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Descriptive Singular Terms

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ABSTRACT: In "Descriptive indexicals and indexical descriptions" Nunberg claimed that only indexicals among singular terms may have descriptive uses, i.e. have non-singular contributions to the propositions they are used to express. In this respect they differ from proper names or definite descriptions. In "Lessons from Descriptive Indexicals", Sæbø shows that this conjecture is untenable, providing examples of the descriptive uses of both proper names and definite descriptions. This paper offers an account of the descriptive uses of all of the singular terms and provides an alternative to Saebø's account. Sæbø's analysis rests on the assumption that the propositional contribution of all singular terms used descriptively is a singular concept. I will argue against this assumption and my proposal will offer a uniform analysis of the descriptive uses of singular terms that is not constrained by it.

KEYWORDS: definite descriptions – descriptive anaphora – descriptive uses – demonstratives – indexicals – individual concepts – proper names – singular terms.

In his seminal work "Indexicality and deixis" (1993), Geoffrey Nunberg introduced the idea that indexicals may have descriptive uses, i.e. non-singular contributions to the propositions they are used to express. In (2004a) he claimed that indexicals in this respect are unlike other

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referential terms, such as names or non-attributive definite descriptions, which do not seem to have such general readings. However, in (2015) Sæbø shows that this conjecture is untenable, providing a plethora of examples of the descriptive uses of both proper names and definite descriptions to bolster his line of argumentation. In this paper I will attempt to support Sæbø's findings and propose a uniform analysis of the descriptive uses of all of the singular terms, providing an alternative to his account. Sæbø's analysis rests on the assumption—shared by many others, such as Elbourne (2005, 2008)—that the propositional contribution of all descriptively used singular terms is an individual concept. In contrast, I will argue against this assumption in section 2.3 and my proposal will offer a uniform analysis of the descriptive uses of singular terms that is not constrained by such an assumption.

The analysis proposed will be based on a generalization of the mechanism of descriptive anaphora which I suggested for the interpretation of the descriptive uses of indexicals in Kijania-Placek (2012, 2015, 2017, 2018). It will not only provide an account of the generation of the propositions expressed in the case of the descriptive uses of singular terms, but will also allow for an explanation of the differing pragmatic availability of such uses between indexicals as well as proper names on one hand and definite descriptions on the other. In the end, I will suggest the consequences of the phenomenon of descriptive uses for the semantics of singular terms.

1. Descriptive uses of indexicals and other singular terms

Indexicals and proper names are usually considered as devices of direct reference (Kaplan 1978, 1989a, 1989b; Perry 1977, 1979, 2012; Recanati 1993; Salmon 1986; Soames 1989; Heim & Kratzer 1998). If definite descriptions are taken to be semantically ambiguous (Wettstein 1981; Devitt 2004), then their referential uses are usually also analyzed as directly referential. This means that such expressions contribute objects to the propositions expressed and, as such, contribute to the expression of singular propositions. However, from Nunberg onwards, the so-called descriptive uses of indexicals have generally been acknowledged whilst indexicals, in their descriptive uses, contribute to the expression

of general propositions (compare Nunberg 1979, 1991, 1992, 1993, 2004a,b; Recanati 1993, 2005; Bezuidenhout 1997; Elbourne 2005, 2008, 2013; Hunter 2010; Stokke 2010; Galery 2008, 2012; Kijania-Placek 2012, 2015, 2017, 2018). A popular example to be found in the literature (Nunberg 1992; Recanati 1993; Jespersen 2012; Kijania-Placek 2011, 2012) is the following:

(1) He is usually an Italian but this time they thought it wise to elect a Pole.
[uttered by someone pointing at John Paul II as he delivers a speech with a Polish accent shortly after his election]

By the use of 'he' in this utterance the speaker is not expressing a contradictory proposition concerning John Paul II that would ascribe the properties of both being an Italian and being a Pole to him, but a general proposition that most popes are Italian.

In general, a proposition might be singular with respect to the contribution of one singular term, while the contribution of another singular term is general, i.e. a distributive property. Thus, the proper name 'Picasso' does not contribute Pablo Picasso to the proposition expressed by:

(2) He donated all his Picassos to MOMA,

while 'he' is used here referentially and contributes a person. Since the contribution of the name is a property of being a painting by Pablo Picasso, the resulting proposition is general with respect to this use of the name.[2] Similar remarks apply to another example of Sæbø's (2015, 1121):

(3) If Mary had been a boy then yes, I do believe England would have remained a Catholic country.

In (3) 'Mary' contributes the property of being a child of Henry VIII by Catherine of Aragon, while 'I' and 'England' have default singular interpretations. (2) and (3) are examples of descriptive uses of proper

[2] For the idea that singularity is a relative feature of propositions, see McKay & Nelson (2014).

names. Other examples have been given by Nunberg (1995), Elbourne (2005), Hunter (2010), Sæbø (2015), Jeshion (2015a,b), Fara (2015a,b) and others. An important category of the descriptive uses of names are the so-called Machiavelli uses (compare Fara 2015b), which are different from (2) and (3) in important respects and will be discussed in section 3.2.1. A representative example is the following:

(4) Dick is a real Einstein,

where 'Dick' has its default referential meaning, but 'Einstein' contributes the property of being an exceptionally intelligent person.

Attributive uses of definite descriptions are by definition descriptive because their propositional contribution is a property depicted by the descriptor of the term. The important question is, however, if definite descriptions have uses in which the propositional contribution is neither the denotation nor the concept which is the descriptor of the term, but a different property altogether. Hereafter I will use the term "descriptive" in a limited sense, i.e. only for uses of definite descriptions which have a general contribution but are not attributive, i.e. whose propositional contribution is a distributive property other than that formed by the descriptor. To exhibit the descriptive uses of definite descriptions thus understood, I will use another example from Sæbø's work. The following utterance should be considered in the context of "a discussion of a snowsled accident where a boy, driving in snowdust and straying from the trail, has hit a tree; the debate is about whether or not the tree was to blame and should be cut down, this discussant arguing that no, the driver was responsible" (Sæbø 2015, 1124):

(5) What if the tree had been a Moose, a deer or another sled, would this still have happened? I believe it would have…

The idea is that since the intended meaning, i.e. 'the entity obstructing the course of the snowsled driven by your son on Pitre Trail Friday', is "costly to express and process" (Sæbø 2015, 1145), it is delivered instead by 'the tree'. In the next section I will briefly discuss the available analysis of the descriptive uses of indexical, proper names and definite descriptions and then propose my own analysis of this phenomenon.

2. Extant analysis of the descriptive uses of singular terms

2.1. Descriptive uses of indexicals

The mechanism of deferred reference was proposed by Nunberg (1993) for the analysis of some uses of indexicals, such as the following:

(6) He is my favorite writer.
[uttered while pointing at a photograph of Nabokov]

The idea stems from Quine's case of deferred ostension where "we point at the gauge, and not the gasoline, to show that there is gasoline" (1968, 195). Its linguistic counterpart is deferred reference, which Nunberg proposed to explain by postulating a distinction between the index and referent of an indexical. According to Nunberg, deferred reference is a two-stage process in which a linguistic expression refers to something in the world by first picking out an element in the expression's context of utterance (an index) and only then referring to (possibly) another element of the context that somehow corresponds to the index. The correspondence is of a pragmatic nature and given by the context. Typically, the referent is an object or property the speaker has in mind, and the index is used to direct the addressee's attention to the referent (Nunberg 1993, 25-26). In the case of (6) the photograph demonstrated is the index, while the person depicted in the photograph—Vladimir Nabokov—is the referent that contributes to the singular proposition expressed. In the same paper, Nunberg claims that the mechanism of deferred reference should be used for the analysis of the descriptive uses of indexicals. We must simply admit that the referent (or, as he sometimes puts it, the interpretation) in deferred reference is either an object or a property (Nunberg 1993, 15, 28-30, 33, 34). The latter case should deliver a general interpretation of the indexical.

The problem with this analysis is that reference to properties does not necessarily result in the generation of general propositions. For example, reference to abstract objects—which is only possible in a deferred way—arguably results in the expression of singular propositions.[3] This means

[3] Examples of deferred ostension to abstract objects by pointing to their exemplifications were given by Quine in (1968). Compare also Kijania-Placek (2012). For criteria of the singularity of a proposition, see Neale (1990).

that the fact that an indexical refers to a property does not by itself provide an analysis of the descriptive uses of indexicals, i.e. uses whose propositional contribution is general. Furthermore, Nunberg holds that deferred reference is characterized by three meaning components (deictic, classificatory and relational) of which the classificatory component concerns the referent and includes features like number and animacy, grammatical and natural gender (Nunberg 1993, 8, 20, 25-26). Since in the case of 'he' the classificatory component includes the requirement that the referent is male, the property of being a pope—not itself being male—is strictly speaking excluded as a potential referent of 'he' as used in (1). We should thus distinguish cases of deferred reference to properties considered as abstract objects from the descriptive uses of indexicals, in the case of which the property itself is not really the *referent* of the term but is still its interpretation. In the latter case, the semantic contribution of the indexical to the general proposition expressed consists in restricting the domain of quantification of a quantifier that constrains the structure of the general proposition.[4] The concept of descriptive anaphora that I propose in section 3 for the analysis of descriptive uses of singular terms may be considered an elaboration of that part of Nunberg's analysis that concerns cases in which the indexical "contributes a property" (Nunberg 1993, 22) in the intended sense. His deictic, classificatory and relational components of meaning would then be limited to the more classic case of deferred reference, when the referent is an object.

I have given detailed arguments against the analysis of the descriptive uses of indexicals proposed by Recanati (1993), Elbourne (2005, 2008), Stokke (2010) and Galery (2008) in Kijania-Placek (2012). Given the absence of space here for such considerations, I will not repeat these arguments as none of the analyses extends to other kinds of singular terms (but see footnote 9 below). The advantage of my analysis is that it captures all kinds of singular terms. To my knowledge, only Sæbø's and Hunter's accounts explicitly concern the whole range of singular terms and I will discuss them in section 2.3 below.

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4 This statement will be slightly amended in section 3.

2.2. Descriptive uses of proper names[5]

Fara (2015a) claims that examples such as (2) and (3) can be analyzed with the help of another of Nunberg's notions: that of meaning transfer.[6] She offers an analogy with other nouns, such as "witch" or "cat" that can sometimes be used to convey transferred meanings of a "person in witch's/cat's costume". The resultant meaning would be available by meaning transfer from the initial meanings of "witch" or "cat" respectively. As Jeshion (2015b) was quick to point out, however, such an analysis of proper names is not available for Fara under her predicativist assumptions. According to predicativists (Burge 1973; Fara 2015a,b; Matushansky 2008; Elbourne 2005, 2008, 2013) "names are predicates in all of their occurrences" (Fara 2015a, 60) and by that they mean "multiply applicable predicates that are true of just those things that are bearers of the name" (Fara 2015b, 251). But while it is quite natural to assume that the meaning of the predicate "person in cat's costume" is a result of an operation on the meaning of "cat", the meaning required for (3)—a "painting by Pablo Picasso"—cannot be obtained from the meaning of "bearer of Picasso" alone, without reference to the relevant referent of the name. Additionally, the process required is not an operation transferring the meaning of one predicate to another—$\langle e,t\rangle \Rightarrow \langle e,t\rangle$—but involves a transfer from an object (Pablo Picasso himself) to a predicate (painting by Pablo Picasso)—$e \Rightarrow \langle e,t\rangle$ (Jeshion 2015b). Jeshion uses the term "coercion" to characterize such a process but does not provide any details as to the exact definition of the process. The analysis proposed below in terms of descriptive anaphora can be considered an elaboration

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[5] Although I am usually careful to use the phrase 'descriptive uses of names', the shortened version "descriptive names" is also used in the literature, in analogy to 'descriptive indexicals'. Yet the kinds of readings discussed in this paper should not be confused with 'descriptive names' in the sense of Evans (1982).

[6] She uses the phrase "descriptive interpretation" but refers the reader to Nunberg (1995) and (2004b), where by descriptive interpretation he means both deferred reference and meaning transfer in the introductions, but devotes both papers exclusively to the analysis of the latter.

of the "coercion" process mentioned by Jeshion.[7] What we need in this case is a relation between Picasso and the distributive set of his paintings, given for example by the property of being a painting by Pablo Picasso. Such a relation will be provided by the mechanisms of descriptive anaphora proposed below.

2.3. Sæbø's and Hunter's uniform analyses of descriptive uses of singular terms

Sæbø's account is based on a postulation of a substitution relation which is to hold between two individual concepts, where "the concept expressed is replaced by another, co-extensional concept (one designating the same individual in the actual world), which is then given a *de dicto* reading" (Sæbø 2015, 1114). Thus, in the case of (5), the concept expressed by the definite description 'the tree' is given the interpretation of 'the entity obstructing the course of the snowsled driven by your son on Pitre Trail Friday' (Sæbø 2015, 1145). I agree that Sæbø's analysis provides correct results for (5). However, it is only applicable to such cases where the replacing concept is an individual one and co-extensional with the original. The following examples, typically considered in the literature as cases of the descriptive uses of indexicals, are thus not analyzable by Sæbø's substitution relation.[8] Let us first consider an example based on Nunberg's (1993):

(7) Today is always the biggest party day of the year.
[uttered on New Year's Eve]

'Today' does not contribute here an individual concept but rather a property of being a day that is a New Year's Eve; any such day. If the term

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[7] In linguistics, coercion is usually associated with the works of Partee (for example 1985) and this reference can indeed be traced back from Jeshion (2015b) through Nunberg (1995, 2004b), to Pusteyovski (1993), to Partee (1985). But her notion of coercion does not include transfers of the e ⇒ <e,t> type (other than the property of being identical with a specific object) that are required here.

[8] It should be noted that in (2015) Sæbø does not analyze quantificational examples, such as (7) below. He does, however, suggest that the account presented there should be considered as a general analysis of descriptive singular terms.

contributed an individual concept, the singularity of the concept would clash with the requirement imposed by the quantifier "always" that requires a range of values to quantify over.[9] The clash would be the same as the original clash between the referential reading of "today" and the requirements of the quantifier that triggered the reinterpretation in the first place. The aim of the reinterpretation was to resolve the clash by supplying an interpretation that would deliver a non-individual interpretation for "today"—a multiply applicable distributive property and thus not an individual concept. In the case of (7) this property restricts the domain of the quantification of the adverb of quantification 'always' (compare Kijania-Placek 2012, 2015, 2017, 2018).[10,11]

Another of Nunberg's examples argues against the requirement of co-extensivity (Nunberg 1990). It is drawn from Peter Weir's movie *The Year of Living Dangerously*. Mel Gibson plays a reporter in Indonesia, Mr. Hamilton, who is looking for arms shipments for local communists and, of course, he would be in trouble if they found out. Hamilton, talking to a warehouse manager and inquiring after the shipments, receives a warning:

(8) – MR. HAMILTON?
BE CAREFUL WHO YOU TALK TO ABOUT THIS MATTER.
I'M NOT P.K.I., BUT I MIGHT HAVE BEEN.[12]

In this example, the replacing concept cannot be co-extensive with the indexical 'I' as the speaker is not a communist and does not warn the warehouse manager against himself but rather against others who are relevantly

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9 See footnote 15 below for a more nuanced characterization of this clash.

10 The assumption of singularity (in the sense of individuality or definiteness, not that of rigidity) of the resulting concept is shared by Recanati (2005) and Elbourne (2005, 2008, 2013), and, I believe, Jespersen (2012). Thus, my criticism of this assumption applies to those accounts as well.

11 A binary structure is standardly postulated for adverbial quantification, regardless of its explicit structure. Thus 'He usually goes on holiday to Italy' would be analyzed as 'Usually, if he goes on holiday, he goes to Italy' (see for example Quine 1941; Lewis 1975, 1986; compare also Kijania-Placek 2012, 2017).

12 'P.K.I.' is an abbreviation for 'Partai Komunis Indonesia'.

similar to himself. Yet, the nature of this warning, its pragmatic force, is only sustained if the interpretation is not (metaphysically) counterfactual, as it was in the case of 'the tree', but concerns the actual situation of the interlocutor, his actual future.[13] These counterexamples are intended to show that Sæbø's account, considered as a uniform analysis of the descriptive uses of all singular terms, is inadequate.

Another account that proposes the uniform treatment of the descriptive uses of singular terms is that of Hunter (2010). Since it is developed in the framework of Discourse Reference Theory, I am unable to discuss it here in any detail for spatial constraints. In a nutshell, her analysis assumes that "One must first determine the standard indexical interpretation […] and then use this interpretation, together with further contextual information, to figure out what is being said" (Hunter 2010, 139). By "standard indexical interpretation" Hunter means a Kaplanian interpretation of an indexical. She thus requires initial saturation of the indexical's contribution, i.e. finding a referent in the context of utterance. This analysis fails for cases such as Schiffer's (1981):[14]

(9) He must be a giant,
[said upon seeing a huge footprint in the sand]

where no intended referent of 'he' is present in the context, so no standard interpretation is available. In what follows I will propose an account of descriptive singular terms that covers examples such as (7), (8) and (9) as well descriptive uses of proper names and definite descriptions.

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[13] For an extensive analysis of this example and especially for an argument against retaining referential interpretation of the indexical under the epistemic interpretation of the modality, compare Kijania-Placek (2012, 2017).

[14] A similar example was also proposed by Loar (1976). Hunter's analysis is originally only intended for quantificational and modal examples of descriptive indexicals. The criticism presented here is thus directed not so much against Hunter's actual analysis but against its generalization to all types of descriptive uses of indexicals.

3. The mechanism of descriptive anaphora

3.1. Descriptive anaphora defined for indexicals

In Kijania-Placek (2012, 2015, 2017, 2018) I proposed an interpretive mechanism which I dubbed 'descriptive anaphora' for the analysis of the descriptive uses of indexicals. Under the descriptive anaphoric mechanism, an indexical expression inherits its semantic value from its antecedent. However, in contrast to classic anaphora, that antecedent stems from an extra-linguistic context: it is an object identified through the linguistic meaning of the pronoun (in the case of pure indexicals) or by demonstration (for demonstratives). The object is used as a pointer to a property corresponding to it in a contextually salient manner and that property contributes to the general proposition. What is important is that the property is not a referent of the pronoun. The structure of a general proposition is determined by a binary quantifier, usually the very quantifier that triggered the mechanism of descriptive anaphora in the first place (see below); the property retrieved from the context serves as a context set that limits the domain of quantification of the quantifier.[15] I will explain the mechanism of descriptive anaphora with the help of (a version of) example (1):

(1a) He is usually an Italian.

Because 'usually' is a quantifier that requires a range of values to quantify over, and because 'he' on its standard interpretation provides just one object, there is a tension in this sentence which triggers the search for an alternative interpretation. The tension is not caused by the fact that John Paul II himself is the standard referent, but it is a tension between the generality of the quantifier and the singularity of the indexical in its default interpretation.[16] The tension would be present, regardless of who the referent was.

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[15] See Kijania-Placek (2012). There I distinguish other triggers for descriptive anaphora, such as the absence of a potential referent in the context (exemplified here by (9)) or pragmatic irrelevance of the referential interpretation (exemplified here by (8)).

[16] In typical cases, descriptive anaphora is triggered by the use of adverbs of quantification in contexts in which they quantify over the same kind of entities that the inde-

Under the descriptive anaphora interpretation, John Paul II is the demonstrated antecedent and his salient property of 'being a pope' is the semantic contribution of this use of the pronoun. 'Usually' is a binary quantifier—$\text{USUALLY}_x(\varphi(x), \psi(x))$—interpreted in accordance with the generalized quantifiers theory (e.g., Barwise & Cooper 1981), which constrains the structure of the general proposition expressed:[17]

$$\text{USUALLY}_x(\text{POPE}(x), \text{ITALIAN}(x)),$$

and USUALLY has the truth conditions of the majority quantifier:[18]

$$M^{gi} \models \text{USUALLY}_x\ (\varphi(x), \psi(x)) \text{ iff } |\varphi^{Mgi} \cap \psi^{Mgi}| > |\varphi^{Mgi} \setminus \psi^{Mgi}|.$$

This analysis gives the intuitive reading for (1): 'Most popes are Italian'.

Other examples of descriptive readings of indexicals are analyzed in a similar way.[19] The problems I posited for Sæbø that concern co-extensivity and the singularity of the resulting interpretation do not arise in the present analysis, because the mechanism of descriptive anaphora gives a property which restricts the domain of quantification and not an individual concept as the semantic contribution of the indexical. As such, the semantic value

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xicals refer to. In such contexts, the generality of the quantifiers clashes with the singularity of the default referential reading of indexicals. Whether there is a clash is, however, a pragmatic matter, as it depends on the domain of quantification of the quantifier, which for most adverbs of quantification is not given as part of the semantics of the word (compare Lewis 1975 and Kijania-Placek 2012, 2015, 2017).

[17] I use the SMALLCAPS font style for formal counterparts of natural language quantifiers and predicates.

[18] In what follows, M is a model, g is an assignment of objects from the domain of the model to individual variables, i is a context, $\models$ is a satisfaction relation obtaining between a sentence (or an open formula) and a model and context, under an assignment; φ and ψ are open formulas, $|A|$ signifies the cardinality of the set A, φ^{Mgi} is the interpretation of formula φ in model M and context i under assignment g, "$\cap$" and "$\setminus$" are the standard set-theoretical operations of intersection and complement (compare Barwise & Cooper 1981 and Peters & Westerståhl 2006).

[19] Some examples require positing covert or implicit quantifiers, see the analysis of (9) and (8).

is not necessarily co-extensional with the referent (i.e. with the individual concept of the referent). The analysis of example (7)

(7) Today is always the biggest party day of the year,

depends on the linguistic meaning of 'today', which delivers the day of utterance. This day serves as the extra-linguistic antecedent pointing to its salient property of being New Year's Eve. This property restricts the domain of quantification for the adverbial quantifier 'always'. As a result the proposition expressed by (7) is 'New Year's Eve (i.e. any New Year's Eve) is always the biggest party day of the year' (compare Kijania-Placek 2012, 2015).

For an adequate interpretation of (8), which I will analyze in the following, more discussed version (Nunberg 1991):

(8a) I might have been a communist,

it was important that the warning concerned the actual situation of the speaker. Yet since the speaker was not himself a communist and was not warning the reporter against himself, the resulting concept could not be co-extensional with the indexical. On the descriptive anaphora account, the speaker serves as the extra-linguistic antecedent of the indexical 'I', but the semantic contribution of the expression is a salient property of this person: being a warehouse manager or just being an Indonesian who is unknown to the reporter. The warehouse manager falls within the extension of the property but, since the property is not an individual concept, is not the only object that satisfies the property. Thus the speaker is able to warn Hamilton not about himself, but of others like himself (compare Kijania-Placek 2012, 2017).[20]

If we consider the example that was problematic for Hunter's account:

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[20] The relevant property supplied by the extra-linguistic context serves the purpose of the context set for the binary existential quantifier which is implicit in this type of modal construction:

MIGHT-HAVE EXISTSx(WAREHOUSE-MANAGER(x), COMMUNIST(x)).

In Kijania-Placek (2012, 2017) I argue for an epistemic interpretation of the modality.

(9) He must be a giant,

it works perfectly well under the descriptive anaphora mechanism, because in the case of demonstratives, the antecedent is not given by the linguistic meaning of those expressions, which is scarce, but by demonstrations. Even though the potential referent is not present in the context, another object—the demonstrated footprint—serves as the extra-linguistic antecedent. This object points to its salient property of being left by somebody. What we get as a semantic value is in fact a property which results from the relation of '*x* is left by *y*' by filling the first argument with the demonstrated footprint. The sentence does not contain an overt quantifier which constrains the structure of the proposition expressed, but in analogy to the use of bare plurals for the expression of a quantified sentences, I postulate a covert binary quantifier for the interpretation of examples like (9).[21] The choice of quantifier depends on the nature of the relation: it is common knowledge that one footprint is usually left by one entity, so the relevant quantifier in this case is the definite description operator.[22] The property generated by the mechanism of descriptive anaphora restricts the domain of the quantifier:

THE$_x$(MALE-WHO-LEFT-THIS-FOOTPRINT(x), GIANT(x)),
– "The man who left this footprint (whoever he is) is a giant."[23]

Thus the mechanism of descriptive anaphora allows for the intuition of the singularity that is present in this case to be accounted for, even though the singularity is not built into the interpretive concept.[24] According to this

[21] For a more extensive discussion of this example and in general of the postulate of covert quantifiers, see Kijania-Placek (2012, 2015). For the postulate of covert quantifiers for the analysis of bare plurals, see Carlson (1977) and Kratzer (1995).

[22] Compare Carlson (1977).

[23] The property of being male is contributed by the features of the pronoun. I leave this aspect out of consideration in this paper but it should be included in a full account. I have also ignored 'must' in this analysis and assumed that it is an evidential (compare Chafe & Nichols 1986 and especially Chafe 1986; see also Kijania-Placek 2012).

[24] "Singularity" in the sense of individuality and not of rigidity.

account, the singularity is a result of combining the concept with a quantifier. In this way the proposed account is flexible and allows for accounting for singularity without making it an intrinsic feature of descriptive uses of singular terms.

3.2. A generalization of the mechanism of descriptive anaphora

The mechanism of descriptive anaphora defined above for indexicals relies on objects present in the context, be they objects delivered by the linguistic meaning of the indexical, or by demonstration. This restriction would not work for proper names as they allow for descriptive uses even if the objects which are their default referents are not present in the context of an utterance. To deploy descriptive anaphora in the analysis of those expressions, we must extend the conception of the mechanism, yet in this respect the required extension is fairly minimal. Already the original description of the mechanism allowed for differences between pure indexical and demonstratives in the way the antecedent was chosen. The differences depend on the nature of the expressions themselves: while pure indexicals—such as 'I', 'here' and 'now'—have a rich linguistic meaning that allows for the identification of the relevant object, for demonstratives—such as 'this', 'that', 'he' or 'she'—the identification is largely dependent on demonstration.[25] The required extension of the concept of descriptive anaphora will thus amount to allowing for more than these two ways—pure linguistic meaning and demonstration—of ascertaining the identity of the object which is to serve the role of the extra-linguistic antecedent, while the nature of the identifying relation will—as in the original case of indexicals—depend on the characteristics of the expressions themselves. Thus, for example, the antecedent of a proper name will be given by the social convention related to a particular use of a name at play in the context of an utterance that connects this use of the name with a particular object—the same convention that gives the default referent for the name when it is used referentially.

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[25] By the claim that pure indexicals have rich linguistic meaning I simply mean that their Kaplanian character is descriptive and in most cases suffices for the identification of the referent in the context of utterance without depending on demonstration.

This extension of the concept of descriptive anaphora permits the analysis of examples such as (2), presented below, but will not suffice to account for all of the descriptive uses of proper names discussed above. This is because (2) is a quantified use of a proper name and in this respect it is analogous to the examples of the descriptive uses of indexicals. However, (4) does not contain a quantifier and no covert quantifier is necessary for the analysis of this use of 'Einstein'. Yet the propositional contribution of the name is still general, making it a case of a descriptive use of a name. As I have already mentioned, however, the quantificational structure of the resulting proposition is either given by the explicit presence of a quantifier in the sentence, like in the case of (7), or is given by a covert quantifier (9). The postulation of a covert quantifier is required only when the sentence itself does not support an alternative structure for the proposition, i.e. when the replacement of an objectual contribution with a property would—to use Frege's terminology—result in an unsaturated semantic structure and thus not a proposition. Thus in the case of (9)

(9) He must be a giant,

since the pronoun's contribution is a property instead of an individual object, the resultant semantic structure would have been a complex property of being somebody who left this footprint and is a giant. This property needs to be inserted in the scope of a quantifier to produce a proposition. Covert quantifiers would thus be postulated when descriptive interpretation is given to a name in an argument position of a sentence. So the quantifier is a product of the structure of the sentence being interpreted and should not be considered an integral part of the mechanism of descriptive anaphora. The generalized definition of descriptive anaphora should thus be the following:

Definition. Descriptive anaphora

- under the descriptive anaphoric mechanism, a singular term inherits its semantic value from its antecedent;
- that antecedent stems from an extra-linguistic context and is an object identified through that aspect of the linguistic meaning of the term, which is crucial for the identification of the term's referent in its default singular uses, i.e. either:

  - by the Kaplanian character of the term (for pure indexicals), or
  - by demonstration (for demonstratives), or
  - by a social convention operative in the context of utterance that links that use of name to its default referent, or
  - by the linguistic meaning of a definite description supplemented by contextual clues (for definite descriptions);[26]
- the object is used as a pointer to a property corresponding to it in a contextually salient manner;
- that property contributes to the proposition expressed but is not a referent of the singular term;
- the property retrieved from the context either serves as a context set that limits the domain of quantification of the quantifier that constrains the structure of the general proposition expressed or constitutes a predicative part of a singular proposition.

Now I will turn to a demonstration of the mechanism of descriptive anaphora as applied to proper names (section 3.2.1) and to definite descriptions (section 3.2.2).

3.2.1 Descriptive anaphora applied to the analysis of proper names

In example (2)

(2) He donated all his Picassos to MOMA,

the name 'Picasso', in its plural form, is used descriptively. Via the descriptive anaphora mechanism, the extra-linguistic antecedent of this use of the name is the person Pablo Picasso and the semantic contribution of the name is the property that results from a salient relation of '*x* is a painting by *y*' with the second argument filled by Picasso himself. This property restricts

[26] In this paper I ignore referential uses of definite descriptions which are cases of misdescription (compare Donnellan 1966). They would probably be amenable to the treatment analogous to that of demonstratives, but this issue requires further research.

the domain of quantification of the binary universal quantifier 'all', resulting in the following proposition ('he' and 'him' are assumed to be directly referential and 'A' stands for the person the pronoun refers to in the context; I disregard the tense in this analysis):

ALL$_x$(PAINTING-BY-PABLO-PICASSO-OWNED-BY-A(x), DONATED-BY-A-TO-MOMA(x)),
– 'He donated all his paintings by Pablo Picasso to MOMA'.

In the analysis of example (3) I will concentrate only on the first embedded sentence. The counterfactual form of the whole utterance requires the consideration of possible worlds in which the following is true:

(3a) Mary is a boy,

while other elements of the context of utterance are fixed, i.e. it is clear from the context who 'Mary' refers to on its default referential and undeferred reading. The mechanism of descriptive anaphora applied to (3a) gives Mary I of England as the antecedent; her salient property of being a child of Henry VIII by Catherine of Aragon is the propositional contribution of the name. Since the name is in the subject position of (3a), the structure of the proposition expressed must be given by a covert quantifier—a definite description:

THE$_x$(CHILD-OF-HENRY-VIII-BY-CATHERINE-OF-ARAGON, BOY(x)),

resulting in the intuitive interpretation of (3): 'If the child of Henry VIII by Catherine of Aragon had been a boy then yes, I do believe England would have remained a Catholic country'.

The analysis of example (4)

(4) Dick is a real Einstein,

does not result in a general proposition (its structure is not quantificational), but the propositional contribution of 'Einstein' is still general (a distributive property). The mechanism of descriptive anaphora gives Albert Einstein as the extra-linguistic antecedent and his salient property of

being an exceptionally intelligent person as a propositional contribution of the name. The structure of (4) does not require a postulation of a covert quantifier and thus the property's contribution remains simply predicative in nature, resulting in the expression of 'Dick is an exceptionally intelligent person'.

3.2.2 Descriptive anaphora applied to the analysis of definite descriptions

I will now return to example (5)

(5) What if the tree had been a Moose, a deer or another sled, would this still have happened? I believe it would have…'

to show how the mechanism of descriptive anaphora works for the descriptive uses of definite descriptions. To repeat, descriptions contribute properties to the propositions expressed in descriptive uses which are distinct from the properties contained in their descriptor parts. Analogously to (3), (5) is a counterfactual claim that invites the hearer to consider a possible world in which (5a) is true:

(5a) The tree is a Moose, a deer or another sled.

Neither a referential nor attributive reading of 'the tree' in (5a) gives a consistent interpretation of (5), because if something is a tree, it cannot simultaneously be a moose, a deer or a sled in either the actual or in a possible world. The required interpretation, as Sæbø correctly noted, requires replacing the concept of a tree that is given by the original expression with another concept somehow related to it. Such a connection is provided by the mechanism of descriptive anaphora. The incomplete definite description, 'the tree' supplemented by contextual clues, gives its actual referent—the actual tree talked about—as the extra-linguistic antecedent, whose salient property of being an entity obstructing the course of the snowsled driven by B on Pitre Trail on Friday' ('B' stands for the accused) is the propositional contribution of (5) and restricts the domain of quantification of the explicitly given quantifier 'the':

THE$_x$(ENTITY-OBSTRUCTING-THE-COURSE-OF-THE-SNOWSLED-DRIVEN-BY-B-ON-PITRE-TRAIL-ON-FRIDAY(x), (MOOSE(x) ∨ DEER(x) ∨ ANOTHER SLED(x)).

As a result, we obtain the following interpretation of the utterance: 'What if the entity obstructing the course of the snowsled driven by B on Pitre Trail on Friday had been a Moose, a deer or another sled, would this still have happened? I believe it would have…'

I believe that (5) and the other examples of descriptive uses of definite descriptions given by Sæbø (2015) and Hunter (2010) do contradict Nunberg's statement that definite descriptions do not allow for descriptive interpretations (2004a, 278).[27] Yet, I still believe that such uses of definite descriptions are more difficult to come by than descriptive uses of names or indexicals. The mechanism of descriptive anaphora allows the observation to be explained in pragmatic terms. When a sentence containing a definite description is uttered, the sheer fact of its utterance makes the descriptive content of the description salient. Despite the fact that the content does not enter the proposition expressed, it diminishes the salience of other properties and influences pragmatic mechanisms, such as descriptive anaphora, that might generate other general semantic values for the expression. The salience may be contextually overridden, but that requires more contextual setting as example (5) shows.

4. Conclusion: The consequences of the phenomenon of descriptive uses for the semantics of singular terms

In this paper I have proposed a uniform interpretation of the descriptive uses of all singular terms, i.e. indexicals, proper names and definite descriptions. To do so I provided a generalization of the mechanism of descriptive anaphora which I have previously introduced for the analysis of descriptive uses of indexicals in Kijania-Placek (2010, 2012, 2015, 2017, 2018). The generalized mechanism of descriptive anaphora relies on an extra-linguistic antecedent, which is an object given by the default semantic

[27] I have previously supported Nunberg's claim in Kijania-Placek (2010), but now find the arguments presented there unconvincing.

mechanism characteristic of the expression in question. This object points to a salient property and this property forms the propositional contribution of the respective term. Depending on the syntactic position of the analyzed expression in the sentence, the property either restricts the domain of quantification of a quantifier or contributes to the interpretation of the predicative part of the sentence. The resulting concept is not an individual concept, but the definiteness is in some cases contributed by the covert or overt definite description quantifier, whose first argument is given by the property obtained by the mechanism of descriptive anaphora. This allows for a uniform treatment of examples calling for individual concepts and those for which the assumption of individuality built into the interpretive concept itself does not give adequate interpretations.

Descriptive uses of singular terms are not the default ones and the process of a descriptive interpretation is triggered by the failures of other interpretations. What is more, the mechanism of descriptive anaphora is semantically dependent on the presumptive referents. Thus the different uses of singular terms, i.e. their referential and descriptive uses, are interrelated in ways that seem to exclude treating them as homonymous.

The fact that the phenomenon of descriptive interpretation is not limited to indexicals but is available for all singular terms seems to require that these kinds of uses were predicted by the semantics of indexicals, proper names as well as definite descriptions. If we assume, as I think we are supposed to, that "to know the meaning of a term is to know the sorts of semantic contribution that the term can make to a larger context, and to have a general understanding of what sorts of context are those in which it will make this or that sort of contribution" (Dancy 2004, 196), we should aim at a semantic analysis of singular terms that would accommodate descriptive uses. Such an analysis would not, however, necessarily lead to a unified account of all singular terms. The semantic differences between indexicals, proper names and definite descriptions should not be sought in admitting descriptive readings for one kind while denying it for others—a move which seems to have been falsified by linguistic data—but rather in what other readings are available to those expressions. For example, while indexicals have at least deictic, (classically) anaphoric, bound, deferred and descriptive readings, they seem to lack (systematic) predicative readings that are available for names, in which a

proper name N contributes the property of being a person called N to the proposition expressed.[28] Although such uses share important characteristics with descriptive uses of indexicals and the descriptive uses of proper names discussed in this paper, they are different in that the generation of the property is not context dependent in the sense that it does not rely on the properties of the default referent of the name that are salient in the context of the utterance.

The phenomenon of the descriptive uses of singular terms appears to be of a cross-linguistic character and is characterized by intra-linguistic productivity and systematicity. The same is true of other kinds of uses of singular terms like the referential and attributive uses of definite descriptions or the referential, predicative, bound or deferred uses of proper names. It follows that the semantics of singular terms seems to call for a systematically polysemous treatment that would encompass all kinds of uses that are characteristic of a certain class of expressions and would postulate mechanisms that underline those uses.[29] Although the details of such treatment lies beyond the scope of this paper and requires further research, the mechanism of descriptive anaphora proposed in this paper may be considered as a contribution to semantic analysis thus understood.

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[28] Compare Burge (1973), Fara (2015a,b) or Matushansky (2008) for the general idea, although they phrase the relevant property in a slightly different manner. Relevant examples include 'There are three Antonio's in my class'. See also Kijania-Placek (2018).

[29] A polysemous treatment of referential and predicative uses of proper names was proposed by Leckie (2013). For an attempt at a polysemous treatment of all uses of proper names, see Kijania-Placek (2018).

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The Fallacy of Naturalism as a Response to the Relativist

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ABSTRACT: This article is a response to Howard Sankey's (2010) 'Witchcraft, Relativism and the Problem of the Criterion.' It seeks to refute two central arguments that are brought forward by Sankey. First, that the relativist is skeptic about norm-justification and second, that naturalism could serve as a response to the relativist. I will demonstrate, by the use of historical cases, that epistemic norms cannot be subjected to empirical evaluation without using the very norms that are the target of analysis. Finally, I will reject the assertion that the conclusion of my critique implies a form of equal validity.

KEYWORDS: Epistemic norms – equal validity – naturalism – objectivity – relativism.

1. Sankey's argument

In his article 'Witchcraft, Relativism and the Problem of the Criterion,' Sankey (2010) aims at a naturalist response to relativism. In doing so, he structures his argument into three parts. First, Sankey makes an argument for epistemic relativism by using the skeptic's strategy, arguing that, whilst "relativism and skepticism pull in opposite directions" (Sankey 2010, 4),

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they agree that "there is no such thing as knowledge or rational justification *in any* objective sense" (Sankey 2010, 4; emphasis added). To be more specific, Sankey argues that the relativist can employ the skeptical problem of the criterion, i.e. that "the attempt to justify the criterion leads either to an infinite regress, circularity or unjustified adoption of the criterion" (Sankey 2010, 5). Therefore, "Justification is an entirely internal matter of compliance with norms that are operative within a belief system" (Sankey 2010, 6). Secondly, to avoid the skeptical conclusion, Sankey uses Chisholm's (1989) particularist approach in which the "claim to knowledge is grounded in particular instances of knowledge which are established before one undertakes the independent task of formulating criteria" (Sankey 2010, 8) or epistemic norms which are employed to justify a belief. Through this move Sankey does not even allow the skeptic to get a foot in the door since, in contrast to a methodist approach starting with criteria for knowledge and epistemic justification, his particularist stance already starts with particular, uncontroversial cases of knowledge and only then seeks to identify criteria. Thirdly, Sankey argues that empirical evaluation can serve as a touchstone against which these epistemic norms can be tested.

2. The objectivities of knowledge and judgment for the relativist

To start with, I deem it necessary to emphasize Sankey's initial argument that the relativist, like the skeptic, denies that there is rational justification or knowledge "*in any* objective sense" (Sankey 2010, 4; emphasis added). In what follows, I will draw upon Lorraine Daston and Peter Galison's (1992; 2007; also Galison 1998) historical study on the notion of objectivity—an epistemic norm[2]—to demonstrate that the relativist can suggest that there is rational justification or knowledge in an objective sense, namely a relativistic one. On this view, the relativist/absolutist discussion must not be confused with a discussion about subjectivism and objectivism.

[2] I will sometimes also refer to epistemic virtues. For the sake of this article those two terms—epistemic virtues and epistemic norms—can be taken synonymously. This is also in line with Daton and Galison (2007, 40), who argue, "Epistemic virtues are virtues properly so-called: they are norms that are internalized and enforced."

"It is no good rejecting relativism merely on the grounds that one believes in the 'objectivity' of knowledge. Relativists can, and do, believe in the objectivity of knowledge. It is only when a case is made for absolute objectivity that relativism is challenged" (Bloor 2007, 256). In what follows, the use of Daston and Galison's illuminating study serves to highlight both the indispensability of the notion of objectivity for the relativist as well as its relativistic character. (I am well aware that Daston and Galison would object to my interpretation of their investigation since they strictly demarcate historicism from relativism; I will reject this demarcation below.)

In their work, Daston and Galison differentiate between three historical phases—the pre-19th, the 19th, and the 20th century—and illustrate the transformations and tensions of objective judgment within and between these periods. In particular, they focus on the question of how objectivity has been practiced, employed, and mobilized, or, in other words, how the epistemic norm of objectivity has been entangled with Western scientific and philosophical practice and thought. The pre-19th-century regime focused on the concept of truth-to-nature. A supposed genius, who was able to "extract a form more perfect than the best objects we find this side of our sensory limits" (Galison 1998, 352), seeks to show the hidden, true, metaphysical image of nature. The 19th century was characterized by a shift towards mechanistic objectivism. Scientists should refrain from any form of interpretation, speculation, and philosophical commitment. The task of a scientist was mechanistic, comparable to a machine. This period was characterized by "the modernity of manufacture, the dynamics of control, and scientific labor management" (Galison 1998, 352) seeking to create a mechanical image rather than a metaphysical one. The 20th century marked a radical shift, which was characterized by two new challengers to mechanical objectivity: structural objectivity and trained judgment. Proponents of the former (such as Frege, Carnap, Poincaré, or Russell) identified objectivity as abstract structures—structures that survive and are unaffected by "translation, transmission, theory change, and differences among thinking beings due to physiology, psychology, history, culture, language, and (as in Planck's fantasy) species" (Daston & Galison 2007, 256). The latter—trained judgment—emphasized that the idea to picture nature "as it is" and, thus, let her speak for herself, was no longer desired. Rather, trained experts created interpreted images through a process of judgment (based on training, familiarity, and experience). In contrast to the pre-19th

century, this image did not seek to display the true workings behind an observable nature, but rather to facilitate teaching, communication, and the summarizing of knowledge through exaggeration.

Without discussing these different knowledge regimes in any detail, what I take to be a crucial insight of this historical study of objectivity is that knowledge *must* always be objective to be recognized as such. Knowledge, in other words, implies objectivity. Neither would an expert, regardless how well-trained, be acknowledged to possess knowledge about nature in the 19th century when intentionally manipulating her empirical observations through exaggeration or any other form of interpretation; nor would a person, regardless how genius-like she might present herself, be acknowledged to possess scientific knowledge about nature in the 20th century without having undergone a pre-defined educational or training process. Thus, objectivity can be considered an epistemic norm that promotes epistemic goals such as truth and knowledge. This norm, however, is always produced and practiced within a particular belief-value system[3] (e.g. in the examples above, the pre-19th, the 19th, and the 20th century systems). In other words, *objective* justification—i.e. the compliance with this collectively and socially accepted epistemic norm—is inevitable for knowledge to be accepted as such within a belief-value community. Objectivity, however, is itself variable and relative to this belief-value system and its desires, fears, technological developments, self-conceptions, philosophical positions, and so forth. In this sense, "scientific theories, methods and acceptable results are social conventions (...), that is, the product of

[3] I use the term belief-value system, as opposed to the often-used notion of epistemic system, to highlight that epistemic concerns are highly interlinked with non-epistemic ones and cannot be strictly demarcated from one another. I doing so, I follow Kinzel and Kusch (2017, 18ff), who discuss Steven Shapin and Simon Schaffer's 'Leviathan and the Air-Pump' (1985), which deals with the conflict between Robert Boyle and Thomas Hobbes. They stress that the controversy between Hobbes and Boyle concerned a range of inextricably linked questions including metaphysics, engineering, credibility, epistemology, aims, politics, and topics. In this way, Kinzel and Kusch seek to highlight that one cannot easily (or at all) isolate questions of epistemology from other concerns and dimensions. Hence, different systems—in which the epistemic norms of 'truth-to-nature', 'mechanistic objectivism', 'structural objectivity', or 'trained judgment' are produced and practiced—should be understood as coherent belief-value systems (as opposed to mere epistemic systems).

collective influences and resources and as peculiar to the culture and its present circumstances" (Bloor 1991, 43-44). 'Being justified' can therefore be understood dialectically; it is a "social status granted by others to the believer" (Kusch 2010, 878).

3. Remarks on Daston and Galison's *Objectivity*

To avoid being accused of putting my own words in Daston and Galison's mouth and giving the impression that they would docilely agree with my relativistic interpretation of their historical investigation, I deem it essential to comment on (one of) their expected objections. Both of them are eager to stress the non-relativistic nature of their historicist study of objectivity. In this context, they argue that "it is a misconception, albeit an entrenched one, that historicism and relativism stride hand in hand, that to reveal that an idea or value has a history is ipso facto to debunk it" (Daston & Galison 2007, 376). In the same vein, they continue, "far from relativizing these virtues, history exhibits their rationale. (...) Truth-to-nature, mechanical objectivity, and trained judgement all combat genuine dangers to knowledge" (Daston & Galison 2007, 376-377). These arguments, however, cannot be granted meekly. In his review on Daston and Galison's (2007) book *Objectivity*, Kusch (2009, 130) puts the main objections to their non-relativistic historicism straight:

> First, relativism is not skepticism; relativism does not equal "debunking." Second, to declare all epistemic virtues equally justified in light of timeless "dangers to knowledge" is to revoke the previous insistence on the historicity of knowledge. And third, and perhaps most importantly, Daston and Galison's quick way with relativism is ahistorical. Only a century ago, and thus very much in the period at issue in Objectivity, the relationship between historicism and relativism was extensively discussed among the likes of Dilthey, Heidegger, Husserl, Nietzsche, Rickert, Simmel, and Windelband. Whatever emerged from this eventually abandoned debate, it certainly included the insight that the historicist can avoid relativism only by either positing a telos of historical development or treating the views of different periods as components of one overall truth. Neither option now seems particularly attractive.

Kusch's third argument deserves further comments. The restricted scope of this article, however, only allows me to briefly sketch the cornerstones of an argument that seeks to reject models of scientific development that posit an overall truth or telos. In doing so, I will reproduce a model brought forward by David Bloor (2007).[4]

Imagine a line AB—A on the left and B on the right end of the line. AB represents knowledge. Add the point C somewhere in the middle. Now, A stands for a prior state of belief and C illustrates where we stand today. The question is obvious: what about B? The first option is to interpret B as absolute truth about reality. Thus, when we improve the accuracy of our theories or reject a hypothesis as false, C approaches B. However, since AB represents knowledge, i.e. beliefs about reality, reality itself can, in fact, not depict B. The line may presuppose reality, but it cannot express it.

> Treating reality as if it could be the end point of the line AB amounts to confusing reality with some theory or assertion about reality. But if the point B registers some theory of reality, then the interpretation of the diagram as a picture of progress is rendered circular. To say we are "getting closer" to reality depends on the tacit assumption that we already know the truth about reality, otherwise we could not enter it onto the diagram. "Getting closer" to B can only describe a process by which one piece of (assumed) knowledge gets closer to another piece of (assumed) knowledge, or one of our beliefs is brought closer to another of our beliefs. Equating B with "reality" is therefore, at best, a way of celebrating (and presupposing) the claims of the current state of understanding. (Bloor 2007, 266)

So, let's try another option. Instead of depicting the line AB of finite length, we can imagine it as being of infinite length. Hence, we can approach truth without running into the troubles of implicitly asserting to already know the truth about reality. However, also this attempt fails since ultimately it makes no sense and would not allow for any progress at all, because "an infinite quantity minus a finite quantity is still infinite" (Bloor 2007, 264). Therefore, instead of considering historical development or progress of knowledge as a progression towards a telos or one

[4] For more detailed discussions on this matter see Bloor (2007) or Kusch (2004).

overall truth, the more viable option is to regard it as "a move *away* from past problems and frustrations, not a move *toward* an unknown terminus" (Bloor 2007, 266). The conception of development or progress as heading towards an overall goal or foregone conclusion revives theological positions such as those of the religious detractors of Darwin's evolution theory, who could not accept it without construing something like a "biological heaven," an ultimate telos (Bloor 2007, 267). "Without realizing what they are doing, today's antirelativists are replaying this old scenario" (Bloor 2007, 267).

The 'move *away* from past problems and frustrations' is evident in Daston and Galison's study, for they demonstrate how the epistemic norm of objectivity and its transformations or transitions are rooted in historical fears. They also show, however, that overcoming fears produces new ones and that gaining knowledge is always tantamount with the production of new ignorance, "unknown unknowns" (Beck 2009), or "unrecognized ignorance" (Merton 1987)—conceptions that stands in stark contrast with the historicist idea of progress as heading towards an overall truth. The often-used metaphor of the light cone exemplifies this understanding of progress. The light cone itself demonstrates knowledge, whilst its border constitutes the unknown. As Albert Einstein put it, "as our circle of knowledge expands, so does the circumference of darkness surrounding it."

The pre-19th-century move away from the Enlightenment's notion of the self as passive, fragmented, and receptive resulted in an active interpretation and selection of sensations (i.e. truth-to-nature) "to bring them under epistemic control" (Kusch 2009, 127). Consequently, this constituted, amongst others, a starting point for a new epistemic fear in the 19th century, namely the danger of excessive interpretation. Mechanistic objectivity was a means to contain this danger. As a result, the rise of objectivity practiced as trained judgment in the 20th century included the claim "that mechanical objectivity had gone too far in excluding the scientific subject" (Kusch 2009, 128). To put the argument above straight, whilst overcoming past problems and fears, each transition and transformation of the epistemic norm of objectivity has also inherently brought about new forms of ignorance and epistemic fears (accompanied by new technologies, topics, politics, metaphysical assumptions, etc.) that have been unknown before. This argument *inter alia* draws into question the historicist idea of treating the different periods as components of one overall truth as well as the positing

of a telos and, therefore, also challenges Daston and Galison's demarcation of historicism from relativism.

4. The problem of the criterion and the fallacious naturalist response to the relativist

I will briefly restate what has been said so far. Objectivity is a socially and collectively accepted epistemic norm that always evolves in a particular belief-value system and can only be understood against the background of this system (which cannot be reduced to an epistemic system, but involves a range of inextricably linked questions including metaphysics, technologies, credibility, epistemology, aims, politics, topics, period-specific fears, self-conceptions, etc.). I have rejected the idea that the transformation or transition of these systems heads towards an overall goal—rather they move away from past problems and, in doing so, will encounter new ones (many of them created through the very transition). In doing so, I sought to highlight that historicism cannot avoid relativism. Hence, different belief-value systems (such as the pre-19th, the 19th, and the 20th century systems) produce different forms of objectivity (e.g. truth-to-nature, mechanistic objectivism, structural objectivity, or trained judgment). This understanding, however, does not diminish the central role of objectivity. Belief is only socially accepted as knowledge *iff* it entails objective justification, i.e. *iff* the believer complies with the collectively accepted social norm of objectivity. Hence, for the relativist, in contrast to what Sankey suggests, there is such a thing as knowledge or rational justification in an objective sense. The relativist only rejects the equation of objectivity and absolutism.

What does this tell us about Sankey's argument about the problem of the criterion? Indeed, not much new. Justification is, in this sense, still "an entirely internal matter of compliance with norms that are operative within a belief system" (Sankey 2010, 6) and, therefore, attempts to justify the criterion will sooner or later run into "an infinite regress, circularity or unjustified adoption of the criterion" (Sankey 2010, 5). Apart from the last aspect—the unjustified adoption of the criterion—I still agree with Sankey. To address this aspect, however, I first have to build up my argument. Therefore, for now, I will focus on Sankey's next *three steps*, which will

get us apart, and address the issue of justification subsequently towards the end of this section.

First, Sankey applies Chisholm's particularist methodical approach starting with "particular cases of knowledge that we possess in order to identify suitable criteria" (Sankey 2010, 6-7). *Secondly*, he argues,

> It is possible to combine a particularist stance with the naturalist view that epistemic norms are subject to empirical evaluation (...) When we proceed in this manner, we employ empirical knowledge which we obtain by means of experience as a touchstone against which epistemic norms may be tested. (Sankey 2010, 8-9)

Finally, he concludes that such a naturalist approach "enables a distinction to be made between epistemic norms for which there is an objective, rational justification, and those for which there is no such justification" (Sankey 2010, 9). In what follows, I will elaborate on these three steps one by one.

Let me start with the particularist methodology and remain with Sankey's example of the Azande's Poison Oracle.[5] The Azande, an African tribe of the Sudan that was studied in detail by Evans-Pritchard (1937), attribute various misfortunes in their daily-life to witchcraft. Additionally, they "employ a number of techniques to determine the action of unseen forces. One technique, which Evans-Pritchard calls the 'poison oracle,' is used to answer a broad range of questions not limited to witchcraft" (Sankey 2010, 2). Sankey argues that these questions include many situations in which "empirical matters of fact are of clear relevance to the question of whether the oracle is able to serve as a reliable guide to the truth" (Sankey 2010, 10). Let the following sentence be such a clear—and empirically verifiable—instance of knowledge: '*It was raining during the night*.' Let us

[5] As a side note, I would like to remark that, whilst representing a standard case in the literature, the Azande's Poison Oracle does not constitute an ideal example for epistemological questions, for it seems at least questionable whether the Oracle has *any* epistemic function at all. Recent analyses (e.g. Leeson 2014) rather suggest that the Oracle serves as a means for social cohesion. Nevertheless, in what follows, I take—for the sake of argument—the Oracle to have an epistemic function to establish a fruitful argument against Sankey.

further assume that the Azande's Poison Oracle states, '*It was not raining during the night*.'

Sankey's next step is to combine particularism with naturalism and to consequently subject the concrete instance to empirical investigation, which seeks to promote epistemic goals such as knowledge and truth. As has been argued above, knowledge, to be accepted as such, entails objective justification, but—and I am stressing this again—objectivity is itself relative to a belief-value system. This means that the result of an empirical test varies in accordance with the practice of the epistemic norm of objectivity (practiced within a belief-value system) as the latter always implicates specific methods. To test empirically whether the Poison Oracle's statement '*It was not raining during the night*' is objectively justified one could, for example, put a beaker outside and, if there is water inside in the morning, conclude that it was raining during the night. Thus, in this empirical evaluation, the conclusion that it was raining is based on a mechanistic measuring of the beaker. However, the same situation could be evaluated differently when being subjected to an empirical investigation that relies on an interpretative form of objectivity. In this case, it could be argued that putting a beaker outside and measuring it in the morning is not enough. The water could also have originated from morning dew or any other influences. In this case, to objectively justify whether it was raining, a trained expert would have to interpret the condition of the water found in the beaker using weather charts and other forms of interpretative means. Even if the empirical investigation based on interpretative objectivity would yield the same result, namely that it was raining, an empirical investigation based on an objective notion of truth-to-nature could still produce different outcomes. The true, metaphysical picture of rain would have to be exposed by a "genius" (e.g. a shaman, witch doctor, etc.). This conception is particularly relevant in the context of the Azande and their complex understanding of witchcraft including scenarios in which it was not the rain, but witches who could have filled the beaker or have made the soil wet.

The question then is: How could we test epistemic practices like the Poison Oracle or the measurement of the beaker, which are said to promote epistemic goals such as truth and knowledge and therefore constitute diverging epistemic norms, empirically without using diverging epistemic norms (e.g. objectivity based on truth-to-nature versus a mechanistic understanding of objectivity)? At first sight, this only seems to be an option

if we consider empirical evaluation universal, supra-historical, or absolute. I will not comment on this flawed comprehension of empirical evaluation since even Sankey explicitly rejects such a view and emphasizes that, because of this rejection, he prefers the 'objective/relative-dichotomy' rather than the 'absolute/relative-dichotomy.' It is worth quoting Sankey (2013, 143) at length here to understand his view on that matter:

> When we speak about absolute epistemic standards, or about standards being absolutely justified, this appears to suggest that epistemic standards are invariant or universally applicable. (...) It is difficult to reconcile such a view with the idea that epistemic norms may be subject to variation across historical and cultural context, as well as intellectual discipline. If we allow that methodological rules undergo variation in the history of science, or that standards of rationality may vary between cultures, this conflicts with the claim that epistemic norms are absolute in the sense of being invariant or applicable in all contexts. But no such problem arises if we work instead with a notion of objectivity. For while we may allow that there is variation with respect to the norms that are actually employed in different contexts, this does not require us to allow that all norms are objectively correct. Different norms may be employed in different periods in the history of science, or in different cultures. But some norms may be objectively better than others. (...) In short, my reason for preferring the idiom of objectivity is that it allows for the variation of norms. It enables us to say that different norms may be employed in different contexts, though not all norms employed in all contexts are equally justified.

Epistemic standards undergo variations across historical and cultural contexts, as well as intellectual disciplines and the notion of supra-historical or absolute norms must be rejected. No objections in this respect. But how, as Sankey (2013, 143) suggests, to assess whether "some norms may be objectively better than others"? The only way to do so is to evaluate norms via norms or objectivity via objectivity. Put differently, as I have tried to illustrate above, empirical knowledge is always obtained via the compliance with a particular epistemic norm of objectivity (e.g. empirical knowledge about the question whether it rained during the night already *entails*, in its method of investigation, a particular epistemic

norm, and thus empirical testing of such norms implies the employment of a norm in this process of testing). Hence, to test a norm via empirical evaluation, as Sankey recommends, implies that the empirical investigator already uses a particular norm. As a result, whatever may be demonstrated regarding an epistemic norm like objectivity, it can only be shown against the background of a particular belief-value system. As Rorty (2009, 9-10) puts it,

> The common message of Wittgenstein, Dewey, and Heidegger is a historicist one. Each of the three reminds us that the investigations of the foundations of knowledge or morality or language or society may simply be apologetics, attempts to eternalize a certain contemporary language-game, social practice or self-image.

To sum up, in contrast to Sankey's understanding of empirical evaluation, I have tried to show that to address the empirical question of whether the Poison Oracle provides objectively justified merits one inherently uses a relative form of objective justification. Put differently, the attempt to evaluate epistemic norms is itself based on the practice of epistemic norms and those, as has been argued, are relative. The empirical evaluation-efforts are therefore always relative to the empirical methods, which vary in accordance with the practice of the relative epistemic norm of objectivity. To conclude what has been said so far, I tried to argue that, contrary to Sankey's argument, a naturalist approach does not constitute a position that could depict the Azande as being unjustified in their beliefs, as an empirical evaluation that seeks to justify an epistemic norm objectively is itself always based on a relative conception of objective justification. Crucially, however, Sankey's (2010, 9) conclusion that a naturalist approach "enables a distinction to be made between epistemic norms for which there is an objective, rational justification, and those for which there is no such justification" must not right away be rejected. The same goes for his remark that the "variation with respect to the norms that are actually employed in different contexts (...) does not require us to allow that all norms are objectively correct" (Sankey 2013, 143). However, and this is the decisive point, we must add that, even if epistemic norms are not arbitrary (because not all knowledge about nature or society will prove socially credible as well as practically utile) fundamentally contradictory epistemic norms can exist.

Empirical evaluation (and justification) will always draw upon these norms and is therefore itself relative.

5. Consequences and equal validity

What are the consequences of the argument that we could not prove the Azande unjustified in their belief through empirical investigation (i.e. through the naturalist approach that Sankey suggests)? Do we have to accept epistemic norms such as the Poison Oracle as being equally valid to, let's say, weather charts or meteorological predictions? The answer to that question is an unequivocal no. I will not grant the Poison Oracle such as status. In this section I will, non-exhaustively, sketch an argument for my rejection of the Azande's Poison Oracle.

To begin with, I cannot grant Sankey's argument that the relativist has to regard epistemic norms (and thus epistemic justification) as merely arbitrary. According to Sankey, the skeptic is skeptical about both norm and belief justification, whereas the relativist is only skeptical regarding norm justification and differs from the skeptic in the sense that she holds on to the idea of belief justification.[6] Sankey therefore construes a position of a relativist who considers all norms equally good or bad, but who can still operate with the norms to justify beliefs. This construal of the relativist position, however, is fundamentally misleading. First, as I have argued, not all norms will prove socially credible in their local contexts even if contradictory norms can exist. As Bloor (1991, 43) puts it, not "anything can be made a convention. And arbitrary decision play little role in social life. The constraints on what may become a convention, or a norm, or an institution, are social credibility and practical utility." In this sense, and in contrast to

[6] Hence, belief justification does not imply norm justification. The argument that the relativist is *only* skeptical about the latter hints at the (correct) idea that she, in contrast to the skeptic, acknowledges that we *can* attain knowledge since we can justify our beliefs by using particular norms. At the same time, Sankey holds the (flawed) idea that the relativist arbitrarily picks any epistemic norm—since, according to him, she considers all of them equally good or bad—to justify a belief. Hence, following Sankey, the relativist can justify beliefs (and thus is not skeptical about belief justification and can attain knowledge) via the use of epistemic norms, which are themselves unjustifiable (thus, she is said to be skeptical about norm justification).

Sankey's understanding, the relativist is not skeptical with regards to norm justification. Norms can be justified, not in an absolute way, but in light of local causes of credibility.

Second, and perhaps most importantly, Sankey's argument only holds if the relativist accepts that all epistemic norms are equally good or bad, i.e. the claim of equal validity. However, cart-carrying relativists such as Bloor, Wittgenstein or Feyerabend have vigorously rejected the idea of equal validity. Whilst not denying that equal validity may be accepted in rare cases, such as beauty or taste, it cannot be formulated as an indispensable condition for relativism. Hence, even if objective, rational justification exists for two contradictory norms, it does not follow that they must be considered equally valid. The relativist can, and should, reject particular epistemic norms. This rejection, however, can never be based on anything absolute, but will always be made against the background of a particular belief-value system and will have pragmatic roots. In what follows, I will roughly outline such a point of view.

At this stage, it is fruitful to remind ourselves of the idea that holding knowledge is closely bound to the practice of specific epistemic norms or virtues. As has been argued, however, these epistemic norms are always produced within a particular belief-value system, which inextricably links epistemic and non-epistemic concerns. Let us briefly consider the 19th century belief-value system,[7] which has been discussed by Daston and Galison. Here, the epistemic norm of mechanical objectivity evolved within a complex network of epistemic (e.g. fears of excessive interpretation, attempts to establish common and comparable scientific standards) and non-epistemic developments (e.g. the invention of photography, new forms of labor management to increase economic efficiency, new dynamics of control, particular gender roles). With regards to the latter, Daston and Galison (2007, 202) argue, "the scientific selves (...) were doubtless inflected by local accents of class and gender: in the ethos of mechanical objectivity, for example, it is difficult to miss the Victorian admonitions to hard work or the masculine overtones of 'unveiling' nature (or in the exclusionary phrase 'men of science')." Hence, non-epistemic virtues or norms play a

[7] The term 19th century belief-value system is only used for the sake of argumentation and is a stark idealization. I will refer to these idealizations of belief-value systems briefly in my conclusion.

crucial role in the production of knowledge. Whilst this already insinuates that the distinction between epistemic and non-epistemic virtues is not as clear-cut as the name suggests, the distinction still provides a reasonable idea that is supportive for the sake of argumentation. Douglas (2000) argues that "non-epistemic values are required in science wherever non-epistemic consequences of error should be considered;" in this sense (non-epistemic) "value-free science is inadequate science" (Douglas 2000, 559). The non-epistemic consequences of the Azande's Poison Oracle are far-reaching:

> The poison oracle, *benge*, is by far the most important of the Zande oracles. Zande rely completely on its decisions, which have the force of law when obtained on the orders of a prince. (...) In many situations where we seek to base a verdict upon evidence or try to regulate our conduct by weighing of probabilities the Zande consults, without hesitation, the poison oracle and follows its directions with implicit trust. (...) No important venture is undertaken without authorization of the poison oracle. In important collective undertakings, in all crises of life, in all serious legal disputes, in all matters strongly affecting individual welfare, in short, on all occasions regarded by Azande as dangerous or socially important, the activity is preceded by consultation of the poison oracle. (...) I do not wish to catalogue all situations in which the oracle may be consulted since this would mean a list of social situations in every sphere of Zande life, and when each sphere is described the part played by oracles is more fitly recorded than in the present place. (Evans-Pritchard 1937, 121-122)

Non-epistemic norms or virtues (e.g. who has the legitimacy to decide important questions of social life) do not merely enter or disturb science at its outskirts, they do not simply serve as constraints for some scientific choices or internal scientific reasoning. They are an inherent part of knowledge production. In short, knowledge relies on the practice of epistemic and non-epistemic virtues. We have seen that the practice of epistemic virtues—such as objectivity—is relative. The same, of course, goes for non-epistemic virtues. Both are produced and practiced in a particular belief-value system. Crucially, however, this does not limit the relativist to define criteria for knowledge to be accepted as valid or rejected as invalid.

The pragmatic relativist weights knowledge in respect of practical considerations and potential consequences and, in doing so, she can reject epistemic and non-epistemic norms in light of her own belief-value system.

Hence, the relativism that I propose here makes a distinction between the agent and the evaluator—what Kusch (2017) calls a dual perspective view. The agent-perspective is based on our contingent historical circumstances and belief-value systems. We find ourselves in these circumstances—we are 'being thrown' into them, to use Kusch's existentialist argument—through socialization, education, and training. The norms of our belief-value community seem or appear to us compelling and without any alternative: "just like perceptual seemings justify perceptual beliefs, so intellectual seemings justify at least some epistemic justification for our [belief-value system][8]" (Kusch 2017, 4692-4693). The second perspective—the perspective of the evaluator as a sociologist or anthropologist, who steps outside her own system—presupposes self-reflection on the contingency of one's own belief-value system. This process of self-reflection will often yield the conclusion that "one's own position lacks a special privilege as compared with others" (Kusch 2017, 4693) and that although other beliefs outside our own system "do not seem right to us in light of our own [belief-value system], they are nevertheless justified given the other [belief-value system]" (Kusch 2017, 4693). Crucially, however, this does not mean abandoning the first perspective from which "our epistemic standards continue to strike us as right" (Kusch 2017, 4693) and which is therefore rational to use. Such a meta-alternation (see Collins & Yearley 1992) can lead to situations in which norms that cannot be shown to be unjustified are nevertheless rejected from our position as agents. The first part of this article was written from the evaluating position and has argued that we cannot prove the Azande unjustified in their belief via empirical investigation since our attempt to do so implicitly relies on the very norms that we seek to test. In what follows, I will argue from the perspective of the agent. It is from this perspective that the argument for equal validity can be rejected.

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[8] In the original quote Kusch uses "epistemic system." In order to avoid a terminological confusion and to stress the inextricable link between non-epistemic and epistemic beliefs, I deem it better to continue with the term belief-value system.

Whilst mainly focusing on non-epistemic virtues in this section, I would like to make one short remark about epistemic virtues of empirical investigations. I deem it important to stress again that my critique of Sankey's naturalist approach concludes that empirical knowledge is relative due to the relativity of the practice of epistemic norms or virtues, but this does not mean that it is impractical or merely arbitrary. Inductively, empirical knowledge based on interpretative judgment—an epistemic norm that, as I would argue, is currently predominantly practiced in most scientific disciplines in the Western hemisphere—has been practically useful and, in this instrumental sense, successful enough in the past. Therefore, I have no reason to reject it. Moreover, I have no reason for treating the Poison Oracle as equally valid in epistemic terms. This rejection may rest on considering the Oracle's predictions as relatively incoherent (by my own lights) and, in the context of the raining-example, as unable to guide me towards practically useful (local) decisions such as an answer to the question whether I should take my umbrella with me or not. The same is true for social inquiries. By my own lights, I clearly regard "a strategy that explicitly acknowledges the need to employ trained judgment" (Daston & Galison 2007, 311) as being superior to strategies such as the Poison Oracle, especially if trained judgment is critical about the "sharp dualism between lay and expert perceptions" (Jasanoff 1998, 98), engages in symmetrical investigations, and puts emphasis on experiences of people directly affected. In the same vein, I have used the term empirical *knowledge* deliberately to demarcate my position clearly from skepticism and to stress that we can gain knowledge through empirical investigation. We do so in a relative way.

Nonetheless, my rejection does not merely rest on epistemic concerns. The various methods of inquiry (e.g. poison oracle, beaker, weather charts), which different norms of objectivity imply, always evolve in particular belief-value systems and thus reflect the broader social climate of these systems. Put differently, they are inextricably linked with political, social, and historical ideas and values (as the gender example above sought to demonstrate). These social components do not pollute scientific knowledge, but are always constitutive of it. This does not imply that knowledge is purely social, but it highlights that the social component is always existent and must be recognized. As a consequence, my position as agent allows me to reject particular aspects of the Azande's belief-value system—such as certain non-epistemic norms—which implicitly constitute their (epistemic)

method of inquiry (i.e. the Poison Oracle). In doing so, I will draw on promising ideas of feminist epistemology.

Longino (1997, 21) outlines some virtues "that are taken as counting *prima facie* and *ceteris paribus* in favor" of a proposed explanation. She states that these virtues include "(...) novelty, ontological heterogeneity, complexity or mutuality of interaction, applicability to human needs, and decentralization of power (...)." Due to the limited scope of this article, I will only focus on one of these virtues, namely the decentralization of power, to outline one specific, but by far non-exhaustive, pragmatic way to reject the Azande's Poison Oracle. As has already been argued, the Poison Oracle as a practiced epistemic norm implicates non-epistemic consequences. In this context, the production of knowledge has significant societal implications. The acknowledgment of the decentralization of power as a crucial non-epistemic virtue in the production of knowledge and in evaluating proposed explanations demonstrates the profound shortcomings of an epistemic norm such as the Poison Oracle. It, thus, constitutes one criterion of its rejection. Evans-Pritchard (1937) exposes the Poison Oracle's centralized, hierarchical, and authoritative nature of producing knowledge as well as its hegemonic character vividly at various passages in his book. It is worth quoting these passages at length here:

> The poison oracle is always the final authority, and if the matter is one involving relations between two persons it must be consulted. For this reason, unless the matter is urgent, they bring all important social questions directly before the poison oracle (p. 168). When I say that the poison oracle, or some other oracle, must be consulted (...), I mean that if a Zande were not to consult it he would be acting contrary to custom and might suffer in social prestige. He might even incur legal penalties (p. 122). I found that when a Zande acted towards me in a manner that we would call rude and untrustworthy his actions were often to be accounted for by obedience to his oracles (p. 124). Some Azande have indeed explained to me their doubts about the honesty of the princes who control the oracles (p. 6). Members of the princely class, *the Avongara*, are not accused of witchcraft, for if a man were to say that the oracles had declared the son of a prince to have bewitched him he would be asserting that the king and princes were also witches. However much a prince may detest members of his lineage he never allows them to be

> brought into disrepute by a commoner. (...) There is an established fiction that Avongara are not witches, and it is maintained by the overwhelming power and prestige of the ruling princes (pp. 9-10). Governors of provinces, deputies of districts, men of the court, leaders of military companies, and other commoners of position and wealth are not likely to be accused of witchcraft unless by a prince himself on account of his own hunting or on account of the death of some equally influential commoner. Generally lesser people do not dare to consult the oracles about influential persons because their lives would be a misery if they insulted the most important men in their neighbourhood. So we may say that the incidence of witchcraft in a Zande community falls equally upon both sexes in the commoner class while nobles are entirely, and powerful commoners largely, immune from accusations (p. 10).

If we agree with Longino (1997, 25), as I tend to do, that knowledge production should "(empower) the many rather than (concentrating) power among the few" it is hard to see how one could practically accept norms like the Poison Oracle, in which knowledge goes hand in hand with a centralization of power, as being equally valid. Similar remarks could be made about practices of knowledge production that rely on the notion of truth-to-nature since, also in these cases, it is only "the moody brilliance of the genius" (Daston & Galison 1992, 83) that vouchsafes objectivity, which is necessary for knowledge to be accepted as such. Genius, however, can, in contrast to expertise for example, not be learned. Someone is born a genius, or she is not. Truth-to-nature, as well as the Poison Oracle, constitutes a practice of an extreme form of power centralization—the power to define and produce "knowledge." Hence, amongst others, the acknowledgment of the decentralization of power as a necessary non-epistemic virtue in the production of knowledge is conducive to the rejection of these and other (theological) accounts that ground knowledge on centralized, hierarchical, patriarchal, unapproachable God's-, Genius'-, Witch doctor's, or Prince's-eyes views. I do not acknowledge these accounts as being equally valid.

I doubt that my argument will convince Sankey, but if he wants to argue for a non-relativist position of knowledge he still owes an explanation that does not run into the problems that I have elaborated on. Thus, the ball is now in the absolutist's court again. All the relativist needs to say—and what

has been said in this article—is that epistemic justification is epistemically circular and that this circularity is not absolute. The relativist, however, does not have to grant all norms equal validity. She can and should define criteria for the validity of knowledge. Unsurprisingly, neither these criteria will ever have an absolute status.

6. Concluding remarks

In this short article, I tried to show that a naturalist response to relativism is fallacious. Needless to say, the argument developed here is not an argument against naturalism per se, but against naturalism as a response to the relativist. As Bloor (2007, 252) argues,

> Knowledge and belief, and the performance of those who know and believe, must be grounded in the natural world, and they are themselves things which are susceptible to scientific explanation. Cause and effect, materiality, the limits of space and time, biological evolution, the working of the brain, the interaction of human beings in society, these alone represent the framework of thinking, including our thinking about ourselves, our knowledge, and our morality. For the relativist, there can be nothing transcendental about the story of human achievement or failure. Neither knowledge nor morality can be supernatural. They are natural phenomena, and any attempt to evade this fact is a lapse into superstition and obscurantism.

In short, Bloor (2007, 252) emphasizes the importance to consider relativism "as the consequence of a yet broader, overarching perspective on the human condition;" a perspective that "might be called naturalism." In this sense, every analysis of the nature of cognitive achievements such as knowledge or justified belief should be both, naturalistic (e.g. taking on board insights from biologists) and sociological.

The relativist does also not fall into a state of illusion as the absolutist, and many forms of foundationalism, tend to do. She is clear about the fact that the assessment of cognitive achievements will always rely on those very achievements, or that the assessment of epistemic norms will always rely on epistemic norms. Should that bother her? I do not see why I should.

Of course, the "description of knowledge and science that results will be no more certain or secure than the scientific theories themselves" (Godfrey-Smith 2009, 150), but the relativist does not doubt that science can figure out something true about the world. In contrast to the skeptic's understanding, science *does* produce knowledge. "The claim is that we have no *absolute* knowledge (...). For the relativist, all our beliefs are the product of, and are relative to, the limits of human nature and our status as human, social animals" (Bloor 2007, 251).

Empirical investigations must always be practiced in an objective way to be granted scientific status. They are, however, never absolute as the very epistemic norms on which they are grounded (such as the practice of objectivity) are relative. Therefore, the principles for empirical assessment (e.g. objective observation) cannot be understood independently of the belief-value system in which they are *practiced*. In this sense, the transformation of objectivity is not merely a change in its wording or meaning, but a change in (cultural) practices. Since the epistemic norm of objectivity, which is inherent in the practice of empirical investigation, is relative, the same must be true for the empirical test result of whether the Poison Oracle is objectively true. Hence, we cannot, without circularity, prove the Azande to be unjustified in their belief. Crucially, however, we do not have to grant them equal validity, even if the rejection of the Oracle can never be based on anything absolute, but will have pragmatic roots in our own belief-value systems.

Finally, I deem it important to stress that the historical cases of objectivity that were discussed in this article constitute simplified, illustrative archetypes. The change of regimes or belief-value systems is never a clear-cut one, but rather a subtle transformation of "moralized virtues associated with active judgment" (Galison 1998, 333). In the context of the study of objectivity, the transformation depended on a variety of factors such as the understanding of the self, gender roles, images and metaphors about society-nature relations, controversies concerning objectivity versus subjectivity, contextual (epistemic) fears, technological developments (such as the invention of photography) as well as the institutionalization of the scientific enterprise and, as a result, a new form of scientific confidence. Whilst these factors are interrelated and often mutually reinforcing, they do not necessarily coincide. In this sense, a fully coherent belief-value system is a myth. The practice of different rationalities and norms overlap, intersect,

and—while still working on the same object of steering—can even contradict each other within one belief-value system (see Foucault 2003 on governance).

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Knowing Subject and External Object in Language and Linguistic Analysis

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ABSTRACT: The claim of linguistics to be a 'science' is connected to its 'objectivity'. The same is true of the philosophy of language. This implies a clear distinction between the language analyst as a 'knowing subject' and linguistic phenomena as an 'external object'. The picture of everyday verbal communication contains the idea of speakers as 'knowing subjects' of verbal signals as 'external objects'. Also, the correspondence theory of truth for natural languages presupposes that the language analyst is a 'knowing subject' who can assess the truth of objectified statements in relation to the factual world. The paper questions those ideas, and suggests that the objective orientation in linguistic analysis is a convenient fiction. It is suggested that analysts and speakers are components in a complex communicational totality, and can never be external objective observers of the verbal communication process. Consequently, a coherence theory of truth is more appropriate for language analysis of all types and for our understanding of speaker behaviour.

KEYWORDS: Coherence – correspondence – external object – knowing subject – objectivity.

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1. Scientific objectivity and language

Linguistics has long claimed to be a 'science' and to have a 'scientific' approach. Linguists of an earlier generation, such as the American structuralists or the Danish theoretician, Hjelmslev (1953), very explicitly tried to put linguistics on a 'scientific' footing, and the 'scientific', nature of linguistics continues to be asserted to this day, for instance on the website of the Linguistic Society of America (online[2]) and in too many works on linguistics to name. This claim seems to rest on the alleged objectivity of linguistic analysis. A typical view is that of Martinet (1989, 6), who says,

> Une étude est dite scientifique lorsqu'elle se fonde sur l'observation des faits et s'abstient de proposer un choix parmi ces faits au nom de certains principles esthétiques ou moraux.[3]

In various contributions to the *Quora* website (online[4]), a number of linguists justify the 'scientific' nature of linguistics by reference to the use of the 'scientific method' in linguistics. Numerous linguists have presented versions of 'scientific method' (Bloomfield 1933, Cook 1971, Sampson 1975, Mulder 1989 among many others). By this, they seem to mean that observation, hypothesis, and testing, controlled by explicit theory (inductive or hypothetico-deductive), ensure that linguistics is a 'science' in the same way that other disciplines are considered to be sciences—and, indeed, those characteristics, along with quantification (found in some areas of linguistics, but rarely relevant in central, qualitative, linguistics), are often met with in expositions in the philosophy of science in one form or another. Such views are so widespread in linguistics that they can be considered dogma. This 'scientific' view implies that there is an observer distinct from, and observing, facts, phenomena, or data. The 'knowing subject' is usually taken to be a being with unique consciousness and/or unique personal experiences or an entity that has a relationship with another entity that exists

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2 https://www.linguisticsociety.org

3 'A study is said to be scientific when it is based on the observation of facts and refrains from choosing between these facts on the basis of aesthetic or moral principles' (trans. PR].

4 https://www.quora.com/why-is-linguistics-considered-a-science

outside itself (various such definitions are easily found in reputable sources online). An observer is a 'knowing subject' in those terms. Linguists, then, are claiming to be objective observers of external phenomena, who can apply 'scientific' methods to arrive at representations, and explanations, of verbal 'reality'. Presumably, philosophers concerned with language hold a similar position. Does this claim stand up to scrutiny? Are language users or observers 'unique', and is language 'external' to them?

This widely held view of the scientific status of linguistics is similar to that in well-known pronouncements on the nature of science, or the scientific point of view, of philosophers of science such as Popper, who asserts, in a similar way to Martinet, that objectivity implies that 'scientific knowledge should be *justifiable*, independently of anyone's whim' (1972a, 44). While Popper denies that any scientific theory can be absolutely justifiable (any theory might be refuted), the objectivity of scientific statements can be supported by 'intersubjective' testing. Objectivity in linguistics, as elsewhere, rests on the impartial observation and recording of speech phenomena and its associated behaviour in speakers as well as on intersubjective agreement about those observations. It also involves the application of clear and reasoned criteria in the analysis of the said phenomena and behaviour to produce descriptive and explanatory models for the understanding. Linguistics has always been concerned with its methods of analysis (or 'procedures' in earlier versions). In all of this, there is the implication that one can distinguish between the linguist-observer and the observed phenomena (and/or behaviour). That is, that the linguist-observer is external to the process of communication, which is 'objectified' or 'reified' for the purposes of analysis. However one conceives of the communication process—in terms of a Saussurean 'speech circuit' (1972/1916), Bühler's <Organonmodell> (1934), or some version of Shannon and Weaver's communication process model (1949)—, the linguist-observer is taken to be an impartial and objective onlooker, and the phenomena are the objects of study. Alternatively put, linguistic analysis involves a radical dichotomy between the knowing subject and the object of study. That, of course, could be said of any science,[5]

[5] This paper is not concerned with natural sciences. Both linguistics and natural sciences employ models and theory-based reasoning, but using and observing language involve being *internal* to the process under observation.

but we may ask whether the analysis of verbal phenomena is really so objective.

Popper's intersubjective agreement replaces a single knowing subject with a plurality of knowing subjects, which may remove the individual whim or allegations of inaccuracy (as well as some of the uniqueness of the experience or consciousness of language), but it is clearly not a sufficient condition of agreement on the nature of phenomena, let alone of the correctness of a statement or theory, since our theories and statements (or even observations) may prove wrong, however many people agree to them—and, of course, it is well known that linguists can, and do, differ in their interpretations of the 'same' phenomena. The notorious case of the morphological status of *cranberry*—i.e. does it consist of a single component or a combination of signs?[6]—(the arguments around which are well discussed by Harris 1973, 66*ff*) is a case in point, as are differences over the phonemic status of affricates, such as [ts]/[c], [tš]/[č], (the problem of 'un où deux phonèmes'—whether a phenomenon is to be analysed as 'one or two phonemes'), or the analysis of syntactic constructions as combinations or dependencies. Nor is intersubjectivity a sufficient condition of objectivity, since the selection of data and methods may also be distorted by current or favoured trends or paradigms. Popper also, of course, puts scientific knowledge in the realm of the '3rd World' (or 'World 3') of rational ideas allegedly existing separately from any knowing subject (1972b, 153 *ff*). Popper's view seems to be an extension of the idea of 'subject invariant' qualities (Harré 1976, 160) leading to an objectivity which is fundamentally quantificational. Most linguists would insist on the subject-invariance of their data, although most linguistic analysis is *qualitative*. When we set up phonemes or grammatical relations for the purpose of accounting for communicational behaviour, it is the transparency of the theory and methods applied to agreed data sets which is supposed to overcome subjectivity. Linguists (and others) typically overlook what Harré calls 'the contribution of the knower to the known' (1976, 21*ff*). In language analysis, that contribution comes from the *selection* of data, theory, and methods, as well as from the position of the linguist-observer (which accounts for differences in interpretation in the cases listed above). That is, linguists also tend to

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6 And, of course, *mutatis mutandis*, for many other expressions. *Cranberry* was a test case.

ignore the arbitrariness of their theoretical positions (noted by Hjelmslev 1953, Ch. 5) and, hence, the theory-laden nature of observations and descriptive or explanatory pronouncements (Rastall 2006a, 2011). This raises the problem of the relationship of the knowing subject to verbal phenomena.

Part of the linguistic conception of language is the view, or observation, that participants in the communication process (human speaker-listeners, sender-receivers) *also* adopt an objective viewpoint with respect to speech signals and their associated messages. That is, the participant in a speech act considers, however briefly (and almost certainly unconsciously before conscious awareness), any speech signal and its message from a number of points of view for the purposes of understanding ('meaning-making') and appropriate response, and, where deeper thought is required, the speaker-participant considers the form and content of speech more extensively. One imagines, then, that the speaker-listener treats the speech signal as an objective fact.

When the poet, Robert Burns, addressed a field mouse as a 'wee, sleekit, cow'rin', tim'rous beastie', we can consider the line from various points of view—a grammatical whole, a compilation of adjectives and their appropriateness, the use of dialect, the metre of the line, the connection to the rest of the poem, etc. In each case, the line is objectified for consideration (see Rastall 2006b for further discussion). It is this objectification, or reification, of a verbal signal or text which is needed for the 'self-referential' or 'meta-linguistic' function of language—using language to discuss other language products.

Mulder (2005, 74) points out that in practice all linguistic analysis and participation in speech acts involve such a reification of verbal products, but that objectified verbal products of whatever kind (sentences, words, references, register effects, etc.) are the creations of our brains engaged in communicative acts. Much linguistic philosophy and logical analysis of propositions similarly presupposes the objectification of sentences. Thus, Strawson's analysis of simple propositions into identifying subject expressions and characterising predicates (1968, 5*ff*) or any other analysis of sentences such as *Snow is white, Mary is Australian*, etc. presupposes the reification of the sentence for the purposes of discussion—in effect, it becomes (as a counter in discussion) a citation form. Again, there is a clearly implied distinction between the speaker-participant, as a knowing subject,

and the speech signal/message as an object or objectified action external to the speaker.

That distinction seems to be inherent in the Shannon and Weaver model of communication (1949) or its variants—a 'sender' forms and transmits a 'signal' to a 'receiver' who decodes it.[7] The linguist, in observing and analysing speech acts, is the knowing subject observing this subject-to-object signalling relation between senders and receivers as itself an object for study, usually abstracting the signal and its message, the text or utterance, for analysis. Similarly, the text or utterance can be analysed in relation to the situational or discoursal context. This position is common in linguistics, and seems to be inevitable for the purposes of analysis, although we may know that a speech act is not a static 'thing' but a dynamic process or event taking place in time with complex transformations of energy and informational state in the participants related to also complex situational and discoursal contexts. The objectified verbal product comes into being through our cognitive processes (as Mulder observes in the same article). But its interpretation also depends on our integration into the speech community.

2. Is there a clear subject-object dichotomy for language and language analysis?

Now, it would be reasonable to ask then whether the relation of subject to object is really so clear-cut both in the case of the linguist-observer and in the case of the real-life participant in a speech act, when we consider that any verbal product is the creation of our brains and is reified as a representation to us. While the objective orientation is useful for the development of understanding and is central to the Cartesian model of rational enquiry, it should be clear that the subject (linguist or participant) and the object (verbal signal/message) are both parts of an inter-connected totality. Viewed, as it were, from the perspective of an alien spaceship, earthbound subjects and verbal objects (texts, utterances) are just components in a complex and interacting whole. Indeed, an *isolated* individual is not part of the

[7] Lakoff and Johnson (2003,140*ff*) criticise the view that 'the speaker puts ideas (objects) into words (containers) and sends them (along a conduit) to a hearer who takes the ideas/objects out of the words/containers' as inappropriate 'objectivism'.

social organism of communication, however much he or she might engage in internal dialogue. Internal dialogue is in part a substitute for social interaction necessary to humans.[8] Human communicative interaction could then be seen as a very complex single organism consisting of interacting individuals, rather as we can see a colony of ants as a single organism of communicating individuals, where acts of communication are a means to the functioning of the whole community by coordinating and integrating individuals into it, and where the individual's orientation and needs are met through communicative integration. We can see that in daily acts of communication, for example in making purchases in shops or requesting and receiving directions, or in the maintenance of social relationships.

The neuroscientist, David Eagleman, makes the point (2015, 133) that, while each individual feels independent from all others, 'each of our brains operates in a rich web of interaction with one another… an enormous amount of brain circuitry has to do with other brains'. Verbal communication, from this perspective, would be a property for the functioning of a community, which determines our speech and its interpretation. The objective viewpoint of the linguist or language analyst would then be just a useful fiction for the purposes of discussion and explanation.

Such would be the view of idealist philosophers such as F.H. Bradley (1897, 99*ff*), who emphasised the difference between the appearances of our lived experience and the inter-connected oneness of underlying reality (in common with a long line of oriental and western monists[9]). The physicist, Carlo Rovelli, makes a similar point about the interconnectedness of the physical universe (2016, 22). The signals and messages of texts and utterances appear to us to be external objects which we know as isolated individuals, but we ignore our integration into a wider social whole and the acquired and unconscious verbal processing which make communication possible. That processing requires the connectedness of individuals into a communal totality. Communication links human organisms into a social whole. That integration into the social organism implies interpretations and appropriate behaviours which come from being *part of* the social totality,

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[8] It is also the normal means to the construction of reality and our representation of it, but that is a different issue.

[9] E.g. practitioners of Daoism and Zen in the east or from Parmenides through Spinoza on in the west.

not separate from it. Bradley says in the same context, 'the secondary qualities must be judged to be merely appearance' (1897, 15). The imagined clear distinction between knowing subject and external object is one of those appearances, and our awareness of the qualities of speech (phonological, grammatical, semantic, social/aesthetic), which are the products of our cognition (whether as speakers or analysts), are also appearances—i.e. they are the way verbal products and behaviours seem to us at an everyday level, not the real, unobservable, cognitive processes giving rise to our awareness of language. As Bradley says in the same chapter, the 'arrangement of given facts into relations and qualities may be necessary in practice, but it is theoretically unintelligible' (1897, 22). The knower and the known, on this view, are parts of a single totality[10] but in which an 'objective orientation' is a useful fiction. All of our judgements, as speaker-participants, about the nature of language involve 'secondary qualities' that are inevitably just the way things appear to us from a given perspective and through the prism of our cognitive processes.

The idea of an 'internal mental model' is relevant here (see e.g. Kintsch & van Dijk 1983, and Johnson-Laird 1983, 2006). Any perceived event or experience, including verbal products, is seen as a 'reality' by reference to, and constructed by, unconsciously formed cognitive 'models'. In the case of verbal products, their reality arises from 'models' as organised expectations from the mass of verbal associations in many dimensions in the brain. Each individual's mental model is different, and so each verbal product is constructed differently. That would partially explain the variations in response to verbal products. The theory that mental models play a major role in constructing reality suggests that the construction of verbal products by the speaker-participant is a matter of secondary appearances rather than objective orientation; they arise via our brain processes. For the language analyst, one must imagine both the integration of the analyst into the communicational totality and an additional layer of anticipations due to his or her preferred analytical model—itself a function of mental modelling through education and experience.

A major issue here is the limitation which, as Bradley (above) suggests, is imposed on us by our senses. The appearances of our lived experience

[10] 'The absolute is not many; there are no independent reals' according to Bradley (1897, 99).

are created through the mediation of our senses. Our cognitive processes which make sense of the physical input to our perceptions, noted by philosophers from at least Berkeley (1710/1910) onwards, further partly determine our representation of the world. The theory of internal mental models is a modern version of this long-standing philosophical viewpoint. Thus, it is a commonplace of science that the real 'ultimate' reality is not as we perceive it (e.g. Rovelli 2016). For example, we can know that grass is not actually green, however counter-intuitive that may seem to the Dr Johnsons among us. While our senses and cognitive processes tell us that grass is an object which has the property of being green in colour, we know that what we perceive is reflected light in the 'green' portion of the spectrum. Chlorophyll in grass absorbs the other wavelengths in the light spectrum. What we see as green is the reflected light, not an inherent property of grass, but that light is interpreted by our cognitive processes as 'green'. If our brains worked differently, we might see that light as another colour in the way that insects see yellow evening primroses as 'blue' in ultra-violet light. The point is that the objective viewpoint of the knowing subject is dependent on both the interaction of the subject with the object and on the interpretation of that interaction by the processes in the subject's brain. This suggests that the state of affairs is no different in the case of the speaker-participant or the linguist-observer; i.e. we must expect that any ultimate verbal reality is not as we perceive or represent it. So, we must ask, is the alleged objective orientation of the linguist-observer or speaker-participant justifiable?

In the case of the linguist-observer, it is obvious that the linguist is not generally a participant in any communication process and is, therefore, in that sense external to it. For example, in considering the structure and communicational functions of imperatives or discourse counters, such as *Pass me that hammer, will you?* or *So, what next?*, the *linguist* is not the recipient of the request or expected to respond with a suggestion. The linguist is concerned with observing what participants in the communication exchange say and do, i.e. having an 'objective orientation'. This is also the (rather simplistic) idealisation described by Quine (1961, 29*ff*) in his account of how a field linguist might learn the meaning of *gavagai* as 'rabbit' in an otherwise unknown language.

The most extreme position adopted by any linguists of the external observer approach was that of American structuralists, notably Zellig Harris

(1951, 1-24). They effectively claimed that the linguist was a kind of observer-analyst who would experiment with speech like test-tube specimens, i.e. applying 'procedures' to a 'corpus of data'. That involved the claim that linguistic analysis could be carried out by purely distributional means and without reference to 'meaning'. It is well known that this idealised approach was never even remotely attainable, or in fact attempted in the extreme ideal form. The reasons for the failure lay in the fact that the recognition of speech components and relations, and their understanding, require awareness of speech functions, the social values of speech, and the diverse parameters of meaning—Quine's idealised picture is closer to the reality of the field worker. Even in a known language, our construction of a relation between *Fred* and *left* in *Fred left*, for example, requires such an awareness—we intuit the connectedness of the signs, which we interpret as a grammatical relation connecting the component signs into a complex sign consistent with the patterns in other combinations. The connection is not overt or formally signalled in either the proper name or the verb—as indeed is the case with most grammatical relations.[11] Furthermore, the determination of where grammatical constructions begin and end (and hence what relations we set up), however, is frequently quite difficult and a matter of arbitrary decision (Rastall 2003). For instance, how many 'sentences' are there in utterances such as: *It's going to rain, I think; Two for the price of one—a good deal, that!* among many others? One's answer depends on one's theoretical and methodological stance; not least, how we define 'sentence'. We 'know' that Burns' mouse is a *wee beastie,* a *sleekit beastie*, a *cow'rin' beastie*, and a *tim'rous beastie*, because we recognise the separate connection of each adjective to the noun, and that implies that the linguist-observer or speaker-participant is *internal* to the communication of the poem; one needs to be an English speaker to recognise the grammatical and semantic connections in Burns' line. All grammatical connections are, in fact, intuited from our knowledge of the language. Furthermore, even the recognition of speech sounds involves our construction of them through cognitive processes. What we hear as [p] or [a] is constructed from our perceptions of sound energy. In analysing Russian, we must be able to

[11] Juxtaposition/sequencing plays a role in the identification of patterns, but not all juxtapositions are interpreted as grammatical relations. In *Fred never left*, *never* is grammatically associated with *left* but not with *Fred.*

recognise and interpret the difference between palatalised and non-palatalised consonants, and in Chinese the different tones as well as the distinction between aspirated and unaspirated consonants, whereas similar features in English are ignored as not communicationally relevant. Conversely, we ignore the clear differences between the consonants in *tea* and *too* or *key* and *coo*, and treat them as communicationally 'the same'. This ability to deal with the specificity of languages comes from becoming internal to the communication process, not from a purely 'objective stance'. Acquiring a language involves becoming communicationally integrated into a community.

We must add that the analysis carried out by the linguist depends also on the theoretical and methodological approach selected by the linguist, and involving many 'short-cuts' which in fact betray the linguist's direct knowledge of the data, such as the intuiting of grammatical connections or what was a relevant phonological difference (as became obvious in the old American structuralist approach; the short cuts undermined the claim to work only with objective procedures). Any theoretical approach (distributionalist, functionalist, etc.) is justified not by observation (which would lead to circularity of argument) but by its logic and general reasonableness. The classification of 'words' into categories such as 'adjective' or 'noun' presupposes the definitions of those terms and methods for assigning words to categories. The identification of phonemes as separately relevant to communication presupposes a theory of communicational relevance, etc.[12] In other words, the linguist-analyst is not in the same position as the laboratory chemist or biologist (as in the old American structuralist ideal)[13] even where the linguist is external to the communication process in the above sense. In addition to perceptual filtering, and cognitive processing of signals, there is the added layer of theoretical and methodological processing of the interpreted signals—applying criteria involving secondary qualities. This is part of the contribution of the knower to the known. In what sense can we then arrive at an ultimate language 'reality'?

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[12] Depending on one's theory: the analysis may not involve 'words' or 'phonemes', 'communicational relevance' etc. at all, but may invoke other concepts.

[13] Natural scientists also use model-dependent reasoning (as noted above), but one does not have to be a fruit-fly to study fruit-flies, whereas we need to be language-users to discuss language.

Among philosophers, the tendency to objectivise language and to adopt an external objective stance is most obvious in the case of the logical atomists, logicians, and empiricists. Wittgenstein in the *Tractatus* (1921/1971) develops the idea of propositions as 'pictures'—'models of reality' which can either agree or not agree with facts (propositions 2.12, 2.21, 4.03). The philosopher in this approach, as knowing subject-cum-observer, must adopt an external viewpoint to judge the agreement or non-agreement of the proposition with reality (and hence empirical truth). Similarly, Ayer (1936/1974) takes the same position with his verificationist theory of meaning. He says (p. 48), for example,

> The criterion which we use to test the genuineness of apparent statements of fact is the criterion of verifiability. We say that a sentence is factually significant to any given person, if, and only if, he knows how to verify the proposition which it purports to express—that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false.

Apart from the obvious (and absurd) restriction of meaningfulness to sentences with a propositional content, it is clear that verificationism (or indeed falsificationism of the Popper variety in scientific statements) implies a clear distinction between the knowing subject and external reality.[14] The same is true of the much less extreme position (noted above) of Strawson (and other language philosophers) in which there are judgements about the 'identification' of the subject and the 'characterisation' of the predicate and their connection (Strawson 1968, 6*ff*). The knowing subject is either the philosopher-analyst verifying the testability of sentences in relation to observable fact, or is the participant in a communicational exchange. In both cases, there is the presupposition of a correspondence theory of truth for ordinary language utterances. Tarski's standard example:

> *'snow is white' is true if and only if snow is white*

[14] It seems to me that Popper's idea of a *World 3* to provide 'epistemology without a knowing subject' (1972: 106*ff*), however useful in other respects, does not remove the need for *someone* (i.e. a knowing subject) to judge whether a proposition is true or false.

is a summation of that position, to which we shall return below. The sort of truth-conditional semantics advocated by logicians such as, among many others, Soames (2005) is a continuation of this approach.

In certain types of investigation, of course, the linguist is internal to the communication process. This is most obvious where interview techniques are used or there are interactions, as in some developmental studies. In the latter cases, investigators must avoid introducing bias or unduly influencing the behaviour of an informant, and adopt an 'objective' stance towards their data. Clearly, on the one hand there have been tendencies to minimise the involvement of the analyst in the communication process and, on the other, to accept the need for that involvement, but to allow for it in arriving at conclusions (everyone adjusts verbal behaviour to that of their interlocutor(s)).

In the case of the speaker-participant who is internal to the communication process, it seems clear that the formation and interpretation of speech signals involve several parameters. This is particularly clear in the processing of verbal signals and written text. In general, the more complex the signal, the more perspectives that are possible on it. Thus, when the poet, Thomas Gray, wrote:

Far from the madding crowd's ignoble strife
Their sober wishes never learnt to stray.
Along the cool, sequestered vale of life,
They kept the noiseless tenor of their way.

we can, among other things, consider the stanza from the point of view of its central meaning, or place in the overall context of the (philosophical) argument of the *Elegy Written in a Country Churchyard*, or its position in 18th Century thought, or we can consider how the grammatical structure and balanced patterns support the rhetorical purpose, or we can look at the iambic metre and vowel lengths to consider their phonological effect in 'lengthening' the lines to correspond to the meaning and improve the poetic effect, or we might consider the different register effects of the lexis. This does not exhaust the list of perspectives, obviously more than for the single line of Burns. On a more everyday level, a speaker referring to a postal delivery (and considering it worthy of comment) might say any one of: *The postman's been, The post's come, Can you check the mailbox?, There's*

some mail for us, etc. The particular utterance will depend on a variety of social and contextual circumstances and motivating factors, along with the construction of a reality (the appearance of a postman and his actions in relation to one's residence). The interpretation and response or attitude to the utterance by the receiver will similarly be varied and involve a range of attitudinal factors.

We can see then that both linguist-observers and participants in communication events can, and do, adopt an apparently objective orientation towards speech signals and messages as a matter of 'lived experience'—i.e. the way verbal reality appears to us.

However, our 'lived experience' of language exists at the 'macro-level' of our everyday reality and in the world of appearances. It is the world in which we see postmen and grass, and recognise actions such as deliveries of post, and in which utterances or written text are objectified as events or things. Our verbal productions also seem to us to be realities as speech signals, and they create realities through the messages that are conveyed. It is a mistake to think that language and reality are somehow separate. The kind of correspondence theory of truth for ordinary language, which we noted above, depends on a separation between the verbal product, utterance, or sentence and some external state of affairs (as Strawson pointed out, 1971, 1*ff*). This may be convenient from some points of view, but it ignores the role of language in *creating* our understanding and social orientations, and it ignores the *conventionality* of language. Furthermore, it ignores the role of our unconscious cognitive processes in forming the macro-level appearances that is our (everyday) 'reality'. The division of language, on the one hand, and 'reality' on the other is a myth.

As we have said, our macro-level reality is a construction which relies on the particular nature of our cognitive processing (including verbal processing). Our everyday sense of reality, including the reality of verbal signals and messages, is built up from micro-level processes- untold millions of tiny neuro-transmitter signals in response to physical inputs such as light photons or sound waves. That construction and representation in consciousness further depends on the way our brains work. From this point of view of the unconscious processing of perceptual information, including the perception of verbal signals, the objective viewpoint in relation to language is also a macro-level representation arising from micro-level processes and complex determining factors of which we are unaware in

everyday life. (Previously deaf people who receive cochlear implants, for example, have to learn to discriminate speech sounds.) Furthermore, the representation and interpretation of verbal signals and their messages depends also on linguistic processing—on the mass of verbal associations in different formal, semantic, and social/aesthetic dimensions. What we take to be the objectified verbal production—sentence, word, etc.—is the result of a complex process of interaction and set of verbal conventions in a totality or communicational world. One might say that the objective viewpoint which seems to involve such a clear-cut distinction of knowing subject and external object is also a part of the appearance world, and hence illusory.

3. The speaker-participant

For the speaker-participant, the interpretation of sound waves as speech and its analysis into apparent 'speech sounds' is an illusion created by our cognitive processes. We know that there are, in reality, no discrete speech sounds.[15] They are our constructions from the perceptual input and our interpretative processes. Similarly, the identification of words and their combinations is a matter of unconscious construction as well as considerable socialisation and education. Meaning associations, speech functions, social values of speech, rhetorical and aesthetic dimensions of verbal signals all depend on unconscious (multiple and parallel) processing. All of the language phenomena that we experience on a macro-level have a coherence and are useful in our social relations and representations of experience, but what all this suggests is that the subject-object dichotomy in relation to language is illusory. Our macro-level representation of the everyday reality of language is the final product of a complex (and unconscious) interaction involving multiple processes adapting communicational means to specific circumstances, and connecting and interpreting physical signals in relation to social or other discoursal contexts and observable reality. But those contexts and realities are themselves the products of unconscious cognitive processes—as is the comparison of language and external reality as it

[15] Our understanding of the relation of acoustic input to perceived speech sounds is too a matter of modelling.

appears to us. In that sense, the radical distinction of subject and object disappears into a complex connectedness. But this also means that the objective stance towards the relation of language and reality is also a product of unconscious processes. Thus, the idea that 'truth' can be characterised for each language 'relative to a time and a speaker' (as Davidson 2005, 225 suggests), while an improvement on the usual correspondence approach (at least for natural languages), still depends on the assumption of a subject-object dichotomy. It is striking, of course, that our separate constructions of verbal reality broadly coincide for the purposes of communication; that can be taken as a function of our integration into the wider social totality, which cannot happen if we 'do not know the language'.

If we take away the appearance of a subject-object dichotomy and treat the objective stance as a convenient illusion, each participant is a component in a dynamic totality of the communicative act, and each participant is a 'node' in the wider network of all those who share the same communication means. Our individual experiences and representations of language differ because our individual brains differ, but we cannot have any verbal construction or representation without being part of that totality. Similarly, the comparison between language and reality-as-it-appears-to-us is a convenient representation of our orientation in the physical and social world. To illustrate the point, we can observe the communication of ants, but cannot be participants in their communication—we cannot enter the 'ant world'. Conversely, ants cannot be participants in human communication. Ants and humans operate in different totalities- obviously of quite different orders of complexity—, but in both cases communicative interaction is a means to orientate the individual in the social world and in relation to physical reality,[16] and in both cases the communicative interaction involves unconscious processes.[17] As a verbal example, we can ask *how* we know that an utterance such as *I'll be there at eight o'clock* is a promise (or commitment, threat, or prediction, as the case may be)? We must be internal to the communication process for the purposes of recognising and interpreting

[16] Work on ants at the University of Würzburg by Franck *et al*. (e.g. 2017) is instructive in this regard.

[17] Of course, some limited human-animal communication is possible in fixed contexts and for specific functions, and vocalisations and behaviours can be indices for other species.

the utterance in its context, and in order to respond and act accordingly.[18] This cannot be known from a purely external viewpoint of the observer. The interpretation and response of the individual in a communication process depends on multiple unconscious cognitive processes (preceding conscious awareness) and integration into the communication community, so the appearance of an objective judgement about an utterance is the brain's representation of the individual's orientation to a verbal signal.

4. The linguist-observer or language analyst

When we come to the linguist-observer, it should be clear that similar points can be made. Linguistic analysis is concerned with the macro-level reality of language, but the recognition and interpretation of sound as speech with its many dimensions of meaning requires that the linguist be internal to the processing of the speech signal. If I acquire recognition of a sound as [õ] in a French utterance, I must use similar cognitive processing of sound as French speakers, but I must also be able to recognise the communicational value of [õ] in French for the purposes of communication in French.[19] This means being internal to French speech communication conventions, even if I am not a participant in a French speech event. Similarly, for the identification of [sõ] as a word, I must be internal to French speech communication to distinguish the [sõ] in *son frère* ('his/her brother') from [sõ] in *le son* ('the sound'), and the latter from [ləsõ], *leçon* ('lesson'). As we come to more complex issues of speech functions, connotations of meaning etc. as in the examples from poetry or pragmatic meaning (above), this need to be internal to verbal processing, even if not a direct participant, becomes increasingly obvious. We cannot operate as purely external observers. Being internal to the communication process means being able to operate with the conventions, associations, grammatical patterns, and social-aesthetic values of the language under consideration and being able to

[18] An interesting case is Mao Zedong's's aphorism *wang3 qian2 zou3* ('go forward') which is ironically exactly homonymous in tones and characters with 'go for the money'. One needs the full Chinese background to get an appreciation of the expression.

[19] Non-French speakers might recognise the sound as different from their own repertoire.

recognise the functions of an utterance in its context. We delude ourselves if we believe that an understanding of language is possible without being internal to the process or that it can be achieved in some purely objective manner through external observation only. That means that the linguist-observer is also not a purely external knowing subject. Observation implies being part of a communicational totality, even if we are not active participants. The same goes for Quine (1961, 13*ff*) when he presents us with an observer's view of 'how words are learnt'. His ideas (however useful), while apparently a matter of empirical observation, are possible only because he understands English- i.e. he is internal to the processes he is 'observing' (and describing).

What the linguist-observer or philosopher can add is, of course, a descriptive/explanatory coherence to the representation of language, but that is achieved through further stages of interpretation in relation to some theoretical or methodological viewpoint. Any such viewpoint is not only in need of justification, it arises from complex processes of education and training as well as unconscious cognitive processes and preferences, which we represent to ourselves in the form of reasoned argument. Those representations of theory and method are themselves, however, the endpoints in a complex communicational totality, rather than a matter of purely impartial decision. Furthermore, as will be obvious, our rationalisations of verbal communication are themselves verbal and, thus, also part of our integration into a complex verbal totality. The fact of needing language to discuss language means that one can never escape the 'loop of language' (Rastall 2000, 215*ff*) to an external, objective viewpoint. That we may be persuaded by an argument or consent to an observation or description is not a sufficient argument for objectivity; it is a matter of reasonableness in our current state of knowledge. Paradigms change.

A connected way of looking at that is to note that the adoption of an objective viewpoint carries with it the idea that objectivity leads to truth or at least an increasingly close approximation to it. That is obvious from Ayer's words (above). Thus, the linguist-observer, the philosopher, or speaker-participant aims to use the 'objective' position of the knowing subject to arrive at truths concerning the nature of language (its 'reality') and/or the veracity of particular utterances. The ideas that verbal representation and understanding depend on participation in a communication process and the unconscious operation of linguistic cognitive processes, and that our

verbal world is produced through verbal conventions not only throw doubt on that objective position, but also on our ability to determine truth through verbal means in ordinary language behaviour. Truth in natural languages will depend on how we look at the world, our attitudes, and verbal conventions, and—for the linguist-observer applying rationally argued but essentially arbitrary procedures linked to a given theory and methodology—the picture of language will be a matter of presenting a coherent construct within a particular type of discourse, rather than an objective real-world truth. As Saussure wrote (1916/1972, 23) concerning the nature of the object of study in linguistics:

> D'autres sciences opèrent sur des objets donnés d'avance et qu'on peut considérer ensuite à differents points de vue; dans notre domaine, rien de semblable. Quelqu'un prononce le mot français *nu*; un observateur superficial sera tenté d'y voir un objet linguistique concret; mais un examen plus attentive y fera trouver successivement trois ou quatre choses différentes, selon la manière dont on le considère: comme son, comme expression d'une idée, comme correspondant du latin *nūdum*, etc. Bien loin que l'objet précède le point de vue, on dirait que c'est le point de vue qui crée l'objet, et d'ailleurs rien nous dit d'avance que l'une de ces manières de considérer le fait en question soit antérieure ou supérieure aux autres.[20]

This means that our linguistic understanding is a set of constructs determined by the theoretical-methodological starting points for analysis. (This seems to be also Hjelmslev's (1953) conception of a constructed reality.) For the speaker-participant also, the objective stance is the creation of a virtual world of constructs. We must now confront head-on the basic tenets of the correspondence theory of truth for ordinary language which is part

[20] 'Other sciences operate with objects given in advance and which one can then consider from different points of view; in our field, there is nothing of the sort. If someone pronounces the [French] word, *nu*, a superficial observer will be tempted to see in it a concrete linguistic object, but a closer inspection will reveal successively three or four different things: a sound, an expression on an idea, a correspondence with the Latin *nūdum*, etc. Far from the object preceding the point of view, one would say the point of view creates the object, and furthermore nothing tells us in advance that one of these ways of considering the fact in question is prior or superior to the others.' [trans. PR]

and parcel of the division between the knowing subject and the external object.

5. Truth as the correspondence of language and fact

Let us consider Tarski's standard (and much debated) example above. We have on the one hand a verbal expression, *snow is white* and on the other the factual question of the colour of snow. The verbal expression is said to be true if and only if the entity named by *snow* indeed has the property of being white. This apparently common-sense position seems to imply that we can recognise as a matter of fact, and without verbal mediation, the entity, snow, except insofar as the expression, *snow*, directs us to an entity in external reality—the real-world value of a variable.[21] Furthermore, we should similarly be able to recognise, again without verbal mediation, what it is to be white, when directed to that property by the expression *is white*. If there were verbal mediation—i.e. if our recognition of real-world snow and the property of being white were dependent on linguistic convention—, then at best we would only be able to determine the truth of *snow is white* for a particular verbal meaning in a particular language. This would be 'truth' for a particular interpretation, and it seems to be Davidson's position (above). Truth in this sense would depend on the particular conventions of particular languages. We could not arrive at any sort of 'ultimate truth' about the non-linguistic world, because the non-verbal entity and property would be identified via an arbitrary verbal definition or meaning. The distinction between the knowing subject and the external object would be far less clear-cut, because the recognition of the external object would depend on the language conventions of the knowing subject's community. We must ask, is there nevertheless an entity, snow, which can be identified separately from an area of experience referred to by the word, *snow*? Indeed, can we identify what it is to be white without the verbal mediation of the word, *white*? And can we specify the connection between

[21] Of course, the same comments apply to white as to green above, i.e. that it is a secondary quality which is a matter of appearance rather than an inherent reality, but Tarski's example is clearly intended as representative of any veridical judgement.

the putative entity, snow, and the property of being white? (And, of course, *mutatis mutandis*, for any other assertion.)[22]

The problem revolves around the conventional nature of language. In English, we find a range of expressions, *snow, slush, sleet, hail*, which refer to similar phenomena. The identification of a meteorological phenomenon as specifically snow (and not slush or hail, for example) seems to depend on verbal conventions in English (and the judgements of individual speakers in speaking), rather than on clear differences between entities. The indeterminate nature of the reference of linguistic signs is well known. It was discussed by Bühler (1934/1990, 75-76) long ago, but its implications have not always been thought through. The 'fuzzy edges' around the reference of *snow* suggest that there is no discrete entity, snow, but rather that there is an indeterminate range of phenomena which can conventionally be referred to by *snow*. In terms of our internal models, we accept as snow whatever meets something in the range of possibilities covered by the English expression, *snow*, i.e. is consistent with our everyday understanding and experience

Furthermore, although there is some dispute over the number of words for snow in 'Eskimo' languages, it is known that there is no general word ('hyperonym') for all types of 'snow', and that there are different verbal conventions from those in English. In one dialect, we find *aput* 'snow on the ground', *qana* 'falling snow', *piqsirpoq* 'drifting snow' and *qimuqsuq* 'snowdrift', all of which would correspond to 'snow' in English, but which would not be 'the same thing' in this variety of Inuit—the internal model would have a reality in which there were four entities. So, can we identify a unique entity, snow, without verbal mediation? It would seem not. We need our knowledge of English to tell us what counts as snow. In other words, our factual world is partly verbally constructed.

The fact that all languages make different conventional distinctions is well-known and obvious to anyone with experience of different languages. Colour terminology is an obvious (if contentious) case in point, but the Russian distinction of *goluboj* ('pale blue') / *s'in'ij* ('dark or intense blue') with no hyperonym for all cases of 'blue' is well established, for example.

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[22] For a further discussion of Tarski's correspondence theory and the 'deflationary theory of truth' touching on connected issues to the ones here, see Stoljar & Damnjanovic (2014) in the *Stanford Encyclopedia of Philosophy.*

Can we, then, identify what it is to be white without verbal conventions? We might point to a range of wavelengths in the visible spectrum, although that hardly corresponds to our lived experience of white.[23] Anyone who has done some home decorating will know the range of possibilities for white paint—rose white, peach white, lace white, grey white, etc. Which, if any, of the interpretations of *white* is intended for snow?[24] Similarly, it can be difficult to match clothes or furniture with different whites. Would *snow is white* be untrue under the interpretation 'rose-white', or would rose-white not be white? 'White' is, of course, a secondary quality, and therefore a matter of appearances—the way things seem to us because of the way our brains work, rather than an ultimate reality, so again it seems we cannot recognise the property of being white without those cognitive processes and the conventions of English.[25] Of course, we could say that anything we judge to be within the range of indeterminacy of *white* can be attributed to whatever we judge to be snow, but that view does not escape, but actually embraces, the verbal contribution to the construction of reality. Furthermore, we are left with the arbitrariness of the distinction between white and grey, or rose-white and pale pink.

While on the subject of conventionality, one must point out that the verb *to be* in English also covers a range of possibilities including universality (*water is necessary for human life*), permanence (*the arctic is in the northern hemisphere*), classification (*gold is a metal*), specification (*Fred is the winner of the race*), definitional (*two and two is four*), temporary state (*the postman is outside*), equivalence (*the morning star is the evening star*), existence (*there is a tree in our garden*), or a combination of functions, etc. Other languages can, and do, have different words for these cases (e.g. in Spanish there is a distinction between permanent and temporary states) and many languages have specific expressions for existential assertion, French

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[23] Bronowski (1979, 126) makes a similar point about 'red'.

[24] The enormous complexity of colour terms and their associations is well illustrated by Jacquet-Pfau in the series, *Dictionnaire de la couleur*, e.g. *Le gris*, 2015. Colour terms and their application are not simply matters of asserting an objective quality.

[25] Russell's empiricist use of 'sense data' (1912, 1-12) does not solve the problem here; it retains the knowing subject and replaces the external object with the brain's response to its appearances while taking no account of linguistic conventions in their organisation.

il y a, German *es gibt*, Chinese *you*[3], etc., which are not parts of the verb 'to be' in those languages.[26] While in English one might *be 22 years old*, in French *quelqu'un a 22 ans*, while in Russian *jemu 22 goda* ('to him 22 years'), and Chinese has different ways of asking the age of children (*ji*[3] *sui*[4]*le*), older children and peers (*duo*[1]*da*[4]), and older people (*gao*[1]*shou*[4] as a term of respect). *Be* also has a conventional range of indeterminacy. It is an unspoken assumption of correspondence theorists that there is only one relation expressed by the verb, *is*, or that it is conveniently the one that logicians use in logic. Again, it seems, we need the verbal interpretation of *snow is white* to understand which facts might be relevant and how they are to be found. We can add that snow—of any sort—can look blue, or even red, in certain conditions, and—as anyone will know—it can look distinctly grubby and grey or black at the end of winter (or at night). It seems the Tarskian approach to truth depends on our being internal to English communication to know what is referred to and predicated (at least in English), and on an assumption of some inherent property of whiteness in a supposedly identifiable entity, snow. This is hardly a sound basis for a correspondence theory of truth—at least for the macro-level reality of language and the macro-level realities it refers to. Rather, it suggests that the verbal product (such as *snow is white*) is *consistent with* at least some (or many) experiences of the things we call (in English), *snow*. Similarly, *the postman is outside* would be consistent with the experience of seeing someone we designate with the term, *postman*, and his location relative to us. Of course, one can add that the lived reality of *outside* is different for all of us, i.e. where we place the postman in our local environment depends on our particular reality.

The conclusion that we cannot determine the truth of a sentence without being internal to the communication system in order to identify entities, properties, and relations again brings into question the dichotomy between the knowing subject and the external object. The appearance of an objective stance for a judgement of correspondence of language with fact ignores the role of conventional verbal mediation in determining what are the relata of the correspondence. One is left with a coherence theory of truth[27] for

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[26] Of course, there is no such thing as a universal verb, *to be*. There are similarities of usage across languages.

[27] Described by, for example, Ewing (1951, 55*ff*).

natural languages and language analysis. Furthermore, the view that language determines the nature of our everyday reality implies that we must reverse the standard doctrine that meaning implies truth. Rather, what we consider to be truth implies meaning, because we cannot assess the coherence of a statement with our model of reality without knowing the meaning of the statement and how it contributes to our model of reality.

6. Conclusions

We have questioned the dichotomy of the knowing subject and external object in the cases of the speaker-participant and the linguist-observer/language analyst. In the case of the speaker-participant, the objective stance of the speaker as a knowing subject towards the signal message is an appearance created by cognitive processes as part of our representation of reality. Whether acting as a sender (with awareness of one's own verbal productions) or as a receiver of verbal signals/messages from others, one must be 'internal' to the communication process; our awareness of language depends on verbal associations, cognitive processes, and the construction of speech which are well beyond individual control, and which are acquired from and consistent with one's language community. The 'knowing' of the knowing subject is a product of those processes and the integration of the individual into a complex totality. That is, the speaker or analyst is neither 'unique', nor 'external'; they are integrated into both the communication process and into the language community on which they are dependent for verbal understanding and activity. Language products as we know them are not 'external objects' but the creations of cognitive processes.

The focus on the objective stance of the speaker-participant is a concern with the verbal behaviour of the individual dissociated from the activity of the individual as a part of a social body and from the socially acquired and non-conscious verbal associations and processes in the brain. All verbal processes are connected to other sets of constructs of the perceptual world, memories, concepts and attitudes. The objective stance, itself in need of closer analysis, allows a representation of the comparison of verbal constructs with non-verbal constructs, insofar as they can be disentangled. This could be seen as the assessment of truth for the individual at the macro-

level of conscious experiences. It is presumably helpful for our social orientation and behaviour.

In the case of the linguist-analyst, similar points apply. Language analysis is not possible unless the language analyst (as knowing subject) is internal to the communication process for the construction and recognition of verbal signals and messages with similar cognitive processes to those of speaker-participants. The objective stance of the language analyst is to that extent illusory. The selection of theory and methods as well as the selection of relevant observations allows a basis for rational discussion, but those selections themselves—i.e. the determination of which parameters of communication are relevant, and which theories and methods to select—are themselves matters of long-term training and subconscious preferences. They can be justified inter-subjectively as appropriate, but cannot be regarded as leading to 'ultimate' truths about the nature of language. The descriptions and explanations of analysts are constructs for the understanding with claims to reasonableness, but indefinitely many perspectives are possible on the same sets of data- as Saussure (above) pointed out.

Furthermore, the macro-level understanding of language is concerned with linguistic reality as it appears to us and the focus is on the language of the individual, rather than on language as a communicative mechanism of the social totality. In looking at natural language statements from the perspective of truth, philosophers have underestimated the conventionality of language and the latitudes of indeterminacy of natural languages involved in the interpretation of sentences, and hence the assessment of their truth. The allegedly objective stance of the philosopher seeking truth as a correspondence of language with fact is again that of the knowing subject observing an external verbal object and comparing it with states of affairs and entities in the non-verbal world (or comparing reified verbal objects for consistency). At least for natural languages and their macro-level appearances in the form of sentences, philosophers, like linguist-observers, are internal to verbal communication for the recognition and construction of verbal products. But the recognition and construction of verbal products involves the implicit application of arbitrary conventions for the reference of linguistic signs and allowance for their indeterminacy of reference. What we regard as snow or white is not simply a matter of facts about the world; what we regard as snow or white involves a large measure of verbal convention and indeterminacy. We thus arrive at truth as it appears to us from

a range of perspectives; but that truth is a matter of consistency of appearances within a given language community, not correspondence with fact—at least for natural languages. If this is correct, then the linguist's claim to scientific objectivity is greatly undermined, and a coherence theory of truth is needed. That is, the 'scientific' status of linguistics cannot rest on a correspondence theory of truth, as it currently does. Its alleged objectivity is illusory. (See also Rastall 2011 for discussion of this point from a different perspective.) A coherence view of truth for our macro-level everyday purposes and in a 'model dependent reality' implies that our notion of truth is dependent on meaning; i.e. our informational model of reality. Hawking and Mlodinow (2010, 216-217) explain 'model-dependent realism' as follows for physical and perceptual systems:

> According to the idea of 'model-dependent realism'...our brains interpret input from our sensory organs by making a model of the outside world. We form mental concepts of our home, trees, other people, the electricity that flows from wall sockets, atoms, molecules, and other universes. These mental concepts are the only reality we can know. There is no model-independent test of reality. It follows that a well-constructed model creates a reality of its own.

Our verbally constructed reality in communication can also be considered a model which can be compared with other models for coherence in terms of its state and predictions. Thus, *Snow is white*, Burns' description of a fieldmouse, Mulder's (1968) description of Chinese phonology, or Popper's *Objective Knowledge* are verbally constructed realities that can be compared with perceptual and other verbal constructs for consistency in multiple dimensions. In everyday practical interaction, that test of consistency applies 'in normal circumstances' for the language under consideration (like boiling point at standard temperature and pressure). 'Coherence' might be seen in terms of 'resemblance' or 'tolerance'—sameness in a relevant respect. Hume's (1748/1968: 192*ff*) account of our sense of constancy of the world and the need for the explicability of change is similar, and also implies a comparison of mental with sensory models. This position implies (as does that of Hawking and Mlodinow) that we may improve or correct our models, but we can never escape models for the understanding (of language or anything else), although Popper's idea of

intersubjectivity provides a further test of coherence with a wider community of thought.

Above we compared human communication with that of ants. Are humans just very complex (and rather self-important) ants? At the least, we should give more attention to the role of language in the totality of human communities and organisation, and to the position of the individual as a component in that totality. That implies a dynamic relationship in which a sense of reality is created in multiple dimensions through a combination of verbal and perceptual experiences with verbal and non-verbal associations and expectations in the internal models. This is a form of 'reality as interaction' (Rovelli 2015, 18). That sense of reality in each individual allows the integration of the individual into the social totality.[28] The linguist's (or language analyst's) job would then be to find coherent accounts of that dynamic.

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[28] This view is consistent with the idea that consciousness is an 'emergent property' and the brain's representation of its activity to itself as part of a connected single totality; this would address the so-called 'paradox of subjectivity' ably discussed by Carr (1999) in the work of Kant and Husserl. The subject 'in the world' and 'for the world' are parts of a single totality.

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Cross-World Comparatives for Modal Realists

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ABSTRACT: Divers (2014) argues that a Lewisian theory of modality which includes both counterpart theory and modal realism cannot account for the truth of certain intuitively true modal sentences involving cross-world comparatives. The main purpose of this paper is to defend the Lewisian theory against Divers's challenge by developing a response strategy based on a degree-theoretic treatment of comparatives and by showing that this treatment is compatible with the theory.

KEYWORDS: Comparatives – counterpart theory – David Lewis – John Divers – modal realism – modality.

1. Divers's challenge to a Lewisian theory of modality

My aim in this paper is to answer a challenge for Lewis's theory of modality, consisting of modal realism plus counterpart theory,[2] which has recently been raised in Divers (2014). The basis of Divers's objection are certain intuitively true sentences containing modal comparatives such as:

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[2] For the main statement and defence of modal realism, see Lewis (1986), for counterpart theory, see in particular Lewis (1968), (1983c), and (1986, Ch. 4, 192ff).

(1) It is true of the tallest actual thing that it might have been taller.
(2) It is true of the fastest actual thing that it might have been faster.
(3) It is true of the actually longest lasting thing that it might have lasted longer.

I will focus on (1) from now on, but the crucial claims made throughout the paper generalize. According to counterpart theory, (1) is true if, and only if, the tallest actual thing has a counterpart which is even taller. This counterpart can either be an object which exists in the actual world or one which exists in a merely possible world. There is no actual thing taller than the tallest actual thing, so the required counterpart must exist in a merely possible world. This can, argues Divers, not be the case, since i) two objects need to be spatiotemporally related in order for them to stand in the 'taller than'-relation and ii) modal realism rules out spatiotemporal relations between objects that exist in different possible worlds. This is a problem, since the Lewisian theory is supposed to respect established 'pre-philosophical' opinions about what is possible.[3]

It is crucial to Divers's challenge that the comparisons in (1) – (3) involve spatiotemporal magnitudes. Comparisons not involving them, such as for example, 'It is true of the longest poem authored by a human that it might have been longer', are not subject to Divers's claim i): The magnitude involved here is that of the number of words or of letters in a poem and there is no reason to think that two poems which are comparable regarding their length so understood have to stand in spatiotemporal relations. For this reason, this and similar cases do not give rise to Divers's challenge. As Divers himself points out, this means that a natural response to his challenge is to deny i) (lemma b in Divers 2014), i.e. the claim that modal comparisons of the sort drawn in (1) – (3) require the compared objects to stand in spatiotemporal relations.

According to Divers, friends of the Lewisian theory of modality who make this move have to face three difficulties. First, Divers suggests that this response might require extensive and deep revisions of their adopted metaphysics of spacetime and modality, second, it might give rise to revenge problems, and third, and finally, it appears that Lewis himself explicitly objected to a particularly natural response strategy which makes

3 See Divers (2013, 186).

this move. (See Divers 2014, 577.) The core idea of this response-strategy is that 'we might simply instate inequalities between numbers in place of relations between non-numerical things. So *x* being taller than *y* requires 'only' that there be numbers *n* and *m* such that …and *n* is greater than *m*' (ibid.). My main aim in this paper is to argue that, even assuming a relatively orthodox Lewisian perspective, none of these three difficulties pose a genuine problem for a response strategy of this sort. I will do that by developing a particular version of the strategy, which I will in the following call the *Degree Strategy*.[4]

2. The Degree Strategy: the basic idea

The main aim of the Degree Strategy is to deliver satisfiable counterpart theoretic translations of Divers's (1) and similar comparative sentences which involve comparisons between degrees instead of objects. To make this clearer, let me introduce a counterpart-theoretic rendering of (1) to give an example of how this might be done:

$$\text{(D1)}\quad \exists v(Aa \wedge Tav \wedge \forall w \forall x((Aw \wedge w \neq a \wedge Twx) \rightarrow x < v) \wedge \exists y(\exists z Cza \wedge Tzy \wedge v < y))$$

Here, *a* is a singular term,[5] *Cxy* says that *x* is a counterpart of *y*, *Txy* says that *x* is tall to degree *y*, *Ax* says that *x* is actual, and < is the *greater than*-relation for degrees. In words, (D1) hence says that there is a degree of tallness *v*, such that it is the degree of tallness of the actual thing *a* which is larger than the degree of tallness of any other actual thing, but that there is a counterpart of *a* which has a higher degree of tallness. The example

[4] For a different response to Divers's challenge, see Noonan & Jago (2017).

[5] The official language of counterpart theory as introduced in Lewis (1968) contains no singular terms, but rather treats names as definite descriptions in the manner described in Russell (1905). The Russellian method requires one to fix the scope of the relevant descriptions in modal contexts, as Lewis points out (ibid, 120f). Regarding the counterpart-theoretic sentences discussed in this paper, these descriptions can be assumed to take wide scope under the modal operator as discussed on p. 121 of ibid, since they correspond to de re modal claims. Officially, (D1) should hence be read as an abbreviation of the respective singular-term-free rendering.

reflects the two core ideas of the Degree Strategy: First, that comparisons of objects in terms of their spatiotemporal magnitudes can always be spelled out in the language of counterpart theory in terms of comparisons between degrees corresponding to these magnitudes. Second, that the resulting counterpart theoretic-sentences do not require the objects whose magnitudes are being compared to be in the same possible world. Both of these ideas, modulo their application in counterpart theory, are well-known from the existing literature on the semantics of modal comparatives. (See e.g. Cresswell 1990, Ch. 5.)[6]

Note that in producing this translation of (1), I did not rely on the manual for translating first-order modal logic into counterpart theory provided in Lewis (1968). Instead of first translating (1) into first order modal logic and then into counterpart theory, I rather directly relied on the resources of counterpart theory to produce (D1). I assume that this is a legitimate move from an orthodox Lewisian perspective, for two reasons.

First, the translation manual in Lewis (1968) was not intended to fix a general methodology for the use of counterpart theory, according to which the one and only way to arrive at Counterpart-Theoretic renderings of a modal sentences is to first translate them into the language of first-order modal logic and then to translate this first translation into the language of counterpart theory. Rather, Lewis introduced the translation manual to make an important point about the expressive strength of the language of counterpart theory:

> If the translation scheme I am about to propose is correct, every sentence of quantified modal logic has the same meaning as a sentence of counterpart theory, its translation; but not every sentence of counterpart theory is, or is equivalent to, the translation of any sentence of quantified modal logic. Therefore, starting with a fixed stock of predicates

[6] Note that (D1) is based on a simplistic implementation of a degree-based semantics for comparatives and that I make no claim that this implementation lives up to the best available linguistic theories of comparatives. (D1) should however serve the purposes of this paper well, since linguistically more sophisticated implementation of the Degree Strategy would have to face the same metaphysical questions which I will focus on in this paper.

> other than those of counterpart theory, we can say more by adding counterpart theory than we can by adding modal operators. (Lewis 1968, 117)

This point is part of his sales pitch for counterpart theory to philosophers who follow the standard approach to the formal treatment of modal sentences in terms of first-order modal logic.[7] A Lewisian who insisted that any translation of a modal sentence of natural language into the language of counterpart theory must proceed via the translation manual, would undermine Lewis's efforts in this direction, since this procedure would preclude counterpart theorists from relying on the additional expressive resources offered by their theory. The second reason is that Lewis himself later explicitly expressed a preference for working directly with the language of counterpart theory.[8]

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[7] This reading is strongly suggested by the first two paragraphs of the paper: *'We can conduct formalized discourse about most topics perfectly well bv means of our all-purpose extensional logic, provided with predicates and a domain of quantification suited to the subject matter at hand. That is what we do when our topic is numbers, or sets, or wholes and parts, or strings of symbols. That is not what we do when our topic is modality: what might be and what must be, essence and accident. Then we introduce modal operators to create a special-purpose, nonextensional logic. Why this departure from our custom? Is it a historical accident, or was it forced on us somehow by the very nature of the topic of modality? It was not forced on us. We have an alternative. Instead of formalizing our modal discourse by means of modal operators, we could follow our usual practice. We could stick to our standard logic (quantification theory with identity and without ineliminable singular terms) and provide it with predicates and a domain of quantification suited to the topic of modality. That done, certain expressions are available which take the place of modal operators. The new predicates required, together with postulates on them, constitute the system I call counterpart theory.'* (Lewis 1968, 113)

[8] *'What is the correct counterpart-theoretic interpretation of the modal formulas of the standard language of quantified modal logic? – Who cares? We can make them mean whatever we like. We are their master. We needn't be faithful to the meanings we learned at mother's knee – because we didn't. If this language of boxes and diamonds proves to be a clumsy instrument for talking about matters of essence and potentiality, let it go hang. Use the resources of modal realism directly to say what it would mean for Humphrey to be essentially human, or to exist contingently.'* (Lewis 1986, 12-13) See also the initial passage in Lewis (1993a, 69).

A crucial question about the Degree Strategy is what degrees are. I will here assume that they are either numbers of whichever sort are semantically required, or in some more complicated cases, such as comparisons of e.g. similarity, ordered or unordered sets of (sets of …) numbers.[9] I will also assume that comparative predicates which take degrees as at least one of their relata implicitly specify a particular measurement scale. Since Divers's challenge is based on comparative sentences involving simple comparisons which can be accounted for by single numbers instead of e.g. sets of them, I will for the most part focus on such cases. Further details of the Degree Strategy will be spelled out in the following subsections in direct response to Divers's three worries.[10]

3. Does the Degree Strategy require deep revisions of Lewisian metaphysics?

Divers's first worry is that response strategies which are based on a denial of i) may 'require extensive or deep revision' (Divers 2014, 577) of Lewisian metaphysics of spacetime or modality. To address this worry for the Degree Strategy, we have to first make clear which metaphysical requirements this strategy imposes. Its three crucial metaphysical requirements are that a) it must accommodate the view that de re-ascriptions of predicates like 'being tall' to possible objects involve a degree of tallness, that b) degrees are comparable across different possible worlds via the 'is (strictly) greater than'-relation $<$, and finally, that c) the ordering of degrees induced by $<$ tracks the ordering of the objects to which they are assigned relative to the relevant dimension of comparison. I take it that any substantial or extensive revision of Lewisian metaphysics which the Degree Strategy might require would be traceable to one or more of these three requirements.

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[9] See Balcerak Jackson & Penka (2017) for a critical discussion of this assumption in the context of linguistic theories utilising degrees.

[10] Note that Schwarzschild & Wilkinson (2002) argue that a linguistically adequate semantics for comparatives requires intervals, rather than degrees, but I will, for the sake of simplicity, stick with degrees. Since intervals are just sets of numbers, all points I am going to make about the degree-based version could easily be generalized to an intervals-based version.

From an orthodox Lewisian perspective, requirement a) poses no special metaphysical problem, since it indeed perfectly matches Lewis's own view of 'properties that admit of degree' (Lewis 1986, 53), for which he suggests a bifurcated treatment: There are both 'families of plain properties: the various lengths, the various masses' and 'relations to numbers, such as the mass-in-grams relation that (a recent temporal part of) Bruce bears to a number close to 4,500' (ibid.). Accordingly, if an object has a mass, then it both has a plain mass-property and stands in various relations to numbers, each of which specifies its mass on a certain measurement scale. This means that proponents of the Degree Strategy can help themselves to relational properties which are already present in Lewis's ontology. Even the more complicated cases at which I hinted, which require degrees to e.g. be sets of numbers pose no problem in this regard. Lewis of course allowed sets of numbers in his ontology and relations between them.[11] Requirement a) imposed by the degree theory hence entails no deviation from standard Lewisian metaphysics.

What about requirement b), the requirement that degrees are <-comparable across different possible worlds? As just pointed out in response to the analogous question about requirement a), the degree theory is conservative regarding orthodox Lewisian ontology, in the sense that it does not require the introduction of new objects which do not already exist according to Lewis. This means that the question can simply be answered by showing that orthodox Lewisian metaphysics satisfies requirement b), i.e. that it entails that degrees are <-comparable across different possible worlds. Or equivalently, and this is the way I will go here, by showing that Lewisian metaphysics cannot fail to meet the requirement. How could it fail to do so? By allowing for at least one of two kinds of variance, first, variance in which numbers, and sets (of sets …) of them, are available from the perspective of different possible worlds, or second, variance regarding which mathematical relations hold between them in different possible worlds.

We can immediately rule out that orthodox Lewisian metaphysics allows for variance with respect to which degrees/numbers exist from the standpoint of possible worlds, since Lewis himself explicitly accepts that

[11] They correspond to properties of numbers and relations which hold between properties of numbers respectively. See Lewis (1986, section 1.5, 50ff).

numbers are 'necessary beings' (Lewis 1983a, 198). By this, he doesn't mean that they exist in every possible world. Rather, according to Lewis, 'numbers […] inhabit no particular world but exist alike from the standpoint of all worlds, just as they have no location in time and space but exist alike from the standpoint of all times and places' (Lewis 1973a, 39). Since degrees are numbers, or sets of them, the first kind of variance which could undermine requirement b) is not allowed by Lewisian metaphysics.[12]

So what about variance regarding the mathematical relations in which degrees, or sets of them, stand from the perspective of different possible worlds? In case of single degrees, the relevant mathematical relation is of course <, the (strictly) greater than-relation. By Lewis's lights, < is an internal relation between degrees, a relation which supervenes on the internal properties of its relata. This means that there can be no variance in whether two degrees are <-related between worlds. Let me explain this in a bit more detail.

To make this point, we need to look at Lewis's definition of an internal relation. According to Lewis, a diadic relation, i.e. an ordered set with two elements in his ontology, is internal if, and only if, 'whenever *a* and *a'* are

[12] In his later writings Lewis (1991) and (1993b), Lewis argues that, given 'some hypotheses about the size of Reality' (Lewis 1993b, 3), mathematical entities and the whole of mathematics can be reduced to megethology, that is, mereology plus plural quantification. There is one aspect of the resulting view which might seem to threaten the Degree Strategist's ability to fulfill requirement b), namely Lewis's neutrality regarding the question of whether sets and in particular the empty set, which serves as the basis for set-theoretical constructions of numbers, are spatiotemporally located (see Lewis 1993b, 13). A Lewisian who adopts Lewis's view should hence be prepared to at least seriously consider the idea that the empty set and with it also the numbers are in spacetime. Indeed, Lewis seriously considers, if not endorses, the idea of identifying the empty set with an arbitrary elementless object, e.g. with the fusion of all ordinary objects in a world (see Lewis 1993b, 9). Does this not mean that there are different empty sets and therefore also different numbers and different degrees in different possible worlds and does this not threaten the Degree Strategy? Not if the Lewisian also follows Lewis in adopting a structuralist view of mathematics (ibid. 15-17). On this view, the same mathematical structure, e.g. that of the rational numbers, might indeed be instantiated by different objects in different possible worlds, but since the structure remains the same across all possible worlds, degrees nonetheless remain cross-world comparable.

duplicates (or identical) and b and b' are duplicates (or identical), then both or none of the pairs $\langle a, b\rangle$ and $\langle a', b'\rangle$ stand in the relation.'[13] For our purposes, it is the identity-version of the definition which is relevant. This is so, because, as mentioned earlier, Lewis assumes that numbers don't exist in possible worlds, but rather outside of them. (See Lewis 1973a, 39.) Since he nonetheless assumes that they should be available in all possible worlds, Lewis stipulates that with respect to each possible world, each number acts as its own unique counterpart, thereby making it de re necessary of them that they exist. (See Lewis 1973a, 40.) Since a duplicate of an object is either a distinct object in the same world or a counterpart in a different world which shares all of the object's perfectly natural properties (see Lewis 1986, 61), this uniqueness assumption entails that the only duplicate of a number is that number itself.

Based on this explanation, we can apply the definition to the <-relation to show that it is satisfied. It tells us that for any two numbers n and m and any pair of their duplicates n' and m', $<$ is an internal relation with respect to n and m if, and only if, either $n < m$ and $n' < m'$ hold or neither of $n < m$ and $n' < m'$ holds. Since we have just seen that n and n' and m and m' are identical, this is trivially the case, since m is either strictly greater than n, or not. So $<$ qualifies as an internal relation between numbers because whether two numbers actually (or possibly) stand in $<$ settles once and for all whether they stand in the relation with respect to all possible worlds. This of course rules out the problematic cross-world variance in whether degrees are <-related.

An important question is still left unanswered, namely whether this mathematical ordering between degrees successfully tracks the relevant dimension of comparison. This is exactly the question at issue regarding requirement c). To meet this requirement, Lewisians have to ensure that e.g. an actual object associated with a height-degree higher up in the <-ordering than an object in a non-actual world also has a greater (plain) height than the non-actual object.

According to Lewis's view, deviant cases in which this is not the case can only arise if there is a mismatch between the object's relevant plain

[13] Lewis (1983b, 356, footnote 16). Note that Lewis switched from using the term 'intrinsic relations' in this definition to 'internal relations' in Lewis (1986); I follow the latter usage.

measurable property and at least one of its corresponding relational properties involving numbers. Degree strategists can therefore avoid the problem by ruling out such deviant cases. They have at least two different ways to do this, one more and one slightly less orthodox.

Let me introduce the slightly less orthodox solution first. It requires one to build a further factor into the degree-based semantics for gradable adjectives, a factor which enforces the required harmonious relation between the degrees associated with the compared object and their corresponding intrinsic properties. This factor could for example be a relation which maps equivalence classes of possible objects to sets of numbers which capture their relevant dimensions on different scales. Height for example would then be treated as a relational property of an object which both involves a degree and a scaling relation, a mapping of all objects of the same height to the relevant degrees, all of course relative to a particular measurement scale. To give an example, if *a* is an object with e.g. a height of three meters, this would mean that the relational property salient to evaluating the truth of a sentence comparing *a*'s height to another possible object would involve the number representing *a*'s degree-in-metres and a scaling relation which maps a set of possible objects which have the same intrinsic height-property as *a* (i.e. its height-duplicates) to the same number representing its degree-in-metres, which in the given example would be 3. In this modified framework, the deviant cases which degree strategists have to rule out would involve a mismatch between the scaling relations involved in the relational properties involved in their analysis of the relevant comparative sentences. Such cases could therefore be ruled out by stipulating that only those comparisons are apt to be true which involve the relevant relational properties which involve the same scaling relation.[14]

[14] One might worry that there is a threat to this approach from possible worlds which are very different from the actual world regarding the quantitative properties of the objects existing in those worlds. A simple example of such a world is one in which all height-properties have always been exactly double that of the actual height-properties. This case is e.g. discussed in Dasgupta (2013). One might argue that height-comparisons between objects in this and the actual world might undermine the proposed modification of the Degree Strategy, since e.g. an actual object and an object from this 'height-doubled' world might be scaled to the same height-in-metres, giving rise to cases in which the latter object has a lower height-in-metres but has a larger intrinsic height than the actual object. A simple way to address potential problem cases of this sort is

This modification of the Degree Strategy also requires no deep revisions of Lewisian metaphysics. The scaling relations on which it relies are metaphysically innocent in the sense that they are just regular relations whose availability is guaranteed by the Lewisians commitment to an abundant view of relations. This means that the further relativization of gradable properties to scaling relations poses no special metaphysical problem over and above those posed by the relativization of gradable properties to degrees, a relativization which is already built into Lewisian metaphysics. Since the same holds for the doubly-relativized properties, Lewisians who rely on the Degree Strategy can meet requirement c) without having to revise their fundamental metaphysics in any significant way. Why then did I call this variant of the Degree Strategy slightly less orthodox? Because it requires degree theorists to rely on relations which, while ontologically unproblematic, are not the simple relations between material objects and numbers which Lewis officially accepts. (See again Lewis 1986, 53.)

The second, more conservative, way to rule out deviant cases leaves the original degree-theoretic semantics as it is and lets the counterpart relation do all the work. In various places, Lewis relies on an ordering of possible worlds regarding their similarity to the actual world. (See e.g. Lewis 1973a.) Such an ordering can be used to restrict the set of relevant counterparts with respect to a particular comparative sentence to those which exist in worlds which are closest to the actual world regarding the measurement structure of the relevant intrinsic quantitative properties. Accordingly, the objects whose degrees are compared in a Degree Strategic translation of such a sentence are always guaranteed to be in worlds which agree on the scaling between the degrees to which they have the relevant spatiotemporal magnitude and their corresponding intrinsic properties. More could of course be said about this and the preceding proposal, but this brief sketches together with what was just said about requirements a) and b) should suffice to illustrate that Lewisians have more than enough resources to implement a version of the Degree Strategy without deeply or extensively revising their metaphysics.

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to invoke a similarity ordering between possible worlds of the sort used to formulate the theory of counterfactuals in Lewis (1973a). The idea would be to rule out that objects from worlds of this sort can enter into comparisons with actual objects.

4. A revenge-problem?

Divers's second worry is that the problem illustrated by (1) and similar comparative sentences could be reinstated for the degree-strategist's surrogate-relation <, the *(strictly) greater than*-relation. In the background again is Divers's assumption i), which says that any two objects need to be spatiotemporally related in order to give us a true instance of a comparative predicate like 'is taller than'.

The degree-strategy explicitly denies that objects need to be spatiotemorally related in order for them to be comparable regarding their spatiotemporal magnitudes. But it does say that the corresponding degrees have to stand in a comparative relation such as <. In order to address this second worry, it still needs to be shown that this latter claim does not entail that the object and its counterpart involved in a (1)-like comparative sentence have to be in the same possible world. This entailment could hold in two cases: First, that two degrees are <-related could imply that they have to exist in the same world. Second, that they are so related could imply that the objects to which they stand in a particular magnitude-on-a-particular-scale-relation have to exist in the same world. I will address both versions of the worry in turn.

Let us first focus on the idea that the fact that the two degrees of lengths, velocities, heights, and so on which are associated with two comparable objects are <-related implies that the degrees have to be located in the same spacetime. This first version of the worry can easily be dismissed. Given the assumption that degrees are numbers, they are not in spacetime at all and can hence themselves not stand in spatiotemporal relations. (See again Lewis 1973a, 39-40.)

But what about the compared objects, i.e. what about the second version of the worry? That e.g. the height-degree associated with one object is higher up on the <-ordering than the height-degree associated with another indeed implies that each of the two objects is in spacetime. This is so, simply because only an object which is in a spacetime can have e.g. the intrinsic mass-property which it has in addition to being related to a certain numbers via a relation such as mass-in-grams. (See again Lewis 1986, 53.) Accordingly, the Degree Strategy cannot completely stay clear of metaphysical claims about the compared objects themselves. That however does not mean that Divers's second worry amounts to a genuine problem for the

Degree Strategy. One of the core ideas of the strategy is that while each compared object has to be in a spacetime, this need not be *the same* spacetime for each of the objects. This is illustrated by degree theoretic translations such as (D1). The Degree Strategy is hence not subject to the second version of the worry either.

One might argue that this is not yet enough to comprehensively address Divers's second worry. So far, I have equated being in a possible world with being in a spacetime. But in Lewisian metaphysics, being in a spacetime is only a sufficient, but not a necessary condition for being in a possible world. A core idea of modal realism is that in order to form a possible world, a collection of (possible) objects needs to be 'glued together' by a special family of relations. Lewis is forced to reject the attractive idea that the 'glue'-role is played exclusively by the actual spatiotemporal relations, because he wants to allow possible worlds that are instead 'glued together' by relations other than them, such as for example the quasi-spatiotemporal relations of Newtonian physics. (See Lewis 1986, 74-76.) So there are possible worlds which are not spacetime in the sense of contemporary physics. (1)-like problem cases could therefore still arise if the Degree Strategic treatment of modal comparatives implied that the relevant objects stand in 'analogically spatiotemporal' (Lewis 1986, 76), rather than spatiotemporal relations.

What does it take for a relation to be analogically spatiotemporal? According to Lewis, such relations have four characteristic properties, namely that of being natural, pervasive, discriminating and external. (See Lewis 1986, 75-76.) To address the objection, I will now argue that the relations between compared objects to which the Degree Strategy is committed do not conform to this characterization.

The first crucial point here is that $<$ itself is not analogically spatiotemporal, since it is, for the reason given in the previous section, an internal rather than an external relation between numbers. This means that the generalization of the first version of the worry to analogically spatiotemporal relations also fails to pose a problem for the Degree Strategy.

This still leaves open the possibility that the Degree Strategy implies that an analogically spatiotemporal relation obtains between the compared objects themselves, instead of between their associated degrees. To address this generalization of the second version of Divers's worry, we first have to get clearer on what the Degree Strategy tells us about the relations which

holds between compared objects. According to the strategy, to evaluate modal comparatives involving spatiotemporal magnitudes such as (1), one has to take into consideration the degrees to which the objects which exhibit these magnitudes are related. Since Lewisian metaphysics operates with an abundant conception of properties and relations, (see e.g. Lewis 1983b, 346) that the Degree Strategists is committed to the claim that the degrees associated with compared objects are <-related means that it is also committed to the claim that these objects themselves stand in a relation.

What sort of relation is this? Informally, it could best be described as the 'is associated with a degree higher up on the <-ordering than the degree associated with'-relation. Does this relation qualify as analogically spatiotemporal? One reason to think that this is not the case is that it is plausibly not a natural relation in Lewis's sense, since it does not 'carve reality at the joints' (Lewis 1983b, 346). This response might not satisfy all critics of the Degree Strategy, since it is based on an intuitive judgement about whether this relation is natural and such intuitive judgements are notoriously controversial.

Fortunately, there is a less intuition-dependent version of the response. According to Lewis, a relation can be denied the status of naturalness if positing its existence would be superfluous since 'we have the resources to introduce it by definition' (Lewis 1986, 77). It is easy to see that the relation which the Degree Strategy requires to hold between two compared objects fails to be natural by this standard: As its name says, two objects stand in it if, and only if, the relevant degree associated with the first is higher up on the <-ordering than the relevant degree associated with the second object. This relation is fully definable in terms of the relation which holds between the degrees. It is therefore not natural and for that reason also not analogically spatiotemporal.[15]

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[15] While Lewis himself somewhat hesitantly accepts naturalness as a necessary condition for analogical spatiotemporality in Lewis (1986, Section 1.6), Bricker (1996) defends the view that the only constraint placed on 'world-glue'-relations is that they have to be external. If 'is associated with a degree higher up on the <-order than the degree associated with' is an external relation, then a degree strategist who accepted Bricker's view would be forced to admit that it can be a 'world-glue'-relation, i.e. analogically spatiotemporal. Degree strategists can simply reject Bricker's view and side with Lewis to avoid this potential problem, but perhaps there is an argument to be made that the 'is

To summarize the argument of this section: Divers's worry is that the same problem which he illustrated using (1) – (3) might also arise based on the relations which the Degree Strategy requires to hold between either degrees associated with compared objects, or these objects themselves. A revenge-problem of this kind could arise if these relations implied that either the degrees or the objects involved have to be in the same possible world. The point made here is that neither the <-relation between degrees itself, nor the relation it induces between the objects associated with these degrees is a relation of the sort which could give rise to such a problem.

A possible objection one might raise against this conclusion is that the arguments provided fail to support it, since they focus on the wrong sort of relation. The idea of the objection is to insist that the relation which holds between two objects which we compare regarding e.g. their height must involve the intrinsic height-properties which objects have according to Lewis. (See e.g. Lewis 1986, 242.) But, the objection goes, the Degree Strategy completely ignores these intrinsic height-properties of objects and relies on a comparison by proxy via the <-relation.

I have two things to say in response to this objection. First, in one sense, it begs the question against the Degree Strategy. The basic idea of the strategy is exactly to provide a semantic analysis which does not involve the sort of relation which according to the objector, it should involve.

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associated with a degree …'-relation is not external. According to Lewis, external relations 'supervene on the intrinsic nature of the composite of the relata taken together' (Lewis 1986, 62). As pointed out earlier, Lewis thinks that objects which e.g. have a size have both a non-relational size-property and stand in relations to numbers which give us their size on different measurement scales. (See Lewis 1986, 53.) Lewis assumes that the non-relational size-properties are internal (see e.g. Lewis 1983b, 355), but it is clear that the relations between objects and the relevant numbers/degrees are not internal, since they do not supervene on the natures of the objects and numbers/degrees taken separately. To put it differently: it is not part of the internal nature of an object that it is associated with the degree of e.g. height-in-meters with which it is associated. This means that it is not part of the intrinsic nature of the composite of any two objects that they stand in the 'is associated with a degree …'-relation, which in turn means that the relation is not external. Things would look different if we were talking about a relation which held between complexes involving both the relevant objects and their associated degrees, but the 'is associated with a degree …'-relation is a relation which holds between objects, not between such complexes. Such complexes are also arguably not the kind of entities which are subject to the modal realists' ban on cross-world relations.

Second, I nonetheless think that the objection points to a legitimate question which the Degree Strategy should acknowledge and try to answer, namely: Is it really unproblematic to exclusively rely on degrees instead of on the corresponding intrinsic properties when trying to account for the truth of sentences involving cross-world comparisons? A natural way of making this question more precise is to spell it out in terms of possible variances in the relation between e.g. intrinsic height-properties of objects and the relations to degrees of height on a certain scape in which they stand. So understood, the question is identical to the question regarding requirement c) which I have already addressed in the previous section. Either way, the objection fails to undermine the answer to Divers's revenge-worry given in this section.

5. Lewis's objection to degree-based semantics for modal comparatives

Divers final worry refers to an explicit discussion of modal comparatives in Lewis (1986, 13). To see what to make of this final worry, we should take a closer look at what Lewis writes in the passage to which Divers refers. As will become clear shortly, it makes sense to quote this passage at length:

> In any case, modality is not all diamonds and boxes. Ordinary language has modal idioms that outrun the resources of standard modal logic, though of course you will be able to propose extensions. […]
> There are modalised comparatives: a red thing could resemble an orange thing more closely than a red thing could resemble a blue thing. I analyse that as a quantified statement of comparative resemblance involving coloured things which may be parts of different worlds.
>
> > For some *x* and *y* (*x* is red and *y* is orange and for all *u* and *v* (if *u* is red and *v* is blue, then *x* resembles *y* more than *u* resembles *v*))
>
> Try saying that in standard modal logic. The problem is that formulas get evaluated relative to a world, which leaves no room for cross-world comparisons.

> *Maybe you can solve the problem if you replace the original comparative relation '…resembles…more than…resembles…' by some fancy analysis of it, say in terms of numerical measures of degrees of resemblance and numerical inequalities of these degrees. After that, you might be able to do the rest with boxes and diamonds. The fancy analysis might be correct. But still, I suggest that your solution is no fair. For that's not how the English does it. The English does not introduce degrees of resemblance. It sticks with the original comparative relation, and modalises it with the auxiliary 'could'. But this 'could' does not behave like the standard sentence-modifying diamond, making a sentence which is true if the modified sentence could be true.* I *think its effect is to unrestrict quantifiers which would normally range over this-worldly things.* The moral for me is that we'd better have other-worldly things to quantify over. I suppose the moral for a friend of primitive modality is that he has more on his plate than he thinks he has: other primitive modal idioms than just his boxes and diamonds. (Lewis 1986, 13-14; my italics.)

In the crucial italicized part of this passage, Lewis first objects to a semantics which introduces degrees into the language of first-order modal logic in order to account for modal comparatives and then argues that a semantics based on his theory of modality better captures the behaviour of modal comparatives in English. The discussion of both theories is not general, but rather focuses on particular modal comparative sentences, namely those involving comparisons of resemblance between colours. It is therefore not at all clear whether Lewis intended this passage to provide a general critique of degree-based semantics for modal comparatives. Based on the quoted passage alone, Divers's claim that 'the Lewisian who would do so [account for modal comparatives using inequalities between numerical degrees instead of relations between the compared objects] must take into account that Lewis (1986, 13) resists this approach to modal comparatives in general and why he does so' (Divers 2014, 577) should therefore be taken with a grain of salt.[16]

[16] Note that Forbes (1994, 39) also seems to accept that Lewis at least meant the 'that's not how the English does it'-part to apply to degree-based theories of comparatives in general.

While the textual evidence fails to conclusively settle the question of relevance, there are two reasons to think that there is still something to Divers's worry and that Degree Strategists have to consider and address the points about degrees made in the italicized part of the quotation.

First, even if it were settled that Lewis did not intend this passage as a general critique of degree-based semantics for modal comparatives, the points he makes might of course still pose a problem for the Degree Strategy.

Second, one may argue that even though Lewis did not explicitly say so in this passage, he must have intended the objection to apply to all degree-based semantics of modal comparatives, since he himself proposed a rival semantics for comparatives at the end of his Lewis (1970). The semantics Lewis sketches there is a supervaluationist semantics which introduces a delineation-coordinate as an additional contextual parameter, 'a sequence of boundary-specifying numbers' (Lewis 1970, 65), relative to which sentences are evaluated.[17] Crucially, this semantics does not rely on degrees.[18]

The relevant part of the quote is the italicized passage immediately following Lewis's concession that a degree-based analysis might produce the right semantic results. This passage contains two claims which Degree Strategists should consider. The first claim is about the semantics of comparatives embedded under 'could', the second claim about the semantics of 'could' in this particular context. Both are linguistic claims about a particular language, namely English, but the second claim also clearly reflects a distinctive aspect of the Lewisian theory of modality. I will now discuss both claims in turn.

The first claim is a claim about the logical form of particular English claims containing modal comparatives. ('The English does not introduce

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[17] The basic idea of the semantics is that a comparative sentence of the form '*x* is *F*-er than *y*.' is true if, and only if, the set of delineations relative to which *y* is *F* is a proper subset of the set of delineations in which *x* is *F*. See Lewis (1970, 64-65).

[18] It should be noted however that von Stechow argues that Lewis's (1970) semantics is 'virtually identical' (von Stechow 1984, 10) to Seuren's (1973) semantics of comparatives, meaning that the two semantic theories produce equivalent results. Seuren's semantics relies on extent variables which range over sets of degrees. This suggests that at least as far as the semantic analysis it produces is concerned, there is no substantial difference between Lewis's theory and a theory which (indirectly) relies on degrees.

degrees of resemblance. It sticks with the original comparative relation, and modalises it with the auxiliary "could".') Now while there are many interesting philosophical questions tied to a Lewisian approach to meaning in general (see for example Schwarz 2014, Weatherson 2013), the most direct way to answer this particular complaint about degree-based treatments of modal comparatives is to reply in kind and to simply point out that it is falsified, both regarding its negative and its positive sub-claim, by recent work done on comparatives in linguistics. The degree-based approach, of which the Degree Strategy is a variant, is a proven standard approach to the semantics of comparatives in natural language semantics. (See e.g. von Stechow 1984, Kennedy 2005, Schwarzschild 2008.) From the perspective of linguistics, a perspective invoked by Lewis himself in the quoted passage by referring to what 'the English' does, there is hence no good reason to accept his first claim. So even assuming that Divers's general reading of Lewis is correct, Degree Strategists would arguably be able to live with this departure from what in this case would be Lewisian orthodoxy.

This leaves Lewis's second claim in the italicized part of the quote. Like the first claim, it consists of a negative and a positive sub-claim.

The negative sub-claim is that in the particular comparative structure which Lewis discusses, 'could' does not behave like the possibility-operator; it does not introduce a possible world, relative to which a comparative phrase is to be evaluated. It should come as no surprise that Degree Strategists fully agree with this claim. After all, they too work with the language of Lewis's counterpart theory instead of the language of first-order modal logic.

The positive sub-claim is a claim about the functioning of 'could' in this context. Lewis writes about this modal auxiliary verb that 'its effect is to unrestrict quantifiers which would normally range over this-worldly things.' This is a generic claim about quantifiers, which strictly speaking leaves it open whether Lewis refers to quantifiers which quantify into the 'original comparative relation' or more generally quantifiers involved in the semantic analysis. If the more specific reading is correct, then Lewis's claim is simply irrelevant to the Degree Strategy, since Degree Strategists do not directly quantify into e.g. a taller-than relation which holds directly between two objects.

If we instead read it as a genuinely generic claim about the quantifiers involved in the semantic analysis of a modal comparative sentence of the

sort discussed in the quote, then again, the degree strategist is in full agreement with Lewis. Quantifiers are per default unrestricted in counterpart theory, but can easily be restricted, e.g. to particular worlds. (See e.g. Lewis 1986, 113.) In Degree Strategic translations of modal comparative sentences, 'could' is indeed taken to act in the way envisaged by Lewis. Consider (D1) again:

$$\text{(D1)}\ \exists v(Aa \wedge Tav \wedge \forall w \forall x((Aw \wedge w \neq a \wedge Twx) \rightarrow x < v) \wedge \exists y(\exists z Cza \wedge Tzy \wedge v < y))$$

In (D1), the existential quantifier which binds the variable *v* ranges only over the degrees of tallness of actual objects in the first main conjunct, but then unrestrictedly over all degrees of tallness in the second conjunct, i.e. in the part which translates the 'could'-claim. So this Degree Strategic translation is perfectly in line with the positive part of Lewis's second claim.[19]

To sum up, Divers's third worry, the worry related to what Lewis writes about modal comparatives in Lewis (1986) on p. 13 does not substantially threaten the Degree Strategy. Putting interpretative problems aside for the

[19] It should be pointed out that (D1) is not the only translation of (1) available to proponents of the Degree Strategy. Since I assume, as pointed out in section 2, that Lewisians need not translate modal sentences from natural language into first-order modal logic in order to then translate the resulting sentence, using the schema provided in (Lewis 1968), into the language of counterpart theory, they could for example instead settle for:

$$\text{(D1*)}\quad \exists u(Aa \wedge Tau \wedge \forall v \forall w((Av \wedge v \neq a \wedge Tvw) \rightarrow w < u) \wedge \exists x(\exists y Wy \wedge \exists z Izy \wedge Cza \wedge Tzx \wedge u < x))$$

This alternative translation differs from (D1) in that its second conjunct now explicitly states, using *Wx* to say that *x* is a possible world and *Ixy* to say that *x* is in *y*, that there is a possible world which contains the counterpart of the actual largest thing *a*.

The alternative translation hence has the advantage of making it more explicit that the object and its counterpart may be in different possible worlds. This advantage is however not at all lost to Degree Strategists who stick to (D1). Given Lewis's (1968) postulates P1, which says that nothing is in anything except a world, and P3, which says that all counterparts are in something, (D1*) is entailed by (D1). So they will still be able to use (D1*) in order to illustrate this aspect of their strategy.

moment, Degree Theories have to deviate from Lewis as Divers understands him regarding one of his two claims, but have a good reason to do so. The other claim also poses no problem since, depending on how one understands it, it is either partly irrelevant to and partly compatible, or wholly compatible with the Degree Strategy.

6. Two further questions about the Degree Strategy

While the previous section completes my response to Divers's three worries about degree-based responses to his objection to the Lewisian theory of modality, the particular example from the quote from Lewis (1986) discussed in the previous section raises two interesting question about the scope of the Degree Strategy.[20]

Lewis's example is that of a modal comparison of resemblances between coloured objects. (See once again Lewis 1986, 13.) The first question tied to this example is of how degree strategists might handle comparisons of colour. The second, how they might handle comparatives like '*w* resembles *x* more than *y* resembles *z*' which involve multiple dimensions of comparison, e.g. resemblance with respect to colour, shape, size, ….

The first question can be answered rather straight-forwardly: To compare colours, Degree Strategists can rely on regions in colours spaces as their degrees. Regions in colour spaces can be represented numerically by sets of tuples of numbers (e.g. in sRGB color space as 4-tuples involving real numbers representing values for red, green, blue, and specifying a white point) and the distances between them can be measured accordingly.[21] Degree strategists can then again rely e.g. on the similarity-relation between worlds in order to ensure comparability of colour-spaces across different possible worlds.

The second question however is much harder to answer: To be fully specific, a Degree Strategic treatment of comparisons of resemblance or

[20] Thanks to an anonymous referee for prompting me to discuss these questions.

[21] Representations of colour spaces of this sort play a major role in efforts to assure consistent colour representations across different display devices. For more information on this, see e.g. the website of the International Color Consortium http://www.color.org/.

more generally, comparative concepts which involve multiple dimensions of comparisons, would require an effective procedure to aggregate the relevant dimensions of comparison into a single ordering of the relevant degrees. Procedures of this sort are of crucial importance in several different philosophical contexts, including e.g. the theory of social choice (see e.g. List 2013), but notably also at a core junction in Lewis's own philosophy. The question of how to aggregate aspects of comparative similarity between possible worlds is an important question about his theory of counterfactuals which crucially relies on this notion of similarity. Lewis discusses this question in several of his works, (see e.g. Lewis 1973a, Section 4.2, 91ff; 1973b; 1979; see also Kroedel & Huber 2013 and Morreau 2010) but I will not attempt to begin to settle in how far Degree Strategists can make use of these discussions to help them answer the second question. Suffice it to say that while Degree Strategists still appear to have their work cut out for them, this further challenge is distinct from, and arguably goes beyond the challenge raised by Divers (2014) which is the main focal point of this paper.

7. Conclusion

To conclude, the Degree Strategy is not directly threatened by any of the three worries raised by Divers: It does not require deep or extensive revisions of Lewisian metaphysics, is not subject to a revenge problem induced by a relation between degrees or objects which are compared regarding e.g. their degrees of height, mass, or of another spatiotemporal quantity, and is also not seriously threatened by Lewis's remark on the treatment of modal comparatives which Divers cites. The Degree Strategy is therefore a live option for Lewisians who are looking for a way to address the general question underlying Divers's (2014) challenge, the question of whether they can account for the truth of counterpart-theoretic sentences involving comparisons between spatiotemporal magnitudes of material object in different possible world.

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Intuition and the End of All –Isms

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ABSTRACT: In my paper, some of the most influential *-isms* in the philosophy of mathematics are discussed with respect to their attitude to intuition. By the end of the all -isms, at first, their tendency to arrive eventually at just the opposite of their previously proclaimed principle is meant. The positive significance to the given tag line is connected with as a simple observation (due to both William James and Wittgenstein) that most of the -isms are justifiable if treated as practical attitudes rather than theoretical systems. Accordingly, intuition's role will be twofold: first, as a reference point with respect to which the given -isms were portrayed as turning into their very opposites; and, second, as the focal point to which all of them might be seen as contributing to intuition's pragmatic reading. Along these lines, the path of intuition might be transformed from an epistemological Calvary—or the path of despair, to use Hegel's words from the beginning of his *Phenomenology* in which one particular theory is replaced by another which is itself later replaced, etc.—into the path of progress in which some traditional dilemmas such as that between mathematical realism and nominalism are solved.

KEYWORDS: Brouwer – formalism – Frege – Hegel – intuition – intuitionism – logicism – phenomenology – pragmatism – philosophy of mathematics.

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1. Introduction

There is a famous remark by Albert Einstein (1998, 890) to the effect that any two -isms can be made the same if they are articulated properly. He made this remark in the context of some correspondence on scientific realism showing, furthermore, some tendencies towards the *pragmatic* standpoint in physics.

In my paper, I would like—as a kind of *dialectical* exercise—to adopt a similar attitude with respect to the main foundational streams in the philosophy of mathematics, including the doctrines of formalism, logicism, structuralism and intuitionism. The concept of *intuition*—to which all these doctrines refer, both in a positive and a negative way—will serve as the focal point upon which this exercise can be performed and eventually be brought to a pragmatic ending. The desirability of such an ending will be another point of my paper.

In general, I will not proceed by way of disambiguation but *phenomenologically*—in Hegel's sense of the word—which is to say, I will follow the given development in the philosophy of mathematics and let the given dialectic simply decide for us what an *intuition* might be.

2. Pure intuition

Let me start with some general remarks on *intuition*. It is a historical fact, which has been amply discussed in the literature, that the concept of intuition in the philosophy of mathematics as well as in philosophy proper has been used in *ambiguous* and often incompatible ways. Charles Parsons' (2009) book *Mathematical Thought and its Objects*, among others, might serve as a reference point proving that intuition has been treated as being of both of an empirical and an intellectual origin, receptive and spontaneous, subjective and objective, *de re* and *de dicto*, irrefutable and fallible, etc. In the end, the most stable property expected from intuition seems to be its *immediacy* going back to its origin in the verb "intueri", "to gaze at". Intuition and the knowledge based on it is thus typically contrasted with knowledge preceded by an inference as a kind of *mediation* and the prospective source of its instability. Let us take these general expectations—immediacy and reliability—as our starting point.

In mathematics, such a general attitude to intuition has an important precedent in antiquity where the demonstrative, *direct* methods of geometrically grounded mathematics had been contrasted with the *indirect*, dialectical methods of logic. As von Fritz (1971), Hintikka (1974) and others have argued, mathematics in antiquity was a science of *epagogic*—i.e. inductive—method as opposed to the *apagogic*—deductive—methods of the dialectic. Among the latter, the indirect proof was the most visible one used by both the rhetorician as well as the Eleatic sophist to justify some counterintuitive and unreliable claims such as that there is no motion, etc. As Grattan-Guinness (2000, 17) has noted, Kant in his controversial separation of mathematics from logic had only been following this old trail despite the existing tendencies to treat both, mathematics and logic, as sciences of the *analytic* method.

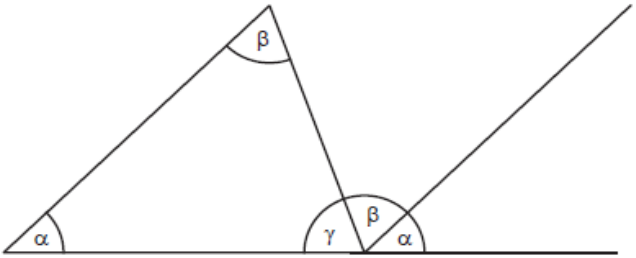
In the light of this, it is understandable why Kant's concept of intuition (*Anschauung*) is basically of *sensuous* origin. By intuition, basically, a representation of an object of our senses, is meant:

> In whatever way and through whatever means a cognition may relate to objects, that through which it relates immediately to them, and at which all thought as a means is directed as an end, is intuition. This, however, takes place only insofar as the object is given to us; but this in turn, is possible only of it affects the mind in a certain way. The capacity (receptivity) to acquire representations through the way in which we are affected by objects is called sensibility. [...] all thought, whether straightaway or through a detour, must ultimately be related to intuitions, thus, in our case, to sensibility, since there is no other way in which objects can be given to us. (Kant 1998, A19/B33)

As a result, in order to justify some sentence, no matter whether of empirical or mathematical origin—e.g., that the sum of the angles in a triangle equals two right angles—one has to intuit something, such as a particular triangle, through our *senses*. As Kant says referring to this very example (the only one he, in fact, gives besides the infamous 7 + 5 = 12), the mere concept or verbal definition of the given object is not enough. Contrary to the empirical context, though, in mathematics the given intuition has an *apodictic* power of the original epagogic method: The demonstration carried out for one particular geometrical figure (such as the given triangle,

see picture) justifies the validity of the given claim for *all* the figures of the given form, and does so not only with some *probability* but with the utmost certainty.

In order to differentiate the empirical, probabilistic, from the mathematical, apodictic, induction Kant introduces the concept of so-called *pure intuition* in which the given demonstration is executed. By this very move, as might be expected, he did not resolve the unclear situation of intuition in mathematics. In fact, he made it worse to the extent that, on the one hand, all the subsequent foundational doctrines, including formalism, structuralism, constructivism and intuitionism, but also logicism, conventionalism, axiomatism, etc., took his concept of intuition for granted, both as their basis and as their target, without, on the other hand, agreeing on at least some of its features as mentioned above. This leaves us, again, with the *immediacy* and *reliability* of pure intuition as something one can at least start with.



3. The logicist interlude

It was undoubtedly the phenomenon of *Non-Euclidian geometries* that made Kant's own example of mathematical justification (the sum of a triangle's angles) spurious: The given demonstration depends heavily on the validity of *Euclid's Parallel Postulate* and is thus *mediated* by it. Similar findings had gradually undermined the idea of spatial intuition's apodictic power and led, as their first fruits, to Poincaré's and Hilbert's conventionalism. Long before this, however, the indisputable success of Leibniz's and Newton's idea of calculus was confronted with its most blatant failures stemming from the uncontrolled employment of some "intuitive"—

both spatial and temporal—notions. This led to calculus' gradual reform which proclaimed all the intuitive references unreliable: That is why Lagrange limited himself to a purely formalistic justification of the calculus and why we shall not find any pictures in Cauchy's textbook.

In the next step of this reform, Bolzano and Frege take the overall counter-intuitionist attitude even further, claiming not only the general *unreliability* of intuition but its complete *uselessness*. The result is the doctrine of *logicism* according to which one can ground the whole arithmetic on logic and on logic only without any further reference to intuition. In the following, I take the case of logicism as the designated one not only because Frege phrased his anti-Kantian agenda in very Kantian terms—as the opposition between the intuition and the concept—but because, to some extent, by being the founder of formal logic in its second-order predicate form he provides an agenda for both the doctrine of formalism and of structuralism. In his *Foundations of Arithmetic* (Frege 1884, § 26), he even plays with the idea that intuitions and, in the end, even particular objects are purely subjective or not communicable and gives, as an example, the projective geometry where the intuition of point can be replaced by an intuition of line, etc., without changing the validity of the objectively valid laws such as that two points determine one line. But this looks rather like a slip of the pen according to Frege's own standards.

In his main counter-intuitionistic attack, Frege, as always careful in his foundational claims, does not phrase the uselessness of intuition as some obvious fact but phrases it explicitly as a promising *hypothesis* to be tested in his *Begriffsschrift* (Frege 1879, IV)—i.e. in the script wholly based on *concepts* thus avoiding hidden references to intuition. Because his foundational interests lie in arithmetic and in arithmetic only, he specifies his target explicitly as the Kantian *intuition of time* (Frege 1884, § 91). And he is quite explicit about what he means by that: Namely the dependency of proofs on the fact that numbers and arithmetical concepts have been introduced *recursively*, i.e. in a way in which the existence of objects and the validity of truths introduced "later", such as 5 or 7 + 5 = 12, depends on the objects and truths introduced "sooner".[2]

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[2] See Frege's critique of Grassmann in Frege (1884, § 6). For a detailed account of this point see my papers Kolman (2015), (2007). The rest of this section is significantly based on these papers.

Recursive definitions, quite common in arithmetic and, one would say, even intrinsic to it, are unacceptable for Frege for purely semantic reasons: The recursive definition of function *f* in which, first, the value for f(0) is set and, then, the value of f(x+1) is introduced by reference to the already set value of f(x), seems to talk about the object *f* sooner than it was definitely introduced. According to Dedekind and Frege, the only standard definition is the *explicit one* in which arithmetical concepts are not introduced *in steps*, as a sequence 1, 2, 3, 4, etc., but *at once* by means of a *single* formula Z(x). To achieve this, one must eliminate the "etc." clause from the recursive formations which Frege was able to do already in his *Begriffsschrift* by using the logic of the second order, particularly the definition of closure:

$$Z(x) := (\forall X)(X(1) \wedge (\forall x)(X(x) \rightarrow X(x+1)) \rightarrow X(x)).$$

In his own words, it was this success which convinced him of the viability of the logicist *hypothesis*.

As we now know, despite the original optimism, the logicist definitions have failed. The reason for this, though, is not the emergence of Russell's paradox, which, as the neo-logicists have shown, is eliminable anyway, but the very nature of *second-order logic*. If they are about to work properly as definitions of closure, the second-order replacements of recursive definitions cannot do without a supposition that there is an *infinite set* in the range of the second-order variable—the set must be *exactly* that of the natural numbers or, at least, of their structure, otherwise some unwanted objects might get into it. This is, in fact, what happens to the first-order Peano arithmetic within its so-called non-standard models.

Now, all the attempts to vindicate the existence of an infinite set by logistically acceptable means, such as Bolzano's (1851, § 13) and Dedekind's (1888, theorem 66) "pure" constructions, prove very graphically that there is no simpler way of introducing infinity than the original recursive, i.e. "intuitive" path, of which the sequence 1, 2, 3, 4, … is a model case. What's more, Frege's and Dedekind's attempts to eliminate the intuitive reasoning by focusing on axiomatic—i.e. formula-producing-systems—completely left aside the fact that the definition of a theorem or derivation proceeds by way of *recursion*. The attempt to eliminate "etc." and all the particular operations with artifacts such as pebbles or abacus from

scientific arithmetic, leaving them to what Frege calls "Kleinkinderzahlen", is doomed to a vicious circle from the very beginning.

4. Three lessons

The lesson from the failure of logicism that is relevant here seems to be twofold:

(1) As for the first part, logicism concluded from intuition's unreliability or from the unreliability of the "gaze" that the alleged reliability of mathematics and knowledge in general must come from the other part of the Kantian distinction, namely from concepts of language. The original tension between clear-sighted intuition and blind symbols is thus turned *upside-down*. Blind symbols and concepts are now the only ones seeing, and intuition is now the blind one because it is unreliable. But this turn did not work. The alleged reliability of logic betrayed Frege at the very beginning, with principles he took not only for being true, but even for being true on analytic grounds.

(2) The second part starts with the supposition that one can do without intuition in the sense of leaving its manifestations to the pre-scientific or psychological level of pre-theoretical counting or drawing diagrams. In any mathematical science worthy of its name, one shall deal only with concepts. In the end, though, the concepts turned out to be not only unreliable but dependent in their goal on the recursive, i.e. intuitive definitions. Thus, ironically, all the remedies suggested by Frege or his followers, particularly type theory, consist in the employment of constructive principles, which is at blatant variance with the original anti-Kantian approach.

Now, based on this two-part lesson, it seems that one might feel compelled to adopt one of the following attitudes toward the logicist failure as far as intuition's reliability is concerned:

(1) First, there is the *attitude of Brouwer* according to which original intuition is reliable enough—one must only keep it sufficiently apart from the blind reasoning of logic.

(2) The second *attitude is that of the early Hilbert* who accused Frege and his logic in general of not being formal or conceptual enough in that they still give, even in their anti-intuitionistic attitude, too much room to intuition by simply presupposing that there is something beyond their formulas which these try to express.

Interestingly, in their foundational endeavors neither Hilbert nor Brouwer fare much better than Frege, ending up with the very opposite of what they promised to achieve.

Brouwer's appeal to a more intuitive mathematics that does not depend on the linguistic schemata but, instead, is anchored in the constructive decision of the *creating subject*, led famously to theorems which almost nobody—including Brouwer's followers—took to be intuitive or even true. From the other side, the variety and artfulness of ways in which Brouwer tried to refute the classical theorems, including such absolute "certainties" as the *principle of the excluded middle*, not only gave rise to the one and only split in modern mathematics but inspired Wittgenstein—after attending one of Brouwer's Vienna Lectures—to enter the second period of his thinking, one characterized by a belief in the plurality of language games as opposed to the intuitively given discourse.

As for Hilbert, his first version of formalism and his attitude to intuition started with the explicit idea that mathematics' certainty consists in concrete but blind symbols and their finite organizations in formulas and formal derivations. The question whether these symbols refer to something—e.g., to infinite entities as Cantor suggested—was bracketed not for being unjustified or unscientific, but rather for being *irrelevant* as far as the issue of foundations is concerned. Gradually, this cautious approach had become an intrinsic one adopting noticeable *transcendental* features: Since the roots of any knowledge are to be identified with a finite (or finitely describable) system of rules and axioms, and finite deductions from them, the certainty of them is also the certainty of given intuition, which, similar to the pure intuition of Kant, is thus not purely empirical but has apodictic features. This is the so-called "finite Einstellung".[3]

In the light of this, one can say that Hilbert and Brouwer represent, in the philosophy of mathematics, certain kinds of *antithetical* positions

[3] I elaborated on this point in my paper Kolman (2009).

reminding one of the early chapters of Hegel's *Phenomenology of Spirit*. Analogously to its starting position of *immediately* given and certain knowledge, which—compared to its own standards—turns out to be the most general and mediated, Brouwer starts with the self-certainty of the given intuition only to end up with the most uncertain and counter-intuitive results. In the next stage, one decides with Hilbert to eliminate the reference to intuition and its object in favor of a meaningless language so as to be forced to acknowledge a new kind of intuition dealing with linguistic artefacts. These examples, of course, make sense only as a part of a bigger story that I have tried to develop elsewhere.[4]

Its lesson, obviously, is not historical but rather *dialectical*. Namely, that there is another, third, secret part of the lesson to be taken from the original logicist failure. And this consists in the conclusion that intuition—even in its pure form—does not have to be *immediate* and *reliable* in the absolute sense of the word. In the same sense in which I do not doubt that this is my hand (to quote G. E. Moore while raising my hand), I will not doubt that the sum of all angles in a triangle equals two right angles. This is not to say that, e.g., by empirical measuring, different results cannot come about, but that they are not typically treated as *counter-instances* to the given claims but as failures to be ignored. And this is what we mean by the given sentences to be *a priori*: That we treat them as irrefutable by standard singular experience because this standard experience—or let us say, with Wittgenstein, the whole stage on which it is played—is defined by their *stability*. But this stability is only a relative one and might be shaken by some drastic change in the situation, e.g., if some secret surgery were performed on me or in the need to measure cosmic distances and times.

5. Pragmatic turn

The *relative* concept of a priori, at which we have just arrived, has been commonly and prominently advocated by, e.g., C. I. Lewis and Ludwig Wittgenstein. What is not so common is the corresponding readjustment of *intuition* to these philosophical needs, the concept alone being abandoned

[4] See my book Kolman (2016a).

rather than justified by modern philosophy, as, for that matter, the development of continental phenomenology and its transition from Husserl to Heidegger testifies. To make the philosophy of mathematics up-to-date, though, one does not have to leave intuition aside as something contradictory and obsolete. What one needs to do is to learn from the development of failed attempts at making the founding principle of mathematics explicit. This is the moment where the phenomenological method of Hegel enters the stage.

As for arithmetic, what has been seen is a repeated pattern of the rebirth of the *constructive* even in the most abstract disciplines of mathematics such as set theory and logic. In light of this, even the "revolution" of Brouwer does not seem to be such a radical break with the whole development but instead represents an explicit acknowledgment of their tacit preconditions to which the systematic use of constructive principles such as *transfinite induction* or situation-dependent formations such as *diagonalization* belong. This does not need to be read as a defense of *constructivism* but simply as an alert that many of the theoretical questions have a *practical* dimension which cannot be eliminated from the foundational debates. As a result, we should enrich the concept of intuition by this practical aspect. Such an adjustment is, in fact, in accord with Kant's original conception of pure intuition which is always explicitly connected to *constructions*—i.e. to doing something—in space and time.

In the realm of geometry, e.g., by claiming that two different lines orthogonal to the third line cannot intersect in any *possible* prolongation, one can mean neither an empirical nor a purely theoretical possibility but a practical and normative one of prolongations that are "good enough" or "acceptable." The mathematicians' talk about the intersection in infinity is thus only a theoretical abbreviation for this practical certainty which, in the context of cosmic distances, loses its original sense. So the discovery of non-Euclidian geometries and their successful applications in physics does not count as an absolute refutation of Euclidian geometry but only as a kind of *proto-theoretical* impulse to revise it with respect to the given context.

Drawing on Lorenzen's work,[5] Stekeler (2008) in *Formen der Anschauung* elaborates on this basic approach to Euclidian geometry starting with the postulates from which the quality of *rectangular solids* (or *blocks*) and

[5] Particularly Lorenzen (1984).

wedges should be measured.[6] A block is defined as a solid fulfilling the following principles:

(1) It has 6 surfaces, 12 edges and 8 corners,

(2) the surface of a block fits on the surface of its copy and on the surface opposite to it thus forming a new block of a bigger size,

(3) through the given inner point a surface of the given block can be uniquely cut up into 4 smaller blocks,

(4) two not necessarily congruent blocks can be brought into (partial) overlap in two arbitrarily marked places on their surfaces,

(5) two blocks lying on the surface of the third block are overlapping already in the case that they touch in two places of their opposite edges,

(6) through the diagonally opposite edges of a given block there is only one diagonal plane cut that divides the block into two rectangular wedges that are copies of each other,

(7) by removing or adding the congruent bodies to a congruent body at the same place the congruent bodies are obtained,

(8) for every two edges of two blocks there is a natural number n such that the edge of the one block after n applications exceeds the edge of the other one.

These postulates are obviously neither axioms in the traditional sense of self-evidently true sentences nor in Hilbert's modern sense of implicit definitions. They are *material norms* defining the given concept by recourse to the pregiven practice of forming the solids and assessing the quality of their form to the extent that it is the very possibility of this practice that guarantees that these postulates are (in)dependent and consistent. By their *completeness*, Stekeler means that they are sufficient to found classical Euclidian geometry in an inferentially-holistic way, forming what is known as its standard model. The basic geometric concepts such as *flat surface*, *straight line* or *orthogonality*—or theorems about them—are taken

[6] See also my review Kolman (2011).

to be the simple (material) consequences of the postulates: plane is the surface that fits on a block, straight is the line fitting on the edge of a block and orthogonal is the angle formed by two intersecting edges of a block. The parallel postulate which is not (formally) deducible from the rest of Euclid's or Hilbert's axioms is a material consequence of the postulates (6) and (8).

Similarly, in *arithmetic* one can understand *Peano axiom*s as material norms expressing the truth about working within the underlying calculi and not as some a priori given truth about some independently given objects. I will come to this in the next, final section. What matters now is that, along these lines, the Kantian concept of pure intuition can be reconstructed in a way which does not have to follow all the details of the Kantian corpus and yet will still remain true to the original idea of his philosophy. This amounts, in the end, to the general insight that the differences one makes do not exist here simply *in itself*, but always *for us* as cognitive subjects. This reading includes also the later rectification of Hegel, by which the a priori structures of reason cannot be interpreted as belonging to the *privacy* of a subject's mind—as some of Kant's followers presumed—but in the joint practice of our orientation in space and time. The general message is simple: One cannot ground any knowledge by merely *looking at* things. This is not only because every act of looking is theoretically charged, but because it is substantially clothed in social agency. The following specification of intuition is given by Stekeler (Unpublished):

> *Anschauung* stands for any possibly *conceptually articulated reference to some object or event in real perception*—such that the same object can or could be perceived by others as well. *Pure intuition* is a label for the mere form of such an objective reference to objects of perception—including the corresponding spatial and temporal transformations of perspectives if there are different observers at different places or if we refer to the same object or event from different times.

Such a practically and socially articulated intuition cannot be infallible simply because I, as the cognitive subject, can never be the absolute guarantee of the corresponding truth. But this feature, as Wittgenstein (and Hegel) have taught us, *makes such an intuition as a prospective basis of knowledge something that is quite impossible in its immediate and infallible form.*

6. Gödel theorems

As in philosophy proper, in the philosophy of mathematics this lesson has been learned the hard way to the extent that there is a kind of official narrative in which Hilbert is the last hero of the old times and Gödel theorems are the living memorial of the last hero's fall. But the situation is already, in fact, much more simply captured by Bernays' laconic remark from his commentary to Hilbert's (1935, 210) collective works:

> it has turned out that in the realm of meta-mathematical reasoning the possibility of a mistake is particularly great.

And this is simply because, e.g., the claims about derivability (and non-derivability) of some figure—despite their being about specific symbols—obviously exceed the "here and now" of the given intuition and point to something which is mediated by its very form. One might call this form the "pure intuition".

Gödel theorems are, in this very sense, not the end of Hilbert's *finitist* approach but rather its correction underlining the mediated and practical nature of our experience. Following Hegel, one can call this feature "infinite" not in the sense that it leads us beyond our "finite" experience—which is the sign of Hegel's famous concept of "bad" infinity—but that it leads beyond its too narrow delimitation: Gödel's unprovable yet true sentence still has to be proven to be true but not in the overly narrow context of Hilbert's methods.[7] What I am aiming at is that Hegel's distinction between bad and true infinity might be fruitfully applied to the concept of intuition and its development with respect to the phenomenon of Gödel theorems.

As his unpublished papers show, Gödel (1995, 310) himself oscillated between the following readings of his results:

(1) "there exist *absolutely* unsolvable diophantine problems [...], where the epithet 'absolutely' means that they would be undecidable, not just within some particular axiomatic system, but by *any* mathematical proof the human mind can conceive" (the diophantine problems

[7] I discuss the mathematical and logical relevance of Hegel's concept of "bad infinity" in my paper Kolman (2016b).

are of the so-called Goldbach type, i.e., of the form $(\forall x)A(x)$, where $A(x)$ is a decidable property of numbers),

(2) "mathematics is incompletable in this sense, that its evident axioms can never be comprised in a finite rule, that is to say, the human mind (even within the realm of pure mathematics) infinitely surpasses the powers of any finite machine."

Accordingly, Gödel might be seen as adjusting Hilbert's narrow, finite concept of intuition by two alternatives, the bad and the true one. The first one, the most popular among the working mathematicians and general public, amounts to claiming that Hilbert's methods do not exhaust "all our mathematical intuitions" we have about the subject. This is, of course, the radical inversion of the original concept of "Anschauung", transforming it from a matter of direct insight based on the sensuously given (e.g., the signs of language) into some mysterious voice from the grave lying beyond our senses (or our language). Such an intuition, of course, tends to be fallible almost by definition.

The other concept of intuition which is in accord with our previous practical delimitations does not lead us *beyond* our language (or the sensuous data in general) but merely beyond its too narrow understanding of a mere artifact. Besides the visual—intuitive—form of the spoken or written signs there is something which gives them life; namely, their use within the whole of human practice that they belong to. In this reading, Gödel theorems might be looked at as elaborating on this distinction between the sign and its use, or between the intuition in the narrower (merely sensuous) and broader (practical) sense.

Following the line of thought indicated in the geometrical case, with axioms interpreted as material norms embedded into the practice of forming solids, I suggest doing the same in the case of arithmetic with axioms interpreted as norms embedded into the practice of calculating and measuring. The continuity between Hilbert's and Gödel's approach to intuition will be secured by replacing the standard difference between the axiomatic theory and its model—which simply copies the ontological difference between the sign and the external object this sign refers to—by two kinds of axiomatic systems and the corresponding concepts of consequence: strongly effective or *full-formal* ($\vdash$) and the more liberal or *semi-formal*

(⊨). Going back to Schütte (1960), both these differences were developed by Lorenzen (1962) in his *Metamathematik* and might be specified as follows:[8]

Full-formal arithmetic, like the arithmetic of Peano, is arithmetic *in the narrower sense* and deals with schematically or mechanically given and controllable axioms and rules. Semi-formal arithmetic or *the* arithmetic *proper* employs—in accord with the infinite nature of the number series 1, 2, 3, …—rules with infinitely many premises, particularly the (ω)-rule A(1), A(2), A(3), etc. ⇒ (∀x)A(x), which is nothing else than the instance of the so-called *semantic definition of truth*. Hence, the significance of semi-formalism is to make us think of semantic definitions as special (more generously conceived) systems of rules (proof systems) which—starting with some elementary sentences—evaluate the complex ones by *exactly one* of *two* truth values.

It is a known fact that the intuitionists and some constructivists (including Lorenzen, but not, e.g., Weyl) question the completeness of this evaluation, arguing that the existence of concrete strategies for proving or refuting every A(N) doesn't entail the existence of a general strategy for A(x). Consequently, a decision must be made whether the infinite vehicles of truth as (ω) should be referred to as rules (1) only in the case when we positively know that all their premises are true, i.e. when we have at our disposal some general strategy for proving all of them at once or, (2) more liberally, if we know somehow that all their premises are positively true or false. The general distinction between the constructive and classical methods in arithmetic is based on this. Now, if one leaves, like, e.g., Lorenzen and Bishop, the concept of effective procedure or proof to a large extent open and does not tie it, like, e.g., Goodstein and Markov, to the concept of the Turing machine, there is still room for an effective, yet liberal enough *semantics* (semi-formal system) and a strongly effective syntax or *axiomatics* (full-formal system). Hence, the constructivist reading does not necessarily wipe out the differences between the proof and truth, as, e.g., Brouwer's mentalism or Wittgenstein's verificationism seem to. And this, in the following way, is where the true concept of intuition comes from:

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[8] The argument given here, and the rest of this section, is based on my paper Kolman (2009).

Gödel theorem affects *only* the full-formal systems, because their schematic nature makes it possible to devise a general meta-strategy for constructing true arithmetical sentences not provable in them. The unprovable sentence of Gödel is of the form $(\forall x)A(x)$, where $A(x)$ is a decidable property of numbers. Now, Gödel's argument shows that this decision is done already by Peano axioms in the sense that all the instances $A(N)$ are deducible and, hence, set as true. So, with Gödel's proof we have a general strategy for proving all the premises $A(N)$ at once, which makes the critical unprovable sentence $(\forall x)A(x)$ constructively true, i.e. provable by means of the (ω)-rule interpreted *constructively*. As a result, there is an intuitive way that transcends the methods of Hilbert's "finite attitude" and that allows us to see why Gödel's theorems did not destroy but instead *refined* Hilbert's finitist—and intuitive—approach in the suggested semi-formal way.

7. Conclusion

In my paper, some of the most influential -isms in the philosophy of mathematics have been first discussed with respect to their attitude to intuition. By the end of the all -isms, their tendency to arrive eventually at just the opposite of their previously proclaimed principle might be meant. But there is a deeper significance to this tag line connected with the suggested pragmatic closure of the paper: This was not meant as a replacement of the given -ism by another one (such as constructivism or pragmatism), but as a simple observation (due to both William James and Wittgenstein) that most of the -isms are justifiable if treated as practical attitudes rather than theoretical systems. Accordingly, intuition's role was twofold: first, as a reference point with respect to which the given -isms were portrayed as turning into their very opposites; and, second, as the focal point to which all of them might be seen as contributing to intuition's pragmatic reading.

I tried to sketch how, along these lines, the path of intuition might be transformed from an epistemological Calvary—or the path of despair, to use Hegel's words from the beginning of his Phenomenology in which one particular theory is replaced by another which is itself later replaced, etc.—into the path of progress in which some traditional dilemmas such as that between mathematical realism and nominalism are solved. This is in accord with Hegel's own intentions and his general idea to look at the desperate—

or negative—nature of knowledge in a cautiously positive way: "this path is the conscious insight into the untruth of knowing as it appears, a knowing for which that which is the most real is rather in truth only the unrealized concept" (Hegel 2018, 52). On such a path, though, there are no signposts or a particular -ism to be mechanically followed and, accordingly, new problems and dilemmas are arising simply because, by "practical", a lot of things can be meant. This has been shown, e.g., by the case of the word "effective" or "effectively calculable" in the context of theorems such as the Church-Turing thesis and the subsequent development of constructive mathematics.

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Essence and Lowe's Regress

NICOLA SPINELLI[1]

ABSTRACT: Some philosophers believe that entities have essences. What are we to make of the view that essences are themselves entities? E.J. Lowe has put forward an infinite regress argument against it. In this paper I challenge that argument. First, drawing on work by J.W. Wieland, I give a general condition for the obtaining of a vicious infinite regress. I then argue that in Lowe's case the condition is not met. In making my case, I mainly (but not exclusively) consider definitionalist accounts of essence. I make a requirement to which definitionalists such as Lowe are committed and which, I venture, should also be palatable to non-naïve modalists. I call it the Relevance Principle. The defence trades on it, as well as on the distinction, due to K. Fine, between mediate and immediate essence.

KEYWORDS: E.J. Lowe – essence – essentialism – Kit Fine – metaphysics – regress.

1. Introduction

Lowe (2008) defends a view he calls Serious Essentialism (Lowe 2008, 45). It is the conjunction of the following claims:

(SE1) Every entity has an essence.

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(SE2) No essence is an entity.

(SE3) No entity is identical with its essence.

(SE4) Essence precedes existence.

(SE5) Essence grounds metaphysical modality.

'Entity' here means any thing at all, from any category in one's ontology; 'essence' means what an entity is; and identity is numerical identity. In Lowe's paper, (SE1) is not explicitly listed as one of the principles of Serious Essentialism. That Lowe endorses it, however, is very clear. Whilst arguing for the reality of essence and essentialist knowledge—i.e., for the claim that things have essences and that we know about the essence of things—he remarks that, in order to 'talk or think comprehendingly' about any thing at all, we must first know 'what the thing is' (Lowe 2008, 35)—which, for him, is just to know its essence. He is therefore committed to claiming that everything liable to be thought or talked about comprehendingly has, in principle, an essence knowable to us, and, a fortiori, that it has an essence.

(SE3) is also not listed among the principles of Serious Essentialism. However, it is implied by Lowe's view that, whereas knowledge of essence is required to talk or think comprehendingly about something, it is not required to see, smell, hear, or be in any way acquainted with the thing in an epistemically impoverished sense (Lowe 2008, 35, footnote 22; also Shalkowski 2008, 56). In other words, so the view goes, when we see a pencil, we do not necessarily know what the pencil is. But we would, if the pencil and its essence were identical. Therefore, they are not. (SE3) also follows straightforwardly from (SE2): if entities were identical with their essences, the latter would be entities, too; but, by (SE2), essences are not entities; therefore, entities and their essences are not identical. One consequence is that (SE3) is not an independent principle of the view. Still, it will help to keep it explicitly listed.

(SE4) means, roughly, that a necessary condition for something to exist is that its existence should not be incompatible either with its own essence or with the essences of existing things (Lowe 2008, 40). So, for example, the round square cupola on Berkeley College fails to meet the first disjunct of that condition, and therefore to exist. The existence of the greatest prime,

on the other hand, is incompatible with the essence of the (existing) integers, i.e., with their characterising properties and relations—an incompatibility that is precisely what the standard Euclidean proof bears out. As for (SE5), though it is paramount to Serious Essentialism as such, it plays no major role in this paper.

I call Eccentric Essentialism the view resulting from taking (SE2) out of Lowe's picture and substituting it with its negation:

(EE1) Every entity has an essence.

(EE2) Every essence is an entity.

(EE3) No entity is identical with its essence.

(EE4) Essence precedes existence.

(EE5) Essence grounds metaphysical modality.

Just like (SE5), (EE5) plays no role in this paper; it is only listed as part of the view for the sake of symmetry and completeness. Now Lowe's opinion is that Eccentric Essentialism should be shunned. The reason is that he holds its characterising principle (EE2) responsible for much metaphysical mischief throughout the history of philosophy (Lowe 2008, 23). This is indeed one of the motivations for Serious Essentialism in general and for (SE2) in particular. Shalkowski (2008) agrees: he thinks that expressions like *the essence of x* lead to the 'mistaken impression' that the essence of—say—Socrates is a genuine thing, on a par with Socrates's beard (compare *the beard of Socrates*), and that they should be avoided ere they hinder our philosophical progress (Lowe 2008, 56-57). I had better make it clear that I will not try to refute that point: trouble may indeed come from the view, and perhaps the culprit is in fact (EE2). Yet the question is: is the impression really mistaken? Or is (EE2) true after all and despite what Lowe and Shalkowski would like to think? If it is true, we should accept it, mischief or no.

Lowe, of course, would agree, and so would Shalkowski. That is why the former endeavours to produce an argument against Eccentric Essentialism, and against (EE2) in particular. It is an infinite regress argument. If it goes through, then Eccentric Essentialism should be discarded—regardless, notice, of any undesirable consequences it may or may not have

on metaphysics or indeed philosophy as a whole. At its barest, the argument is as follows:

> Eccentric Essentialism entails a vicious infinite regress.
> Therefore, Eccentric Essentialism should be discarded.

I submit that Lowe's argument does not go through, and that reasons to reject Eccentric Essentialism should be sought elsewhere (if at all).

More specifically, my position is that Lowe's regress does not follow from Eccentric Essentialism, and that the overall argument is therefore unsound. I mean the full-blown view: no characterising claim needs to be weakened to avoid the regress. If I am right, the Eccentric Essentialist is able to get everything their serious counterpart can achieve while remaining true to the notion—which is the only source of disagreement between the two—that essences are entities in their own right. Or if they cannot, it is not because of Lowe's regress.

Here is my strategy. I first reconstruct Lowe's argument, which in the original paper is somewhat underdeveloped, based on J. Wieland's work on infinite regresses (Section 2). I also distinguish two possible construals of the regress, and pick one as my target (Section 3). I make a requirement on essentialist discourse by invoking what I call the Relevance Principle—which is widely accepted and to which Lowe is committed anyway (Section 4). I then elaborate on essence and propose a distinction, due to Kit Fine, between immediate and mediate essence (Section 5). Strictly speaking, this is not required for my case; but it does help make the logical situation more articulated and nuanced. Finally, I argue that Lowe's overall argument is unsound (Section 6).

2. Lowe's Argument

Is Eccentric Essentialism a tenable view at all? According to Lowe, it is not. Here is the argument:

> If the essence of an entity *were* just some further entity, then *it in turn* would have to have an essence of its own and we would be faced with an infinite regress that, at worst, would be vicious and, at best, would

> make all knowledge of essence impossible for finite minds like ours. To know something's essence is not to be acquainted with some *further thing* of a special kind, but simply to understand *what exactly that thing is*. (Lowe 2008, 39)

The premises of the regress are (EE1) – (EE3). By (EE1), every entity has an essence. Thus, for any arbitrary entity x there is $E(x)$, the essence of x. By (EE3), $E(x) \neq x$. So far, so good. By (EE2), however, $E(x)$ is itself an entity. And since quantification in (EE1) is unrestricted, so that it applies to *all* entities, $E(x)$ has its own essence, $E(E(x))$. By (EE3), $E(E(x)) \neq E(x)$. Also, since $E(x) \neq x$, $E(E(x)) \neq x$: there are no essentialist cycles. It is easy to see that, with all the premises in place, every entity generates an infinite sequence of essences.

We have then, says Lowe, two scenarios. The best-case scenario for the Eccentric Essentialist is that knowledge of essence becomes impossible, at least for finite creatures. This side of the argument is epistemological. If grasping the essence of an arbitrary entity x involves grasping infinitely many essences, then it seems that, our minds being unable to cope with an infinite amount of information, we are never in a position to grasp the essence of anything. This, of course, is a challenge for Eccentric Essentialism, and one of great importance. Yet I will not discuss it. That is because I am more interested in what Lowe takes to be the *worst*-case scenario for the eccentric essentialist: being faced with a vicious infinite regress. This, on the face of it, and as Lowe seems to think, is a metaphysical rather than an epistemological issue. What is at stake here is not the claim that the essence of an entity a is knowable to us, but rather the claim that a has an essence to begin with. Obviously, if the latter claim turns out to be false, then the former will be false too. The converse, however, does not hold. Priority, then, lies with the metaphysical side of Lowe's argument.

But there is work to do to understand what Lowe's metaphysical argument exactly is. It is clear why, by (EE1) – (EE3), for every entity we have an infinite sequence of essences. But there is no principled reason why an infinite sequence should, as such, be a regress—let alone a vicious one. Lowe, on the other hand, does not explicitly say what he means when he suggests that this particular series is a vicious infinite regress. But what *is* a vicious infinite regress?

Wieland (2014) is the state of the art when it comes to regimenting infinite regress arguments. Wieland gives two theories of infinite regress arguments. On the Paradox Theory (Wieland 2014, Ch. 2), an infinite regress argument yields a conclusion that is shown, by independent means, to be paradoxical. The arguer is then in a position to refute the core premise of the regress (the claim that gets the regress going). Consider, for example, the following version of the classic guardian regress. There is at least one reliable person. But a person is only reliable if they are guarded by a guardian. Yet a guardian is a person, and for them to be a well-functioning guardian they have to be reliable. They will then need to be guarded by a guardian of their own. And so on. Therefore, there are infinitely many people. If there is one reliable person, then, there have to be infinitely many reliable people. But this is a paradox, because there aren't infinitely many reliable people (this needs to be assumed or proved independently: it does not follow from the regress). Therefore, there is no reliable person.

On the Failure Theory (Wieland 2014, Ch. 3), an infinite regress argument shows that an alleged solution to a problem fails because it requires solving infinitely many further problems of the same nature as the first. Consider the following version of the (also classic) reasons regress. In order to justify a proposition (problem), you provide a reason for it (alleged solution).[2] (We assume that reasons are propositional in nature.) But for a proposition p to be a reason for proposition q, p has to be justified first. You then have to provide a reason for q first. And so on. Thus, you will never justify any proposition: because, before you do so, you have to provide infinitely many reasons. Therefore, the alleged solution (providing reasons) fails to solve the problem (justifying propositions).

On the face of it, Failure regress arguments are stronger than Paradox ones, because the arguer need not, in addition to developing the regress, independently assume or show that the conclusion of the latter is paradoxical (Wieland 2014, 26). The failure regress, in other words, is self-sufficient. It is not immediately clear how Lowe thinks of his (again, somewhat underdeveloped) argument. Considerations of charity, however, suggest that, if a Failure version of it is available (and, as I will show, it is), it should be preferred.

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2 Another version of the regress features beliefs rather than propositions.

Failure infinite regress arguments come in two varieties, depending on whether the problem, whose alleged solution the argument is meant to refute, is a universally or an existentially quantified problem. In our case, a problem of the first sort would be how we can say of every entity what the entity is. A problem of the second sort would be how we can say of *any* entity what it is. The solution Lowe wants to refute is: we say what a thing is by appealing to a further entity, the essence of the thing of which we wish to say what it is. Note that if this solution fails to deliver in the case of the existentially quantified problem—whereby if we appeal to the essence of entities, understood as a further thing, to say what entities are, we cannot in fact say what any entity is—then it follows, as a sheer matter of logic, that the solution will not deliver in the case of the universally quantified problem (Wieland 2014, 29). Simply put, if the solution does not work for any thing, it will not work for every thing. As a consequence, if an existential version of the Failure version of the argument is available, it should be preferred.

Among Wieland's argument schemas, the relevant one is thus Failure Schema B (Wieland 2014, 22). Let S be an agent, x,y objects in the relevant domain K, and φ,ψ predicates. Then:

Failure Schema B

1) For all x in K, if S has to φ x, then S ψs x.
2) For all x in K, if S ψs x, then there is a new item y in K and S first has to φ y in order to φ x.
3) For all x in K, if S has to φ x, then there is a new item y in K and S first has to φ y in order to φ x. [from 1-2]
4) S will never ψ any item in K. [from 3]
5) If S ψs any item in K that S has to φ, then S will never φ any item in K. [from 1-4]

Though rigorous, the schema is only semi-formal. It can, however, be wholly formalised. Wieland gives a natural-deduction version (31). Semi-formal, however, is precise enough for present purposes.

Lowe's regress can be naturally reconstructed as an instance of Failure Schema B. Let S be an arbitrary agent, K an unrestricted domain, x,y

unrestricted variables ranging over *K*, φ the predicate *say what x is*, and ψ the predicate *appeal to a further entity, the essence of x*. (EE1) is rendered by the unrestrictedness of *K* and *x,y*, while (EE2) and (EE3) are built into ψ. We then have:

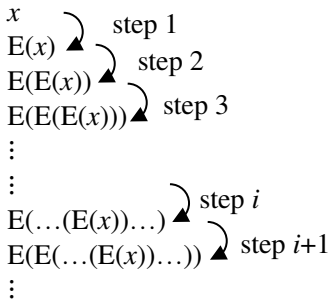
Lowe's Regress

1) For all entities *x*, if *S* has to say what *x* is, then *S* has to appeal to a further entity, the essence of *x*.
2) For all entities *x*, if *S* appeals to a further entity, the essence of *x*, then there is a new entity *y* (the essence of *x*), and *S* first needs to say what *y* is in order to say what *x* is.
3) For all entities *x*, if *S* has to say what *x* is, then there is a new entity *y* (the essence of *x*), and *S* has to say what *y* is in order to say what *x* is. [from 1-2]
4) *S* will never say what any entity is. [from 3]
5) If *S* appeals to the essence of any entity of which *S* has to say what it is, then *S* will never say what any entity is. [from 1-4]

On this reconstruction of the argument, the initial problem is saying what an arbitrary entity *x* is. To do so, we appeal to its essence, E(*x*), construed—as per (EE2)—as a further entity, distinct—as per (EE3)—from *x*. But since E(*x*) is an entity, in order to appeal to it to say what *x*, we first need to know what E(*x*) is (otherwise, what would we be appealing to?). That is to say, we need to appeal to its own essence, E(E(*x*)). And so on, *ad infinitum*. The solution to the initial problem is indefinitely postponed, the problem is never solved, and the regress is vicious.

Note that while the wording of the Failure Schema, and thus of my reconstruction of the regress, is somewhat epistemological, the argument itself is not. The reason is that the regress does not trade at all on the agent's cognitive abilities. What gets it going (and keeps it going) is not the agent and what they can or cannot come to know, but the relations between an entity and its essence (if the latter is a further entity). It is because of them, not because of the agent's epistemic profile, that the alleged solution fails. *x* is what it is only due to E(*x*). But if E(*x*) were not what it is, it would not be in a position to determine what *x* is. Thus, *x* is what it is only if E(*x*) is

what it is. Therefore, x is what it is only due to what E(x) is, namely, due to the essence of E(x): a further entity E(E(x)). But if E(E(x)) were not what it is… and so forth. It follows that, to be what it is, x needs infinitely many essences of essences. The latter are logically and metaphysically prior to x, in the sense that they are a necessary condition for x to be what it is. It is this priority that makes the regress vicious—and, again, not epistemological. Here is a way to represent the situation:



> x, E(x), E(E(x)), …, E(…(E(x))…) is a vicious infinite regress if and only if (the completion of) step i is founded on (the completion of) step i+1.

The arrows, head to tail, should be read '… is the essence of …'. E.g., the first arrow says that E(x) is the essence of x. While every entity is a term of the sequence, every arrow is a term-to-term step. Steps may or may not be founded on one another. According to (my reconstruction of) Lowe, each step i is founded on step i+1. Foundedness is to be cashed out as follows: 'If … were not the essence of __, __ would not be the essence of ---'. So step 1 is founded on step 2 because if E(E(x)) were not the essence of E(x), E(x) would not be the essence of x. The box on the right, specifying what it is for an infinite sequence to be a vicious regress, is a rendering of Wieland's 'first needs to' wording in Failure Schema B. Lowe's claim is that the sequence x, E(x), … meets the condition.

I should mention that, on an alternative reconstruction (based, however, on the same schema), the initial problem is not saying *what* an arbitrary entity x is, but *that* the entity is—i.e., that it exists. By (EE4), that too involves appealing to its essence, to the essence of its essence, and so on. Call this the 'existential alternative' to my official reconstruction. There are reasons to think it leads to a weaker case for Lowe, but they will be better appreciated if presented at the end of the paper, after the official essentialist reconstruction has been discussed in full.

3. Two ways to construe the regress

There are two ways to construe the essence of an essence, and therefore two ways to construe Lowe's regress (in its official, essentialist form). By (EE2), given an entity x, its essence E(x) is itself an entity. E(x) may be thought of as a property, perhaps a complex one, or as a complex of properties (a set, a structure, or what have you), or a proposition. Relations may be allowed in essences, too. Assume, for the sake of simplicity, that the essence of x is a property, and that so is the essence of E(x), E(E(x)). Let x be Socrates, and E(x) the property of being human. What property is E(E(x))? One option is: the property of being an abstract object—for the property of being human is essentially an abstract object. Call this the *objectual* essence of E(x). Another option is: the property of being an animal—for to be human is essentially to be an animal. Call this the *generic* essence of E(x). Lowe's regress can be developed in two ways, depending on whether the essences of properties figuring in it are objectual or generic.

There are reasons to prefer the objectual to the generic construal of the regress. One is that if generic essence is chosen then Eccentric Essentialism becomes implausible regardless of whether Lowe's argument is or is not successful. For if essence is generic, then the thought that there may be some entities that have no essence gains plausibility. Take for instance the property of being good. If G.E. Moore is right, it is unanalysable; that is to say, there is no simple or complex property F such that 'To be good is to be F' is true (Moore 1993). What is, then, the essence of being good in the generic sense? Plausibly, there is no such essence. This, notice, is true of any unanalysable property (if there are any, which sounds plausible to me). But then (EE1) is likely to be false, and the whole view goes with it. Incidentally, and this is a second reason to go objectual rather than generic, (EE1) and (SE1) are identical; so that, if the generic construal is preferred, and the above reasoning is sound, Lowe's Serious Essentialism is in trouble, too.

A possible response is as follows. It is true that, on the generic construal, if there are unanalysable properties, then some properties have no essence and (SE1), as well as (EE1), must go. But it need not go entirely. To accommodate the difficulty, it is enough to say that only *some* entities have no essence. Others, however, do. These are all the properties that are not unanalysable. And since, by all appearances, they will be neither scarce

nor uninteresting, this may be a bullet that the Serious Essentialist is prepared to bite. (SE1) can then be modified as follows:

(SE1*) Some, but not all entities have an essence.

But the move is unsuccessful. If (SE1*) is true, then there is at least one regress, setting off from some entity *x*, that involves an entity that has no essence. (I say at least one, but, unless there is only one unanalysable property, there will be more.) If that entity is *x* itself, then there is, in fact, no regress. Otherwise, the regress stops as soon as the entity that has no essence is reached. Therefore, if the Serious Essentialist opts for the generic construal of the regress and, as they must, for (SE1*), they end up with a heavy loss of generality: at least in some cases either the regress does not even get started or, if it does, it is not infinite (nor, therefore, vicious). In all this, notice, (EE2), the Serious Essentialist's bête noire, is completely idle: the regress flounders without the need arising of defending the claim. The Eccentric Essentialist, then, if at all interested in generic essence, might as well follow suit and weaken (EE1) to:

(EE1*) Some, but not all entities have an essence,

Leaving everything else untouched, including the controversial principle, they would be no worse off than their Serious counterpart.

All things considered, then, charity suggests we should pick the objectual construal of Lowe's argument and leave the generic to one side.

4. Essence and the Relevance Principle

In this section I pave the way for my defence of Eccentric Essentialism by introducing what I call the Relevance Principle. This is intended to make official a restriction that, as the recent literature shows, sensible essentialist discourse ought to be subjected to. These days, when it comes to essence, the two main camps are the definitionalist and the modalist. The restriction first surfaced within the former, but was then endorsed by quite a few modalists. I will review the two accounts, and then work my way to the principle.

The essence of an entity is what the entity is. The modal account of that notion has it that an entity *x* is essentially *F* (where *F* is a property) if, necessarily, if *x* exists, it has *F*. In an article, Fine (1994a) levelled a number of charges against the view. While I cannot hope to do justice to them, let alone expound them comprehensively, it is possible to get a feel for the overall case from the following.[3] If the essence of an entity is supposed to be what the entity is, to endorse the modal account of essence is to believe that an essentialist truth about an entity—a truth that spells out what the entity is—is just a (*de re*) necessary truth about it. But, intuitively, not every (*de re*) necessary truth about an entity spells out its essence: some are simply not informative as to what the entity is. For example, Socrates is necessarily distinct from the Eiffel Tower if he exists, and necessarily belongs to singleton Socrates (i.e., to the set whose sole member is Socrates). He is also such that if the 2008 financial crisis was the result of a global conspiracy, then the 2008 financial crisis was the result of a global conspiracy. Yet it seems that none of the following exchanges would make a sensible discussion of essence:

– What is Socrates?
– He is distinct from the Eiffel Tower.

– What is Socrates?
– He belongs to singleton Socrates.

– What is Socrates?
– He is such that if the 2008 financial crisis was the result of a global conspiracy, then the 2008 financial crisis was the result of a global conspiracy.

Whatever Socrates's essence is, being distinct from the tower, belonging to the set, or the 2008 disaster cannot be part of it. And that is because, by all appearances, they have nothing to do with what Socrates is.

[3] Hale (1996) and (2013), Lowe (2008) and Mulligan (2004) all offer additional arguments against the modal account. All of them (except perhaps Hale 1996) also share Fine's specific worries.

As I have said, the modal account faces yet other difficulties, more or less proximate to the foregoing, on which I cannot expand. The upshot, however, is that necessity, though it may well be a necessary condition for essentiality, is not a sufficient one. Hence, the modal account, or at least the unqualified version of it that the definitionalists criticise, should go. The suggestion is that we should instead think of the essence of *x* simply as *what x is*, and take this as a primitive notion to be understood on the model of real definition.

The definitionalist case against modalism turns on the idea that *relevance*, beside perhaps necessity, is a necessary condition for essentiality. This is never argued for by definitionalists, and is rather left to intuitions. It has, as one might say, the value of a principle:

> **Relevance Principle**: Whatever belongs to the essence of an entity has to be *relevant* to the question as to what the entity is.

The modal account fails, so the definitionalist argues, because necessity (or modality in general) is insensitive to relevance. Now, there is more than one way to cash out relevance, of course, and some will be more welcome to the metaphysician than others. Fine talks of necessity being too coarse-grained to capture essence—and perhaps the concept of *grain*, being less compromised with pragmatics and information structure theory, has more metaphysical appeal than that of relevance. Be that as it may, what the definitionalist arguments make clear is that, if a statement of essence is an answer to the question, 'What is…?', anything that has little or nothing to do with the latter cannot be part of the former.

Several modalists have tried to resist the definitionalist's charges. Yet, interestingly, most of them defend the modal account by qualifying it in a number of ways—all of which are meant to, among other things, accommodate relevance (or fine-grainedness, or some form or other of having-to-do) by making modality sensitive to it. It is the case of, among others, Della Rocca (1996), Gorman (2005), Zalta (2006), Correia (2007), Wildman (2013).

The Relevance Principle is paramount to my defence of Eccentric Essentialism against Lowe's regress. It is therefore welcome both that Lowe, a definitionalist (2008, 2012), accepts it, and that the principle, or some version of it, is in fact endorsed by several modalists. It allows me to remain

agnostic as to which notion of essence I work with in this paper: *prima facie*, any will do—providing it satisfies the principle.

6. The defence

Here is an interesting point from Fine (1994b). It is in the essence of Socrates that he is a man; it is in the essence of the property of being a man that it is a property; is it then in the essence of Socrates that his essence is a property? Or: it is in the essence of singleton Socrates that it contains Socrates as its sole member; it is in the essence of Socrates that he is a man; is it then in the essence of singleton Socrates that its sole member is a man? Fine suggests we should answer these questions in the negative, and I think he is right. To state the essence of Socrates is to answer the question, 'What is Socrates?' But there seems to be a difference between the bearing that being a man on the one hand, and having a property as (part of) his essence on the other, have with respect to the question as to what Socrates is. To answer the question, 'What is Socrates?' by saying that he is a man is acceptable; to answer it by saying that he is something whose essence is a property is intuitively infelicitous. It also seems to me that purveyors of qualified modalism should agree (it will then be a question of cashing out the distinction in modal terms).

We should, then, Fine suggests, distinguish between *mediate* and *immediate* essence. Briefly put, the immediate essence of an entity x only includes what has a direct bearing on the question as to what x is. The immediate essence of any entity in the immediate essence of x, on the other hand, which has only an indirect bearing on what x is, is only in its mediate essence. Mediate essence, Fine points out, is subject to chaining: if the immediate essence of the immediate essence of a is in the mediate essence of x, so is the immediate essence of the immediate essence of the immediate essence of x—and so on. More about this in a minute. Immediate essence, however, is not subject to chaining: everything that is directly relevant to what x is, is already in it, and nothing else is (Fine 1994b, 1995).

This leads me to my first objection to Lowe's argument. If we buy the Finean distinction, and I think we should, then there is a sense of essence with respect to which the notion that the essence $\mathrm{E}(x)$ of x is an entity

distinct from x, and has its own essence, does not involve any vicious regress: immediate essence.

The problem from which the regress supposedly starts is saying what x is. In order to solve the problem, we appeal to a further entity, the essence of x. If, however, by 'essence' here we mean 'immediate essence', then no regress sets off: because what $E(x)$ is, the immediate essence of x and an entity in its own right, is, has no direct relevance to what x itself is. Think again about Socrates, the property of being human, and the property of being a property. Therefore, although $E(x)$, the immediate essence of x, is itself an entity and thus has its own essence $E(E(x))$, the latter is not required in order to specify what x is: only $E(x)$ is. In other words, x's being immediately essentially $E(x)$ is not founded on $E(x)$'s being (immediately) essentially $E(x)$—just as Socrates's being immediately essentially human is not founded on the property of being human's being (immediately) essentially a property: because, again, what the property of being human is, is not directly relevant to what Socrates is. If that is so, then our problem—saying what x is—is solved right at the outset.

This is not to deny that if $E(x)$ were not what it is, then x would not be what it is either—so that there is a transitive dependence of x on each $E(\ldots)$ in the sequence. But that dependence is a modal fact. By the Relevance Principle, that does not make it an essentialist fact. Instead, the test for essentiality is relevance. And the test for immediate essentiality is direct relevance. And it seems that what $E(x)$ is, i.e., $E(E(x))$, just *isn't* directly relevant to what x is—even though, in the absence of $E(E(x))$, and of all the $E(\ldots)$ behind it, and if they were not what they are, x would not be what it is. The reason, then, why Lowe thinks that step i of the sequence is founded on step i+1 is disconnected from the question as to what x is: for what $E(x)$ is, and what would happen if it weren't what it is, and so forth, is not directly relevant to that question. If the issue is immediate essence, those modal facts, sacrosanct as they may be, are immaterial.

At this point the Serious Essentialist might say: this is all very well, but there still is a sense of essence which *is* subject to Lowe's regress, namely, mediate essence. Because even if it is only $E(x)$ that is *directly* relevant to what x is, all the other terms of the sequence are still *indirectly* relevant, and therefore in x's mediate essence. So that, at the very least, the solution to the problem of saying what x is in the mediate sense is indeed beyond reach.

There is, however, a case against this. The mediate essence of x does indeed involve an infinite chain of essences. However, recall Section 2, the infinite chain is also a vicious regress only if the completion of each step i is grounded on step i+1 being first completed. In other words, the chain is a vicious regress only if the fact that, e.g., E(E(E(x))) is in the mediate essence of x is what the fact that E(E(x)) is the mediate essence of x is founded on. But, it seems to me, that is not so. Take E(E(x)). It is in the mediate essence of x *not* because of the relations it has with E(E(E(x))), but because E(x) is the immediate essence of x and E(E(x)) is itself the immediate essence of E(x). In other words, what makes E(E(x)) indirectly relevant to what x is, and thus what makes it true that E(E(x)) is in the mediate essence of x, is not the fact that E(E(x)) stands in some relation (immediate essentiality) with E(E(E(x))), but the fact that x is immediately essentially E(x) and E(x) is immediately essentially E(E(x)). This, note, despite the fact that if E(E(x)) did not stand in the relevant relation (immediate essentiality) with E(E(E(x))) it would not be in a position to be the immediate essence of E(x). In general, it is step i and those preceding it that ground step i+1, not the other way round. The essentialist chaining starts from x and proceeds, immediate essence after immediate essence, 'what it is' after 'what it is', direct relevance after direct relevance, through the E(…)—it does not go throughout the E(…) to x, which would indeed make it impossible for x to be reached (because there are infinitely many E(…)). But then there is no vicious infinite regress as far as mediate essence is concerned: only an innocuous infinite chain.

A final remark on the existential alternative mentioned at the end of Section 2. Recall (EE4): essence precedes existence. That is, an arbitrary thing x, in order to exist, has to have an essence which, moreover, must be internally consistent and compatible with the essence of other things. But, by (EE2), essences are entities. Hence, for the essence of x to exist it too must have an essence, in turn internally consistent and compatible with the essence of other things. And so on. But then it looks like we have a new regress. The initial problem to solve is not saying what x is, but that x exists. And since solving it requires solving infinitely many problems of the same sort type (one for each of the essences involved), the essentialist solution fails. This is why we have a regress, and a vicious one. The reason why it is a new regress is that essences are needed not so much because they determine what entities are, but because, by doing so (and under certain

conditions), they allow them to exist. The 'what' (*quid*) contribution of essences, so to speak, is bypassed, and it is only important in that it makes the 'that' (*quod*) contribution possible. So, in the existential alternative, every essentialist step (recall the arrows in the Section 2 diagram) is founded on the next *not* because of what entities and essences are, but because, whatever they may be, the existence of each of them requires the existence of its essence. Here is how to cash out this foundedness modally: 'If … did not exist, it would not be the essence of ___, and therefore ___ would not exist'. For example, if E(x) did not exist, it would not be the essence of x, and therefore x would not exist.

My defence has no bite, here. For, if successful, it only shows: 1) that essences beyond E(x) are not immediately essential to x; 2) that, even though they are mediately essential to x, that is because of the preceding, not the following, essentialist steps. But the new difficulty, as far as x is concerned, is purely existential and, as such, is not touched by issues of relevance. The reason why the existence of x requires the existence of infinitely many essences is not that these are all somehow essential *to x*—which is the notion the Relevance Principle is meant to undermine. It is, rather, that each essence in the sequence must exist and therefore, by (EE4), requires *its own* essence. Essentialist steps are here, from the *quidditas* point of view, independent of one another: it is only existence that is transmitted through the chain.

It thus looks like (EE2), although it does not indefinitely postpone essence, does, with a little help from (EE4), indefinitely postpone existence. Yet, I think, appearances deceive. The main reason is that, recall, we are dealing here with objectual essence. But incompatibilities between essences are all and only about generic essences. Take the property of being the greatest prime. It is incompatible with the essence of the integers, and that is why there is no greatest prime. But the incompatibility has nothing to do with the objectual essence of the property—which includes items like the property of being a property, the property of being an abstract object, and so on. What does the work is the generic essence of the property of being the greatest prime, which includes the property of being such that any integer greater than the greatest prime will have other divisors than 1 and itself. But then we are back to the problems of the generic regress—and, as we have seen, they are problems for the Serious as well as for the Eccentric Essentialist. Moreover, finessing the point, (EE2) can be

amended so that it requires that it is only necessarily existing entities that can be objectual essences. That way, once incompatibilities have been sorted out by generic essence, the existence of objectual essences is guaranteed. The amendment, I conjecture, is not foreign to the spirit of Eccentric Essentialism: someone so keen on seeing essences as entities—such as properties and so on—would probably not refrain from seeing properties as necessarily existents.

If all of this is correct, then Lowe's regress is not a good reason to discard Eccentric Essentialism. True, the epistemological side of Lowe's argument still needs addressing (though it looks like the distinction between mediate and immediate essence could go some way towards doing so). My aim here, however, was never to secure the view against all of its difficulties, but only to show that Lowe's regress is not among them.

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Organon F 25 (3) 2018: 429-430

Note on Russell and the Materialist Principle of Logically Possible Worlds

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This note would be best placed at the end of my seventh comment (my 2018, 273) on Vacek's second doubt (Vacek 2017, 265) about my book (my 2016). But as a stand-alone note, there is no need to keep track of that. The question is how or whether new or hitherto "alien" objects can come into being in any possible world, or at least in the actual world.

Possible worlds are defined by which objects are in them, past, present, and future, and by their interrelationships. Although within many worlds, objects begin or cease to exist from the temporal viewpoint (sub specie temporis) of temporal observers within those worlds, *by definition* of any possible world, all of its objects are already there. They are included in the very definition of that world. For a possible world is not identifiable except in terms of all (and only) the objects it includes and their interrelationships. We may, if we wish, say that the definition of a world takes a timeless viewpoint (sub specie aeternitatis). But what is important is not that the viewpoint is timeless, but that the inclusion of objects in a world is purely definitional. (That is the reason *why* it is a timeless viewpoint.) This already solves the problem of new objects for Russell and Leibniz. Namely, by definition of "possible world," there are not and cannot be any new objects "coming into" any possible world, either by moving from one possible

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world into another one (as if we were carrying a chair from one room to another, and perhaps somehow also still leaving the chair in the first room), or by simply coming into being out of nothing (ex nihilo) in the possible world in question, or in any other way. We may, if we wish, say that the temporal viewpoint is limited, relative, and illusory compared to looking at spacetime as a whole. But what is important is that this is a definitional matter. To change the objects (their existence, their properties, or their relationships) in the slightest is by definition to change the possible world into a different possible world. And as is often noted, this is so regardless of whether we finite humans are able to state the complete definition of any possible world. To criticize this solution is to criticize Russell (and Leibniz), not my book. (Russell wrote a book on Leibniz.)

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Hubert Dreyfus and Charles Taylor: *Retrieving Realism* Cambridge, MA: Harvard University Press, 2015, 171 pp.[1]

This short book presents a well-structured critique of the traditional Cartesian epistemology. It offers a philosophical outlook which, as declared at the beginning, is supposed to retrieve realism and hence avoid extremes in philosophical discourse; such as extreme realism, reductionism or scientism, on the one hand, vague relativism and subjectivism on the other. In the bigger picture, Dreyfus and Taylor try by means of this book to answer a traditional philosophical question "What is the world we live in?" therefore even "What is real?"

The book is divided into eight chapters. In the preface, there is a dedication to Rorty, whose thoughts and objections are discussed in throughout the book. At the beginning, we are presented with a metaphor of a picture, a concept borrowed from Wittgenstein (PI §115), in which we are somehow stuck in. This picture is mediational and it accompanies us the whole life; it affects our way of thinking, behaving and our language. It is a legacy of modern epistemology since Descartes which presents knowledge of things mediated only through inner states/ideas/representations. Our main objective, according to the authors, therefore, is to break ourselves free from the above-mentioned picture in order to see clearly what surrounds us. With clean sight, we are able to avoid many common mistakes while constructing a theory. The authors clearly describe this picture as "not fully explicit"; it is rather "a kind of captivity because it has prevented us from seeing what is wrong with this line of thought" (the mainline epistemological thinking). Provided we manage to identify the picture would be like grasping a big mistake.

Authors claim that it is the idea of grasping external reality by means of internal representations which veils the picture-free reality surrounding us and makes it easy for us to misunderstand the world. What seems to bother them the most is, on the one hand, the claim that in any mediational theory, knowledge gets to us "only through" such representations, ideas, etc., and on the other, that this picture is being taken as given.

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In order to define this picture, so that we could escape it later, one must, according to the authors, identify the four interwoven strands. We can consider them to be attributes of any mediational picture: 1) it has the "only through" structure, 2) the content of our knowledge can be analysed into clearly defined, explicit elements, 3) when justifying our believes one can never go beyond/below these defined elements since they have the status of immediate givens and 4) it has dualist sorting, e.g. with the mental-physical distinction. Needless to say that in some cases one or another strand can be broken, in other words, missing.

As the authors mention, there have been many attempts to deconstruct the meditational picture by breaking the dualistic point of view, such as linguistic turn, materialist turn or Kant's critical turn. However, these all still kept the "only through" meditational element, therefore failed and stayed captured. The major turn in deconstructing mediational theories was made in the 20th century. At the same time alternative contact theories were being elaborated; they were based on the rejection of Cartesian and Empiricist epistemologies.

A contact theory soughed by the authors should capture that we are in a direct contact with the world and this contact is primordial, therefore we cannot escape it. We grasp the world and the framework gives its sense to our "grasps of reality". It points out reembedding of thought and knowledge in the bodily and social-cultural context in which it takes place. Furthermore, such a theory has a temporal depth and has to be seen as having holistic attributes.

The authors further claim that one must understand that our grasps of the world cannot be just representational and that knowledge, properly understood, not only consists of representations but it is lodged within individual minds in the first place. Our grasps are shared and then secondarily imparted to each one of us.

Heidegger and Merleau-Ponty presented an example of successful deconstruction of the mediational view, therefore Dreyfus and Taylor used some of their thoughts as a basis for their own contact theory. On the other hand, they considered Rorty to be a "hidden representationalist", although he was trying to free himself of representations. May that be as it is, it must be acknowledged that debates with Rorty gave the authors some precious perspectives and possibly, if Rorty had not died before the book was finished, it might have helped Dreyfus and Taylor to explain some uprising questions considering their contact theory, to which I am coming back later in this review.

Dreyfus and Taylor's own conception, presented almost at the end of this book, was given a name "pluralistic robust realism". It is basically built upon two pillars: on the one hand, it is phenomenology—the thoughts of Heidegger and Merleau-Ponty, and on the other, the critical perception of Rorty. Its crucial terms

include grasps of reality, embedded coping, embodied agent, direct access, affordances, and temporality.

The first pillar can, therefore, be interpreted as a holistic understanding of reality and people living in it; it is an attempt to combine exact science and philosophy in the frames of understanding the reality surrounding us. Our knowledge is influenced by culture and society, therefore, has to be taken as a whole. It claims that a person is embedded in reality and forced to act on it. We are in contact with this reality and we cope with it (deal with it)[2] on an everyday basis—we cannot escape this contact because it is primordial. Objects of reality are neutral but they have their affordances which are filled according to the context and time-dependent when grasped (by us). This may characterize the phenomenological part.

The second pillar encompasses debating and reacting to Rorty. He claimed that all objects, even those studied by natural science, are only intelligible on the background of our embedded coping so that the idea of a view from nowhere is literally unintelligible. In other words, we cannot achieve the view of nowhere because we are in a way limited by our own form; we are human and we perceive as humans. The authors, however, considered this as "deflationary" realism and tried to offer their "robust" realism instead, claiming that in order to understand the status of the structures studied by natural science, we have to make sense of an independent reality.

Not only here did the authors part ways with Rorty. He banned the picture, claiming there was none. He also pointed out that we should get away from a number of philosophical dichotomies which have supposedly outlived their usefulness. However, Taylor and Dreyfus argued that in order to escape the mediational picture we need to identify it and deconstruct it. Thus they found Rorty's approach to knowledge representationalistic. As representations feed the mediational picture, Rorty never managed to escape it.

However, while explaining why their realism is plural, the authors agreed with Rorty at least in one part. They shared his opinion there is no one language for correctly describing nature but right after they counter Rorty, that there could well be many languages each correctly describing a different aspect of reality.

Dreyfus and Taylor explain the position of pluralistic robust realism subsequently: 1) there are multiple ways of interrogating reality (the plural part), which 2) reveal truths independent of us—such truths that require our thinking to be revised and adjusted in order to grasp them (the robust realist part) and where (3) all attempts fail to bring the different ways of interrogating reality into a single mode of questioning that yields a unified picture or theory (so they stay plural)."

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[2] Dreyfus used the term "coping with things", Taylor used "dealing with things".

Rorty's critical reception pointed out the drawbacks of this philosophical concept. For example, his claim concerning outlived dichotomies supports a thought which I personally could not get rid of while reading this book. The authors highlighted a number of dichotomies throughout the centuries, arguing that we have to stop distinguishing between the inner and the outer, mind and body etc. However, they keep one of the dichotomies themselves in their plural robust realism and it stands right in the name. On the one hand, they declare that we are in direct access with reality and we cope with it, but on the other, they keep the science view of the world when presenting this concept as "realism", therefore the dichotomy of inner and outer still lives.

Taking into consideration the above-mentioned argument, I cannot help but see some similarities with the constructivist point of view, namely Goodman's. The realist view of science is at the first sight the reason why the Dreyfus-Taylor's concept seems to be incompatible with Goodman's worldmaking concept. However, in his *Ways of Worldmaking* (1978) Goodman, as a pluralist, admits that there may be the one world "W" but it would be uninteresting and boring. Furthermore, there are other similarities, for example, affordances could be well interpreted as Goodman's versions, contexts influence the way we see and perceive the world, etc. Dreyfus and Taylor tried to avoid any form of non-realism or anti-realism, but Goodman was neither; he proclaimed himself an irrealist. It remains unanswered why Goodman and his worldmaking, or any kind of constructivism as such, has not been taken into consideration while composing the pluralistic robust realism.

The above-mentioned criticism may be considered irrelevant when compared to such a complex theory which the authors managed to create. Clearly, it is not easy to describe everything they wanted to show. It is possible to see that they expected to be criticized therefore they had tried to support their claims by many other authors as evidence; whether against their way of thinking or to justify it. Sometimes the examples of authors are just too many and that might distract the readers, though. The book itself is somehow a piece of art, a little chaotic but it makes sense in the end. Furthermore, the concept of pluralistic robust realism is definitely worth to be explored deeper and discussed more. Because as the authors say—we cannot escape it.

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Vladimír Marko: *Four Ancient Arguments about Future Contingencies* Comenius University in Bratislava, 2017, 369 pp.[1]

An interesting monograph by Vladimír Marko, dedicated to ancient logic and entitled *Four Ancient Arguments on Future Contingencies* (in Slovak: *Štyri antické argumenty o budúcich náhodnostiach*), saw the light of day at the end of the previous year. Marko decided to analyse four selected ancient logical arguments that form the *loci communes* of the ancient, medieval, and even contemporary discussions of logicians, philosophers, and thinkers in general that are interested in the formal aspect of our thinking about the world. This is a rare publication in Slovakia, the Czech Republic, and indeed the wider Central Europe region, because only a handful of authors from it deal with ancient logic.[2] One can read between the lines in the *Preface* that Marko's ambitious aim is to use this book to make ancient logical arguments popular, and even revive their spirit and thus integrate them into the mysterious process of initiating the young to study logic. In this review, I try to estimate the prospects of Marko's book when it comes to fulfilling this aim.

The publication, which is described at the very beginning—and rightly so—as a "scientific monograph", is approximately 370 pages long. Marko presents here the results of his many years of research in ancient argumentation. The book is equipped with two indexes while the extensive bibliography gives separate lists of historical sources, the newest critical editions of Ancient Greek, Roman, and even some Medieval and Byzantine authors and the texts by modern authors. It contains also the index of the acronyms—those mostly used in the renowned dictionaries and critical anthologies of the source texts—what permits easier orientation in the otherwise complicated pile of bibliographical references.

In addition to the *Preface* and the *Introduction*, the book is composed of core chapters dedicated to particular ancient arguments. The introductory chapter is of great methodological importance, because it points to possible difficulties in the

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[2] To give an example, let us mention F. Gahér with his important works on Stoic logic, namely Gahér (1994, 2006).

interpretation, study, and teaching of ancient logical theories. Marko draws our attention to several possible scientific approaches towards the source texts, and he comparatively considers the advantages and disadvantages of every methodological approach. Above all, he tries to demonstrate that ancient texts in general cannot be read and interpreted as isolated and individual units. According to Marko, this is a mistake made by both students and some researchers when they insufficiently take into consideration many relevant layers of the text that should be approached in terms of the so-called "principle of charity" (Wilson 1959; Davidson 1974; etc.). I consider this part of the text to be an especially valuable and useful methodological manual for anyone who deals with ancient sources. It does not matter whether they are texts with a totally logical focus and whether the interpreter approaches them with the motivation to undertake a logical analysis. I recommend Marko's *Introduction* to all students of philosophy, logic, and the history of thinking in general—especially at postgraduate level—as well as to any philologists who want to approach the subject of their own research interest in a truly scientific fashion.

In terms of length, the first chapter considerably exceeds the others; it has a sort of mini-monographic character supplemented with a summary and even an appendix. The extent of this part of the book corresponds to the seriousness, importance, and notorious reputation of its subject area—reflections of *Aristotle's tomorrow's sea battle*. The second chapter, humorously called *Looking for the Lazy Argument Candidates*, deals with a lesser known but all the more academically attractive "Lazy Argument" or "Idle argument" (ἀργὸς λόγος). It is trying to prove, from the viewpoint of the logically driven fatalism, how our efforts are in vain, for example, when taking care of our own health or even in the fulfilment of duties required to get credits. I consider this part of the book to be exceptionally attractive and accessible to readers and students, since the use of formal logical means to analyse the argument is reduced to a necessary minimum. That is why I recommend incorporating the Lazy Argument as a possible topic into specialized courses on ancient philosophy or the history of logic. A philosophically interesting part of the chapter is the subchapter *Many Faces of Fatalism* (pp. 144-172), where Marko discusses different interpretations of fatalism—not only those originating from the ancient times but also those provided by present-day authors. Another important chapter of the monograph—the third and the shortest one—deals with a lesser known ancient argument known as "The Reaper". The chapter's title, *Some Sketchy Notes on the Reaper Argument*, corresponds to the reduced extent of this part. It is a refreshing intermezzo before the stirring finale of the official part of the work. It provides an extensive analysis of Diodorus' *Master Argument* (περὶ δυνατῶν). Apart from the interpretation of the ancient argument itself, it contains quite an

extensive (I do not know whether complete, but definitely "exhausting" for readers) list of pioneering reconstructions of the argument in question. I must admit that in this part, the analytically not-so-well oriented reader can get lost, and this is why I recommend it only to logically proficient readers. The book also contains two appendices—one dedicated to an argument on the rival grammatical conceptions of the anomalists and the analogists and another one dealing with Cicero's attempts to translate the term ἀξίωμα.

Marko's analysis of particular ancient logical arguments is very complex, systematic, and thoroughgoing. It reveals his historical and philosophical thoroughness as well as the *acribia* of the logical and analytical approach. Functional references and quotations of the source texts (in Greek and Latin) are not merely emblematic decorations of the text—as it sometimes happens, unfortunately—with works from ancient times; rather, they demonstrate that Marko does in fact work with the original sources and consults the respectable critical editions of ancient authors.

The reviewer is traditionally required to express some objections or criticism, or to reproach the author for mistakes in the assessed work. When I admit that I was looking for mistakes in Marko's text only with greatest difficulties, I do not mean just to flatter the author. I do not dare to judge the details of the formalized analysis of particular ancient arguments—I will gladly leave this task to other, more analytically focused, reviewers. As a Classical philologist, I was vainly looking for mistakes in Ancient Greek and Latin, terminological misunderstandings, and ambiguousness. In the whole publication, I found only one typo in Greek, specifically in the phrase δι ᾽ ἑνός λήμματος λόγος on p. 194. One thing I do not understand is why Marko mostly used only Latin transliterations of ancient Greek terms and original passages in the chapter on "The Reaper". This question is more pressing, because right in this chapter the philological dimension of the text is perhaps most noticeable. According to my opinion, the use of Latin transliteration instead of the Ancient Greek polytonic alphabet is not appropriate here.[3]

I know that with this review I did not live up to the expectations of the readers of *Organon F*, who expected that I would dive into an erudite criticism of Marko's formalized analyses of ancient judgement-based schemes. I repeat that I will gladly leave this task to other more qualified peers. My aim was rather to emphasize what

[3] The argument that we commonly come across transliteration in newer books of Anglo-Saxon provenance is not fully persuasive. To transcribe ancient Greek in Latin script is the same barbarianism as using speakers at a classical concert. Marko's ancient logical symphony does not deserve such a thing.

makes Marko's publication useful for a less analytically erudite reader. I also wanted to answer the question of whether Marko's ambition expressed in the *Preface* is well grounded—we learn in between the lines that by this book he would also like to enrich the teaching of the history of ancient philosophy and the history of logic. My conclusion is that at least some parts of the book—especially the methodologically oriented *Introduction* and some analytically less demanding parts (in the chapter on "The Lazy Argument")—can certainly be used in teaching the subjects in question. Finally, I would like to ask Marko for something unusual. It would be helpful if some of the future editions of his book were adapted to the needs of non-analytically focused readers, particularly students. A much larger audience would then be able to enjoy his *opus magnum*—if not in the form of a grand symphony of logic, then at least in the form of its piano transcription.

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Philosophy of Fiction: June 27, 2018, York University (UK)[1]

Fiction, the semantics of fiction, the metaphysics of fiction, and fictionalism belong to the pivotal topics in contemporary analytic philosophy. Given the interest in the topic, it is not a surprise that events dedicated to its problems abound. One such event was hosted at the Department of Philosophy, University of York.

The workshop entitled Philosophy of Fiction took place on June 27, 2018 and provided a vivid platform for contemporary research in the philosophy of fiction. The workshop commenced with Peter Lamarque's (University of York) "Fiction as a Practice", arguing that there are plausible ways of looking at a fiction more as a practice than a theoretical investigation. Bjørn Jespersen (VŠB Technical University of Ostrava) in his "Vulcan Revisited: Is the F an F?" provided a hyperintensional account for nonexistents and a hyperintensional account of requisites by means of Transparent Intensional Logic. Gregory Currie (University of York) investigated the role of "The Narrator within the Fiction", revisiting and discussing his earlier views. Robin Le Poidevin (University of Leeds) tried to explain "Truth in the Dream", comparing it to truth in fiction. Martin Vacek (Slovak Academy of Sciences) in his "Do You Want To Be an Ersatzer? You can Have Fiction for Free" proposed a unified treatment of fictional characters and alien individuals within a particular metaphysical picture of modality. Finally, Daniela Glavaničová (Slovak Academy of Sciences, Comenius University in Bratislava, University of York), who organized this workshop, presented a hybrid view on "Fictional Names and Negative Existentials".

The workshop was unique from at least two points of view. From the aesthetic point of view, it took place in a beautiful medieval building King's Manor in a magnificent city with a rich and impressive history. From the academic point of view, it provided an opportunity to present and discuss intriguing philosophical ideas and views, and opened a new dimension of international cooperation between

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the Slovak Academy of Sciences and the UK researchers, the dimension not acknowledged till these days.

Martin Vacek