Namely-Riders: An Update

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I here recall Ryle's analysis of Heterologicality, but broaden the discussion to comparable analyses not only of Heterologicality but also other puzzles about self-reference. Such matters have a crucial bearing on the debate between representational and non-representational theories of mind, as will be explained.

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[1] The most remarkable thing about the so-called Heterologicality Paradox is that a straightforward logical analysis of the case shows there is no proper paradox. The appropriate analysis, moreover, has been published more than once, yet even its author did not see that there was no problem.

[2] If the reader will take the trouble to consult, for instance, the 4th edition of Irving Copi's Symbolic Logic (Copi 1973), then, in the chapter about the theory of types, he will find the definition of heterologicality, and then a supposed proof of the contradiction:

Het'het' iff -Het'het'.

But Copi has to explicitly assume ''het' is univocal' at one place in this 'proof', so his otherwise thorough analysis shows instead that the contradiction is not derivable as a matter of logic, but rests instead on a contingent premise - which therefore Reductio requires we deny.

[3] I will have some things to say later about Copi's blindness to his assumption. Indeed there are comparable blindnesses with many other 'self-referential paradoxes' which have impeded understanding of these matters, as we shall see. But in a volume celebrating the achievements of Ryle, it is appropriate first to realise that Ryle (Ryle 1951), saw the relevant point:

> ...what we are asked to decide is whether 'heterological' and 'homological' are themselves heterological or homological...But to ask this is to suppose that 'heterological' and 'homological' do stand for philological properties...And this supposition is false.

Ryle then went on to make his well-known point about the unavailability of namely-riders in this case:

> If unpacked the assertion that 'heterological' is heterological would run:- 'Heterological' lacks the property for which it stands, namely that of lacking the property for which it stands, namely that of lacking the

property..." No property is ever mentioned, so the seeming reference to such a property is spurious.

[4] I made a rather similar point myself, in Slater 1973, in an article Ryle kindly accepted while editor of *Mind*. For although there is trouble with the definition

'x' is H iff 'x' is not x,

there is no problem with

'x' is not self-applicable iff 'x' is not x,

so long as it is recognised there is a variable in the now fully expressed predicate, i.e. that 'self' is a pronoun replacing "x". For

'x' is -x iff 'x' is not x

is perfectly unobjectionable.

[5] It is useful, however, to keep in mind the Copi version of this kind of point, for, from his 'proof' we see that what is necessary is just that

if 'het' just means one thing (Het) then Het'het' iff -Het'het',

and that leads us to a clear proof of Ryle's point in the form:

'het' does not just mean one thing.

But the fact that a condition has been suppressed, which must be negated, is a parallel feature in several other cases. Thus a demonstration that the Liar (and associated) paradoxes do not arise, given a proper logical analysis was provided in this style by Sayward (Sayward 1987), following work by Goodstein, and Prior. For the T-schema presumes univocality of the sentence said to be true, so the necessary fact is not that schema itself, but instead

if s only means that p then Ts iff p,

and the now explicitly stated assumption shows the paradox does not arise when

s = '-Ts',

without a further premise, that

s only means that -Ts.

But the contradiction which would otherwise be available means that this further premise must be denied.

[6] The key point which maybe obscures this, even for many people today, is that the shift between the two latter indented statements involves a shift from direct to indirect speech. In the historical times when it was thought there were paradoxes of self-reference this distinction, significantly, was not formally expressible in the published logics. It only started to become formally available with the development of modal, and general intensional logics in the late 50's, and early 60's, since 's says that p' is formally parallel to 'a believes that p' etc. Even with the first given formalisation of indirect speech, by Goodstein (Goodstein 1958), which led to an understanding of grammatical operators, and the development of the propositional attitude logics, blindness to the direct/indirect distinction was present. For in Goodstein's initial case, if

A says that everything A says is false,

then we can prove

Something A says is true,

and

Something A says is false

and even Prior, in Prior 1958, was puzzled by this kind of result. Thus, if there was a sentence in a book, which said that everything said in the book was false, then Prior originally thought that, by the above kind of proof one could show by logic that there must be a second sentence in the book! Certainly something said in the book must be true, and something said in the book must be false, but that does not require there to be two sentences in the book - since we are dealing with 'saying that', i.e. indirect rather than direct speech. I made this point in Slater 1986, showing that all that is required by logic is that two different things are meant by any sole sentence in the book, i.e. that it is not univocal, as in the previous cases above. Prior himself started to see the distinction himself, in the chapter 'Tarskian and non-Tarskian Semantics' in Objects of Thought (Prior 1971, Ch 7). In particular, if the operator form 'it is true that p' is written 'Vp' then we can say, as he did:

Ts iff (p)(if s means that p, then Vp).

And from this can be derived Sayward's conditionalised T-schema - given that Vp is logically equivalent to p, i.e. that operator-truth is the null, identity modality.

2

[7] Now if the non-univocality of the Liar, and Heterologicality were just ordinary ambiguity then one might get a very proper demand for some namely-riders: 'It is a bank' has two senses, which can be specified, namely 'It is a money bank' and 'It is a river bank'. But the

lack of namely-riders is essential, in some cases. The general demand that we should be able to discriminate meanings, or implicit sayings, in parallel to explicit sayings is, of course, a common feature of compositional theories of meaning, as in, for instance, the Tractarian Picture Theory of Meaning. But meanings must be non-compositional otherwise there would be real paradoxes.

[8] This was shown by Thomason, in his and Montague's arguments for an operator analysis of propositional attitudes - of which 'it is true that' above is, of course, just one example. For Montague had shown, quite generally, in Montague 1963, that 'Indirect Discourse is not Quotational', as Thomason put it (Thomason 1977). But Thomason not only supported Montague, he also showed that a comparable argument proved that indirect discourse was not about any structured intensional objects either. Thus it was not, for instance, about some semantic analogues of sentences in a 'Language of Thought'. This led Thomason (Thomason 1980, see also Slater 1998, Ch 2), to the conclusion that representational theories of mind, such as Fodor's, had to be inconsistent, when fully worked out. As Asher and Kamp said (Asher and Kamp 1989):

> To happy-go-lucky representationalists, Thomason (1980) is a stern warning of the obstacles that a precise elaboration of their proposals would encounter...[W]ith enough arithmetic at our disposal, we can associate a Goedel number with each such [representational] object, and we can mimic the relevant structural properties of, and relations between such objects by explicity defined arithmetical predicates of their Goedel numbers. This Goedelisation of representations can then be exploited to derive a contradiction in ways familiar from the work of Goedel, Tarski and Montague.

Asher and Kamp, nevertheless, were at the time trying to re-construct a representational theory of the attitudes despite this, on account of a difficulty they thought they had seen in non-representational theories. Asher and Kamp had produced what they took to be a demonstration that such non-representational accounts must fail (e.g. Asher and Kamp 1989, p94). For if an arithmetisation of the notion of expression is formulated, by means of some relation which says that the formula with Goedel number n means that p, then it seems that fixed points can be generated, and standard difficulties with self-reference lead naturally to contradictions. But Asher and Kamp's 'proof' presumes that the expression relation is 1-1, and there is no guarantee that this is so. Comparable consideration of self-referential forms generated with an expression relation which does not presume univocality lead to no trouble, as above (Slater 1986, 1991). Indeed there is a simple consistency proof which guarantees such an operator analysis cannot lead to contradiction in Goodstein's original paper on the operator approach. It follows that different meanings cannot always be discriminated in different sentences as compositional theories of meaning would require. In Goodstein's original case, for instance, we can go on to talk about

that thing which A says which is true,

and

that thing which A says which is false,

but there need not be any way in which we can specify them further.

[9] In close connection with this, there is the otherwise largely technical question of just how the quantification involved with operator constructions completely works. What is the quantification over? Propositional quantification is normally taken to be substitutional, but one very forceful argument against such an account is that then there would not seem to be a stateable truth condition in the following kind of case (c.f. Loux 1998, p150):

(Ep)(there is no linguistic expression in English for the thought that p).

Indeed, Loux has made this difficulty his entire basis for rejecting Prior's account. For, of course, in such a case there expressly cannot be any namely-riders, if the statement is true. But it is not the case that the only complements in operator constructions are explicit used sentences, they can also be propositional descriptions. Thus we can say, for example, 'Tom believed what Peter said'. And such a description can be provided in Loux' case, even if, necessarily the proposition in question cannot be given directly. Thus we can talk about 'that proposition there is no expression for in English', which can be formalised using an epsilon term at the propositional level (see Slater 1989.) And then the truth condition in the above case is that that proposition there is no expression for in English is indeed inexpressible in English - which follows the standard epsilon definition of the quantifiers. Once the 1-1 nature of Asher and Kamp's expression relation is removed (c.f. Turner 1991, p11) we must accept that there may be things which can only be gestured towards, and not explicitly said.

[10] These points clearly bear, as well, on the possibility of replicating operator accounts with syntactic ones in the manner of des Rivieres and Levesque 1986 - see also Reinhardt 1980, Schweizer 1993. Isomorphic structures have been constructed which seem to show that non-representational accounts of the attitudes are (materially) equivalent to certain representational ones. But that would, as before, conflate indirect and direct speech. Des Rivieres and Levesque notably do not consider languages which include their own expression relations, and indeed have to explicitly exclude self-referential cases (des Rivieres and Levesque 1986, p126f), so they have not faced up to all the questions which self-reference and expressibility in a language produce.

[11] Neither, in the more discursive philosophical tradition has Stephen Schiffer, for instance. Schiffer, in his book *Remnants of Meaning* (Schiffer 1987), argues very tightly, but non-mathematically, against both a 'sentential' and a 'propositional' construal of attitude reports, which, at a distance, parallels Montague's and Thomason's formal work quite well. Moreover, he also defends substitutional quantification for properties and propositions, like Prior (Prior 1971, p35), and a non-compositional theory of meaning, which we have seen is required by the operator account. Schiffer, however, would still

prefer a predicative account in terms of some causal 'Language of Thought'. Thus he wants to assert at one place (Schiffer 1987, p206):

(p)(Es)(Harvey believes that p iff R(Harvey, s)).

But while this quantifies over 'p', rather than 'that p', as the operator theory requires, it gives, as before, a demonstrably false account of the operator construction. For since s is a syntactic entity the biconditional is falsified directly by Montague's work showing that operators are not equivalent to meta-linguistic predicates. The central grammatical difference is that operators take sentences to make other sentences, whereas predicates take names to make sentences. And that means that operators do not describe, but merely qualify the propositions they act upon. Furthermore, although Schiffer uneasily settled for the use theory of meaning, he did not see the further central fact about operators in that connection, namely that, even is s means that p the operator form alone incorporates the meaning of the associated syntactic string: it does so simply by using rather than mentioning 'p'.

[12] Fodor crucially forgot this possibility about meaning when arguing for his theory that propositional attitudes are relations to internal representations. For instance, in 'Propositional Attitudes' (Fodor 1991) he argues for relations to internal representations, rather than external speech. Certainly in indirect speech the words in the complementary clause gain their meaning expressly by being internalised. But they are internalised just by not being merely mentioned, and so they are still a form of external speech. Also they are then internalised by the interpretative reporter not the agent, and their internalisation is just a matter of adoption and use, not structured representation.

[13] Fodor's type of internalism is explicit in his discussion of 'Objection 2' to the above paper, although it is possible he would be happier with propositional attitudes being relations to what his internal representations are supposed to represent, namely propositions and facts. Such items might more easily be thought of as 'in the world', and so there is a little remarked quasi-externalism in Fodor's remark, regarding 'Objection 1', that propositions are the mediated objects of propositional attitudes. But given Fodor's first formulation of his theory above, which is more well-known, Montague's and Thomason's work applies, as with Schiffer. Likewise Goedel's result applies, and also Tarski's Theorem, forcing a hierarchy of metalanguages, which was shown to be impossible in the late 50's again (Cohen 1957, c.f. Haack 1978, p144). For a recent reiteration of the impossibility see Turner 1991, p7. None of these issues are addressed by Fodor, yet, as Thomason had previously said, they are final against his language of thought account, on the above common presentation.

[14] But there are difficulties also with Fodor's 'mediated' theory, as he presented it at the place above. For he there stressed the need for internal representations on the basis that propositions 'don't have forms', believing that a theory claiming 'empirical repute' would have to apply in virtue of the form of the entities in its domain, and so making propositions a problem. He is right that (non-reified) propositions do not have forms - they are structureless, in Stalnaker's terms (Stalnaker 1976), for the very reasons Fodor gives -

despite his wanting a compositional theory of meaning. But introducing immediate objects with forms does not get over the problem of formlessness (if it is one), since the relation between the immediate object and the proposition still brings in a formless 'entity'. In short, 'a believes p' is maybe a 'relation' between a person and something formless, but re-writing it as, say,

(Es)(a Bel s.s means p)

does not eliminate the element which has no form. The point is obscured if the theory is presented as first above, namely as equating 'a believes p' with 'a Bel s'. Schiffer argues, as we saw, that the relevant semantics is non-compositional (Schiffer 1987, Ch7), and that, of course gets at this another way. The extra clause needed, 's means p', like Asher and Kamp's expression relation, is properly another 'relation' with a formless entity. And that also negates Fodor's 'decisive' argument against Schiffer, at the end of Fodor's review of Schiffer's book (Fodor 1990). For Fodor there tries to defend compositional semantics by insisting that there must be distinctive truth conditions for distinct expressions of Mentalese. But given the above points about the sometimes necessary absence of namely-riders that cannot be guaranteed.

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[15] Does the above have any further relevance to the other puzzles about self-reference which gripped the minds of philosophers and logicians earlier this century? I mentioned before the blindness which Copi seemed to suffer when thinking he was driven to an inescapable contradiction in connection with Heterologicality. A very similar blindness has also clearly affected many discussions on Truth - those which take Tarski's T-schema to be unexceptionable. But somewhat similar blinkers would seem to have been worn in other cases of self-reference.

[16] This is very clear in the case of Berry's Paradox, since Graham Priest, for instance, in his derivation of the seeming contradiction requires the denotation relation, as with Asher and Kamp, to be 1-1 (Priest 1983, p162). But if 'the least ordinal not definable in less than 19 words' defined, i.e. uniquely referred to some object, then that object both would and would not be definable in less than 19 words. Hence some identifying descriptions cannot have a unique denotation. To think otherwise, indeed, is to forget the function of demonstratives like 'this' or purely referential terms like Donnellan's 'the man drinking Martini', whose semantics does not determine, in either case, a unique referent. In such cases there is no 'namely-rider' without a further parameter being given. And there is no problem about 'the least ordinal not definable in less than 19 words' then not correctly describing what it refers to, since that merely means it is non-attributive, and gains its reference gesturally from the context in which it is used. I have formalised the general point about pure reference in my many discussions of the epsilon calculus, and in particular I have given the specific analysis of Berry's case in Slater 1988, p211.

[17] Priest, of course, has been so persuaded of the inescapability of contradictions that he has proposed we accept there are true ones. But you might as well say that we should accept there could be composite primes: it is in the definition of contradictories that they

cannot be both true, so Priest's 'dialethism' is easily met with firm resistance (Slater 1995). And, sensibly enough, more appropriate approaches to the phenomenon of inconsistent information (rather than inconsistent reality) have been presented (Brown 1999). It was Russell's Paradox in Set Theory, of course, which magnified interest in the notion of self-reference, and it was that particular paradox which inspired the other main 'paraconsistent' logician, Newton da Costa in his development of non-classical logical systems (da Costa, 1986). Da Costa constructed systems which withdrew from classical logic merely to the extent of not affirming '-(A&-A)'. But it is only recently that a start has been made on an approach to sets which makes such an allowance for absurdity itself absurd.

[18] Thus Harry Bunt has formulated a general theory of 'ensembles' which includes sets as a special case (Bunt 1985, see pp262-3 in particular). Bunt does not develop the idea quite to the best effect, but given there is a condition on an ensemble which is required to produce a set, then the Abstraction Axiom, like the T-Schema is not unconditional, and traditional problems with self-reference in connection with it then merely return one to the reverse case - namely to where no definite number of discrete elements is determinable, as when (but not exclusively when) the predicate in question is a mass term. That gives a theory of continua which involves indeterminate numbers, not excessively large ones (c.f. Bostock 1974, 1979), and so one blindness which has held people to many paradoxes in set theory would seem to involve forgetting that with an amount of stuff no number can be specified without an extra parameter - the nomination of a unit. But there may be discrete items which have no total number, as we shall see.

[19] As Mary Tiles, amongst the few, has noted, Frege's logical symbolism does not discriminate between count and mass terms (Tiles 1989, p151). It therefore suggests, even if it does not assert, that the concept of number is applicable to all concepts whatever. The point exposes clearly the omitted presupposition in Set Theory, which strictly should allow sets to be formed only when there is a determinate number of discrete elements to be collected together. Cantor himself appreciated that there could not be a number, or a set of all things because of his, Cantor's Paradox, and for a time the question of the 'limitation of size' was a preoccupation of thinkers, wondering which infinities might still be consistently numbered. Cantor could, in the end, only offer as a postulate that Aleph Zero is consistent (Hallett 1984, p175), but a stronger point is made by Tiles (Tiles 1989, p63), who reminds us that there is indeed an inconsistency even in Aleph Zero once we attend not to the 'cardinality' or 'power' of the natural numbers, but the size or number of them:

> When two sets (whether finite or infinite) can be put into one-one correspondence with each other they are then said to have the same power or cardinal number...in the infinite case it will mean that an infinite set may have the same cardinality as a proper part of itself. This is, to say the least, paradoxical if one is thinking of number as a measure of plurality, of the size of a discrete collection. The size of a whole should always be greater than that of any of it proper parts. There must surely be more natural numbers than there are even numbers (indeed twice as many)...

In other words, if there were a determinate number of natural numbers it would be the same as the number of the even numbers, because of the 1-1 correspondence, but also it would be bigger, since the even numbers are a proper part of the natural numbers. Hence there is no determinate number of the natural numbers - and therefore no set of them (c.f. Boolos 1998, p306). Here indeed was a paradox about infinity which teased most theorists until the end of the nineteenth century, even if it has been forgotten by succeeding generations of set theorists. But its resolution is in a similar manner to those before: a presupposition of determinacy, now determinacy of the number of some discrete elements, must be negated. What is the number of the continuum? What is the number of natural numbers? There is no namely-rider, in either case.

[20] Tiles thinks that Cantor's proofs of non-denumerability, nevertheless, still require us to discriminate between infinities. She goes on:

> Th[e] consideration [above] by itself would suggest that the fundamental distinction is that between finite and infinite, and that infinite sets are without number not only because they cannot be exhaustively counted but also because even the notion of cardinal number, as a measure of size or numerosity, can get no grip there. And this would have been the correct conclusion to have drawn had it not been for Cantor's discovery that not all sets have the same cardinality, i.e. there are infinite sets which cannot be put into 1-1 correspondence with each other.

But given there are no completed infinities the facts about non-denumerability have to be looked at rather more carefully. In a constructivist account of real numbers they are represented by functions, and then naturally one cannot have a function such that

$$fm(n) = fn(n)+1.$$

But this merely shows that we must discriminate amongst the denumerable between what can be enumerated, and what cannot. No difference in size is involved, merely a difference in being able to specify the function in question - give it as a 'namely-rider' in an extended sense.

[21] Now, if we define 'real numbers' not in terms of impossible Platonic limits but merely convergent sequences of rationals, then we are identifying 'real numbers' with certain functions, since sequences are functions from the natural numbers. But the name 'real number' is then strictly a mis-nomer, since a function is not a number, even if each of its values is one. There is no numerical representation, as a result, of the length of the diagonal in a unit square, for instance. This length is available geometrically, but all arithmetic can do is produce a function which generates a series of approximations. The function is then properly just a representation of the geometric ratio, but naturally is not equal to it. The 'irrational number' is not available 'extensionally' only 'intensionally' it may be said. But the difficulty for arithmetic is strictly more acute, since properly there is no number available at all, in this case.

[22] And the functions which should replace them are 'non-denumerable' merely because there is no enumerating function of them, as Skolem's Paradox has otherwise indicated. For while all computable functions of one variable are enumerable, there is no way to specifically enumerate just those which have completely defined values - otherwise the halting problem would be solved. Hence the ordinal numbers of those functions which generate 'the real numbers', although denumerable are not enumerable. There is, in other words, a more general kind of expression, which is like that for a 'real number' except certain decimal places may be undefined. These are enumerable, but diagonalisation does not produce a further one of them, since neither fm(n), nor fn(n)+1 need equal anything. Amongst the functions which generate these expressions are all the real-number functions, since, to any depth, each string of values, say of 0's, and 1's must be taken. But we cannot, in general, determine which these real-number functions are. Even if fn(x) determines a 'real number', which function it is is only determinable from its ordinal place amongst all computable functions, not from its ordinal place amongst the real-number functions. And that has the result that, if the latter ordinal place is 'n', then fn(x) is not a calculable function of n. Of course if one specifies a sequence just of real-number functions that makes it the case that which function is the nth in that sequence is determinable from n, and fn(n)+1 (modulo the base) will then be a further, distinct real-number function of n. But it is only the specification of such a sequence which makes fn(x) a function of both x and n, and so there is no further diagonal function in the general case, merely a denumerable list which cannot be specified, only gestured towards.

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