RUSSELL AND MACCOLL: REPLY TO GRATTAN-GUINNESS, WOLEŃSKI, AND READ

JAN DEJNOŽKA

In the December 1999 special edition of *Nordic Journal of Philosophical Logic* on Hugh MacColl, Ivor Grattan-Guinness and Jan Woleński describe my discussions of Russell and MacColl in superficially true but unfortunately misleading ways. After replying to them, I proceed to my main topic, whether we can impute S5 to MacColl in light of Stephen Read's paper in the same special edition denying that MacColl has S4 or even S3. I argue that MacColl has an S5 formal modal logic with invariant formal certainties and impossibilities, and following Read, a T material modal logic with material certainties and impossibilities which can vary relative to fresh data, and that MacColl writes these logics using the same generic notation.

1. Grattan-Guinness: Does Russell have an Implicit Modal Logic?

Grattan-Guinness (1999, n.6) correctly reports that Thomas Magnell (1991) had the best of me (1990) in our exchange in *Erkenntnis* on whether Russell has a modal logic. Magnell's two chief questions were, Where is Russell's modal logic in his writings? and What is its strength? I indeed failed to explain these things in my 1990 paper, which was mainly concerned with Russell's underlying theory of modality.

Unfortunately, Grattan-Guinness's report is 11 years out of date. I answer both of Magnell's questions in my 1996 book, and I answer the questions a second time in my 1999 book. My 1999 book describes Russell's earlier and later implicit modal logics and where they are located (pp. 3–4, 6, 8, 9, 16, 20, 53, 62–64, 68, 79–80, 96–97, 111, 120, 165, 187, 199). It has an entire chapter entitled "The Strength of Russell's Modal Logic", and has a note criticizing Magnell's paper in detail (1999, p. 197, n.5). I state in the preface that the book supersedes my 1990 paper precisely because that paper failed to distinguish Russell's theory of modality from his implicit modal logics (my 1999, p. ix).

In my 1996 book I say:

Thomas Magnell (1991) asks two basic questions. First, if Russell has a modal logic, where may we find it in his writings? In *Principia*, though not announced as

Nordic Journal of Philosophical Logic, Vol. 6, No. 1, pp. 21–42.

such. The modal [theory] I call "MDL" is the key to reinterpreting *Principia* as functioning as a modal logic. Why expect poor Russell to rewrite *Principia* when he can explain how to reinterpret it in a few brief lines?

Second, if Russell has a modal logic, what is its S1–S5 strength? MDL is the basis of Russell's modal logic. But we cannot look directly to MDL for the answer. MDL predicates 'necessity' of propositional functions, not of propositions. MDL says only that *F(x)* is necessary with respect to *x* just in case *F(x)* is always true[, that *F(x)* is impossible with respect to *x* just in case *F(x)* is always false, and that *F(x)* is possible with respect to *x* just in case *F(x)* is not always false]. Now, a fully generalized statement which is always true with respect to every one of its variables is necessary without qualification. This, Russell says, is how he intends to analyze logical truths (1994a; the unpublished ms. transcript is cited as c. 1903–5). But he later adds that a logical truth is true in virtue of its logical form, since he comes to realize that full generalization alone is not a sufficient condition of logical truth. Call this new modal logic "FG–MDL*". MDL is just a stepping-stone to FG–MDL*. Now, logical form is timeless and unchanging. Thus in FG–MDL*, whatever is possible is necessarily possible. And that is the distinctive axiom of S5. Thus Russell has the strongest of the S1–S5 logics without admitting any modal entities or even modal notions; "always true" is a veridical notion. In fact, FG–MDL* is stronger than S5. Insofar as Russell admits $(x)(x=x)$ as a logical truth (see PM *24.01; PLA 245–46), FG–MDL* is S5+ I, where I is $\Box(x)(x=x)$. Of course, Russell did not intend that FG–MDL* have a specific S-strength, since he developed FG–MDL* while unaware of C. I. Lewis's S-logics. (Dejnožka 1996, p. 290, n.6)

Thus Russell's early modal logic is implicit in his thesis that logical truth is purely general truth. His later modal logic is implicit in his revised thesis that logical truth is pure generality plus truth in virtue of form. The identification of these implicit logics is due to Gregory Landini. The texts in Russell are well known, especially the famous 1919 (pp. 199–205).

I also answer Magnell in my 1999 book:

I now answer Magnell's two main questions: Where did Russell present his modal logic, and what degree of modal strength does it have? My answers: In *Principia*, using the three equivalences as the key to unlock it; and the same or nearly the same as Hughes' and Cresswell's S5+ I, though Russell would have been unaware of those much later writers (Dejnožka 1996: 290, n.6). [Russell's three equivalences are: *Fx* is necessary with respect to $x=$Df *Fx* is always true; *Fx* is impossible with respect to $x=$Df *Fx* is always false; and *Fx* is possible with respect to $x=$Df *Fx* is not always false.] (Dejnožka 1999, p. 16)

I repeat these points in more detail:

My final topic is Magnell's reply to my 1990 *Erkenntnis* paper. Magnell raises two main objections to my claim that Russell has a modal logic. First, "if Russell ever had a modal logic, we might expect him to have advanced a systematic study of valid forms of modal inference.... [But] Russell did not set out a modal logic in any of his writings that I am aware of." Second, "If there is a modal logic implicit in Russell's work, it need not, of course, be equivalent to one of the S-logics. But if there is even a vague outline of a modal logic to be found, we should be able to indicate its position, whether below S1, between S1 and S5, or beyond S5 with some degree of confidence." But Magnell cannot find even a vague outline. He says that MDL "cannot be more than a small part of even a modest theory" (Magnell 1991: 172–173, 176).

As to the first point, Magnell must be unaware of *Principia Mathematica*. Far from modest, MDL is as grand and robust as *Principia* itself. For MDL is an interpretation of *Principia*. MDL is a formula for reinterpreting *Principia* as a modal theory. FG–MDL* is the set of logical truths in *Principia*....

As to Magnell's second point, Magnell's question is misplaced. MDL is but a building block. It is of FG–MDL* that we properly ask what degree of strength it has. The answer is that FG–MDL* is stronger than S5. (Dejnožka 1999, p. 96)

In my 1999 book, I report a distinguished predecessor in attributing a modal logic greater than S5 to Russell. Nino Cocchiarella (1987) finds an S13 modal logic in Russell. Again, I find what Hughes and Cresswell (1972) call S5+I, that is, S5 conjoined with $\Box(x)(x=x)$, in Russell. I is not in historical S5, nor anywhere else in Lewis that I can find, but it is so often added today that many people now equate S5+I with S5.

As Grant Marler kindly notes, and as I note from the work of Arthur Prior and Kit Fine (Dejnožka 1999, p. 64), it is trivial that standard quantificational logic is S5, if you interpret the logical modalities appropriately. But Russell interprets the logical modalities appropriately, since he says that the concept of logical necessity adds nothing to the concept of logical truth (see Dejnožka 1999, pp. 4, 10). And Russell's interpretation of the logical modalities is express and repeated. Thus I am not attributing S5 to Russell merely because he has a quantificational logic. If I did, that would mean that everyone who accepts quantificational logic also accepts S5, which is absurd (see Dejnožka 1999, p. 16). Russell's concept of logical necessity as truth under any interpretation anticipates Carnap, Tarski, McKinsey, Beth, Kripke, Almog, and Etchemendy, and has antecedents in Venn and Bolzano (Dejnožka 1999, p. 15), not to mention MacColl.

2. Woleński: Does Russell Reject MacColl and Modality?

Happily, Woleński cites my 1999 book. But his first sentence reads, "Frege and Russell, the fathers of mathematical logic, were not very interested in modalities and relations between them[1]" (Woleński 1999). His note 1 adds, "See Rescher 1974 and Dejnožka 1999 on Russell and his objections to MacColl and modal logic" (Woleński 1999). This is superficially true. It is true that Frege and Russell devote comparatively little time to modalities, and it is also true that Rescher and I discuss Russell's objections to MacColl on modality. But Woleński's statements as quoted above are totally misleading because they appear to imply the myth my book combats. This is Rescher's myth that Russell rejects MacColl's theory of modality because Russell dislikes modalities. In fact Russell accepts a modified version of MacColl's theory of modality, and he expressly gives MacColl the credit for it in a "smoking gun" text. In my 1999 discussion of the possible origins of Russell's theory of modality in Frege, Venn, Peirce, MacColl, Bolzano, Leibniz, or Aristotle, I say:

> As the reader may have suspected for some time, my view is that MDL is a modification of MacColl's theory of a "certainty," an "impossibility," and a "variable." The chief reason is that Russell expressly says so:
>
> > Mr. MacColl speaks of 'propositions' as divided into the three classes of certain, variable, and impossible. We may accept this division as applying to propositional functions. A function which can be asserted is certain, one which can be denied is impossible, and all others are (in Mr. MacColl's sense) variable. (LK 66)
>
> Russell's earliest published statements of MDL are generally replies to MacColl, and generally use 'certain' in place of 'necessary' for a propositional function's being always true, following MacColl. See Ivor Grattan-Guinness (1985–86: 118–19). Russell's innovation is to predicate modalities of propositional functions instead of propositions as MacColl does.... (Dejnožka 1999, p. 117)

Thus Russell adopts MacColl's theory, but with two modifications. First, logical necessity is now predicated directly of propositional functions and only derivatively of propositions. (A fully general proposition is logically necessary just in case every propositional function it contains is logically necessary.) And second, the theory is seen not to require Unreals (Dejnožka 1999, pp. 117–18). Russell states his MacCollian theory of modality in nine works over a period of thirty-six years (Dejnožka 1999, p. 4). If Woleński wants to report my views, that is what he should be reporting. Unfortunately Woleński's message, "Russell was not very interested in modalities; see Rescher and Dejnožka on Russell's objections to MacColl," even if it is technically correct and is very carefully and neutrally worded, unavoidably conveys the opposite of what I hold. It sounds for all the world as if I agree with Rescher that Russell rejects MacColl and modalities. By lumping me together with Rescher without explaining that I repudiate Rescher's myth, Woleński makes it sound as if Rescher and I are at one.

Pace Rescher, Russell and MacColl courteously agree that they have more in common than not (Russell 1906, p. 260: "the points of difference are small compared to the points of agreement;" MacColl 1907, p. 470: "The differences between Mr. Russell's views and mine are mainly due, I think, to the fact that we ... do not always attach the same meanings to ... words;" see Dejnožka 1999, p. 118).

3. Read: What is the Strength of MacColl's Logic?

I proceed to discuss whether S5 can be imputed to, i.e., found implicit in, MacColl.

Even though I impute S5 to Russell and find the origin of Russell's MDL in MacColl, imputing S5 to MacColl on the basis of his trichotomy of certain, variable, and impossible propositions would be as wrong as imputing S5 to Russell on the basis of MDL. MDL is not a modal logic, but only a building block. I impute S5 to Russell on the basis of FG–MDL*, or at least on the basis of FG–MDL. FG–MDL* is the thesis that logical truth is purely general truth plus truth in virtue of immutable logical form. FG–MDL is the thesis that logical truth is purely general truth, where purely general truth is a special kind of truth in virtue of logical form, namely truth in virtue of purely general form (Dejnožka 1999, p. 64).

Construed as a sort of propositional function modal logic, MDL by itself yields only S2 (Dejnožka 1999, p. 194, n.2). And Russell's MDL is different from its MacCollian origin. Russell replaces MacColl's certain, variable, and impossible propositions with necessary, possible, and impossible propositional functions. Material certainty (explained below) seems closer to epistemological necessity than to logical necessity, "variable" means "contingent" (MacColl uses π to mean "possible"), and MacColl and Russell are famous for disputing the difference between a proposition and a propositional function. Also, imputing S2 to MacColl is uninteresting. People have been trying for decades to impute S3 (McCall 1967, not claiming success) or T to him (Read 1999, claiming success).

To impute S5 to MacColl in a neo-Russellian way, we would need to show that FG–MDL*, or FG–MDL, or something like them, is logically implicit in MacColl. Only then could we show that for MacColl a logically possible proposition is necessarily possible in virtue of its form, and thus cannot change its modal category. The notion of form need not be exactly Russell's.

More simply, we can impute the distinctive axiom of a neo-Russellian S5 to MacColl if we can show that for MacColl a proposition cannot change its modal category, so that a possible proposition necessarily belongs to the category of possible propositions. That is how I conceptualize the question in Russell's case.

Read interprets MacColl as claiming that "it is impossible that a variable (contingent) element be either certain (ε) or impossible (η)" (Read 1999, sect. 6, p. 2). Does this mean that a variable element cannot change its modal category, which is at least analogous to accepting the distinctive axiom of S5, or does it mean that if an element is variable, then it can neither be certainly nor impossibly variable, which is analogous to rejecting that axiom? I would think the former. But Read alleges a systematic functional formula-exponent ambiguity in the modal symbols he is interpreting, such that a formula suffixed by a predicated exponent does not count as iteration (Read 1999, sect. 6, pp. 1–2). This seems to throw both answers to my question into doubt, since both predicate a suffixed modal operator of a formula.

Worse, Read alleges that MacColl "explicitly rejects $a^{\varepsilon}:a^{\varepsilon\varepsilon}$ (the characteristic axiom of S4) and its like" (Read 1999, sect. 4, p. 4), and even denies that MacColl has S3 (Read 1999, sect. 4, p. 5; Read's $\Box A \rightarrow \Box\Box A$ is equivalent in S4 to $\Diamond\Diamond A \rightarrow \Diamond A$; both are S4 tautologies). This seems to preclude S5, and even to throw a wet blanket on imputing the distinctive axiom of S5 to MacColl, though Read does not discuss the question.

Worst of all, and going directly against my conceptualization, Read quotes MacColl on "when the statement α or β may belong sometimes to one and sometimes to another of the three classes, ε, η, θ...." (Read 1999, sect. 4, p. 5). Thus the modal status of a proposition is "relative to the data" (Read 1999, sect. 1, p. 2). Indeed, MacColl expressly states that a proposition can change from variable to certain or to impossible if we obtain "*fresh data*" (1906a, p. 19, giving an example). Thus a neo-Russellian S5 seems out of the question, since MacColl's modal categories seem easily mutable.

But on the very same page in his 1906 book, MacColl speaks of "formal certainty" and "formal impossibility" (1906a, p. 19). And in his 1902 paper he admits a:

> **province of pure logic**, which should treat of the relations connecting different classes of propositions, and not of the relations connecting the words of which a proposition is built up.... Take, for example, the proposition "*Non-existences* are *non-existent*". This is a self-evident truism; can we affirm that it implies the existence of its subject *non-existences*? **In pure logic** we have $\eta^{\eta} = \varepsilon$, or more briefly $\eta^{\eta\varepsilon}$, which asserts that it is certain that an impossibility is an impossibility. (MacColl 1902, p. 356, boldface emphasis mine)

The singular "its subject" suggests that the "is" in "is an impossibility" is the "is" of predication, not the "is" of identity. And the superscripts in $\eta^{\eta\varepsilon}$ indicate predicates (MacColl 1906a, p. 6). Thus it seems that in pure logic it is certain that it is impossible that an impossible proposition be true. Combining the texts of 1906a and 1902, it seems that it is *formally certain* that it is *formally impossible* that a *formally impossible* proposition be true. Thus it seems that MacColl's province of pure logic contains immutable modal categories, namely formal certainty and formal impossibility.

Pure logic is purely general:

> *Pure logic* may be defined as the general science of reasoning considered in its most abstract sense.... In other words, Pure Logic is the Science of Reasoning considered

with reference to those general rules...which hold good whatever be the matter of thought. (MacColl 1880, p. 48)

Pure logic consists of logical terms ("permanent symbols") and variables ("temporary symbols") which stand for statements (MacColl 1880, p. 49). Thus pure logic "is the logic of *statements* or *propositions*" (MacColl 1902, p. 352; see 362). So perhaps it is within the statement calculus of pure logic, with modal operators such as formal certainty, and logical truth as truth under any interpretation, that we might find a neo-Russellian S5 logically implicit in MacColl.

The big question is whether formal certainty is best interpreted as relative to fresh data. This would seem consistent with the 1906a generic definition of certainty considered by itself. But one uses this definition in abstraction from MacColl's definitions of formal certainty and material certainty at one's peril. The generic definition is that A^{ε} if and only if "A is *always* true within the limits of our data and definitions, that its probability is one" (MacColl 1906a, p. 7). It sounds for all the world as if certainty is a genus of which formal certainty and material certainty are the species in such a way that any formula which is certain can be affirmed indifferently as either formally or materially certain. As we shall see, this is not true at all, and the whole point of the formal-material distinction is that a formula can be materially certain but not formally certain, so that you cannot tell whether A^{ε} is true or false from the formula itself, but only from the context, which tells you whether ε is to be understood formally or materially.

There is a historical progression in defining certainty. In 1896–97 MacColl says that statements which are "*necessarily and always false* ... may be called *absurdities*, *impossibilities*, or *inconsistencies*," and gives "$(2 \times 3 = 7)$" as an example (1896–97, p. 157). In 1897 he speaks of "an *impossibility*, like $2+3=8$" (1897, p. 496). So far, impossibilities seem to be ordinary logical impossibilities. But in 1902 he says, "A^{η} asserts that A is *impossible*—that is it *contradicts some datum or definition*" (1902, p. 356), arguably implying relativity to a fresh datum or definition. This fresh definition itself seems to revise his earlier definition. Then in 1903 he seems to resolve the tension by allowing propositions to fall not only into the classes of truths and falsehoods, but also into "various other classes..., for example, into *certain*, *impossible*, *variable*; ... or into *formal certainties*, *formal impossibilities*, *formal variables*" (1903, p. 356). He says of other logicians, "Many of their formulae are ... *not formal certainties*; they are only valid conditionally" (1903, p. 356; see 359; see also 1905a, p. 393; 1902, p. 354), implying that formal certainties are unconditionally valid. In his 1906 book he distinguishes formal certainties and formal impossibilities from material certainties and material impossibilities (1906a, pp. 16, 17, 19, 97):

A proposition is called a *formal certainty* when it follows necessarily from our definitions, or our understood linguistic conventions, without further data; and it is called a *formal impossibility*, when it is inconsistent with our definitions or linguistic conventions. It is called a *material certainty* when it follows necessarily from some special data not necessarily contained in our definitions. Similarly, it is called a *material impossibility* when it contradicts some special datum or data not contained in our definitions. In this book the symbols ε and η respectively denote certainties and impossibilities without any necessary implication as to whether formal or material. When no special data are given beyond our definitions, the certainties and impossibilities spoken of are understood to be formal; when special data are given then ε and η respectively denote material certainties and impossibilities. (MacColl 1906a, p. 97)

I shall call ε and η MacColl's *generic symbols*. MacColl is telling us that he uses these generic symbols to express what I shall call MacColl's *specific modalities*. Formal certainty, formal impossibility, material certainty, and material impossibility are MacColl's specific *modalities*. Thus all the specific modalities, formal and material alike, are expressed in MacColl's notation. In the quotation, MacColl is telling us how to tell which specific modalities are expressed by ε (either formal certainty or material certainty) and by η (either formal impossibility or material impossibility) in any particular formula. By parity of reason, θ and π are also generic symbols, and we should also be able to tell on any given occasion whether θ expresses formal variability or material variability, and whether π expresses formal possibility or material possibility. Each generic symbol has two uses: formal and material. All the generic symbols are used "without any necessary implication as to whether formal or material." That is, you cannot tell from a mere occurrence of a generic symbol whether it is being used formally or materially. You must learn that from the context. In a word, the test is definitional. A generic symbol's use is formal if and only if its use is definitionally or conventionally certain, impossible, variable, or possible. The test is confusingly stated, since MacColl speaks of "data not necessarily contained in" definitions, but does not explain what he means. But he also says in the same year that definitions and linguistic conventions *are* data: "Suppose we have no data but our definitions or symbolic and linguistic conventions" (1906b, p. 515); "*it follows necessarily from our data*, which are here limited to our definitions and linguistic conventions" (1906b, p. 516). Assuming traditional containment theory of deducibility, it seems he simply means that a certainty is formal if and only if it is necessarily contained in, and so follows from, our definitions or linguistic conventions. In any case, the test of material modality seems to be the truth-relevance of data not contained in definitions or linguistic conventions, and that sounds like relevance of empirical evidence in an ordinary sense.

Thus the historical trend seems to be toward a province of pure logic with immutable specific modalities, namely, the formal modalities. But the question remains whether formal modalities are relative to definitions or linguistic conventions. Can a proposition change from formally certain to formally variable or formally impossible if we obtain *fresh definitions* or *fresh conventions*? The question is serious, since MacColl says on the first page of his book:

> There are two leading principles which separate my system from all others. The first is the principle that there is nothing sacred or eternal about symbols; that all symbolic conventions may be altered when convenience requires it, in order to adapt them to new conditions, or to new classes of problems. (MacColl 1906a, p. 1)

But it would be curious to say that formal certainties such as 2+2=4 are mutable because they are relative to definitions or linguistic conventions, since a popular definition of analytic truth is truth in virtue of definitions or linguistic conventions. Can truths which are analytic in this sense ever change into falsehoods? For example, if we redefine "2" so that 2+2=5, what bearing would that have on arithmetic for MacColl? And if propositions, which are a type of statements for MacColl, can change from formal certainties to formal impossibilities, what about the "meaning" or "information" he says they "convey" on different occasions of use (1906a, p. 2); see 1906b, p. 515? It is that information or meaning which many would call the proposition. MacColl says,

> I do not say that the same *information* may be sometimes true and sometimes false, nor that the same *judgment* may be sometimes true and sometimes false; I only say that the same proposition—the same *form of word*—is sometimes true and sometimes false. (MacColl 1907, p. 470)

One may also question whether MacColl's modal terms "follows necessarily" (used twice), "not necessarily contained," "contradicts," and "inconsistent with," which occur within MacColl's definitions of the four specific modalities in the block quotation of 1906a (p. 97) are to be interpreted relative to MacColl's definitions, or whether they are primitive terms, or whether they introduce circularity. This question, too, is serious, since the second leading principle of MacColl's book is:

> [T]he complete statement or proposition is the real unit of all reasoning. Provided the complete statement (alone or in connexion with the context) convey the meaning intended, the words chosen and their arrangement matter little. (MacColl 1906a, p. 2)

Can MacColl's context principle excuse circularity as mattering little? Are his definitions of formal certainty and formal impossibility merely explanations or elucidations, where Gottlob Frege distinguishes among definition (*Definition*), explanation (*Erklärung*), and explication (*Erläuterung*) (Dejnožka

1996, pp. 73–74)? Perhaps so. Insofar as definitions are given of subsentential expressions, MacColl's context principle might make datanic definitions matter little in a way it could not make datanic statements matter little.

Perhaps the most basic question is: Do the a priori sciences ever really change, and in what sense or senses, for MacColl? What about the propositions of his own calculus? Would he say they could be changed into formal impossibilities in any genuinely controverting sense? What about the meanings or information they express as he uses them in his book?

It seems that formal certainties and formal impossibilities cannot change in truth-value:

> Some logicians say that it is not correct to speak of any statement as "sometimes true and sometimes false"; that if true, it must be true always; and if false, it must be false always. To this I reply...that when I say "A is sometimes true and sometimes false," or "A is a *variable*," I merely mean that the symbol, word, or collection of words, denoted by A sometimes represents a truth and sometimes an untruth. For example, suppose the symbol A denotes the statement "Mrs. Brown is not at home." **This is not a formal certainty, like 3>2, or a formal impossibility, like 3<2, so that *when*** *we have no data but the mere arrangement of words*, "Mrs. Brown is not at home," we are justified in calling this *proposition*, that is to say, *this intelligible arrangement of words*, a *variable*, and in asserting A^{θ}. (MacColl 1906a, pp. 18–19, boldface emphasis mine)

MacColl writes as if he thinks certain syllogisms really are valid and certain implications really do obtain. He expressly equates a syllogism's validity with its formal certainty. He says, "to render the given syllogism $AB:C'$ *valid* (i.e. a *formal certainty*)" (1906a, p. 36). Thus "the syllogism will become a formal certainty, and therefore valid" (1906a, p. 36); see 1904a, p. 468. Again, he says of other logicians, "Many of their formulae are ... *not formal certainties*; they are only valid conditionally" (1903, p. 356), implying that formal certainties are unconditionally valid; see also 1905a, p. 393; 1902, p. 354).

MacColl says that "in formal logic, as in mathematics, it is convenient, if not absolutely necessary, to work with symbolic statements whose truth or falsehood in no way depends on the mental condition of the person supposed to make them" (1906a, pp. 82–83); see 1904e, p. 879. He says "that formal logic should not be mixed up with psychology—that its formulae are independent of the varying mental attitude of individuals" (1906a, p. 60; see also pp. 82–83). What then of the varying definitions and linguistic conventions of individuals? He says, "In order to make our symbolic formulae and operations as far as possible independent of our changing individual opinions, we will arbitrarily lay down the following definitions...." (1906a, p. 83). Thus, far from making formal certainties truth-relative to fresh definitions, MacColl seems to offer fresh definitions to help ensure their immutability. He says that pure logic "has the immense advantage of being independent of the accidental conventions of language," notably the subject-predicate distinction (1902, p. 352).

MacColl says little about what meanings are. He does distinguish synonymy from mere logical equivalence, showing concern for the identity conditions of meanings (1906a, p. 14); 1904a, p. 69. It seems to me that statements' meanings are intensions or connotations, not extensions or denotations (MacColl 1906a, p. 92), and that specifically they are truths or falsehoods statements represent:

> [W]hen I say "A is sometimes true and sometimes false," or "A is a *variable*," I merely mean that the symbol, word, or collection of words, denoted by A sometimes represents a truth and sometimes an untruth. (MacColl 1906a, p. 19)

Thus it seems that meanings are immutable, and what varies is which meanings are represented on various occasions of a statement's use. The suggestion becomes that formally certain and formally impossible statements never vary in the meanings they represent. The best explanation might be that in pure logic, as well as in pure mathematics, nothing particular which could vary is ever denoted. In pure logic, there is no Mrs. Brown. (You could introduce variables ranging over logical operators, including modalities, but that is not in the picture suggested by the texts.)

Perhaps the most convincing texts showing that the formal certainty of formal certainties does not change relative to fresh definitions or fresh linguistic conventions are the texts against non-Euclidian geometries. Here MacColl accuses the non-Euclideans of changing the accepted meanings of terms, stating:

> But on this principle of arbitrarily changing the commonly understood meanings of words and symbols we might plausibly or paradoxically maintain that January has 37 days, February 34, and the whole year 555. We need only slyly change the base of our common arithmetical notation from ten to eight....
>
> **Every formula ... has its limits of validity, namely, the accepted conventional meanings of the words or symbols in which it is expressed. Otherwise, we might legitimately convert any false statement into a true, or vice versa, by simply agreeing to change the ordinarily accepted meanings of the words or other symbols in which it is expressed** .... The Euclidean geometry seems to me to be the only true one,... chiefly, because it is the only system that frankly accepts the customary conventions of ordinary language. (MacColl 1910, pp. 187–188, boldface emphasis mine; see 191–193 for more detail)

Commentators impliedly differ on the mutability of MacColl's modalities. Werner Stelzner makes the dependence of a proposition's truth-value on context the basis of his interpretation of MacColl (Stelzner 1999). This is basic to material modality. But Grattan-Guinness reports that MacColl rejects non-Euclidian geometries so vehemently that he assigns them to his Unreals (Grattan-Guinness 1999, sect. 6. p. 1), a strange position for a contextual, not to say definitional, relativist. Grattan-Guinness adds, "Although he writes in a conciliatory way, MacColl may have been arguing for his system as *the* correct logic" (Grattan-Guinness 1999, sect. 6, p. 1). This latter claim seems mistaken in one sense. MacColl says:

> Modern symbolic logic ... is a progressive science; it can lay claim to no finality or perfection. But, in the form which I have given it, it has now one great merit which it never possessed before; it has become a *practical* science. (MacColl 1903, p. 364)

But it seems to me that Grattan-Guinness is perfectly correct on MacColl on geometry, and that this is because of MacColl's formal modalities, which are specific to mathematics and logic. And insofar as pure logic consists of formal certainties, Grattan-Guinness is right about the logic too. On MacColl on non-Euclidean geometries as impossible and unreal, see MacColl 1905a, p. 397; 1905b, p. 74; 1906b, p. 508 and especially 513 n.1, "Non-Euclideans seem to forget" certain formal certainties and impossibilities; {mac06b}; 1904b–f.

Read does not see that MacColl ever expresses reduction (Read 1999, sect. 4, pp. 4–5). But that depends in part on what might count as reduction in MacColl's notation. I am not convinced by Read's claim that ε and η behave differently as formulae and as exponents (Read 1999, sect. 4, p. 4; sect. 6, p. 1). Read admits that his interpretation of MacColl, by treating ε and η differently depending on whether they occur as formulae or as exponents, makes some of MacColl's formulae ill-formed, and admits, "For MacColl [these formulae] support the identification of ε and η as element and exponent" (Read 1999, sect. 6, p. 1). Surely it is disingenuous to say "Nothing warranted use of the same symbol except" for the formulae that do warrant it (Read 1999, sect. 6, p. 1). This seems to be an Achilles' heel of Read's interpretation. For example, if η is synonymous with A^{η}, as MacColl seems to intend (Storrs McCall calls ε "an arbitrary certain proposition," 1967, p. 4–546), what is the difference between "In pure logic we have $\eta^{\eta} = \varepsilon$, or more briefly $\eta^{\eta\varepsilon}$, which asserts that it is certain that an impossibility is an impossibility" (MacColl 1902, p. 356), which is Read's Theorem 3.2 (3), and $\neg\Diamond\neg\Diamond A \leftrightarrow \Box A$, or $\neg\Diamond A \rightarrow \Box\neg\Diamond A$? Read argues that "$a^{\eta\varepsilon}$, for example, means $(a^{\eta})^{\varepsilon}$, not $a(^{\eta\varepsilon})$, so that the fact that, say, $\eta^{\varepsilon} = \eta$ is irrelevant to such possible reductions of exponents" (Read 1999, sect. 4, p. 4). Read's premise is true (MacColl 1906a, p. 7), but his conclusion ignores that by "$\Box\neg\Diamond A$" we mean precisely $\Box(\neg\Diamond A)$, not $(\Box\neg\Diamond)P$, which is not even well-formed, since modal operators operate on statements.

Read's functional ambiguity also seems to conflict with MacColl's second leading principle, that "the words chosen and their arrangement matter little" (MacColl 1906a, p. 2). The principle does not seem restricted to ordinary language as opposed to formal notation. The subject–predicate distinction which the formula–exponent distinction formalizes is an obvious example, and for MacColl the principal example. MacColl says, "In pure logic ... 'A struck B' and 'B was struck by A' are exact equivalents, and any symbol we choose to represent the one may also be employed to represent the other" (1902, p. 353). Frege found the subject–predicate distinction arbitrary as early as 1879 *Begriffsschrift* (1967). Green grow the rushes, ho!

It might be objected that after stating his second leading principle, MacColl concludes the introduction to his book by saying that subjects and predicates are permanently fixed after all:

> Let us suppose that amongst a certain prehistoric tribe, the sound, gesture, or symbol S was the understood representation of the general idea *stag*. This sound or symbol might also have been used, as single words are often used even now, to represent a complete statement or proposition, of which *stag* was the central and leading idea. The symbol S, or the word *stag*, might have vaguely and varyingly done duty for "It is a stag," or "I see a stag," or "A stag is coming," &c. Similarly, in the customary language of the tribe, the sound or symbol B might have conveyed the general notion of *bigness*.... By degrees primitive men would learn to combine two such sounds or signs into a compound statement, but of varying form or arrangement, according to the impulse of the moment, as SB, **or** BS, **or** S_B, **or** S^B, **&c., any one of which might mean** I see a *big stag*, **or** "The *stag* is *big*," **or** "A *big stag* is coming, **&c**.... Finally, and after many tentative or haphazard changes, would come the grand chemical combination of these linguistic atoms into the compound linguistic molecules which we call propositions. The arrangement S^B **(or some other)** would eventually crystallize and *permanently* signify "The *stag* is *big*," and a similar form S^K would *permanently* mean "The *stag* is *killed*." These are two complete propositions, each with distinct subject and predicate. On the other hand, SB and SK **(or some other forms)** would *permanently* represent "The big stag" and "The killed stag." These are *not* complete propositions; they are merely qualified subjects waiting for their predicates. On these general ideas of linguistic development I have founded my symbolic system. (MacColl 1906a, pp. 3–4, boldface emphasis mine)

My reply is that there is no inconsistency here. MacColl's second leading principle and its principal example show that for MacColl the subject–predicate distinction is logically arbitrary. MacColl is merely adding that as a matter of historical accident terms crystallize into being permanently regarded and used as subjects or predicates. These are precisely the conventions of natural language which MacColl says "matter little" "[i]n pure logic." The distinction is between pure logic—with the statement or proposition as "the real *unit* of all reasoning"—and our merely historical conventions of slicing propositions into subjects and predicates. Far from contradicting his second leading principle, MacColl's interpretation of the development of natural languages as including logically arbitrary conventional divisions of terms into subjects and predicates, however long the conventions may happen to last, is the main *consequence* of his second leading principle. That is the whole *point* of saying that our ordinary divisions of terms into subjects and predicates are logically arbitrary. Through its principal example, the second leading principle is the logical *foundation* of the "general ideas of linguistic development" on which MacColl founds his symbolic system. Frege says exactly the same thing: the conventional division of subjects and predicates in natural languages is logically arbitrary.

In this connection, note that in his 1906 book, MacColl begins by defining normal script A as subject and its superscript as predicate, but soon lets A be a statement and its superscript an operator on the statement (1906a, pp. 4, 18–19). Following Read's approach, one might see that as another blatant functional ambiguity poor MacColl falls into. But charity suggests that MacColl simply sees statements as *being* subjects. That may seem odd, but it is also a step on the road to the mature Frege's theory that a statement is a logical subject-name of an object, either the True or the False—a theory Frege was prepared to defend tooth and nail. It is also in line with MacColl's calling subsentential statements such as disjuncts "terms" (1880, p. 50). To explain his second leading principle, MacColl says:

> Grammar is no essential part of *pure* logic. The student of pure logic need know nothing of grammar, absolutely nothing. The grammatical structures of statements are matters with which he has no special concern. (MacColl 1880, p. 59)

The truth is that since pure logic is a logic of statements (MacColl 1902, p. 352; see 362), in pure logic *all* subjects are statements. MacColl expressly affirms this: "In pure logic, the subject, being always a *statement* ..." (1902, p. 356, emphasis MacColl's). Thus it is hard to find MacColl guilty of a simple, blundering ambiguity between subjects and statements. Perhaps, then, the functional ambiguity Read sees between subject η and predicate $^{\eta}$ is likewise not really there to be seen, in MacColl's bold new conception of subsentential grammar as mattering "absolutely nothing." In fact MacColl expressly affirms this too, and precisely in his pure logic: "In pure logic ... 'A struck B' and 'B was struck by A' are exact equivalents, and any symbol we choose to represent the one may also be employed to represent the other" (1902, p. 353). Of course, MacColl would be the first to tell us that in ordinary language people generally have no trouble telling nouns from verbs from the context (1902, p. 363; compare 1910, p. 342

n.1, "it is scarcely possible to mistake a statement for a ratio"). That does not detract from my point. For that matter, MacColl could just as easily deem modal operators subjects and statements predicates. What difference would it make? Absolutely none to inference, since the new statements would be "exact equivalents" of the old.

For MacColl, statements *are* certainties, impossibilities, and variables (1902, pp. 353, 356. 368; 1910, 190). And even if Read is right that normal script and superscript modal symbols function differently, ε and A^{ε} are still "exact equivalents." Thus the functional difference makes no inferential difference. MacColl would be the first to tell us that Read's difference matters "absolutely nothing." Whenever we see ε, we can just rewrite it as A^{ε} and preserve all inferences. That should be obvious on the face of it anyway. Thus we can rewrite Theorem 3.2(3) as $A^{\eta\eta\varepsilon} = A^{\varepsilon}$. That looks like a reduction to me. In fact, the very formulae Read disingenuously brushes under the rug as "ill-formed in my canon" are the keys to the kingdom of iteration across the statement-operator divide: $a^{\varepsilon} = (a = \varepsilon)$ and $a^{\eta} = (a = \eta)$ (Read 1999, sect. 6, p. 1).

That MacColl admits iteration as well formed is obvious in any case from formulae such as $A^{\eta\varepsilon\varepsilon}$ (1900, p. 79), so the only issue is which axioms are implicit in him.

As to vacuous operators, what about MacColl's formally certain "truism[s]" (1906a, p. 78; 1902, p. 356)? Perhaps "true in virtue of definitions or conventions" is not informationally vacuous, but there is certainly something empty about definitional truisms, and thus about all formal certainties.

It might be objected that my own Achilles' heel is MacColl's saying, "In this book the symbols ε and η respectively denote certainties and impossibilities without any necessary implication as to whether formal or material" (1906a, p. 97). Thus it seems that Read and Stelzner are right to interpret MacColl's modalities indifferently as to whether they are formal in this text or material in that text. Read says that MacColl "first introduced" his symbols "relative to the data" and "[s]ubsequently generalized them to stand also for certainty *tout court*, that is, [for] necessity, for impossibility, and for contingency" (Read 1999, sect. 1, p. 2). Thus Read might well make this objection (he also inverts my history, which is unimportant).

My reply is that the objection falls into the very trap MacColl so carefully warns his readers against. This is the trap of thinking you can tell from a generic symbol considered by itself whether it expresses a formal modality or a material modality. There is no generic logic in MacColl. There are generic symbols which express formal modalities in some formulae and material modalities in others. Thus there are two modal logics, one formal and one material, and you need to look to the context to tell which formulae belong to which. Even if you suppose a generic modal logic of which the formal modal logic and the material modal logic are species, and generic modalities such as generic certainty, it seems that for MacColl pure logic and pure mathematics are S5 in virtue of their formal certainty, and not in virtue of any supposed generic certainty. The natural suggestion is that formal certainty is a concept of form, while material certainty, and by extension the supposed generic certainty, are not. The natural suggestion is that fresh data are empirical, or at least contingent, or at least nonformal ("material") in nature. (That MacColl calls statements data, 1902, pp. 365, 366 does not detract from this, since statements can be empirical, contingent, or material.) Are we prepared to hold the alternative view, that MacColl outQuines Quine by letting individual logical truths be directly revised into logical falsehoods in the light of fresh empirical experience?

Here I see Read as uncritically following Storrs McCall's lead. When McCall cannot find even S3 implicit in MacColl, McCall looks only at generic symbols, overlooking the fact that sometimes they express formal modalities (McCall 1967).

It might be objected that Read is interpreting MacColl's express modal logic while I am focusing on a sub-system which is implicit at best. Not so. I am focusing on one of MacColl's two express modal logics. For example, MacColl expressly assigns his express formula, $\eta^{\eta} = \varepsilon$, which is Read's Theorem 3.2 (3), to the province of pure logic. Calling the formal modal logic a sub-system alters nothing. The fact remains that it seems to be S5, and Read brushes it under the rug.

My argument is rather general. I have not determined from each of MacColl's formulae whether it concerns formal modalities or material modalities, so as to see what his formal modal logic, as opposed to his material modal logic, would be exactly. I hope my suggestion to look for the relevance of empirical data to the modal status of propositions may be helpful in this task. But as a general rule an ordinary language metalanguage should be controlling over the formal notation it explains. Is it wise to investigate generic formulae in the abstract while ignoring an ordinary language statement plainly telling us generic symbols express different modalities on different occasions? As Hegel might ask, who is being abstract here (Hegel 1966)?

The definition or test of MacColl's distinction between formal and material modalities is not as clear as one might wish, but it is clear that he has a distinction that divides formal sciences from material sciences. The division is so traditional that an interpretation of his notation which failed to reflect it would seem simply inadequate. As Grattan-Guinness says, MacColl is still in the world of Euclid.

There would be nothing wrong with Read's using $\eta^{\eta} = \varepsilon$ as Theorem 3.2 (3) in a material T, even if the formula really belongs to a formal S5. That is innocently using a logical truth as a thesis in a probability calculus. The theorem's status as part of the formal S5 would simply be a Stealth airplane too hard to detect with the material T radar.

Where Read goes wrong is where he quotes MacColl on "when the statement α or β may belong sometimes to one and sometimes to another of the three classes, $\varepsilon, \eta, \theta$ ..., we may still accept [various formulae] as valid, but *not* their converses, [notably] $\alpha^{\varepsilon} : \alpha^{\varepsilon\varepsilon}$" (Read 1999, sect. 4, p. 5). Read would have done well to ponder the key word "when" more closely. On the face of it, it means when and only when. Thus on my reading MacColl implies that when the statement α or β may *not* belong sometimes to one and sometimes to another of the three classes—that is, when α or β is a *formal* certainty or *formal* impossibility—we may accept $\alpha^{\varepsilon} : \alpha^{\varepsilon\varepsilon}$ as valid. That is, on my reading MacColl is in effect asserting that $\alpha^{\varepsilon} : \alpha^{\varepsilon\varepsilon}$ is false in material modal logic, but implying that it is true in formal modal logic. Does that not make all the sense in the world?

But then there cannot be a generic logic with generic certainty in MacColl. For how are you going to classify $\alpha^{\varepsilon} : \alpha^{\varepsilon\varepsilon}$ and all the other formulae MacColl lists after the key word "when"? If they are formally certain but not materially certain, are they generically certain or not? If you default to material certainty as the test of generic certainty, then the difference between material certainty and generic certainty collapses. Likewise for defaulting to formal certainty.

If anything, the formal modalities are deeper than the material. The very proclamation that material modalities can vary according to fresh data is itself a formal certainty! The probability calculus belongs to formally certain mathematics, and MacColl's foundational interpretation of probability, i.e., his material modal logic, belongs to the province of pure logic.

Does MacColl ever expressly state that formal modalities never vary? I found three texts. The first is unclear, but the other two are "smoking guns." The first text is:

> On the other hand we get [a certain formula], for θ means $(\theta_{\tau})^{\tau}$, **which is a formal certainty, and a certainty cannot be a variable, since certainties and variables form two mutually exclusive classes by definition**. (MacColl 1906b, p. 514, boldface emphasis mine)

On the face of it, this is a discussion of formal certainty as static. But if MacColl means only that, generically speaking, no statement can be a certainty and a variable at the same time and relative to the same data, so that if formal modalities are static in nature, this would ensure that a formal certainty can never become a formal variable, the question is begged as to their static nature.

The second text is a "smoking gun." MacColl states that statements which may vary in modal status are by that very fact "unlike" formal certainties and formal impossibilities:

Take the statement $A^{\theta\theta}$. This (**unlike** *formal certainties* such as ε^{τ} and AB: A, and **unlike** *formal impossibilities* such as θ^{ε} and $\theta : \eta$) may, in my system, be a *certainty*, an *impossibility*, or a *variable* according to the special data of our problem or investigation. (MacColl 1903, p. 360, boldface emphasis mine)

The third text is another "smoking gun." It says that all and only formal modalities are "permanently" the modalities they are, while all and only material modalities "may change" their "class." We are asked to imagine nine statements, some certain, others impossible, and others variable. We then pick a statement at random and call it A:

Where then is the error in the first argument? It consists in this, that it tacitly assumes that A *must* either be *permanently* a certainty, or *permanently* an impossibility, or *permanently* a variable—an assumption for which there is no warrant. On the second supposition, on the contrary—a supposition which is perfectly admissible—A *may change its class*. In the first trial, for example, A may turn out to represent a certainty, in the next a variable, and in the third an impossibility. When a certainty or an impossibility turns up, the statement A^{θ} is evidently false; when a variable turns up, A^{θ} is evidently true; and since (with the data taken) each of these events is possible, and indeed always happens in the long run, A^{θ} may be false or true, being sometimes the one and sometimes the other, and is therefore a variable. That is to say, on perfectly admissible assumptions, $A^{\theta\theta}$ is possible; **it is not a *formal* impossibility** ....

But, with other data, A^{θ} may be either a certainty or an impossibility, in either of which cases $A^{\theta\theta}$ would be an impossibility. For example, if all the statements from which A is taken at random be exclusively variable, θ_1, θ_2, etc., then, evidently, we should have $A^{\theta\varepsilon}$, and not $A^{\theta\theta}$. On the other hand, if our universe of statements consisted solely of certainties and impossibilities, with no variables, we should have $A^{\theta\eta}$, and not $A^{\theta\theta}$. **Thus the statement $A^{\theta\theta}$ is formally possible; that is to say, it contradicts no definition or symbolic convention; but whether or not it is materially possible depends upon our special or material data.** (MacColl 1910, pp. 197–198, boldface emphasis mine)

In this remarkable text, MacColl uses or virtually uses a model statement universe to prove that $A^{\theta\theta}$ is formally, i.e., logically, possible because in that one possible universe, $A^{\theta\theta}$ is true. In effect, MacColl is equating being formally possible with being true in at least one possible world. The plain implication of the text is that $A^{\theta\theta}$ "*must* ... be *permanently*" formally possible, since the contrast is to the variability of $A^{\theta\theta}$'s material possibility depending on which universe we stipulate, i.e., on our "material data." By the way, the temporal connotations of the expressions "permanently" and "may change its class" (not to mention "fresh data") should not be taken to suggest that MacColl has a temporal logic. Time is not of the essence in this text. The text suggests that MacColl's logic of statements is a logic of all possible statement meanings, if it is not also a logic of all possible statements. If so, we can see why pure logic should be S5. For all the statements in the model are stipulated, and so is the whole model, so no fresh data will be relevant to use of the model to show that the statement in question is formally possible. Such use of hypothetical models is consistent with the fact that MacColl's semantics is that there is only one universe (1907, p. 471). Thus his intended model is simply the world, just as it is for Frege and Russell (Dejnožka 1999, p. 72; see 3, 101), though of course they reject any Unreals.

To sum up, it is more reasonable than not to find implicit in MacColl a material T and a formal S5. The S5 is neo-Russellian because the classes of formal modal statements are logically permanent. MacColl's distinction between formal and material modalities is basic. Read brushes it under the rug. But this is not to reject Read so much as to subsume his important paper into a more complete perspective. In fact, I am simply assuming Read is right that the material logic is T.

MacColl, of course, would have been unaware of later systems like T and S5. Nor do I wish to make it a self-fulfilling prophecy that the formally certain formulae of his notation work out to S5. I claim only that any interpretation of his notation which fails to result in S5 for formal certainties would be simply inadequate in that any such interpretation would be inconsistent with his ordinary language texts metalinguistically describing what formal modalities are. I leave the task of working out the formally certain formulae to others. Perhaps MacColl's formulae might somehow be inconsistent with his metalinguistic description of his modal symbols. But if so, I would call it a flaw in the execution of his conception. And if not, we default to a formal S5.

My criticism is that MacColl is inconsistent. He criticizes non-Euclidians for altering the ordinary meaning of "straight line," but he himself alters the ordinary meaning of "statement" and "proposition" so that the truth or falsehood of statements and propositions is variable. He says that speaking of a statement as "'sometimes true and sometimes false' ... is purely a matter of convention" (1910, p. 192). He defends this by saying that "words are after all mere symbols ... to which we may give any convenient meaning that suits our purpose" (1910, p. 350). Yet in the very same paper, he cries bloody murder when non-Euclidians offer new definitions of "straight line" and other terms (1910, pp. 187–188). He says in an earlier work, "Contradictions and obscurities are the necessary result" (1904c, p. 213), on a par with maintaining "with the strictest logic that $6 \times 4 = 30$" by shifting to base eight, or even with a village lad saying he can swim the Atlantic, where the local pond is named "The Atlantic" (1904c, p. 214. Indeed, his is the deeper alteration, since logic is deeper than geometry. MacColl appeals to ordinary usage, arguing that saying a statement is sometimes true and sometimes false is like saying an event happens many times (1907, p. 472). But on MacColl's own use of "judgment," "meaning," and "information," a logic of judgments or statement meanings or statement information would have invariant modalities across the board, and would seem more basic than a mere logic of linguistic statements. And to change a meaning safely, MacColl requires only a new definition, plus no risk of confusing the new with the old (1902, p. 362). I think the non-Euclidians give at least as much fair warning of their new meanings as MacColl does of his, *pace* MacColl (1910, p. 187), and their changes are plain enough to risk no confusion. To my mind, the worst ambiguity is MacColl's own use of "certain" to mean formally certain or materially certain as told not from the symbol, but only from the context. MacColl gives fair warning, but buries it on p. 97 of his book, exactly ninety pages after his generic definition of A^{ε}. I might be the only one who has noticed it.

MacColl's two requirements for safely changing an expression's meaning jointly suggest that what he has in mind is merely that if a statement's certainty is relative only to definitions or linguistic conventions, *understood as remaining the same*, then the statement is immutably, formally certain:

> Now, a statement is called a formal certainty when it follows necessarily from our formally stated conventions as to the meanings of the words or symbols which express it.... (MacColl 1902, p. 368)

(As before, the "necessarily" seems circular.) MacColl's relativistic material modalities remain an important part of a complete perspective on MacColl on modality. Indeed, they are half the story—but only half the story.

I thank the anonymous reviewer, whose every comment helped, and Layman Allen for answering a question on S4.

Bibliography

Cocchiarella, N. 1987. *Logical Studies in Early Analytic Philosophy*. Ohio State University Press, Columbus, Ohio.

Dejnožka, J. 1999. *Bertrand Russell on Modality and Logical Relevance*. Ashgate Publishing Ltd, Aldershot, England.

Dejnožka, J. 1996. *The Ontology of the Analytic Tradition and Its Origins: Realism and Identity in Frege, Russell, Wittgenstein, and Quine.* Littlefield Adams, Lanham, Maryland.

Dejnožka, J. 1990. The ontological foundation of Russell's theory of modality. *Erkenntnis*, vol. 32, pp. 383–418.

Frege, G. 1967. *Begriffsschrift, a formula language, modeled upon that of arithmetic, for pure thought.* In Jean van Heijenoort (ed.), *From Frege to Gödel.* Harvard University Press, Cambridge, Mass.

Grattan-Guinness, I. 1999. Are other logics possible? MacColl's logic and some English reactions, 1905–1912. *Nordic Journal of Philosophical Logic*, vol. 3, no. 1, pp. 1–16.

Hughes, G. E. and Cresswell, M. J. 1972. *An Introduction to Modal Logic*, 2nd ed. Methuen, London.

Hegel, G. W. F. 1966. Who Thinks Abstractly? In *Hegel: Texts and Commentary*, trans. and ed. by Walter Kaufmann. Doubleday Anchor, Garden City, N.Y.

MacColl, H. 1910. Linguistic Misunderstandings. *Mind*, n.s. 19, pp. 186–199, 337–355.

MacColl, H. 1907. Symbolic Logic (A Reply). In *Mind*, n.s. 16, pp. 470–473.

MacColl, H. 1906a. *Symbolic Logic and Its Applications.* Longmans, Green, and Co., London.

MacColl, H. 1906b. Symbolic Reasoning (VIII). *Mind*, n.s. 15, pp. 504–518.

MacColl, H. 1905a. Symbolic Reasoning (VII). *Mind*, n.s. 14, pp. 390–402.

MacColl, H. 1905b. Symbolic Reasoning (VI). *Mind*, n.s. 14, pp. 74–81.

MacColl, H. 1904a. Symbolic Logic. *The Athenaeum*, no. 3989, pp. 468–469.

MacColl, H. 1904b. Symbolic Logic. *The Athenaeum*, no. 4005, pp. 149–151.

MacColl, H. 1904c. Symbolic Logic. *The Athenaeum*, no. 4007, pp. 213–214.

MacColl, H. 1904d. Symbolic Logic: A Correction. *The Athenaeum*, no. 4008, p. 244.

MacColl, H. 1904e. Symbolic Logic. *The Athenaeum*, no. 4026, pp. 879–880.

MacColl, H. 1904f. Symbolic Logic. *The Athenaeum*, no. 4027, p. 911.

MacColl, H. 1903. Symbolic Reasoning (V). *Mind*, n.s. 12, pp. 355–364.

MacColl, H. 1902. Symbolic Reasoning (IV). *Mind*, n.s. 11, pp. 352–368.

MacColl, H. 1897. Symbolic Reasoning (II). *Mind*, n.s. 6, pp. 493–510.

MacColl, H. 1896–97. On the Calculus of Equivalent Statements (Fifth Paper). *Proceedings of the London Mathematical Society*, vol. 28, pp. 156–183.

MacColl, H. 1880. Symbolic Reasoning (I). *Mind*, vol. 5, pp. 46–60.

Magnell, T. 1991. The extent of Russell's modal views. *Erkenntnis*, vol. 34, pp. 171–185.

McCall, S. 1967. MacColl, Hugh. In Paul Edwards and Arthur Pap (eds), *The Encyclopedia of Philosophy*, vol. 4, pp. 545–546. Collier-Macmillan, New York.

Read, S. 1999. Hugh MacColl and the algebra of strict implication. *Nordic Journal of Philosophical Logic*, vol. 3, no. 1, pp. 59–83.

Russell, B. 1919. *Introduction to Mathematical Philosophy*. Allen and Unwin, London.

Russell, B. 1906. Review of Hugh MacColl, *Symbolic Logic and Its Applications*. In *Mind*, n.s. 15, pp. 255–260.

Stelzner, W. 1999. Context-Sensitivity and the Truth-Operator in Hugh MacColl's Modal Distinctions. *Nordic Journal of Philosophical Logic*, vol. 3, no. 1, pp. 91–118.

Woleński, J. 1999. MacColl on modalities. *Nordic Journal of Philosophical Logic*, vol. 3, no. 1, pp. 133–140.