

Margaret MacDonald's Scientific Common-Sense Philosophy

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Margaret MacDonald (1907-1956) was a central figure in the history of early analytic philosophy in Britain due to both her editorial work as well as her own writings. While her later work on aesthetics and political philosophy has lately received attention, her early writings in the 1930s present a coherent and, for its time, strikingly original blend of common-sense and scientific philosophy. In these papers, MacDonald tackles central problems of philosophy of her day: verification, the problem of induction, and the relationship between philosophical and scientific method. MacDonald's philosophy of science starts from the principle that we should carefully analyze the elements of scientific practice (particularly its temporal features) and the ways that scientists describe that practice. That is, she applies the techniques of ordinary language philosophy *on actual scientific language*. MacDonald shows how "scientific common-sense" is inconsistent with both of the dominant schools of philosophy of her day. Bringing MacDonald back into the story of analytic philosophy corrects the impression that in early analytic philosophy, there are fundamental dichotomies between the style of Moore and Wittgenstein, on the one hand, and the Vienna Circle on the other.

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1. Introduction

Given her prominent role in the early history of the journal *Analysis* (which she edited from 1948-1956)¹ and in the publication of Wittgenstein's 1934-1935 lectures with Alice

¹ According to Waithe, *A History*, MacDonald was, along with C.A. Mace, Gilbert Ryle and Susan Stebbing, a "co-founder" of *Analysis*. Her name, however, does not appear on early mastheads (which only list the editor Arthur Duncan-Jones, Stebbing, Mace, and Ryle), nor is it mentioned in MacDonald's own history of

Ambrose (Ambrose, *Wittgenstein's Lectures*), Margaret MacDonald (1907-1956) was a central figure in the *institutional* history of early analytic philosophy in Britain. While her later work on aesthetics and political philosophy has lately received attention (e.g., Wolff, "Analytic Political Philosophy"), her early writings in the 1930s present much more than "criticisms of several contemporary philosophers" (Waithe, *A History*, 364) but a coherent and, for its time, strikingly original blend of common-sense and scientific philosophy.

In these papers, MacDonald tackles central problems of philosophy of her day: verificationism, the problem of induction, and the relationship between philosophical and scientific method. On my reading, MacDonald's philosophy of science starts from the principle that we should carefully analyze scientific practice (particularly its temporal features) and the ways that scientists describe that practice. She applies the techniques of ordinary language philosophy to study *actual scientific language* and in so doing articulates a unique version of the view that philosophy of science should attend to scientific practice.

My aim here is twofold: to give the first systematic treatment of her views on these topics and to explain why they have not received the attention that they deserve. In sections 2 and 3, I will analyze her argument against verificationism and her solution to the problem of induction. In section 4, I identify a common thread of "scientific common-sense philosophy" in these and other publications from the 1930s, setting it in the context of some of her most important interlocutors. In section 5, drawing on Eileen O'Neill's work, I suggest why, despite her editorship of the flagship journal of analytic philosophy and a

the journal (MacDonald, *Philosophy and Analysis*), nor in the notice of her death (Saw, "Dr. Margaret MacDonald"). I could not determine precisely her involvement in the journal before World War II.

variety of publications on hotly debated questions in that period, she nevertheless was not recognized as a major figure.

2. Against Verificationism

Before I begin, it will be helpful to say something about her life and intellectual milieu. Born in 1907, she was awarded a first-class degree at UCL in 1932 and then a PhD under Susan Stebbing, who supported her financially during her studies. From 1934-7, MacDonald was Fellow at Girton College, Cambridge, where she attended the lectures of Moore and Wittgenstein, both of which are cited in her early publications. She was a librarian at St. Hilda's College, Oxford from 1937-1941. In 1946, she became lecturer at Bedford College, where she was appointed Reader in 1955, a year before her untimely death.²

MacDonald's first major publication, "Verification and Understanding", which appeared in 1934 in the *Proceedings of the Aristotelian Society*, targets a view she finds in Peirce, C.I. Lewis, and the Vienna Circle, that "the whole meaning of a proposition is given in a set of conditional propositions about the experiences which would verify it" (MacDonald, "Verification and Understanding", 143). While staying neutral on the question of whether there are unverifiable propositions, MacDonald shows that the meaning of a sentence cannot *consist in* its means of verification.³

² The fullest discussion of her life and work is now Kremer, "Margaret MacDonald", superseding Addis, "Margaret MacDonald", 601-5.

³ Elsewhere MacDonald frequently commits herself to the empirical verifiability of all meaningful propositions. She wields it against McTaggart's theory of the self (MacDonald, "Russell and McTaggart").

Before going into her argument, it will be helpful to reconstruct the position that she argues against. For, unlike some early discussions of verificationism (e.g., Black "The Principle of Verifiability"), MacDonald is aware that a wide variety of philosophers on both sides of the Atlantic had defended some version of the thesis.⁴ Indeed, Schlick is the only member of the Vienna Circle cited by name in the text,⁵ and "Wittgenstein and the Viennese School" are only mentioned in a footnote.⁶ Instead, she focuses on the formulations of verificationism in the earlier pragmatists Peirce and Lewis.⁷

In empirical science, MacDonald is happy to accept, the "criterion of observability" is very useful indeed. According to this criterion, unless one's assertion is connected to possible experiments or observations, one is talking a lot of claptrap. Moreover, she even accepts that at times "the scientist does tend to identify what he understands with the means

She argues for the senselessness of hylo-morphism by arguing "it would be logically impossible to verify" it (MacDonald, "The Philosopher's Use of Analogy", 299). Finally, she argues against Engels' claim that all things are really processes (and its contradictory held by "substratum theorists"), concluding that the claim is "non-significant" because it is "not resolvable by more empirical observation" (MacDonald, "Things and Processes", 8).

⁴ Stebbing, *Logical Positivism and Analysis* quotes many of the same passages from Americans as MacDonald, and as Stebbing's dissertation concerned the American pragmatists, it is likely that she is MacDonald's source.

⁵ She cites Schlick, "Form and Content". Stebbing, *Logical Positivism and Analysis* explicitly relies on MacDonald's verbatim report of these lectures.

⁶ Compare Wittgenstein's own statement of the principle of verificationism in McGuinness, *Wittgenstein und der Wiener Kreis*, 47 ff, 97 ff, 243 ff. The same ideas can still be found in his Cambridge lectures, e.g., Ambrose, *Wittgenstein's Lectures*, 19.

⁷ See Misak, *Verificationism*, for verificationism in the pragmatist tradition.

of its verification” (“Verification and Understanding”, 144). However, scientists more usually understand verification in a way that is quite different from the positivist-pragmatists: “verification is usually employed in science and elsewhere, not to establish the meaning of propositions, but to prove them true” (“Verification and Understanding”, 144). The crux of MacDonald’s argument against verificationism is that their conflation of distinct notions of verification take them far afield from the plausible scientific principle and lead them to paradoxical results.

The problematic results are most apparent when it comes to statements about the past.⁸ To verify a statement about Queen Elizabeth I’s death, for example, we must rely on various present documents or remains. That is the only reasonable way for us to know about those events. It would be preposterous to say that the meaning of “Queen Elizabeth I died in 1603” *consists in* reading such documents. But that is precisely what one must say if the meaning of the proposition is the means of verifying it, at least if verification retains its ordinary understanding. That is to say, “X is verifiable” seems to only be true if X can be verified by taking some action in the present or future and thereby being able to determine whether X is true or false. So if we are to retain the ordinary meaning of “verify”, then the positivist must either say that statements about the past are *unverifiable* and meaningless or else that they are not really about the past at all, but really about the present documents, etc.

While her argument is clearest when it comes to propositions about the past, the point is perfectly general. If one wishes to verify, for instance, an empirical generalization, the meaning of the general proposition (say, “all swans are white”) cannot be the same as

⁸ Stebbing, *Logical Positivism and Analysis* and Stebbing, Russell, and Heath, "Communication and Verification" also focus on examples concerning the past, but use them to argue for the stronger claim that there are meaningful propositions which are *unverifiable* in the sense that is of interest to the positivists.

the way of telling whether it is true or false (looking at the particular swans), as that would conflate our evidence (the particular observations) with what it is evidence for (the generalization).

The result of MacDonald's argument is that one must distinguish the following:

(1) the ways in which I come to understand a proposition, (2) that which determines the truth or falsity of the proposition, (3) the evidence on which I base my belief in its truth or falsity. ("Verification and Understanding", 153)⁹

The verificationist seeks to identify these:

The question "What does this sentence mean?" is identical with (has the same answer as) the question: "how is this proposition verified?"... (Schlick, "Form and Content", 181)

Schlick's identification, however, leads directly to MacDonald's Queen Elizabeth I problem. The ways in which I come to understand a proposition in general have to be somehow connected to my own personal history and the things with which I have been acquainted. But the details of my own personal history are relevant neither for what determines the truth or falsity of a proposition unconnected with that history nor for discovering the appropriate evidence for coming to believe the proposition to be true or false.

What is important to note here is that MacDonald's argument (unlike, e.g., Lewis "Experience and Meaning") does not claim that there is an *extensional* difference between meaningful propositions and those that can be verified. Her claim is merely that the method

⁹ Stebbing ("Communication and Verification", 170) makes a related distinction in her discussion of verificationism between "(1) establishing a certain proposition as true, (2) understanding what difference its truth or falsity would make, (3) knowing the evidence upon which my belief in its truth or falsity is based." While the third item in both lists is the same, the first and second go in different directions.

by which a proposition is verified is distinct from its meaning and that both of these are different again from what determines the truth or falsity of that proposition.¹⁰

In sum, MacDonald does not object to the verificationist's claim that that all and only meaningful propositions can be verified (which she elsewhere assumes), but that the identification of meaning and method of verification contravenes the established and sensible usage of scientists themselves. Moreover, these two claims are derived, it seems, from reflections on what scientists say and do, since they act as if all meaningful propositions are verifiable, but do not identify the means of verification with the meaning of what is verified.

3. Induction

In her work on verification, MacDonald drew attention to the way in which scientists use the term "verify" to rule out prominent philosophical accounts of the meaning of informative propositions. In her work on the problem of induction, she does the same with the notion of "hypothesis".

¹⁰ This connects to Roman Ingarden's 1936 objection to verificationism that, in order to know how to verify a claim, one must already know what it means, so that the verificationist has switched what is posterior (verification) with what is prior (meaning). See Pelletier and Linsky ("Verification"). While MacDonald wouldn't object to this, her argument is rather broader. First, she objects to the identification of the meaning with the means of verification, not with the idea that one is prior to the other. Thus, even if the verificationist scrapped talk of priority and simply held onto the biconditional that X is the meaning of p iff X is the way to verify whether p, then MacDonald's argument would still stand. Second, Ingarden's objection only distinguishes two things, which correspond roughly to 1 and 3 in MacDonald's list, while MacDonald also distinguishes what determines the truth value of a proposition.

It is important to begin, however, with how MacDonald understands the problem of induction. For her, it is essentially about predicting future events from past experiences. This temporal feature of inductive reasoning is essential, because for her “temporal reference...is the chief characteristic of empirical generalizations” (MacDonald, Ryle, and Berlin, "Induction and Hypothesis", 22). The problem with such predictions is that we have no “universal guarantee...that our conclusions to the future will always be valid” ("Induction and Hypothesis", 20).

She begins by critiquing “rationalist” and “empiricist” solutions to the problem. The problem with rationalists, she argues, is that they obliterate the difference between logical or mathematical propositions and empirical generalizations by assimilating the latter to the former. Thus we do not have an invalid inductive argument:

1. These n swans have been observed to be white.
2. Therefore, all swans are white.

We instead have a deductively valid argument with a suppressed premise:

1. These n swans have been observed to be white.
2. [Causal information built into the observation of the n swans that implies “If these n swans have been observed to be white, then all swans are white”.]
3. Therefore, all swans are white.

On this view, what grounds the goodness of inductive arguments is their connection to these sorts of deductive arguments, even when premise 2 cannot usually be known. Moreover, premise 2 should not be seen really as a new premise but a sort of requirement of “complete insight” into the observations mentioned in premise 1. If we have this, so the rationalist says, we can deduce the empirical generalization from 1 alone. However, MacDonald rightly points out that this means that it would be logically inconsistent for

someone to assert premise 1 with full knowledge of the situation and deny the conclusion, but that seems completely wrongheaded. The rationalist support for their unintuitive claim is that otherwise regularities in nature would be an accident or a miracle. But in ordinary language miracles and accidents are things that happen *contrary* to our expectations. The rationalist therefore obliterates the difference between those events which we expect and those that we don't by calling all of them accidental, so that "there will be no point in calling anything an accident" ("Induction and Hypothesis", 24).

MacDonald then critiques the empiricist response of Ramsey, Peirce, and Ayer. On their view, empirical generalizations are not propositions at all but rules for future behavior (so that "Every X is ϕ " would be understood as "If you meet an X, treat it as having ϕ ") or how to interpret future experience ("Induction and Hypothesis", 25), because even though one cannot know them to be true (as future experience may contradict past experience), it is reasonable to act as if they were. Empiricists therefore adopt empirical generalizations as mere "hypotheses" which are revised continuously but never finally.¹¹ MacDonald argues against this view in two stages. First, she shows that, if empirical generalizations are rules, then one could be said to know them but not to believe them. Second, the logic of the term "hypothesis" requires that it is possible to know whether they are true, reducing the empiricist position to contradiction.

The first step of her argument supposes, with the empiricists, that empirical generalizations are rules. But rules, unlike propositions, are neither true nor false. We can, however, be said to *know* rules when, for example, we say that we know the rules of a

¹¹ A version of this view remains in the literature on formal learning theory, on which only infinite inductive rules are ever justified. See, e.g., Kelly, *The Logic of Reliable Inquiry*.

game. There, MacDonald argues, we know simply by “understanding and being able to apply them” (“Induction and Hypothesis”, 27), drawing on Wittgenstein's recent work on understanding linguistic expressions.¹² But if empirical generalizations were like that, then we would not be able to understand them as rules at all, since then it would not make sense, as the empiricists wish, for us to speak of believing them. On the other hand, MacDonald takes it to be axiomatic of propositions, which *can* be believed, to be knowable. Here she seems to be appealing to some form of the verificationist principle that all meaningful propositions can be empirically verified. Thus, it seems, the empiricist is caught in a dilemma. Either empirical generalizations are rules, in which case they are knowable, or else one can be said to believe them, but in that case it must also be possible for one to know them, contrary to their solution to the problem of induction.

In the second stage, MacDonald argues that “hypothesis”, as used in science and in ordinary life, is used for something “advanced to explain certain empirical facts. It is opposed then to other propositions which are known to be true” (“Induction and Hypothesis”, 27). Citing examples from 19th century chemistry and 20th century physics, she argues that once a scientist has confirmed a hypothesis, they no longer speak of it as a hypothesis, but rather as something known.¹³ What is the nature of this knowledge?

¹² For example, see Wittgenstein, *Big Typescript*, 22 ff, or Ambrose *Wittgenstein's Lectures*, 92.

¹³ It seems likely that MacDonald's interest in hypotheses was in some way derived from Wittgenstein, who devoted a significant amount of energy to understanding them from his discussions with the Vienna Circle through most of the 30s. See McGuinness, *Wittgenstein und der Wiener Kreis*, 99, 255 ff, Wittgenstein, *Philosophische Bemerkungen*, 282-5, *Big Typescript*, 117-122, Ambrose, *Wittgenstein's Lectures*, 78. However, his position differs from hers in significant ways, which are extremely relevant for MacDonald's discussion of induction. In particular, according to Wittgenstein, a hypothesis is not a proposition (*Satz*), but

According to MacDonald, it is distinctive of empirical knowledge that, although it is factive, it is nonetheless fallible. Sometimes it turns out that, although we did everything right, we made a mistake and were wrong. In such cases we did not know. But the mere possibility of being wrong does not mean that we do not know here and now ("Induction and Hypothesis", 29). Moreover, scientific discourse does not tend to include, when discussing, say, Gay-Lussac's law, any of the markers of doubt or uncertainty, such as the subjunctive "may" or attitude verbs like "believe" or "suppose", further suggesting that these are not mere hypotheses. If we take this seriously, then scientists take empirical generalizations to be knowable. This is a problem for the empiricists because they want to hold both that empirical generalizations are hypotheses and that they cannot be known. But the considerations of scientific language suggest that *if* they are hypotheses, then they can be known and furthermore that holding a generalization as a hypothesis presupposes that there are other known generalizations which the hypothesis can explain.

MacDonald uses the results of the previous discussions to build her own solution to the problem of induction. The refutation of the rationalists showed that one does not know empirical generalizations through deduction, while the discussion of the empiricists shows that we do in fact know empirical generalizations. How is this possible? First, she claims that the use of the word "knowledge" when talking about empirical generalizations is *sui generis* and fundamentally different from "knowledge" when applied to cases of mathematics and logic. Second, what justifies predictions about the future has nothing to do with discovering some general relation of "causation" holding, but rather simply "the rather a law for the construction of propositions (*Gesetz zur Bildung von Sätzen*). In this respect, he seems to be more or less closely following Ramsey (Hacker, *Wittgenstein's Place*, 71), and thus subject to the same criticism.

application of accredited methods to determine the dependence of happenings upon each other” (ibid, 34).¹⁴ That is, inductive procedures do not need any further, external, philosophical justification. Rather they are justified internally by the particular practices of science.

To see how innovative this position was at the time, it is instructive to look briefly at the commentaries of Ryle and Berlin. Both express incredulity that empirical generalizations can be known at all, because any such “knowledge” must be fallible, since they assume knowledge to be analytically infallible (“Induction and Hypothesis”, 38 and 69). Indeed, Berlin believes that it follows logically from knowledge being fallible that it is equivalent to “subjective certainty”. But on this point, it seems to me that MacDonald has clearly won out. Fallibilism is by far the standard view in contemporary epistemology.

Ryle's and Berlin's objections show the extent to which MacDonald's views about induction and empirical knowledge were quite surprising for their place and time. While her work is clearly in the tradition of Ramsey and Wittgenstein of the 1930s, neither of them appeal to scientific practice nor do they conceive of hypotheses as possible objects of knowledge. History, however, has to a certain extent vindicated MacDonald's fallibilism, although as far as I know no one has articulated this same solution to the problem of induction.

4. Science and Philosophy

¹⁴ Her conclusion, although not the argument for it, is close to Ramsey "Truth and Probability" and Strawson *Introduction to Logical Theory*, who also take induction to be a fundamental mode of inference.

We have seen how MacDonald carved out original and plausible views concerning widely discussed questions of her time. Here I will step back and try to get a better glimpse of her *Weltanschauung*. Doing so helps us to appreciate the extent to which she is not merely a thinker of interest on a small subset of narrowly circumscribed problems, but as someone whose methodology is broadly applicable to a wide variety of philosophical areas.

The challenge with giving this account, which is admittedly somewhat speculative, is that MacDonald herself tends not to step back and tell us explicitly why she does what she does. With good reason. MacDonald considered herself squarely in the burgeoning "analytic" tradition, which, as she herself described it, deliberately eschews "long, very general and abstract metaphysical speculations about possible facts or about the world as a whole" and instead promoted work "on limited and precisely defined philosophical questions" (MacDonald, *Philosophy and Analysis*, 1). Her own work is a prime example of this.

But as a historian, it is possible to elucidate what MacDonald herself did not, just as historians of skepticism may be dogmatic about their own histories of skepticism. I will begin with her own explicit discussions of her place in philosophy, and then bring together several scattered remarks on method as well as general observations from the two previous sections to paint a general portrait of her early work on philosophy of science.

How did MacDonald characterize her own philosophical bent? She was one of the first to use the term "analytic philosophy" to describe her own views in 1936, after Collingwood polemically coined the term in 1933 (Beaney, "Historiography"). In the same essay, she closely links analytic philosophy with logical positivism as one overarching

philosophical project.¹⁵ For her, analytic philosophy made use of a "logical-analytic method" that was neither deductive nor inductive, but rather consisted in "elucidating the meaning of expressions" (MacDonald, "Russell and McTaggart", 323).

In giving this broad characterization, MacDonald only intends to distinguish this philosophical outlook from McTaggart's methodology, which aimed to deduce substantive metaphysical theses from evident propositions. She therefore does not give specific details about her own philosophical positions that would make her stand out among the analysts. From the preceding discussion, I believe the case can be made that both her self-conception was entirely plausible and that she nonetheless adds an important new dimension to our understanding of early analytic philosophy.

One very basic way in which MacDonald challenges our customary understanding of the period is the breadth of her philosophical worldview. Her early papers discuss not only the Cambridge analysts and logical positivists, who might perhaps be considered the most cutting-edge philosophers of their time, but also American pragmatists, idealists, and even Friedrich Engels.¹⁶ Of the latter, the idealists and Engels provided general targets

¹⁵ She opposes McTaggart to Russell and "his followers" whom she names as Wisdom, Stebbing, Ayer, all of whom practice "analytic philosophy" on 322-3 and then later calls McTaggart's opponents "the logical positivists", whom she refers to in the first-person plural "We" (326). Wittgenstein's *Tractatus*, Carnap, and "other writings of the Vienna School" are freely used to explain and defend ideas that were attributed to Russell earlier in the paper. So it seems at the very least for the purposes of comparison with McTaggart, that MacDonald does not see a fundamental divide between the Cambridge and Vienna strands of what has come to be known as "early analytic philosophy".

¹⁶ Her discussion of ancient and early modern philosophers is relatively minimal, but this is not surprising given the topics of interest to her. Aristotle and Descartes receive a fair amount of discussion in "The Philosopher's Use of Analogy". Hume is briefly discussed in "Induction and Hypothesis".

against whom she could help to define what was then a more amorphous movement but would coalesce in the post-war era as the tradition of analytic philosophy. The pragmatists, especially Peirce, were seen by MacDonald as fellow travelers with the positivists.

MacDonald's uniqueness in her methodology comes out most clearly in her careful attention to the language that scientists actually use. In the previous sections, we saw how she dissected "verification" and "hypothesis" to solve philosophical problems. She especially wields her analyses of these terms to argue against other philosophers who claim to take the work of scientists seriously.¹⁷ The way she discusses these terms in some ways closely resembles the way that Wittgenstein discusses notions of "rule" or "know" in his own later work. This is not surprising, since MacDonald attended Wittgenstein's lectures in the 30s precisely when he was developing the views that would eventually appear in the *Philosophical Investigations*. However, Wittgenstein's focus there seems exclusively on the everyday and eschews the technical.¹⁸ MacDonald by no means scorns everyday use of various terms, but is unique in this period in applying those very same analytical techniques to scientific language to show how philosophical problems are confused and can be resolved.

The appeal to scientific language shows how MacDonald takes the results of scientific practice seriously, not as something that would overthrow common-sense as a whole, but which could show philosophical assumptions to be grossly mistaken. On the

¹⁷ In this way, her criticism of the Vienna Circle has some affinities with those of Reichenbach, "Logistic Empiricism in Germany". I have seen no clear indication that either was aware of the other. Moreover, MacDonald's focus on the language used by scientists has no clear parallels with Reichenbach.

¹⁸ For Wittgenstein on "ordinary language", see e.g., McGuinness, *Wittgenstein und der Wiener Kreis*, 45; Ambrose, *Wittgenstein's Lectures*, 102.

other hand, she is not overly deferential to scientists. She admits above, for example, that occasionally scientists misleadingly talk about the methods of verifying a proposition as if they constituted the meaning of that proposition, even if they usually distinguish them. Rather than simply being mouthpieces for contemporary scientists, then, MacDonald seems to advocate the use of the kind of critical discernment championed by Stebbing.¹⁹ But MacDonald did think that on certain occasions science could correct common-sense. For example, she argues that the common-sense distinction between solid, liquid, and gas as a distinction of kind and not of degree is shown to be false by means of experiments ("Induction and Hypothesis", 32).

MacDonald's 1938 "The Philosopher's Use of Analogy" is her clearest statement of how scientific language works, with which she compares philosophers' use of language:

Again, consider the difference between what Lavoisier was doing when he discovered oxygen and the nature of combustion and when he reformulated the chemical vocabulary. The second involved defining new chemical terms and redefining old ones. It was wholly concerned with the uses of words and so far resembled philosophical activity. But it resulted from the first activity of empirical discovery which was not verbal. There is, I suggest, no such distinction in the activities of philosophers. (295)

Philosophers who aim to use special technical language in the manner of scientists to explain various features of our experience merely make new analogies and extend language. For example, hylomorphists have extended the notion of "analysis" when they

¹⁹ In "Things and Processes" and "Review", MacDonald alludes to her agreement with the criticism of Eddington in Stebbing, *Philosophy and the Physicists*.

say they are “analyzing” bodies into form and matter in the way that one might “analyze” a complex machine into its elements. “Analysis” in the latter use is meaningful, since there one can physically take apart the machine, which one cannot do in the hylomorphic case. The hylomorphist does not explain substance with the new terms, but only re-describes it. This can be seen from the fact that, unlike in the Lavoisier case, no new empirical predictions can be made. Furthermore, philosophical puzzles arise from the inadequate understanding of the analogies that are used. The examples of impressing wax and reforming gold give some sense to the distinction between form and matter, but mislead philosophers into thinking that such a distinction can always be made. Even though MacDonald’s emphasis here is on the use of ordinary language and not scientific language to solve philosophical problems, her comparison with serious cases of scientific language still drives the argument.

The method that MacDonald ends up endorsing for philosophy is "understanding how language is ordinarily used, how certain uses of it have provoked these [philosophical] problems and how it has been misused in many alleged solutions" (312). As is clear from what came immediately before, this language (somewhat confusingly) includes "ordinary and technical vocabularies". Her suggestion then, it that the dissolving of philosophical problems, because they are merely verbal, only comes from clear attention to language as a whole.

Moreover, the attention that MacDonald pays to scientific language is not random. One recurrent interest visible in the papers discussed above is the role of time in the scientific process. And this is not surprising, given her claim quoted above that “temporal reference...is the chief characteristic of empirical generalizations”. In her discussion of verificationism, time was the most straightforward means of elucidating the difference

between the meaning of a proposition and the determination of its truth. But more generally, MacDonald takes it as axiomatic of verification that the scientist attempts this in the present or future. Unlike in Schlick "Meaning and Verification", for MacDonald it matters that scientists in fact can carry out verifications, which means carrying them out in the here and now. If we are interested in science as it is actually practiced, this is what we care about, not the mere description of logical possibilities. Similarly, when it comes to induction, MacDonald (unlike Stebbing in *Modern Introduction to Logic* or Ryle in the symposium) takes the problem to be *fundamentally* about the prediction of future on the basis of past events, not about atemporal reasoning from sample to generalization. This stems from the very same concern with what scientists actually must do. Observation and prediction do not exist in a Quinean universe of tenseless facts. They are the activities of temporally situated inquirers.

The understanding of empirical science as a temporally situated human practice, unlike a logically perfect ideal, is a significant positive result of MacDonald's methodology and sets her philosophy of science apart from her contemporaries. This can be discerned most clearly by situating MacDonald in relation to her four most profound influences: the pragmatism of Peirce, the positivism of the Vienna Circle,²⁰ Stebbing and the Cambridge School of Analysis, and the middle/late Wittgenstein. In the ordinary tellings of the history

²⁰ MacDonald often treats pragmatists and positivists together, even though they arose in very different circumstances and had distinct developments. From her early work on verificationism, for example, she treats the positivist and pragmatists as having essentially the same view and argues in MacDonald ("Language and Reference") that Carnap's attempt to give a formal description of language is compatible with Peirce's theory of signs. For connections between American pragmatists and early analytic philosophers in Europe, see Misak ("Influence of Pragmatism on the Vienna Circle").

of analytic philosophy, these constitute four rather distinct strands, which can be most easily discerned by their respective views on the relationship between philosophy and the empirical sciences.

MacDonald agrees with the pragmatists and the positivists in their repudiation of metaphysics due to its lack of content and their embrace of a thoroughgoing empiricism. She further agrees with the positivists that philosophical problems tend to arise from language, although her solution is not to construct an ideal symbolism in which the problems do not arise. Rather, she takes seriously the language used by scientists themselves.

MacDonald and her mentor Stebbing also are somewhat unique in their interest in both the burgeoning scientific philosophy represented by Russell and the Vienna Circle as well as in the sorts of observations of ordinary language and belief that Moore and Wittgenstein were taking notice of. Moreover, they agree that common-sense and the results of empirical science are not in direct conflict. However, MacDonald disagrees with Stebbing's metaphysical conception of analysis (which Stebbing herself soon abandoned), preferring to see problems as either in need of empirical investigation by the special sciences or else linguistic in nature.

She agrees with the later Wittgenstein that philosophical problems very often arise from a misuse of language (indeed, from "misleading analogies in the use of our language" Wittgenstein, *Big Typescript*, 408), but unlike Wittgenstein believes that philosophers should engage closely with the language of science. This scientific bent explains, to some extent, her disagreement with Wittgenstein about the proper understanding of "hypothesis". While both took the notion to be philosophically important, only MacDonald used

examples from the history of science to try to figure out how to solve the problems that are involved with it.

In this way, MacDonald has much in common with what has been called the broad "ordinary language" stream of analytic philosophy (Rorty, *Introduction*) in seeing philosophical problems arising from misunderstanding ordinary language, but not because there are defects in ordinary language that could be resolved by an ideal language. Indeed, her position on this matter strongly resembles later Wittgenstein and is perhaps even more extreme than J.L. Austin, who himself only took reflections on language to be preliminary (Austin, "A Plea for Excuses"). On the other hand, she also has much in common with the "ideal language" branch of the tradition (such as Russell, early Wittgenstein, and the Vienna Circle), since she had a strong interest in science.²¹

Indeed, one of the most fascinating results of the present study is that MacDonald falls squarely between the major divisions of early analytic philosophy. Consider, for example, the structure of Ayer et al, *The Revolution in Philosophy*. Here we see distinctions, e.g., between logical positivism and the Cambridge School of Analysis that resonate even in more rigorous contemporary histories of analytic philosophy, such as Beaney, *The Oxford Handbook*. If what has come before is correct, her work could not with any justice be categorized under any of these headings, not because she was talking about something else entirely, but because she has both significant points of agreement and disagreement with most of these thinkers.

²¹ It is interesting to note in this connection that Stebbing herself has been characterized as falling between these two traditions by Chapman, *Susan Stebbing*, 173 ff.

5. Disappearing Ink

I hope to have shown that MacDonald's philosophy of science is interesting, plausible, and fundamentally different from other thinkers in the early analytic tradition. In this section, I transition to consider why her work was not given the consideration that it so clearly deserves. Drawing on Eileen O'Neill's work, I suggest that the fact that she, like Stebbing, was not included in histories of analytic philosophy was perhaps the decisive reason that she was lost from philosophical memory.

First, a brief look at citations during her lifetime:

- "Verification and Understanding": 2.²²
- "Induction and Hypothesis": 2.²³
- "The Philosopher's Use of Analogy": 5.²⁴
- "Russell and MacTaggart": 1.²⁵

On the other hand, lack of citation is the rule for the vast majority of articles during this period, so perhaps this is not particularly telling.

So one must look elsewhere to see how her writings were initially received. One place to look is at important early collections of analytic philosophy:

- "Verification and Understanding" is listed as "Suggested further readings" in Feigl and Sellars (*Readings*).

²² Reeves ("Theory of Descriptions") and Stebbing, Russell, and Heath ("Communication and Verification").

²³ MacIver ("Some Questions"), although he only uses it for a reference to Moore's lectures and Will ("Problem of Induction").

²⁴ Bergmann ("Two Types"), Donagan ("Recent Criticisms"), Emmet ("The Use of Analogy"), Smart ("Descartes and the Wax"), and Stebbing ("Some Puzzles").

²⁵ Taube ("Positivism, Science, and History").

- “The Philosopher’s Use of Analogy” and “The Language of Political Theory” were anthologized by Flew (*Logic and Language*).
- “Things and Processes” is reprinted in MacDonald (*Philosophy and Analysis*).
- “Natural Rights” in Laslett (*Philosophy, Politics, and Society*).
- “Ethics and the Ceremonial Use of Language” in Black (*Philosophical Analysis*).
- “Sleeping and Waking” further reading in Hospers (*An Introduction*).
- “Some Distinctive Features of Arguments used in Criticism of the Arts” is reprinted in Elton (*Aesthetics and Language*).²⁶

The presence of a variety of MacDonald’s works in important collections in the early history of analytic philosophy does at the very least indicate that her work was taken seriously by at least some of her contemporaries. Indeed, MacDonald was named one of the “most active” young philosophers in Britain by Black (“Relations”).

One of Eileen O’Neill’s most important arguments concerning the fate of early modern women philosophers in her celebrated “Disappearing Ink” is that, in order to be remembered, philosophers must be included in standard philosophical histories. Moreover, the type of philosophy that early modern women produced was sidelined in the 18th and 19th centuries, thus leading to their almost complete exclusion from histories.²⁷ As we will see, MacDonald’s name is almost totally absent in the earliest histories of analytic philosophy

²⁶ This was originally printed as her contribution to the Aristotelian Society Symposium Hannay, Holloway, and MacDonald “Criticism of the Arts”.

²⁷ As O’Neill correctly notes, this argument does not completely explain why early modern women philosophers have so entirely disappeared, which she argues is due to the aftermath of the French Revolution (“Disappearing Ink”, 37 ff). I omit discussion of it here only because it is the first mentioned argument that seems to explain MacDonald’s disappearance best.

from the 1950s. Her name is not mentioned at all in (the admittedly very selective) Urmson (*Philosophical Analysis*), the first history of analytic philosophy (Beaney "Historiography"), nor Ayer et al (*The Revolution in Philosophy*), nor Warnock (*English Philosophy since 1900*). Her name comes up 4 times in Passmore's extremely comprehensive *A Hundred Years of Philosophy*: twice for her role as editor of *Analysis*, once for her review of Ryle's *Concept of Mind*, and most tellingly once as a footnote citing Moorean attacks on McTaggart. There is no mention of the fact that her attack on McTaggart was made from a completely different direction from Moore's, nor that MacDonald was comparing McTaggart with *Russell* and his followers (whom she names as Wisdom, Stebbing, and Ayer), *not* with Moore. In fact, Moore is never even *mentioned* in the article, in which the ideas of the Vienna Circle and Wittgenstein loom much larger. Never are MacDonald's own views discussed in any of the early histories of analytic philosophy. Despite her relatively high standing in British analytic philosophy in the first half of the 20th century, the fact that she never made it into any of these volumes in a serious way helps to explain why her name was forgotten in subsequent generations.

But why was her name excluded from these histories? Two explanations present themselves. The first is sociological: institutional sexism in Britain hindered, even if it did not completely prevent, women from reaching positions of prominence. In particular, women still had very limited opportunities for study and employment especially at centers such as Oxford and Cambridge.²⁸ Although MacDonald, like Stebbing, eventually obtained permanent positions in London, this only came after a long period of professional

²⁸ Chapman, *Susan Stebbing* (especially 38-40, 79-80) clearly documents the challenges faced by MacDonald's supervisor in gaining a permanent position.

precarity.²⁹ A second, compatible, reason is that her views were not neatly characterizable with respect to the main traditions represented by her male colleagues. Thus we see two very brief mentions of the very same article, “Russell and McTaggart”, characterizing MacDonald alternatively as a logical positivist (Taube, who at the very least uses MacDonald's self-characterization in that article) and a Moorean (Passmore). Such a hermeneutical technique entails that we will learn nothing new from MacDonald that we could not learn from some men. I will argue for this thesis by looking at two sets of commentaries on her work in the 1930s by Gilbert Ryle, Isaiah Berlin, and A.M. MacIver. By making this claim, I am not suggesting that this kind of misunderstanding cannot happen to men, or indeed to anyone. I am only assuming that it disproportionately affects those in marginalized groups and can therefore explain why their views have so thoroughly dropped out of the history of philosophy.

One very clear illustration of how she was received by her contemporaries came in Ryle's and Berlin's commentaries on "Induction and Hypothesis". Ryle, in fact, only discusses MacDonald's views in the first 3 pages out of his 27-page commentary, the remaining pages dedicated to his own solution to the problem of induction. Ryle's main contention is that, while MacDonald's points about language are all well and good, they do not themselves touch upon the substantive issue of induction:

The philosophical perspicacity required for the solution of these puzzles will be something more than nicety of stylistic taste, which is what, if I am not doing her an

²⁹ See Kremer ("Margaret MacDonald") for a striking comparison between MacDonald's and Ryle's professional academic careers.

injustice, Miss MacDonald by her example recommends us to rely upon as our Open Sesame. ("Induction and Hypothesis", 38)

If I am right about her general philosophical outlook above, there is much more to MacDonald's philosophy than legislating "stylistic taste". And even though it is by no means unusual to be polemical in Aristotelian Society commentaries in this period, the condescension in Ryle's prose when discussing his colleague's work is palpable. Indeed, he never even takes the time to argue why the points brought up by MacDonald are not relevant to the problem as he describes it. Ryle's commentary thus exemplifies perfectly what Miranda Fricker has called "testimonial injustice".³⁰

Berlin in his commentary, by contrast, substantively engages with MacDonald's argument, listing eight objections. However, he still fails to see her views except as approximations of the correct views of other, male, philosophers. For example, when contrasting the correct typology of knowledge with MacDonald's, Berlin essentially assumes Russell's distinction between knowledge by acquaintance and knowledge by description, claiming that it is only when understanding this notion of knowledge that the problem of induction is so much as intelligible and faulting MacDonald for not having discussed the problem under these headings. Her view is reduced to "when we know it we know it, but sometimes we do not; and even when we do we may still be mistaken, of which one can only say that it is either tautological, or fallacious, and in either case extremely obscure" (ibid, 73).

³⁰ Fricker (*Epistemic Injustice*), Anderson ("Epistemic Injustice").

A second example comes from her exchange with A.M. MacIver in *Analysis* in 1937-8.³¹ In "Language and Reference", she argued that the type-token distinction from Peirce was compatible with Carnapian views of language. MacIver in his response takes issue with MacDonald's claim that one can find tokens of a type (e.g., the number of tokens of the word 'the' on this page) without any reference to meaning. Who precisely is correct in this dispute is not of primary interest. Rather, MacIver seems incapable of appreciating MacDonald's views as a response to the question at hand. He admits freely at one point that he was arguing more against Stebbing's views than MacDonald's, as if they were the same, and somehow finds a way to fault *MacDonald* because she "pays no attention" to philology and phonetics, in which he has "an amateur interest". In fact, "The Philosopher's Use of Analogy" makes clear that MacDonald does know something of philology and attempts to systematically separate the philologist's interest in language with the philosopher's. But the more important point is that the details of Tagalog phonetics are not in any obvious way relevant *at all* to their debate. I do not mean to suggest here that I have proved that MacIver was an outright sexist. However, MacDonald suffers from a kind of misunderstanding by him that disproportionately affects women.

These examples of extended engagement with her work in the 1930s strongly suggests that her male contemporaries did not engage with her views on their own terms, instead forcing them into the molds of other (male) philosophers. This had the predictable outcome of leading to her exclusion from the early histories of analytic philosophy.³² While the exclusion of a thinker from the history of philosophy, rightly or wrongly, can happen

³¹ MacDonald ("Language and Reference"; "Reply"; "Further Reply"), MacIver ("Token, Type and Meaning"; "Rejoinder"; "Last Words").

³² See Chapman, *Susan Stebbing*, 69 ff for a similar dynamic with Stebbing and John Wisdom.

irrespective of sexism, it is reasonable to think that implicit or explicit sexism did play a role in MacDonald's case. She lived and worked in a time that was especially difficult for women (especially those, like her, of lower class background). Moreover, her views were misunderstood in a way that is more common for women to experience than men. Together, these explain to some extent why she was so thoroughly written out of the early histories of analytic philosophy.

6. Conclusion

MacDonald's work on philosophy of science in the 1930s was important and original in its own time and place. Given the current interests in the philosophy of scientific practice,³³ it is also relevant for contemporary debates. My modest aim here was simply to bring out one way in which her work stands out. There is much still to be done to understand her place in the history of early analytic philosophy. Two major questions: first, how does her early work connect to her later writings on ethics, political philosophy, and aesthetics? Second, do any of her distinctive views influence other thinkers, even if they do not directly acknowledge it?³⁴

The story of why she was largely forgotten can be instructive for those interested in reviving women in the history of philosophy more broadly. First, we have seen general confirmation of O'Neill's thesis outside of the early modern period that being excluded from early histories of philosophy constitutes a major bottleneck for female philosophers being transmitted. This means that, in order to recover female philosophers, historians must

³³ See, e.g., Chang ("The Philosophical Grammar of Scientific Practice")

³⁴ On her influence on Ryle, see Kremer ("Margaret MacDonald").

search for their subjects outside of such histories. Second, we must guard against the temptation to impose the categories of a female philosopher's male contemporaries, simply because they became more prominent in subsequent history. While it is generally sound advice to try to interpret thinkers charitably, it has additional importance when it comes to marginalized groups, since in this case, historians risk missing an important argument or view just because the person who made it was a woman.

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