Polanyi, Popper and Methodology: A Reply to S. Richmond

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1. Introduction

It is sometimes said that Polanyi's ideas have been summarized, analyzed, explicated, clarified and reconstructed often enough and that we should rather use his theories as a rich source of inspiration and a common platform to work on than go on repeating them. However, when accounts of his work appear that contradict the standard interpretation, one is forced to go back to the old texts in order to check the merits of these accounts. A good example is Sheldon Richmond's recent comparison of the theories of Polanyi, Popper and the historian of art E.H. Gombrich.[1] Apart from a rather surprising interpretation of Polanyi's philosophy of science, Richmond also presents a Popperian critique of it. My aim in this paper is to discuss critically his interpretation of Polanyi's epistemology and to show that his criticism fails.

One of Richmond's central questions is "whether there can be a Popperian reply to Polanyi's critique of methodology''(113). This is interesting because we already have a Popperian reply to Polanyi, namely, Popper's own *Logic of Scientific Discovery*. In the preface to the first English edition of 1959, Popper indicates that the book is an attempt to get rid of pseudo-psychological and ''subjective'' methodologies and that it is meant to

> save the sciences ... from an obscurantist faith in the expert's special skill, and in his personal knowledge and authority; a faith that so well fits our 'post-rationalist' and 'post-critical' age, proudly dedicated to the destruction of the tradition of rational philosophy, and of rational thought itself.[2]

Apart from this anti-Polanyian *cri de coeur* and a few scattered allusions in footnotes, it is hard to find any direct exchange of views, critical or otherwise, between what Joseph Agassi has called "the two outstanding philosophers of the mid-century," Popper and Polanyi.

What we do have, however, and what I take to be the most extensive and explicit Popperian criticism of Polanyi, is Alan Musgrave's doctoral dissertation *Impersonal Knowledge: A Criticism of Subjectivism in Epistemology* (University of London 1969).[3] Although Richmond does not seem to have considered secondary sources such as this, his own search for a Popperian reply to Polanyi's "critique of methodology" seems interesting in its own right. Especially since he maintains boldly that Popper's methodology as a system of conventional rules for discussing theories not only contradicts, but in fact refutes Polanyi's critique. This claim is crucially important for Richmond's whole endeavor because, as he himself points out (113), his overall position rests on a double analogy: (a) if Polanyi's critique of methodology parallels (in part) E.H. Gombrich's critique of aesthetics, and (b) if Popper's theory of methodology refutes Polanyi's critique, then (c) Gombrich's Polanyianism is mistaken also. But of course if (b) is wrong, Richmond's position collapses and (c) fails, at least as far as the analogies go. My concern is neither with the analogies between Polanyi and Gombrich nor with Richmond's critique of the latter, but rather with (b), the viability of Richmond's "Popperian reply" to Polanyi's critique of methodology.

In order to establish whether Polanyi's critique of methodology is worthy of a more elaborate examination, Richmond raises the preliminary question of its consistency: (a) "Can Polanyi's theory of personal knowledge be true of itself?" and (b) "Is Polanyi consistent on the issue of the role of disagreement in science and philosophy?" (114). These are important questions so let us have a look at how they are dealt with.

2. Is Polanyi's Epistemology Consistent?

Can Polanyi's theory of personal knowledge be true of itself? According to Richmond, Polanyi has a theory "that no scientist can articulate his know-how without inhibiting his research."[4] He then goes on to tell us that if Polanyi's theory of personal knowledge is true of itself,

> then we contradict Polanyi's theory that no scientist can articulate his know-how without inhibiting his research. If ... Polanyi's theory cannot be true of itself, then Polanyi's theory is a counter-example to itself. (115)

Unfortunately, this seems seriously confused. Certainly, the piano player who is focussing her attention on the movement of her hands and fingers instead of on the music, will seriously inhibit her performance during a recital. But while practising she will be shifting her attention constantly and consciously between the particulars (touch, finger setting etc.) in order to improve her skill. Or, to mention a few other Polanyian examples, "[m]otion studies, which tend to paralyze a skill, will improve it when followed by practice" (*TD* 19) or "[t]he formal rules of prosody may deepen pour understanding of so delicate a thing as a poem" (*TD* 20). And in the case of research, discovery or coming to know in general proceeds by an alternating "see-saw" analysis and integration (cf. *KB* 129).

There is no need for further examples. The first part of a Polanyian rebuttal of Richmond's claim is simply that tacit awareness of clues and focal attention are mutually exclusive only at a particular point in time. It is possible, and sometimes even necessary, to shift one's attention to subsidiarily functioning particulars (though it may be difficult to retrieve them) within the problem situation. The second part, of course, is that explicit knowledge of rules cannot replace the activity or practice in which these rules are embedded. Knowing the rules of tennis by heart does not make one a good player. Thus whatever rules or procedures Richmond's scientists may come up with, these rules can never wholly replace the tacit operations involved in the practice of problem solving.

As regards the role of rules in scientific practice, I think it quite clear that Polanyi is neither saying there are no rules, nor that "methodology is worthless" —as Richmond wants us to have it — but that any rules that can be laid down for the guidance of research can be but "vague maxims" (cf. *PK* 125).[5] I think Polanyi's critique of "methodology" is summed up nicely in the following statement:

> It is only when we are confronted with a live scientific issue, that the ambiguity of the formal processes and of the various attenuated criteria of scientific truth becomes apparent, and leaves us without effective guidance (*PK* 150).

Note, however, that no effective guidance is not the same as no guidance at all. Richmond is totally misconstruing Polanyi's position by saddling it with the false dilemma between "either you have an exhaustively specifiable set of rules of method or you have no rules at all." Hence presumably his identification of the "no rules at all" option with tacit knowing: "[r]ules of method cannot guide science, because scientists depend on inarticulate sources of knowledge for progress" (114).

Pace Richmond, there simply is no Polanyian "theory" that scientists cannot specify their know-how without inhibiting their research. Consequently, there is no "serious flaw in Polanyi's theories" either.

Is Polanyi consistent on the issue of disagreement? In conducting his second consistency test, Richmond maintains that according to Polanyi

> ... disagreement is a block to scientific progress; only when scientists agree is there progress in science ... [o]nly when scientists agree on the fundamental views of nature, can they apply their views and so progress on the understanding of nature. (116)

To repeat, no evidence is produced to show that Polanyi really held this view, either explicitly or by implication. In the latter case, we would expect some textual evidence, but Richmond only comes up with the well-known passage (*PK* 151) where Polanyi, preparing the way for Kuhn, talks about *temporary* incommensurability of competing theories and the role of persuasion in debates between their proponents. Reliance on research traditions involves agreement on certain fundamentals and is necessary for any field of inquiry. But consensus need not, and indeed should not be total. Without some measure of dissent from scientific consensus no discovery, and without discovery no progress at all. Dissent already starts, for instance, in the first step to discovery which, according to Polanyi, is to find a good new problem, one "that no one has yet sighted." or to find a new way of solving a known problem (cf. *KB* 202; *TD* 22). In general, Polanyi is in fact warning that the dangers of scientific consensus "are an unceasing menace to scientific progress" (*KB* 94).

I conclude that Polanyi's position on the matter of scientific controversy does not warrant Richmond's claim that he "disallows disagreements in science" (116). Hence, the question concerning the consistency of Polanyi's theory of personal knowledge of which Richmond makes so much, turns out to be a non-issue. Let us now have a closer look at Richmond's Popperian reply to Polanyi's historical, psychological and logical criticism of methodology.

3. Historical Criticism: History of Science Versus Rational Reconstruction

According to Richmond, the theory of personal knowledge does not specify the procedures of discovery and testing and therefore it cannot have normative implications and it cannot be used as a guide by scientists.

To a certain extent, Richmond is right. But, of course, Polanyi's epistemology is not meant to specify rules but rather to give an account of the general conditions under which discovery and progress are reasonably possible at all. Whether this is a weakness of Polanyi's philosophy of science wholly depends on whether, and in what sense, philosophy of science should be conceived of as the watchdog of science ("methodology") or not.

As a true Popperian, Richmond sees its task as normative and so he construes the controversy between Polanyi and Popper as a dilemma between the history of science and methodology (cf. p.146). In contrast to Popper, Polanyi is said to advocate the view that the history of science should dictate the philosophy of science (117).

What to think of this? To begin with, it should be pointed out that Polanyi's philosophy of science is but part of his overall epistemological program, the critique of objectivism and the development of a viable alternative. Given his aims and his new conception of knowing, it seems quite natural for him to concentrate on and emphasize the *informal* aspects of scientific discovery. It seems odd to expect him to engage in what his logical positivist and critical rationalist opponents were already so busy doing. For instance, discussing the question of how the rationality of scientific progress could be safeguarded, inductively or deductively, and specifying explicit, preferably formalized, rules or procedures. However, this is asking too much charity of a true Popperian, so I will not pursue this issue further.

I think it is a caricature of Polanyi's position to say that he advocates the primacy of the history of science. As we have seen, Polanyi's point is not so much historical but rather meta-methodological: live scientific issues of real importance cannot be solved by applying algorithms. I think there is not a shred of evidence to suppose that Polanyi believes that selection and testing of hypotheses, though ultimately personal acts, are not also subject to rules. Nowhere is Polanyi even suggesting that methodologically "anything goes" or that the maxims of scientific procedure are unimportant. Scientists are guided by these maxims but since maxims are not algorithms, success cannot be guaranteed.

Next, it seems to me that the controversy between Popper and Polanyi is not at all about the alleged primacy of the history of science. For Polanyi, who in this respect comes close to the position of the later Wittgenstein, practice is primary. Just as Popperians use examples from the history of science in order to find support for their theories, Polanyi uses historical examples in order to support his attack on method fetishism by showing that many rules proposed by the "philosophers." if in fact applied to the letter, would have inhibited progress (at least in the natural sciences). In addition, his examples show that scientific standards or rules, instead of being a priori or universal, change over time:

> To every change in scientific value, from Kepler to Laplace to Einstein, there has corresponded a change in scientific method, which can be formulated in changing maxims of procedure (*PK* 170).

At this stage it might be interesting to point out very briefly some differences (and similarities) between the Popperian and the Polanyian approach to the history of science as can be found in their treatment of the Copernican revolution. Let us start with the Popperians.

4. Intermezzo: Popperian and Polanyian History of Science

I. Lakatos's and E. Zahar's beautiful account of the Copernican revolution is an attempt to reconstruct that episode such that it can be shown that science progresses in a rational way.[6] That is, they try to show that there are, with hindsight, good objective reasons to adopt Copernicus's heliocentric assumption at the time of its proposal — irrespective of the psychological or historical question whether or not Copernicans like Kepler and Galileo actually believed in, or were aware of, these reasons.[7]

Indeed, from Popper to Lakatos (and to Richmond, I should add), the Popperians did their utmost to keep philosophy of science within the bounds of the (allegedly) deductive context of criticism. Only the objective "third world" of logical contents of thought and language matters and so true epistemology is "epistemology without a knowing subject." Where this leaves the Popperian admonition to adopt the critical attitude remains a mystery. From a Polanyian point of view the question here is: who is to adopt that attitude if we are supposed to proceed on the assumption that there is nobody to adopt it? Or, what "objective" account can be given of the Popperian doctrine of fallibilism that human beings are prone to error if the possibility cannot be excluded that errors may be due to the incorrect use (by human subjects) of reliable methods?[8] Finally, it seems that (sophisticated) Popperian reconstruction of the history of science (as in the case of the Copernican revolution) allows for any measure of irrationality and dogmatism on the part of individual scientists (such as the Copernicans). Since the context of discovery is a matter of psychology (or sociology) only, it is irrelevant what scientists actually believe.

Polanyi's interests are markedly different. As Richmond recognizes, he naturalizes epistemology by bringing the heuristic and "psychological" dimensions of knowledge acquisition within its scope and thus within that of applied epistemology (philosophy of science). In doing so he was (with Quine) one of the first to jettison the Kantian distinction between the contexts of discovery (fact) and justification (norm). Unlike Popper, part of Polanyi's aim is to explain how coming to know is reasonably possible and (partly) for that reason he proposed innovative conceptions like, for instance, tacit knowing, intuitive foreknowledge, intellectual beauty and personal judgement.

All this can be traced in Polanyi's account of the Copernican revolution.[9] Again very briefly, for him the problem is how Copernicus's system could vastly exceed Ptolemy's in its anticipations though both had about the same explicit (empirical) content.[10] His solution is to a considerable extent identical to that of Lakatos, but it *also* attempts to specify the grounds on which "Copernicus fastened his hopes during thirty years of travail and ... he and his followers claimed, against bitter opposition, that the heliocentric system was real."[11] In addition, Polanyi correlates the logical, the psychological and the aesthetical realm by his suggestion that, ultimately, the heuristic superiority of the Copernican system lies in its appearance, that is, in its aesthetic qualities like harmony, depth and coherence. According to Polanyi, these aesthetic criteria in science are marks of intuitive truthlikeness and intimations of reality.[12] It goes without saying that on his account the appreciation of these qualities is a matter of fallible tacit knowing, not of mechanically applying a set of rules.

5. Psychological Criticism: Commitment versus Universal Criticism

According to Richmond, Polanyi holds that two requirements of the methodological approach, namely the critical attitude towards theories and the universality of discussion, are psychologically impossible to meet (cf.p.113). A critical attitude towards a theory involves appreciation. In order to appreciate a theory, however, one has to interiorize a-critically the tacit framework in which it is embedded. Thus, according to Polanyi, "one can only appreciate a theory if one is a-critical of it" and therefore critical methodology is impossible (119). Worse even, the tacit frameworks in question are conditioned by local culture. Hence "people from different cultures with different implicit premises cannot have discussions" (113).

Richmond's reply to his own construal runs as follows. Popperian methodology only proposes conventions for the procedure of discussing theories "as impersonal trials seeking truth" (119). It does not describe actual attitudes of scientists but it can be used to change them. For example, Richmond suggests that we could agree to ignore our tacit presuppositions towards theories and proceed to discuss them as impersonal trials. Similarly, scientists from different cultures can agree "to ignore their tacit views and ... to apply the agreed-upon rules to their disagreements" (*ibid*.).

My rejoinder can be brief: Richmond has failed to grasp the meaning of "a-critical" as an important characteristic of tacit knowing (cf., e.g., *PK* 264). The mental act of integration which constitutes tacit knowing (like personal judgement, choice and decision) can as such, that is, *at the time of its making*, be rash, premature or unwise but not critical.[13] It would be a mistake, however, to think that this means that an a-critical mental act of integration could not be performed in order to criticize something else, or could not itself be criticized afterwards. Similarly, tacit frameworks are interiorized in a largely a-critical way and function at subsidiary levels, but it does not follow that one cannot be critical of particular elements of the theory one is working on or even of the particulars of the framework in question (insofar as they are specifiable).

Finally, we have Richmond's claim that Polanyi's theory of knowledge implies the impossibility of cross-cultural discussion. If true, this would imply that Polanyi advocates total incommensurability of interpretative schemes and thus a radical relativism of truth (and that would make his position utterly inconsistent). But Richmond does not present a shred of evidence for his claim, which I find totally incomprehensible.

6. Logical Criticism

On Richmond's account, Polanyi's logical criticism of methodology is based on Plato's Meno paradox. We are told not only that Polanyi adopts Plato's solution, but also that

> [g]iven Meno's dilemma that methodology either finds and so is unnecessary, or that methodology cannot find and so is useless, the consequence is that methodology cannot guarantee certainty (120).

Again we are forced into a dilemma: either give up certainty (Popper's choice) or give up methodology (Polanyi's choice). If certainty goes, we accept that no finality can be reached. Methodologies are just conventional guidelines for improving our trials by asking how we can more quickly expose error.

Again, the tendency to simplify matters leads to distortion and confusion. First of all, Polanyi's solution to the Meno paradox can hardly be classified as "Platonic." Tacit knowing is not necessarily a kind of retrieval from memory, nor as is suggested elsewhere some "inner, hidden source of knowledge" (110) but rather a capacity of cognizing agents for solving problems. More importantly, it is totally incomprehensible to me how the theory of tacit knowing could be taken as an attempt to safeguard certainty. This is supposed to be Polanyi's position:

> Polanyi's idea is correct that science cannot guarantee truth. However, he is too utopian in expecting that science should guarantee truth (120).

But what does this mean? Unfortunately, no explanation is forthcoming. If we are supposed to come up with bold conjectures, I would say that, yes, according to Polanyi, science is our best bet if we want to have reliable knowledge. Precisely for that reason, science itself (as an institution) should be safeguarded. If this is utopian, Popper and the overwhelming majority of philosophers of science, not to mention scientists themselves, are equally utopian. As Richmond seems to acknowledge, Polanyi is convinced that certainty cannot be had. But he does seem to overlook that Polanyi, like Popper, is not only an anti-foundationalist but, as I have tried to show elsewhere, an impeccable fallibilist also.[14] This brings me to a final point, Richmond's own Popperianism.

7. How "Popperian" is Richmond's Reply?

Not only Richmond's rendering of Polanyi is highly problematic, his summary of the Popperian view of methodology as mere "conventional rules for evaluating theories" (117) is questionable too.

To begin with, the rules of logic which play a crucial part within the Popperian system are not conventional at all. On the contrary, they are, at least according to the Popperians themselves, about as near to "absolutes" as one can get. Surely, there is nothing particularly "conventional" to *modus tollens*, the logical backbone of early (naive) falsificationism.[15]

Next, Popper's rejection of induction and his ban on *ad hoc* strategies to protect theories from falsification are just too central to his program to have them treated as mere conventions or topics for discussion. Similarly, the decision to jettison the non-deductive heuristic ("irrational") context of discovery from philosophy of science and to consider only its ("objective") products, the ready-made theories, worthy of philosophical attention can hardly be seen as a convention, let alone as a proposal for discussion.

This brings me to a final point. According to Richmond, when there is a clash between scientific practice and philosophical rule Popper holds that "scientists and philosophers should *discuss* not dictate" (118). But this is puzzling because what now remains of the normative role of philosophy of science? As Richmond acknowledges, the whole point of normative Popperian philosophy is to legislate: "Philosophers of science ... ask what *should* scientists do?." I do not think that the ambiguity can be solved by suggesting that Popper thinks "there should be a dialogue between philosophers of science and scientists" (*ibid*.). Of course this would nicely legitimize the role of the philosopher of science, but apart from the fact that to prescribe dialogue remains a prescription all the same, it also seems watering down the normative aims of Popper's epistemology.

To sum up, whatever the merits of Richmond's other interests, his account of the similarities between Gombrich and Popper and Gombrich and Polanyi, his Popperian reply to Polanyi fails. Not only are Polanyi's ideas frequently misconstrued, but his own position is in several respects less Popperian than he is suggesting.

ENDNOTES

[1] Sheldon Richmond, *Aesthetic Criteria: Gombrich and the Philosophies of Science of Popper and Polanyi*, Series in the Philosophy of Karl R. Popper and Critical Rationalism (ed.) K. Salamun, Vol. VI, Amsterdam/Atlanta, GA: Rodolphi, 1994.

[2]K. R. Popper, *The Logic of Scientific Discovery*, London: Hutchinson, 1959, 23.

[3]As the title suggests, Polanyi is considered a paradigm example of a so-called "subjectivist" epistemology. Musgrave's thesis was written under Popper's supervision whereas various versions were commented upon by J.W.N. Watkins and I. Lakatos. J. Agassi might be an exception but it is questionable whether he should be taken as a Popperian (cf. his "Genius in Science." *Philosophy of the Social Sciences* 5 (1975), 145-161) . Musgrave's criticism of Polanyi is extensively discussed in my *Michael Polanyi's Post-Critical Epistemology*, Amsterdam 1988,159-226.

[4]Richmond, *Aesthetic Criteria*, 115. The textual evidence for this claim are *PK* 56 and *The Study of Man* (*SM*) 31 with examples of skills (piano and tennis playing) and the thesis that subsidiary and focal awareness are mutually exclusive.

[5]Cf. also *PK* 311, where Polanyi rejects "the vain pursuit" of a *formalized* scientific method and advises us to abandon the search for *strict* criteria and *strict* procedures: "The scientist's procedure is of course methodical. But his methods are but the maxims of an art which he applies in his own original way to the problem of his own choice." Quite rightly, the Popperian Lakatos interpreted Polanyi as advocating a "case law approach." by contrast to Popper's "statute law" approach. In my view Lakatos even comes close to becoming a Polanyian when he points out that the scientific standards, as applied by the scientific elite in particular cases, have constituted the main yardstick of the philosopher's *universal* laws. Cf. Lakatos, "History of Science", *The Methodology of Scientific Research Programmes*, Cambridge 1978, esp. pp.136-138. Interestingly, though, Lakatos makes two exceptions: when a research tradition degenerates or when a new bad one is founded. As examples of the latter he mentiones "some of the main schools of modern sociology, psychology and social psychology" (Lakatos, *ibid*., 137). If Lakatos is right, this might explain Richmond's concern for an autonomous and legislative role of philosophers of science as a methodology. It may well be that certain fields of inquiry are in need of (better) rules, whereas the natural sciences can proceed on their own steam. In that case Polanyi, at least insofar as philosophy of science proper is concerned, does not distinguish sufficiently between the state of the art in the various sciences. But neither does Richmond.

[6]Cf. Lakatos (and E. Zahar), "Why did Copernicus's Research Programme Supersede Ptolemy's?", *The Methodology of Scientific Research Programmes*, p.168-192.

[7]Briefly, the objective reasons are in conjunction: Copernicus's theory was superior to Ptolemy's because: it predicted a wider range of phenomena, it was corroborated by novel facts (though only much later) and it showed more heuristic unity. Lakatos and Zahar, *ibid*., p.189.

[8]For an interesting criticism of Popper's attempt to eliminate the knowing subject, cf. Susan Haack, "Epistemology With a Knowing Subject", *Review of Metaphysics* 33(1979), 309-335; see also my *Michael Polanyi's Post-Critical Epistemology*, p.218ff.

[9]Cf. M. Polanyi, “Science and Reality”, *British Journal for the Philosophy of Science* 18 (1967), 177-196. For a more elaborate comparison of the Lakatos-Zahar and the Polanyian account than I can undertake here, see my *Michael Polanyi’s Post-Critical Epistemology*, p. 138-145.

[10]Cf. Polanyi, “Science and Reality”, 189. Interestingly, his answer is in part similar to that of Lakatos and Zahar: the Copernican system was superior because it explained the main features of Ptolemy’s system, it predicted the relative orbital radii of the planets and it had greater heuristic power.

[11]Polanyi, *ibid*., 185; the objective ground was Copernicus’s theory which explains planetary loopings and predicts a plausible sequence of orbital radii.

[12]As far as I can see, Richmond does not go into the issue of the important role of *aesthetic* criteria like, e.g., “intellectual beauty.” within Polanyi’s epistemology.

[13]Cf. also *PK* 314 where it is pointed out that mental acts cannot be corrected “at the moment of acting.”

[14]See my *Michael Polanyi’s Post-Critical Philosophy*, p.22,192f.,210; see also *PK* 271, 314f., 404.

[15]Since Richmond (cf. p.9, 25f.) keeps his Popperianism safely confined to Popper’s position of the *Logic of Scientific Discovery* (1959), he does not have to address the awkward problem that naive falsificationism (a theory is refuted when it is contradicted by observation statements) is untenable, as later developments in the philosophy of science (including critical rationalism) bear out.