# THE DILEMMA OF THE MODERN MIND AND THE LIMITS OF RULES: POLANYI'S CRITICISM OF POSITIVISM (1946-1952)

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## ABSTRACT

*Starting in 1946, Polanyi begins to criticize a comprehensive system of ideas that he names positivism. His criticism is twofold. On the one hand, it has the narrow aim of pointing out the inconsistencies of a positivist account of science, according to which the essence of scientific objectivity lies in establishing rigorous mathematical relations between measured variables employing fixed rules. On the other hand, it examines the broad assumptions underlying this view, namely radical empiricism and skeptical doubt. The present paper analyzes both aspects of this criticism, stressing its crucial role in the development of Polanyi's philosophy.*


## Introduction

In a paper titled, "The Roots of Tacit Knowledge: Intuitive and Personal Judgment in Polanyi's Early Writings (1939-1946)", I proposed a historical reconstruction of the idea of tacit knowledge by analyzing the concepts of "intuitive judgment" and "personal judgment".[1] The present paper continues that historical reconstruction by examining another crucial step in the development of Polanyi's philosophy, namely his criticism of positivism between 1946 and 1952.

Starting with a criticism of planned science, Polanyi reflects on the functioning of science, focusing on its institutional arrangement and the factors leading to its growth. In particular, Polanyi draws attention to scientific discovery, emphasizing the crucial role of "intuitive judgment" and "personal judgment". Indeed, scientists rely on undisclosed abilities to make discoveries, abilities that allow them to integrate scattered particulars in a coherent solution. According to Polanyi, the crucial steps leading to scientific discovery are a matter of intuition, creativity, instinct, and personal commitment. For these reasons, I concluded that making discoveries relying on an intuitive and personal judgment is something analogous to knowing "tacitly".

  

This account of scientific discovery has an important consequence. Indeed, since intuitive and personal judgment cannot be formulated in exact terms, we cannot provide a set of definite rules for replacing their use. Thus, in view of the fact that these abilities are crucial in the process leading to new discoveries, we can conclude that we cannot formulate rules for making discoveries. As we shall see in the present paper, this is one of the main aspects of Polanyi's criticism of positivism.

The second relevant aspect of this criticism lies in the fact that it involves the broad assumptions underlying the positivist view of science, namely radical empiricism and skeptical doubt. Polanyi names the philosophical framework established on these principles "the modern mind". According to Polanyi, the modern mind creates a self-contradictory ideal of knowledge because it leaves no room for the personal participation that makes knowledge meaningful. This false idea of knowledge, which Polanyi will call "objectivism" from *PK* onward, has moral and political repercussions as well. Against this false idea of knowledge and its consequences, Polanyi elaborates his fiduciary program, according to which the process of knowing is the realization of the conjunction between understanding, believing and belonging (see Polanyi 1947b, 9). Although the criticism of the modern mind is only sketchy in the period under consideration (1946-1952), it is still important because it shows the interplay between critical and constructive thinking in Polanyi's philosophy.

The criticism of positivism had a crucial role in the development of Polanyi's thought. As I showed in my earlier paper (Tartaro 2021), Polanyi didn't develop the concept of tacit knowledge by starting from the maxim "we can know more than we can tell". Rather, his reform of the very idea of knowledge originated with an effort to show that there is something more than what we can tell and that this has to be regarded as knowledge. Polanyi's criticism of positivism had exactly this role. Indeed, the faulty logic of positivist accounts of science demonstrates that scientific knowledge cannot be established by applying explicit rules to primarily given observation. As Polanyi (1947b, 9) states, "science is not based on the mere evidence of our senses". On the contrary, it always involves beliefs that we hold in virtue of our belonging to a group sharing these beliefs. In other words, the value of this criticism lies in demonstrating that not all knowledge is always explicit. Only once this point has been established is it possible to inquire into the tacit dimension of knowledge. This inquiry, which is Polanyi's most important contribution to the epistemology and philosophy of science, thus has its roots in the problems I'm going to consider.

## Radical Empiricism and Skeptical Doubt: The Principles of the Modern Mind

The analysis of the modern mind is a first attempt to disclose the origins of the 20th-century crisis. According to Polanyi, the reasons for this crisis and its tragic implications "can be traced back to the very beginnings of modern civilization as it emerged from the Middle Ages" (1945, 8). Polanyi points out how the rise of the modern age is characterized by a struggle against the medieval worldview, in particular against Aristotle's philosophy and the authority of the Church. Radical empiricism and skeptical doubt were the two principal means of the modern mind in this struggle against authority:

> Cartesian doubt and Locke's empiricism became then the two powerful levers of further liberation from established authority. These philosophies and those of their disciples had the purpose of demonstrating that truth could be established and a rich and satisfying doctrine of man and the universe built up on the foundations of critical reason alone.

Self-evident propositions or the testimony of the senses, or else a combination of the two, would suffice (*SFS*, 61).

Here Polanyi clearly states the twofold function of the two principles. On the one hand, they are employed as tools against established authority. On the other hand, they lay the groundwork for a critical reason. In a later section, we shall see how the combination of these two principles not only establishes the critical reason but also determines its failure and, consequently, how it motivates the quest for a *post*-critical reason. For the time being, however, we can just focus on the first aspect.

The role of skeptical doubt is to provide a systematic elimination of unwarranted assumptions implied in our way of thinking. As Polanyi states: "To assert any belief uncritically has come to be regarded as an offense against reason…We feel in it the danger of obscurantism and the menace of an arbitrary restriction of free thought. Against these evils of dogmatism we protect ourselves by upholding the principle of doubt that rejects any open affirmation of faith. For the past three centuries the principle of doubt has been continuously at work on the elimination of all uncritical affirmations of faith" (1952b, 217).

Once the unwarranted affirmations of faith are abolished through skeptical doubt, only propositions based on the testimony of the senses are justified. According to the modern mind, the truth can be established on the basis of unquestionable hard facts because empirical evidence is cogent and has the power to compel the assent from any rational human being (cf. Polanyi 1947a, 10). The role of radical empiricism, thus, is to provide a method allowing to formulate propositions that can resist the skeptical doubt. In this way, it is possible to reach a solid foundation for knowledge. The consequences of these principles are far-reaching. Since only self-evident propositions (e.g., logical tautologies) and the testimony of the senses can guarantee reliable knowledge, science is considered to be a mere organization of experience through the application of logical rules.

## From the Modern Mind to a Positivist View of Science

As shown in the previous section, both skeptical doubt and radical empiricism played a leading part in the establishment of modern science. Polanyi's reconstruction of the beginning of modern science can be summarized as follow: (1) modern science is founded on a critical struggle against all authority; (2) the exercise of radical doubt is a means to demolish any a-critically accepted authority; (3) only auto-evident proposition and hard facts gathered with empirical methods can stand up to this radical doubt; (4) thus, science, being the refusal of all authority and the rational activity par excellence, is a mere collecting and organization of these hard facts (Polanyi 1947a 10-11 and 1949, 14).

The positivist account of science pushes further this interpretation, because "the movement set out not only to liberate reason from enslavement by authority, but also to dispose of all traditionally guiding ideas, so far as they are not demonstrable by science. Thus, in the positivist sense truths become identified with scientific truth and the latter tend—by a positivist critique of science—to be defined as a mere ordering of experience" (Polanyi 1949:14). The interrelation between the principles of doubt and empiricism and the view of science as "ordering of experience" or a "calculating machine" is thus quite plain. Polanyi provides a grotesque picture of this conception in the following passage:

> A passionate affirmation of what some scientists believe science to be was given in recent years by the distinguished American psychologist, Clark L. Hull, in his *Principles of*

> *Behaviour*. The essence of scientific objectivity lies, he says, in establishing rigorous mathematical relations between measured variables. Given the values of one set of variables, science predicts exactly the value of another set. A genuine scientific theory must operate like a calculating machine, which, once the keys representing the dividend and the divisor have been depressed, determines the result automatically (Polanyi 1950, 2).

Thus, from this perspective, scientific knowledge is conceived as objective, impersonal, verifiable, and unambiguous.

Polanyi criticizes the positivist account of science from various perspectives. History of science, for instance, supplies evidence that the essential features of scientific discovery do not reside only in establishing rigorous mathematical relations between measured variables. For example, the cases of Copernicus, Kepler, and Newton, show how scientific discovery is a process in which "each new phase re-states that which was known before. Each reveals that its predecessor was the embryo of a truth wider and deeper than itself" (Polanyi 1945, 9). Thus, scientific progress is not a process of accumulation, as the positivists claim. Furthermore, capital studies in heuristics, such as the ones of Poincaré and Polya, reveal how the essential phase of discovery represents rather a process of "spontaneous emergence" than a formation of a mathematical relation between sets of values. Moreover, mathematical relations empirically established are not by itself a mark of scientific discovery, as Polanyi makes clear when he refers to the alleged link between the period of gestation of rodents and multiples of $\pi$ (cf. Polanyi 1947a, 11). Eventually, Polanyi's criticism of the positivist account of science is also based on a reflection on the limitations of the rules and the procedures of scientific practice. We shall focus mainly on this last criticism in what follows.

## What Rules Cannot Do: Polanyi's Arguments Against Positivism

As stated in the introduction, a consequence of the crucial role of personal judgment in scientific practice is that the rules alone are not able to lead to discovery, verification, or falsification. Polanyi's criticism aims to demonstrate that the positivist account of science is false and, *a fortiori*, the assumptions on which this conception is based are faulty as well. Nevertheless, Polanyi doesn't regard the rules framing scientific practice as dispensable. Nor does he claim that procedures and formalization are of no use. On the contrary, this critical analysis leads Polanyi to a deeper understanding of the meaning of scientific rules in light of personal judgment. Rules prove to be "rules of art," which are such only when embedded in practice. They are not strict codifications prescribing unambiguous application, but rather vague maxims leaving a significant margin to personal judgment.

### *Rules and Discovery*

The main target of Polanyi's criticism is the idea that scientists can make discoveries simply by applying fixed rules to given empirical observations. In *SFS*, Polanyi sets the problem with the following question: "Given any amount of experience, can scientific propositions be derived from it by the application of some explicit rules of procedure?" (*SFS*, 7). For the sake of argument, Polanyi concedes both that the relevant experience is provided in the form of numerical measurements and that we already know which sets of values are in mutual connection. So, for example, we now face two sets of numerical values, and we know that one can be represented in the terms of the other. Despite this significant concession, however, we cannot obtain a scientific law through the application of rules and procedures intended as a mechanical activity. Indeed, as Polanyi states,

> There are many forms of mathematical series—such as power series, harmonic series, etc.—each of which can be used in an infinite variety of fashions to approximate the existing relationship between any given set of numerical data to any desired degree. Never yet has a definite rule been laid down by which any particular mathematical function can be recognized, among the infinite number of those offering themselves for choice, as the one which expresses a natural law (*SFS*, 7).

We can restate this in mathematical terms. Starting from a series of couples ($a_1$,$b_1$), ($a_2$,$b_2$),…,(an,bn), with ai ∈ A, bi ∈ B, we are looking for a rule or a procedure so that we can determine a single function connecting values of the set A to values of the set B. Yet there are many of these functions. For instance, if our couples were (0,0), (1,1), (2,2), (3,3), (4,4), (5,5), the function *f(x)=x* would be appropriate, but the function *g(x)=x-sin(πx)* would be suitable as well. Nor can we overcome this problem by adding new values. Indeed, although new observations, i.e., new values, could allow us to discern between *f(x)* and *g(x)*, because they imply different predictions in, let's say, x=1.5, many other functions would still be available. That is due to the fact that we are just adding new couples to our series. Now we have ($a_1$,$b_1$), ($a_2$,$b_2$),...,($a_n$,$b_n$), ($a_{n+1}$,$b_{n+1}$),...,($a_{n+m}$,$b_{n+m}$), but the situation is unaltered, because we have only extra *m* couples. An infinite number of functions are still available, and no rule (e.g., "make new observations and select the one which predicts rightly") allows us to select a particular one. Polanyi explains this point as follows:

> It is true that each of the infinite number of available functions will, in general, lead to a different prediction when applied to new observations, but this does not provide the requisite test for making a selection among them. If we pick out those which predict rightly, we still have an infinite number on our hands. The situation is in fact only changed by the addition of a few more data – namely, the "predicted" data—to those from which we had originally started. We are not brought appreciably nearer towards definitely selecting any particular function from the infinite number of those available (*SFS*, 7-8).

At first sight, Polanyi's argument seems to state simply the thesis of the underdetermination of theory by data. In a certain sense, this is true. Polanyi acknowledges that different mathematical functions, i.e., different scientific laws or theories, can account for the same empirical evidence. At the same time, however, Polanyi does not share the conventional anti-realistic and skeptical interpretations and implications of the thesis. When providing this argument, Polanyi doesn't mean to prove that scientific discovery is unachievable, or that scientific laws don't describe anything "real", or that they are only conventional accounts of reality. Instead, the argument aims to show that scientific discovery is not the result of the application of rules and procedures as the positivist account holds. Polanyi summarizes his point as follow: "I am not suggesting that it is impossible to find natural laws; but only that this is not done, and cannot be done, by applying some explicitly known operation to the given evidence of measurements" (*SFS*, 9). As we will show in the next section, according to Polanyi, scientific theories concern reality, and they are susceptible to genuine verification, too. However, there are no rules that underlie the process of verification.

*Rules for Verification*

Even though positivism is unable to provide explicit rules for discovery, the argument sketched above doesn't amount to a complete refutation of this conception. Indeed, making recourse to the distinction between the context of discovery and the context of justification, advocates of a positivist account of science could argue that the criticism misses the point. The generation of a new idea or hypothesis and the validation of that hypothesis are two different processes. While the former is not subject to an analysis in terms of rules and procedures, the latter is precisely the circumstances where rules come into play to answer questions of justification and validity. Thus, the rules cannot lead us to discovery but can guarantee that the outcomes of the process of discovery are sound and justifiable.

According to Polanyi, positivism is wrong even on this point. As far as rules of verification are concerned, Polanyi provides at least three arguments. The first one is just a consequence of what we said in the previous section. Indeed, having proved that scientific propositions are not strictly derivable from experience, *a fortiori*, further observations cannot give any conclusive confirmation about the validity of these propositions. Let us consider again, for example, the rule of successful prediction. Even though it is often conceived as an *experimentum crucis*, the fulfillment of a prediction does not lead automatically to the confirmation of a scientific proposition. This is due to the fact that adding new observations is not enough for determining which function holds between the measured values. New evidence can discriminate between competing functions involving different predictions, but an infinite number of functions would still be available (See Polanyi 1950, 28).

As a second argument, Polanyi provides a case study proving that correspondence with observations does not always imply the validity of a scientific proposition or theory. I will not go into detail about this argument, but will simply show the conclusions drawn by Polanyi. In this case, both the fulfillment of predictions and the reproducibility of experiments are concerned. According to Polanyi, even the most rigorous criteria of verification may be fulfilled, and nevertheless, they may lead to apparent confirmation of a false proposition in science. The case proposed by Polanyi (*SFS*, 78-79) confirms this conclusion. Moreover, a further example makes manifest that "our reliance on reproducibility suffers from a fundamental weakness. It is always conceivable that reproducibility depends on the presence of an unknown and uncontrollable factor which comes and goes in periods of months or years and may vary from one place to another" (*SFS*, 79). Different criteria of verification, such as the reproducibility of the experiments or the fulfillment of prediction, always leave room for conceivable doubts on the reliability of the results. Thus, the question about how we can validate a scientific proposition remains open because we do not know how to determine the reasonableness of these doubts through strict and explicit rules.

Polanyi's last argument is the most compelling one because it clearly shows that verification is not reducible to the application of explicit rules and fixed procedures. To prove this point, Polanyi proposes the following example:

> Suppose a player of roulette observes the numbers of reds and blacks that turned up in a hundred consecutive throws. He may plot them in a graph and derive a function in the light of which he will make a prediction. He may try it out and win. He may try it again and win. And win a third time. Would that prove his generalisation? No; in our view, it would only prove that some roulette players are very lucky—i.e., we would consider the fulfillment of his predictions as mere coincidences (Polanyi 1950, 28).

In this case, the player of roulette behaves as the scientist does according to the positivist point of view. She gathers observations, establishes correlations, and represents them in a mathematical form. Thus, starting from a series of couples (throw1, color1), (throw2, color2)…(thrown, colorn), he determines a function allowing him to predict the value of colorn+1 corresponding to the thrown+1. At this point, his law needs to pass some test to be confirmed. The results have to be reproducible; different methods have to lead to the same results; and, above all, predictions have to be fulfilled. Now, let us suppose that the law discovered by the player of roulette succeeds in meeting these criteria. Despite the evidence, we cannot conclude in favor of the validity of the law. We would prefer to say that the fulfillment of the predictions is only a coincidence, and the player is merely lucky.

Discussing the deeper reasons why we reach this conclusion is beyond the scope of this article. However, the main point is that this example allows us to show that the fulfillment of predictions in terms of observations is not in itself capable of validating a scientific statement. In general, any rule for verification cannot definitively validate a law or a theory. Consequently, given that there are valid scientific statements, we have to admit that verification is based on a different ground. Before displaying on what this process is grounded, we shall show that falsification as well is not a process consisting of the application of rules and procedures.

*Rules for Falsification*

Polanyi's considerations on verification are only a part of an alternative understanding of the relationship between theory and experience. The arguments above demonstrate that a scientific theory cannot be considered a mere ordering of experience, because observations alone, and a mathematical articulation of the measurements derived from them, can not lead to discovery. Nor, as argued in the previous paragraph, can experience alone validate a scientific statement. Now we can point out that the opposite holds too, namely that adverse observations don't lead automatically to the disposal of the theory. As Polanyi writes in "Scientific Beliefs," "the current positivist story that a scientist immediately drops a hypothesis the moment it conflicts with experience is a pure myth. No true scientist acts in this clumsy manner" (Polanyi 1950, 28).

Against this idea, Polanyi argues that, as in the case of verification, rules of falsification are not applied mechanically and thus always leave room for conceivable doubts about the reliability of the outcomes. In particular, Polanyi points out how some potential falsifications are explained away, for instance, through the addition of an *ad hoc* hypothesis or the reduction of the adverse observations to anomalies, i.e., an unsolved and partially unimportant problem of the theory.

Before going into details, consider a possible positivist objection to Polanyi's argument. An advocate of the positivist view of science could argue that even though it is true that scientists explain away potential falsifications in their daily practice, this is what they do but not what they should or ought to do. In other words, she could support the idea that any account of science, and in particular accounts of falsification, should be normative and not just descriptive. Thus, scientists explaining away anomalies behave incorrectly, because they do not follow the rule prescribing to reject a theory or a scientific statement each time these are refuted by experience. In this respect, the compliance with the rule according to which every time predictions fail the theory should be rejected is a sign of good science, in opposition to the bad practice of scientists explaining away potential refutation of their theory.

Without going into details about the alternative between a normative and a descriptive account of science, Polanyi's reply to this criticism aims to show that the process of explaining away adverse observations is not in and of itself an inappropriate practice in scientific research. Indeed, the positivist objection relies precisely on this assumption. As Polanyi states:

> The process of explaining away deviations is in fact quite indispensable to the daily routine of research. In my laboratory I find the laws of nature formally contradicted at every hour, but I explain this away by the assumption of experimental error. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discovery. Such things have often happened in the history of science. Yet I shall continue to explain away my odd results, for if every anomaly observed in my laboratory were taken at its face value, research would instantly degenerate into a wild-goose chase after imaginary fundamental novelties (*SFS*, 17).

Moreover, explaining away anomalies also has a much more valuable role than this. Indeed, when scientists dispose of contradictions to their theory by calling them anomalies, they pave the way to a further expansion of the theory itself. Indeed, if theories were abandoned each time they face adverse experiences, these theories could never advance. Scientists would behave in a clumsy manner if they did so. As Polanyi writes: "It is true enough that the scientist must be prepared to submit at any moment to the adverse verdict of observational evidence. But not blindly" (*SFS*, 17). A strict and mechanical application of the rules of falsification, such as the one that prescribes abandoning the theory when it does not fulfill the prediction, would paralyze scientific research. This is the reason why the abandonment of a theory cannot be decided exclusively by the application of some rules. As Polanyi summarizes this argument: "Scientists will often tolerate such contradictions to their theory, regarding them as anomalies which may be eliminated in the course of time by an amplification of the theory. Whether they should abandon a theory or not in any particular case can be determined by no fixed rule" (Polanyi 1950, 30).

## Rules as Rules of Art

Polanyi's argument definitively demonstrates that scientific processes such as discovery, verification, or falsification cannot be carried on through the application of fixed rules and explicit procedures. At the same time, given that they are actual processes commonly found in scientific practice, the other goal of this criticism is to show that there has to be a different ground, other than the explicit one of rules and procedure, on which discovery, verification, and falsification are based. It is my conviction that this critical debate with positivism that occurred around the forties is a crucial step in the development of Polanyi's thought. Indeed, it is exactly the reevaluation of the function of rules that allows Polanyi to develop the first insights about the "intuitive judgment" into the concept of "personal judgment".

Before closing this section, we need to make clear the meaning of Polanyi's conclusions on the function of rules. Indeed, we have to avoid a possible misunderstanding which could follow from the considerations presented so far, namely that Polanyi's criticism amounts to disposal of any kind of rules in favor of a complete intuition-based account of scientific discovery, verification and falsification. This is not the case. Even though these reflections foreshadow the preeminence of the tacit over the explicit, the goal is not to get rid of the explicit dimension as something pointless in scientific practice. On the contrary, Polanyi aims to show rules in a different light. Rules of scientific research are not unambiguous prescriptions to be followed mechanically, as rules for multiplication are, for instance (*SFS*, 44). On the contrary, they are rules of art, namely "vague rules embodied in the art of scientific research" (*SFS*, 44). This kind of rule is not subject to an unambiguous interpretation. As Polanyi states: "How can we ever interpret a rule? By another rule? There can be only a finite number of tiers of rules so that such a regression would soon be exhausted. Let us assume then that all existing rules were united into one single code. Such a code of rules could obviously not contain prescriptions for its own reinterpretation." (*SFS*, 44).[2] Thus, because they are not incapable of precise formulation, they leave room for personal and intuitive interpretation at each new application. If so, this also implies that "every process of reinterpretation introduces elements which are wholly novel" (*SFS*, 44), i.e., each new application and interpretation modifies the very meaning of the rules themselves.[3] It is exactly in this context that personal judgment comes into play. In order to use and apply the vague rules embodied in the art of scientific research, scientists have to resort, in various degrees, to their own personal judgment. As Polanyi concludes:

> We may conclude that just as there is no proof of a proposition in natural science which cannot conceivably turn out to be incomplete, so also there is no refutation which cannot conceivably turn out to have been unfounded. There is a residue of personal judgement required in deciding as the scientist eventually must what weight to attach to any particular set of evidence in regard to the validity of a particular proposition (*SFS*, 17).

## The Dilemma of the Modern Mind

The failure of the positivist account of science makes clear that the modern mind establishes science on the wrong basis. The dilemma of the modern mind, as I call it, lies in the contradiction between a "false theory" and a "right practice". Indeed, the modern mind expects science to be based on radical empiricism and skeptical doubt. If science were actually founded on these principles, any scientific discovery would not be possible. At the same time, however, the progress of science in the last three centuries clearly shows that scientific progress is possible. Thus, the practice of science is based on different premises than the one assumed by positivism.

Polanyi's reflection on the real foundation of science is beyond the scope of this article. However, it is worth noticing how this criticism of positivism disposes of both radical empiricism and the skeptical doubt employing the idea of personal judgment. Indeed, the twofold function of personal judgment is, on the one hand, to integrate observations in a coherent theory and, on the other hand, to establish the boundary between reasonable and unreasonable doubt. The first function reduces the role of the principle of empiricism because a scientific theory is no longer a matter of gathering indiscriminately new observations. Instead, the second function restrains the application of skeptical doubt. Indeed, a conceivable doubt is always possible. Positivist accounts of science miss exactly this point. In the struggle against any authority, they adopt a kind of doubtful attitude toward any a-critically accepted statement. At the same time, however, they try to avoid the skeptical consequences of this approach by relying on empiricism, namely on observations organized through explicit rules. This line of thought aims to establish the truth of some statements beyond any conceivable doubt. Nevertheless, Polanyi's criticism shows that this position is untenable because of the very nature of the rules. Indeed, since each rule needs to be interpreted before its application, rules alone can never determine by themselves their own application, because every application can be made out to accord with the rule employing a specific interpretation. That is the reason why doubt is always conceivable. Since it is up to the scientist to decide if doubt is reasonable on any occasion, it is only personal judgment—not rules and procedures—which allows us to explain how these processes are really fulfilled in the scientific practice.

## Conclusion

Polanyi's criticism of positivism paves the way for his constructive postcritical philosophy. Indeed, the rejection of the objectivist tendency of positivism doesn't culminate in an untenable subjectivism. On the contrary, Polanyi's proposal aims to overcome the duality between objective and subjective. Indeed, scientific knowledge is neither absolutely valid nor is it relative to the subject. Instead, it is a *belief* we are *committed to* and that we retain with *universal intent*. In this respect, science has a *fiduciary* foundation. Although Polanyi further develops these concepts afterward, his reflection during the forties is a crucial step in their development. In conclusion, I propose that the following quotation shows the intricacies between the criticism of positivism and its dangerous consequences, the reflection on personal judgment and the constructive project of a post-critical philosophy:

> We can now discern the fundamental fallacy of the positivist model of science. It tries to construct a machine which will produce universally valid results. But universal validity is a conception which does not apply outside the commitment situation. Any reference to it is merely a manner of expressing our submission to an ultimate obligation and can appear only as part of a fiducial declaration. The attempt to construct something universally valid, prior to any belief, is logically nonsensical. Science can never be more than an affirmation of certain things we believe in. These beliefs must be adopted responsibly, with due consideration of the evidence and with a view to universal validity. But eventually they are ultimate commitments, issued under the seal of our personal judgment. At some point we shall find ourselves with no other answer to queries than to say, "because I believe so." That is what no set of rules, or any model of science based on a system of rules, can do; it cannot say, "because I believe so". Only a person can believe something, and only I myself can hold my own beliefs. For the holding of these I must bear the ultimate responsibility; it is futile, and I think also ignoble, to hunt for systems and machines which will take that burden from me. And we, as a community, must also face the fact that there is no system of necessary rules which will relieve us from the responsibility of holding the constitutive beliefs of our group or of teaching them to the next generation and defending their continued profession against those who would suppress them (Polanyi 1950, 34-35).

## ENDNOTES

[1]Editor's note: The article was published in the last issue of *Tradition and Discovery*. See the References for full bibliographic information.

[2]This is a straightforward formulation of a part of the Wittgensteinian "rule-following paradox", namely the regress of interpretation.

[3]The understanding of the rules for discovery, verification, and falsification as rules of art has further implications for the question "what is science?" First of all, it implies that science is a kind of art. Being an art, its rules "only can be transmitted only by teaching the practice which embodies them" (*SFS*, 44). But this kind of transmission is typical of traditions, such as artistic or workshop traditions. In this respect, thus, science itself is a tradition. In particular, given the constant process of reinterpretation

of its rules, science is a tradition that constantly renews itself at every stage of transmission. This subject, and the theory of tradition that it implies, however, is beyond the scope of this article.

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