



## How The Non-Existence of a Scientific Method Ought to Affect Ex-Post Research Evaluation

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### Abstract

*In this article, it is argued that, given the lack of a method scientists may have used to minimize their failures, a strategy applying strict criteria for ex-post evaluation of research, by rigid count of their outputs, will stifle rather than promote creativity and progress of rational knowledge. As a result, a somewhat ineffective science policy would ensue.*

### Introduction: the fruitless search for a <<science method>>

History of science is littered with tentative results and unsuccessful efforts. Many an attempt made by epistemologists at identifying a straightforward, universal pattern in the search for new, original rational knowledge have proven hardly any fruitful.

Popper proposed that science proceeds along with elimination of unfit hypotheses, which would lead scholars closer and closer to truth - he in fact referred to gradual verification of “falsifying hypotheses” rather than a naïve concept of one-off falsification of theories; also, he was well conscious that an ultimate truth may never be reached and – according to the lesson by Xenophanes – we could be unaware of having come to such truth even if we luckily had [1].

But Lakatos contended that, actually, the refutation of a prediction does not always bring about the immediate rejection of the correspondent theory: such an explanation might remain in use as long as a new one, providing more accurate predictions, becomes available [2]. This suggestion provides a reasonable account for the stand-off of theoretical physics during the second half of the nineteenth century.

Kuhn, on turn, proposed that the most important progresses in rational knowledge might be interpreted as changes of “scientific paradigms”, whose revolutions take place whenever anomalies can no longer be justified without a radical shift in the structure of world understanding [3]. His interpretation correctly justifies even puzzling, reversible mutations in the approaches to the interpretation of

universe, such as the double switch in ancient astronomy from Aristarchus’ heliocentric system to Ptolemy’ geocentric and back again in the Renaissance to the heliocentric, Copernican one – but according to this interpretation, some changes in scientific paradigms may even follow swaps in vogues, which seems inconsistent with an indisputably rational description.

A vigorous, thoughtful endeavor to clarify science’s aims and their role in establishing a possible set of cognitive rules which may underpin the progress of rational knowledge has been made by Laudan [4], but he points out that such an attempt looks virtually hopeless considering its history. A view driven by commonsense and deep methodological analyses of many great epistemology scholars’ works appears in the end to be the one set forth by Feyerabend [5], who simply denies any <<Method>> of science might ever be built up or found out.

Summing up, the lack of a standardized procedure to maximize success while performing scientific research brings about an idiosyncratic, inevitable uncertainty of R&D outcomes, which tends to show up ex-ante regardless of scientists’ abilities and efforts. Indeed, just by singling out the sprinkling of few lucky, rewarding research projects within the vast amount of unavoidably failed attempts that mess up the advance of theoretical and empirical knowledge it is often impossible to tell apart the many brilliant and hard-working scientists from the few mediocre and lazy ones.

This consideration entails an inescapable, but little noticed, consequence for science policy: the need for a very conservative use of ex-post penalties to <<punish>> researchers for their

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supposed failures, since most of these are not failures, just inevitable steps within a normal path in the accumulation of new, original, rational knowledge.

### **Conclusions: discoveries without publications**

Both the whole process and every single step of the path leading to scientific progress ought to be labelled as discoveries. This is true within hard sciences, in abstract terms when a theory is devised and discussed and in empirical ones whereas experiments are designed and performed. Such representation should also be correct for social science, regarding the ideation of and debate on models and their tests. Even within humanities, in a sense, the birth of interpretations which replace current narratives can be described as a consequence of the discovering previously unknown accounts.

On the other hand, given that science aims at gaining reproducible results, doing research could be identified as making efforts concerning reproducible discoveries by putting forth new, original theoretical and empirical knowledge or criticizing from within the consistence of already accepted rational knowledge. Many, not to say most, of these efforts inevitably will not result into publications.

The critique, in particular, might not reach the publication or quotation stage in those disciplines where a strong bias exists against criticism of knowledge proposed by entrenched scholars. Shall we perhaps say the efforts not acknowledged by individual publications do not deserve to be considered as genuine scientific endeavors? If so, a large part of meaningful research activities would possibly go unnoticed. This is precisely the risk inherent in a carpet application of research evaluation only based on harsh, uncompromising count of publications and quotations.

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