Worrall's Rule and a Critique of Standard Empiricism[1]

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Abstract: *'Standard empiricism' is a name that Nicholas Maxwell has given to the methodology which insists that in science no substantial thesis about the world can be accepted as a permanent part of scientific knowledge independent of evidence and certainly not in violation of the evidence. Maxwell suggests that standard empiricism is a current, official, orthodox conception of science and it is very widely upheld. He also argues that standard empiricism has some fundamental deficiencies and it is untenable. On the other hand, Nicholas Maxwell admits that standard empiricism is rather immune to his criticism as it has a strong defensive mechanism built in it, the mechanism which does not allow any metaphysical discussion into science.* (Maxwell, 1998)

However, there have been studies that allow us to believe that standard empiricism itself is not consistent with the norms it states. In other words, anthropological, sociological and historical empirical studies show that it is very hard to find such an "ideal" science. The aim of my article was to explore implications of this belief. I tried to assess how strong this 'empirical' argument is and whether it is applicable to standard empiricism at all. The tool I used was John Worrall's "rule" which says, "Other things being equal, working scientists have accepted the theory A as a better theory than B if, and only if, A was better than B; moreover, we can tell whether A was better than B by applying the criterion of scientific merit supplied by the methodology M" (Worrall, 1976).

[1] This article proceeds from my master's thesis *Possibilities of Critique of Nicholas Maxwell's Concept 'Standard Empiricism' in Comparison With Aim-Oriented Empiricism – Metaphysical and Empirical Arguments*, which was supervised by professor Rein Vihalemm and docent Endla Lõhkivi and was defended at the University of Tartu in June 2010.

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Introduction

One of the distinction lines used in the mapping of the twentieth-century philosophy of science is a position based on which different authors describe their relationship between methodology and practice of science. By and large the views may be divided into two opposite groups. The first of them favors separating methodology and practice of science completely – that is, normatively prescribing rules to which argumentation should correspond to be called scientific. According to the second approach, practice of science is preferred as the basis for characterization of successful scientific work in epistemology, the normative account is supposed to emanate from what is actually going on in scientific communities, institutes, and so on.[2]

Any particular theory of science (in the sense of metatheory) can position itself between those opposite views in different distances. Most such theories of scientific method have both sides represented, the only question is to what degree.

Approaches where the normative aspect is strongly present can be analyzed, valued and criticized with philosophical tools by showing the strengths and weaknesses of norms under question and inferences following from these norms. Approaches with the main focus on what actually is happening in the scientific practice can be criticized by describing (empirically) practices which collide with the specific account of science. (This kind of approaches risk falling into relativism of only locally validated claims in which case it is actually not criticizable at all, as critique is in some ways universal.)

Therefore it seems that a normative methodology has certain likable traits; a normative methodology can theoretically give something to science, for example, pointers for negative or positive heuristic if we are looking to the future. If we are concerned with the past, it can give us some other kind of understanding of science and knowledge which attempts at explaining what the reasons for success of scientific enterprise are.

[2] Positivists are probably the most famous representatives of normative epistemology and the Strong Programme of SSK with a relativist sociology of science is on the other end of this scale.

One of the most important features of a normative methodology is that it is possible to criticize it using philosophical arguments; at least we are entitled to suppose that. But this is not always the case. In my opinion there is a plausible situation of norms being fixed in a way which makes it impossible to criticize them. I agree with Nicholas Maxwell's point of view that the current official and orthodox conception of science is what he calls standard empiricism. Standard empiricism is a methodology which insists that in science no substantial thesis about the world can be accepted as a permanent part of scientific knowledge independent of evidence and certainly not in violation of the evidence (Maxwell, 1998, p. 37).[3]

Nicholas Maxwell (1998) has presented solid arguments against such kind of conception of science and has proposed as an alternative an aim-oriented empiricism, an approach free of problems of standard empiricism.[4]

If history of science has allegedly shown the success of science, then history of philosophy of science has shown its own weakness. This is exactly what philosophy of science has frequently been accused of. My question is: if Nicholas Maxwell's diagnosis of science is adequate and scientists actually accept on declarative level only claims that can be empirically tested, which means any philosophical critique to standard empiricism has no power at all, then what can be done to evaluate this methodology (so that it could have any influence on scientific practice)?

If the empirical evidence is primary, then it should at least be possible to criticize scientific norms in the cases where discrepancy between declarative conception and actual scientific practice is increasing and to do so by referring to empirical (anthropological, historical, sociological) studies.

In principle, this 'empirical argument' is borrowed from non-normative approaches to science. I am aware of this problem, therefore I am not claiming that 'empirical argument' could in any way replace traditional philosophical analysis. My aim is much less ambitious. I would like to suggest one additional possibility of critique and investigate under which conditions this possibility can be used. The latter is my main goal as empirical arguments are used anyway.

[3] Nicholas Maxwell distinguishes between two different types of standard empiricism: bare and dressed. Bare standard empiricism is what I have cited here (and at which the following 'empirical argument' is targeted); dressed standard empiricism allows simplicity considerations in addition to empirical consideration to determine the choice of theory of science. (Maxwell, 1998, p. 37)

[4] These problems are the problems of induction, the problems of simplicity, the problems of evidence, and the problems of scientific progress (Maxwell, 1998, p. 45).

To be more precise, if we simply have a norm "only empirical evidence can determine what should be considered as part of scientific knowledge and what not", then under what conditions can we say that "historical or sociological studies have shown that actual practice of science does not correspond to that norm"? Hereinafter I am going to use this claim also in the wording "standard empiricism does not correspond to its own norms".

For this purpose, I will provide an overview of the classic gap between a description and a norm, and try to overcome it using a tool that Rein Vihalemm (1981) calls "Worrall's rule".

I will conclude that for criticizing standard empiricism, Worrall's rule has quite a limited power as standard empiricism will most probably be confirmed by the rule and also this rule combined with standard empiricism may not escape the circularity problem.

Hume's guillotine

If there is a need to attack a norm with a description, the first and most obvious obstacle to overcome was formulated already in the 18th century. Nowadays it is called either Hume's law or Hume's guillotine. This law expresses that normative claims cannot be derived from descriptive claims. Here it points to the fact that it is impossible to say that standard empiricism is a weak theory of method by relying on the evidence found to the claim that actual science does not meet these requirements. But is this gap impassable?

Hume's law was written as a small comment at the end of a chapter[5] dealing solely with moral issues. This part of the volume tells about virtue and vice in general and Hume's argument as to what leads to his so-called law consists of a conviction that moral distinctions are not derived from reason.

Hume thinks that it is commonly presupposed that reason has no power to influence our actions and affections. The effect of moral decisions on our acts, on the other hand, is very direct. Reason is passive in relation to actions and moral decision is active. The basis of an active principle can by no means be an inactive principle. And if a principle is inactive it stays so irrespectively to what it has been applied: moral sphere or the natural world. (Hume, 2003, p. 294)

[5] Book 3. *Of Morals*; Part 1 'Of virtue and vice in general'. Sect. 1. 'Moral distinctions not deriv'd from reason', see Hume, 2003, pp. 293–302.

Reason is utterly inert and cannot incite or impede any action or passion, because reason is discovering truth or falsehood. Truth or falsity is a correlation or disagreement with the actual relation of ideas or with matter of facts. Thus everything to which such correlation or disagreement cannot be applied, is not true or false nor is it object of our reason. Hume believes it to be obvious that such correlation or disagreement is not applicable to our passions, wishes or actions, as they are primary facts and realities, complete in themselves and do not hold any relation to other passions, wishes or actions. Hence it is not possible to consider them either true or false and, accordingly, consistent or inconsistent with reason. (Hume, 2003, p. 295)

If morality proceeds from truth or falsity, then the source of immorality would be a factual error and there would not be different levels of moral assessment (more immoral, less immoral) because morality can then only be concordant with or contradictory to reason. (Hume, 2003, p. 296)

At the end of this chapter there is a passage referred to when speaking about Hume's law or Hume's guillotine. This passage expresses a disputable thought that the transition from 'is'-claims to 'ought'-claims is not right.

> *I cannot forbear adding to these reasonings an observation, which may, perhaps, be found of some importance. In every system of morality, which I have hitherto met with, I have always remark'd, that the author proceeds for some time in the ordinary way of reasoning, and establishes the being of a God, or makes observations concerning human affairs; when of a sudden I am supriz'd to find, that instead of the usual copulation of propositions, is, and is not, I meet with no proposition that is not connected with an ought, or an ought not. This change is imperceptible; but is, however, of the last consequence. For as this ought, or ought not, expresses some new relation or affirmation, 'tis necessary that it shou'd be observ'd and explain'd; and at the same time that a reason should be given, for what seems altogether inconceivable, how this new relation can be a deduction from others, which are entirely different from it. But as authors commonly do not use this precaution, I shall presume to recommend it to the reader; and I am perswaded, that this small attention wou'd subvert all the vulgar systems of morality, and let us see, that the distinction of vice and virtue is not founded merely on the relations of objects, nor is perceiv'd by reason.* (Hume, 2003, p. 301)

Hume's law has been disputed in moral philosophy[6] as well as concerning any normative propositions. Here I would like to point out two possible solutions to overcome this gap.

Jonathan Harrisson finds in his book *Hume's Moral Epistemology* that it is even logically impossible to accept certain factual claims and to deny normative conclusions arising from these. For him it is not an accidental double meaning of the words 'right' and 'wrong' – on the one hand these terms are the main concepts of the moral realm, but on the other hand these words also mean the 'right' or 'wrong' means for some specific purpose. And the latter is logically derivable from the actual matters of fact. It would be utterly irrational to choose a 'wrong' way to Rome if the goal is to arrive in Rome, or to give purposely incorrect answer to the question about the time. (Harrisson, 1976, pp. 74–76)

Would this approach help us to criticize standard empiricism? Unfortunately not, because in order to criticize methodology of science with this kind of instrumentalist account we need to know what is the aim of the science as whole. If we know the overall aim, we could assess the means of achieving it, that is, to assess whether the methodological norms will help to reach that aim.

As there is no agreement about such general aim, it is possible to "borrow" the purpose from the methodology under question, that is, from standard empiricism. The aim of science according to this methodology is to gain as much trustworthy (empirically proved) knowledge as possible. For that, on the other hand, some kind of general (independent from empirical evidence) presupposition about the world is needed to be able to infer from it a question whether standard empiricism helps us to gain trustworthy empirical data. Standard empiricism in principle forbids this kind of presuppositions.

Another possibility to overcome the gap of Hume's law is the suggestion Tõnis Idarand makes in his article 'Põhjendamise probleem ja empirism väärtusfilosoofias' (The problem of grounding and empiricism in axiology, Idarand, 1993) where he tries to find an answer to the question: is it possible to admit applicability of deductive-logical justification model in grounding of value-claims and at the same time agree with Hume's law? Finding the answer to that question rises from the fact that the moral discussion actually uses logic. Idarand suggests contextualism as one possibility. Contextualism solves the problem by accepting among assumptions also value claims in addition to factual claims. The assumption is taken as an axiom and is not substantiated in

[6] See, e.g., Searle, 1964.

itself which ensures logical transition from presumptions to value-inferences. (Idarand, 1993, p. 67)

Hence to make a connection between descriptive and prescriptive or to say that the argument "standard empiricism does not correspond to its own norms" is correct, an additional assumption is needed which would build the bridge across the gulf. John Worrall proposes exactly such kind of additional assumption for assessing methodologies.

Would Worrall's rule help to bridge the gap?

John Worrall (1976) presents in his article "Thomas Young and the 'refutation' of Newtonian optics: a case study in the interaction of philosophy of science and history of science" one way to overcome the gulf of normative and descriptive claims. He thinks that the method which "does the job" lies in adding to assumptions a sentence according to which practicing scientists confirm with their choice of theory (rather with estimation of the rationality of this theory) the value-estimation prescribed by a methodology.

Description of Worrall's rule

John Worrall tries to show that methodology can be presented in a historically testable way. He wants to convince readers that although historiography is by large driven by normative thoughts, it does not mean that all history claims are normative or that testing philosophy against history of science is circular. Worrall believes that normative methodology can improve history of science.

The rule under question is as follows:

* Other things being equal, working scientists have accepted theory A being better than theory B if, and only if, A was better than B; moreover, we can tell whether A was better than B by applying the criterion of scientific merit supplied by the methodology M.

In a footnote on page 167 Worrall gives a formal explanation to his "rule".

He uses the marking 'CP' to express presumption that other circumstances are equal, '$A >_M B$' to express a statement that A is better than B according to methodology M, and 'P(A,B)' to express a claim that A was historically preferred to B. Worrall states that we should regard every methodology as confirming that

$(*_M)$ $CP \rightarrow (A >_M B \leftrightarrow P(A,B))$.

The first direct confirmation of methodology M is that if the 'initial condition' that $A >_M B$ is entered to the $*_M$ rule and *ceteris paribus* is presumed, then $*_M$ implies P(A,B), a claim which is confirmable by historical investigation.

$*_M$ implies that $(*'_M)$ $(A >_M B \wedge \neg P(A,B) \rightarrow \neg CP$

From that arises the second confirmation for M. Initial conditions inserted to $*'_M$ are '$A >_M B$' and ¬P(A,B) lead to ¬CP. This is another claim which is independently and historically testable.

If it turns out that all historical evidence refers to the absence of any disturbing facts during rivalry of A and B, then ¬¬CP should be considered historically confirmed and therefore $(*'_M)$ and $*_M$ and together with that methodology M are refuted. (Worrall, 1976, p. 167)

John Worrall has three claims

1) Adding this rule to methodology enables us to test methodology against the history of science.

Methodology should give us general criteria which would enable us to assess scientific theories, that is, which theory is better than the other, or to create a ranking list of theories. This assessment, on the other hand, has some implications to the decisions of scientists' decisions and actions. Worrall claims that methodology determines the scientists' acceptance of a given ranking; however, it does not mean that a scientist has to work with the theory methodology has estimated as the best, because by developing background ideas of a theory which has been recognized as less rational, a scientist may attain an excellent theory by the merits of the same methodology. (Worrall, 1976, pp. 161–163)

For the association (methodology + *) to be testable against history, that is, having descriptive implications, an additional condition is needed – one which says that every attempt that tries to save a methodology by referring to the external factors has to be done in a way that it is specific and testable against the historiography of science. An articulated conclusion has to be brought out as to why the conditions were not equal for the theories. (Worrall, 1976, p. 165)

2) Although there are no purely descriptive historical facts (or claims), it does not mean that methodology cannot be testable against history of science nor that Worrall's rule is circular (Worrall, 1976, pp. 168–169).

3) Normative methodology can be helpful for the historiography of science.

Although the concepts historians use are provided by methodology, it does not mean that all history of science is "normatively interpreted" or "soaked in methodology", it could yet be influenced and improved by explicit application of methodology (Worrall, 1976, p. 172).

Usage of specific methodology can provide historians with proper terminology for expressing historical facts in a lucid and concise manner. For example, methodology of research programs is a further development of such theory of method that describes science solely by changing theories. It is very difficult to find one corpuscular theory or wave theory of light; instead, there are corpuscular theories and wave theories. (Worrall, 1976, p. 172)

Another way how methodology can help historiography is by widening the field of sight. Methodology provides researchers with a heuristic – not only with a set of problems but also with means to solve these questions. (Worrall 1976, p. 173)

The problems, limitations and potential applications of Worrall's rule

The first limitation in the application of Worrall's rule is the fact that it is obviously impossible to test any relativistic methodology. Why? Because relativistic methodologies cannot be testable in principle.

Barry Gower (1997) emphasizes one not very widely recognized point that if we accept the connection of method and relativism we do not have the ability to support our claim with evidence. Relativism says that if there is no right way to argue and justify the decisions made in science, then we can justify principles only by referring to local beliefs, habits and needs. Rational is what we consider rational. However, it seems to be obvious that the critique of methodology in the light of practice of science can work only if the practice is successful. But relativistic methodology does not provide us with tools to recognize the success or, to be more precise, to differentiate between actual success and apparent success. (Gower, 1997, pp. 247–248)

Secondly, the historical contexts which allow us to use Worrall's rule are limited to periods where there are at least two (in principle there could be more) clearly distinguishable rival theories between which scientists have to choose. Hence in a period of Kuhnian normal science when scientists are working with one approved theory the Worrall's rule cannot be applied. This is why it is possible to imagine that a scientist can be mistaken over a long period of time. Because of the deficiencies of ruling, theories are not evident enough to make the scientists choose between them.

The third and most important limitation for the current topic is that it is clear that the rule under question connects norm and description but does not provide a shortcut to compare adequacy of two different methodologies. We can assess methodologies one by one by placing them into historical context. This limitation, as we soon will see, plays a very important role in relation to standard empiricism and its critique.

Standard empiricism and Worrall's rule

Worrall's rule is structured in a way that with its help it is possible to falsify methodology. If the falsification fails, then the methodology is confirmed. Shortly put, this rule is a yes–no system, and (using Hume's vocabulary) a normative claim is either in correlation with a fact or not, and it is impossible to find out which methodology is more or less coherent with actual history of science compared with other methodology. It is very plausible that more than one methodology can be confirmed by this rule.

Likewise it is very much possible that standard empiricism itself will be confirmed by the Worrall's rule. The main reason why I think so is that standard empiricism is a very "indulgent" theory: it only calls for empirical adequacy. These accounts of science which have other requisites besides empirical adequacy are very likely to be more falsifiable.

An even bigger problem lies with a threat that, in my opinion, in terms of standard empiricism the circularity problem is hard to avoid. The following will explain why I think so.

As already briefly mentioned, Worrall denies that the circularity problem is something that we should be worried about when we talk about his so-called rule, although some philosophers, Lakatos and Agassi among them, have claimed that historiography is influenced by methodological considerations. This has lead to the conclusion that testing methodology against history of science is circular and therefore damaged because historical case studies which have been chosen to test methodology, probably confirm the methodology. (Worrall, 1976, pp. 168–169)

Berry Gower has expressed a similar opinion. He affirms that with inventive and perhaps impudent mind any practice can be reconciled with any principle. Take, for example, falsificationism. If one wishes to refute falsificationalism – every genuine test of a theory is a test of falsification of this theory – with the claim that it does not correspond to reality, one will find themselves in a deadlock, and has to

conclude that history of science and this thesis meet very well, because first of all one has to determine which is the *genuine* test of a theory. (Gower, 1997, p. 247)

Worrall asserts that his rule can become circular for methodology only if one decides who is a scientist and who is not relying on the same methodology one wants to test. But it is also possible to decide on the basis of general opinion. Let us assume that (a) general opinion is shaped, influenced by and containing not only descriptive propositions but a mixture of descriptive and normative claims and (b) the set of persons whom general opinion considers scientists is vague. Accepting (a) affects Worrall's rule only if those normative considerations are systematically taken from the same methodology which is being tested. And according to Worrall this is not the case. As far as (b) is concerned, we can be liberal in involving the set of scientists, if we also allow external factors that could explain discrepancies between their actions and what is advised by methodology, factors like the lack of intelligence and mathematical ability. (Worrall, 1976, p. 165)

So according to Worrall the aforementioned general opinion is an escape way from the circularity problem. But is this applicable also when standard empiricism is concerned?

I think not. Because if we agree with Nicholas Maxwell's claim that standard empiricism is an officially accepted methodology of science to which the cause of the success of science is attributed, then it is more than probable that science is defined by the ideology of standard empiricism and the majority of history of science is written according to it. So it is inevitable that circularity that Worrall wants to avoid sneaks in. Standard empiricism will be tested against the history of science which defines the concepts of 'science' and 'scientist' by the rules of standard empiricism. It would be avoidable if general opinion through which history 'is given' to us actually contained material originating from very different methodological assumptions and is written during very different periods of time.

The objectivity (in the sense of avoiding circularity) would be better ensured with application of Rein Vihalemm's 'historiographical apparatus', which is, in a way, a further development of Worrall's general opinion. According to Vihalemm, only such assembled historian–methodologist–scientist's opinion can be used as historiographical apparatus that contains in itself accounts of different people who all have knowledge about science, but who have very different methodological assumptions as their history has been written during different periods of time and different development stages of science. (Vihalemm, 1981, p. 13)

Possible applications of Worrall's rule

While the mechanical testing of standard empiricism with Worrall's rule will very likely result in it being confirmed, then only way to use this rule for critique is using it in comparison with some other kind of methodology which is also confirmed by Worrall's rule. But as we previously saw, the toolbox of Worrall's rule does not contain direct comparison. So should it be concluded that Worrall's rule is completely useless as it probably does not refute standard empiricism directly (as standard empiricism is a very "indulgent" theory) and at the same time does not provide ways to compare methodologies which have been confirmed by Worrall's rule? In my opinion it is not utterly useless.

One advantage of methodologies with uttered normative component over purely descriptive is that they give another dimension to the explanation of science. They help us to understand science in the sense of success. The norms of standard empiricism do not contribute to understanding. The problem of standard empiricism is basically the same as the problem of positivism – an absolute lack of logic of discovery. Standard empiricism with its indulgency also has little to give. So if the aim is to preserve the advantages of normative methodologies, every methodology that provides some criteria for assessing empirical facts besides just collecting them should be preferable. This, of course, applies in case this other is confirmed by Worrall's rule.

John Worrall had a third argument for justifying his rule, namely a claim that methodology of science is helpful for historiography of science. If we turn our focus to this argument and emphasize that testing methodology is at the same time testing historical understanding and historiography, the statement that one methodology explains more than the other becomes an important difference. And here it is not even essential whether we talk about history of science in a sense of intrinsic rationality or history in general, because according to Worrall's rule the methodology can be confirmed by history also by referring to the external factors.

The main problem of standard empiricism is that after finding out the empirical success of a theory, a historian of science is left in a pool of loosely connected facts, with no clue about their relevance. In other words – looking at the history while armed with standard empiricism a big part of actual practice of science becomes mystical.

Conclusion

The aim of the present article was to determine which conditions are relevant for being able to criticize standard empiricism with the 'empirical argument'. How strong is the argument that standard empiricism does not correspond to the norms it states, that is there is no such ideal science as standard empiricism suggests?

I found that there are possibilities (tools) to overcome Hume's classic is–ought problem by adding a normative sentence as an assumption to the empirical description of science (in this case, to history of science). It could be, for example, an increase in the role of decisions that practicing scientists make. Or to claim that practicing scientists with their assessments of a theory give a value assessment which either confirms or refutes methodology. John Worrall provides us with precisely this kind of tool.

The second part of the article was dedicated to the question whether Worrall's rule is powerful enough to open up standard empiricism to the critique, and concluded that unfortunately its application to standard empiricism is limited. First of all, general opinion does not inevitably solve circularity problem and second, standard empiricism is so indulgent conception that it will most probably be confirmed if tested against history of science.

But this does not, however, mean that Worrall's rule is utterly useless for that task. Namely the very same property that makes standard empiricism so strong against any critique also makes it idle for understanding science. Any methodology which, similarly to standard empiricism, is confirmed by Worrall's rule, but which entails more than just a requirement of empirical adequacy, that is, which besides the process of retrospective justification also explains how the result is reached and why it is decided to take certain direction of research, should be preferable also with respect to Worrall's rule, because its wider aim (and implicit need) is to assess historiography itself, to give pointers towards adequately operating historiography. Hence the methodology allowing that is certainly better, but this is precisely the limit where the possibilities of Worrall's rule in criticizing standard empiricism end.

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