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Contents

Methodological Objectivism and Critical Rationalist 'Induction'

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This paper constitutes one extended argument, which touches on various topics of Critical Rationalism as it was initiated by Karl Popper and further developed (although into different directions) in his aftermath. The result of the argument will be that critical rationalism either offers *no solution to the problem of induction at all*, or that it amounts, in the last resort, to a kind of *Critical Rationalist Inductivism* as it were, a version of what I call *Good Old Induction*. One may think of David Miller as a contemporary representative of what I consider as the 'no solution' version of critical rationalism, while Alan Musgrave stands for the version of 'critical rationalist induction'. Popper's own writings admit of either interpretation.[1]

1 Objectivism

Popper states as one of his[2]

(P$_1$) ...principal methods of approach, whenever *logical* problems are at stake ...to translate all the subjective or psychological terms, especially 'belief', etc., into *objective* terms. Thus, instead of speaking of a 'belief', I speak, say, of a 'statement' or of an 'explanatory theory'; and instead of an 'impression', I speak of an 'observation statement' or of a 'test statement'; and instead of the 'justification of a belief', I speak of 'justification of the claim that a theory is true', etc.

Even though it is not difficult to recognize what Popper is driving at with this meta-philosophical principle, it is not difficult either to see that he simply does not get it right: why should, for instance, a '*claim* that a theory is true' be less 'subjective' than a '*belief*' that a theory is true? Does a *claim* not presuppose a *claiming subject*, just as a *belief* presupposes a *believing subject*? Concerning these questions it is not of much help either that Popper distinguishes[3]

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[1]This depends on whether we look at his position up to the mid-1950s, which is covertly inductivistic (notwithstanding his clamours to the contrary), or whether we think of his later views, which belong to the 'no solution' kind (notwithstanding ...).

[2]Popper (1972), Chapter 1, § 4.

[3]Ibidem, Chapter 3, § 1.

(P$_2$) ... two different senses of knowledge or of thought: (1) *knowledge or thought in the subjective sense*, consisting of a state of mind or of consciousness or a disposition to behave or to react, and (2) *knowledge or thought in an objective sense*, consisting of problems, theories, and arguments as such. Knowledge in this objective sense is totally independent of anybody's claim to know; it is also independent of anybody's belief, or disposition to assent; or to assert, or to act. Knowledge in the objective sense is *knowledge without a knower*: it is *knowledge without a knowing subject*.

The unquestionable part of this distinction is that it differentiates between *attitudes* of persons towards statements (or, as I would prefer, towards propositions) on the one hand, and the *statements* (propositions) *themselves* on the other. Either is sometimes referred to as 'knowledge'. But the same goes for 'claims', 'assertions', 'beliefs', and so on, because *all* such terms are ambiguous in this respect. They sometimes refer to *propositional attitudes* (which Popper deems 'subjective' or 'psychological') and sometimes to the *contents* of such attitudes, that is, *statements* or *propositions* (which Popper calls 'objective'). The distinction is one between *knowing* and *the known*, *claiming* and *the claimed*, *asserting* and *the asserted*, *accepting* and *the accepted*, *believing* and *the believed*, and many more of these. On the one side there are *subject-related*, that is, *pragmatic*,[4] notions involving *persons* who know, claim, assert, accept, or believe certain propositions, while on the other side there are *objective*, that is, *semantic*, notions involving just *propositions* and, as we shall see, their truth values, their truth conditions, and such objective properties as can, in essence, be explicated in terms of truth conditions. All this has been made entirely clear by Carnap as early as 1950.[5] Thus, it is not a question of the *terms* being 'subjective' or 'objective' (cp. P$_1$), but a question of their *meanings* which may be ascertained from the respective occasions on which these terms are used.

That it is important to distinguish between the objective semantic and the subject-related pragmatic meanings of such terms can be demonstrated by the following simple consideration: It is *logically possible* that there exists some person X who believes (or claims seriously the truth of) both h and $\neg h$ at the same time[6] (or, at least, that X believes inconsistent propositions). But it is *logically impossible* that both h and $\neg h$ are true, that is, that such *contents* of X's beliefs or claims — what *is* believ*ed* or *is* claim*ed* by X — can 'occur', as it were, at the same time.

This example should also illustrate that the distinction between the semantics and the pragmatics of the involved terms carries over to the *attributes* going with them: restricting ourselves to purely objective logical and/or sem-

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[4]For the distinction of pragmatics, semantics, and syntax as parts of semiotic, cp. Carnap (1948), pp. 8ff.

[5]Cp. Carnap (1950), especially §§ 11f.

[6]This is so at least as long as we do not *define* the notions of believing and of seriously claiming in such a way that only consistent beliefs are beliefs. This, however, would not be advisable, because most people hold at least *some* inconsistent beliefs.

antic considerations, we may delimit the *truth conditions* of propositions, characterize, say, a proposition q as being a *logical consequence* of some proposition p, or *contradicting* some other proposition v, or, if we have some appropriate *logical weight function*, we may say, for instance, that the logical probability (or the logical support, or the corroboration, or any other weight) of p, given q, has some value r. For all these attributions we don't need *any* reference to persons or attributes of persons at all.[7] They are *objective* in the sense that they obtain quite *independently of whether or not there exist any persons* who consider, believe, deny, or even like or detest the involved propositions. And all such attributions can be characterized in *purely logical and/or semantic terms* in virtue of their objective semantic properties. As long as we remain in the realm of semantic considerations, no reference to persons or properties of persons is needed or called for.

However, as soon as we introduce pragmatic considerations, we are dealing with entirely different matters. For instance: Given any two *propositions p* and *q*, where p is 'knowledge' or a 'belief' in Popper's *objective* sense, that is, is a known (or believed, or otherwise entertained) proposition, while q is *not* 'knowledge' or a 'belief' in the objective sense (is *not* a known or believed, or otherwise entertained, proposition) — which *purely semantic* characterization can be given in order to delimit one from the other *in this respect*? Obviously none. There is no other way than to move on to *pragmatic* considerations, involving persons and/or their attributes (such as, for instance, their propositional attitudes): the *known* (or *believed*) proposition is the one where there exists at least one person who knows (or believes) it, while there is no such person for the other proposition. As not every proposition is known or believed, we simply *cannot distinguish purely semantically* (that is, without reference to persons) such propositions which do constitute knowledge in Popper's objective sense, from those which do not. And this is also the case with all other notions of the discussed kind, such as 'belief', 'claim', and so on.

So, can we then pose the *problem of induction* and/or a possible *solution* of it in a purely objective manner? Popper proposes to restate Hume's logical problem '...in an objective or logical mode of speech'. Here is one of the many versions he gives of what he considers to constitute the *logical problem of induction*:[8]

(P$_3$) Can the claim that an explanatory theory is true or that it is false be justified by 'empirical reasons'; that is, can the assumption of the truth of test statements justify either the claim that a universal theory is true or the claim that it is false?

Note that Popper asks expressly (in a shortened version, taking only the second half of P$_3$ after 'that is') whether the *assumption* that some singular

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[7]Nor do we need any reference to 'science as an institution', in which Jarvie (2001) is interested.

[8]Popper, ibidem, Chapter 1, § 5.

propositions (test statements) are true can justify either the *claim* that some general proposition (universal theory) is true or the *claim* that it is false.

This, however, indicates a fatal confusion: depending on which way we understand the involved terms, we have either (a) an objective logical problem *but not the problem of induction*, or we have (b) a (subject-related, or 'subjective', as Popper calls it) version of the problem of induction *but not a logical problem*.

If we start with case (a) where we understand 'claim' and 'assumption' *objectively*, that is, as *propositions*, then we get:

(oP$_3$) Can the *proposition* that some singular propositions (test statements) are true *justify* either the *proposition* that some general proposition (universal theory) is true or the *proposition* that it is false?

As in this case the 'justifying' relation takes propositions as arguments and, thus, must be an objective semantic relation, it would be better not to speak (misleadingly) of justification, but, rather, of logical consequence, logical implication, or some other suitable and semantically explicable relation (such as logical probability, for instance). And indeed, even though it may sound somewhat exaggerated to call it a logical *problem*, this is, after all, a purely logical question to which there is a trivial answer: yes, true singular propositions can prompt the logical consequence (and, thus, 'justify' in some far-fetched sense) that some general proposition is false. Furthermore, it is also the case that no general proposition can be a valid consequence of (and, thus, be 'justified' by) a consistent set of singular propositions. However, I don't know of *any* relevant philosopher of the past century who would have denied *this*. In particular, none of the Logical Empiricists, Popper's prime targets in his anti-inductivist crusade, would have claimed anything to the contrary. As far as this *logical* question is concerned, there has always been full unanimity. Thus, if the 'solution' of the problem of induction consists in nothing more than in the mere recognition of the asymmetry of falsifiability and verifiability, then this would constitute neither an original nor a particularly specific achievement of Karl Popper, but, rather, a commonplace hardly deserving any further discussion.

And, finally, this was not, and is no version of, Hume's Problem of Induction: Hume did not ask for a logical triviality (whether certain propositions follow, or do not follow, from certain others, or whether there exist any other logical relations among propositions), nor was he sceptical about the validity of some logical relations (such as entailment), but he asked whether we (humans) can reasonably believe (or know), that is, can be *justified in our believing*, some general propositions, given (or granted) that we have nothing more than justified belief (or knowledge) of some singular propositions to start with. Even though this way to put it is not exactly Hume's, [9] the decisive point should be clear: Hume did not ask for the reasonableness of certain *logical relations* (whatever it should mean to talk about the 'reasonableness'

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[9]I had to make some terminological adaptations for a clearer fit with the present context.

of *objective* matters) but for the reasonableness of certain *propositional attitudes*. Thus, by 'objectivizing' the matter, Popper lost track of the very *problem* that he claimed to have 'solved', or, as Musgrave aptly puts it on several occasions, he simply *changed the subject*.[10]

This leads us to case (b): Let us then take the terms 'assumption' and 'claim' at their face values as denoting *propositional attitudes* of persons and adapt P$_3$ accordingly:

(sP$_3$)   Can the *assumption* (of some person X) that some singular propositions (test statements) are true *justify* either the *claim* (of person X) that some general proposition (universal theory) is true or the *claim* (of person X) that it is false?

In this case the 'justifying' relation takes *propositional attitudes* ('assumption' in the sense of *assuming* and 'claim' in the sense of *claiming*) as arguments, as demanded by Hume's problem, but this is *not a logical relation*. What we are asked for is to explicate 'justification' or, what amounts to the same, 'reasonableness' and 'rationality' as *attributes of, or relations among, propositional attitudes*, and we can easily see why this must be the case. If we tried to explicate these terms in a purely semantic manner, nothing of any interest would come up, indeed, any such notion would remain totally superfluous: we might as well get on without it and speak of the truth, or some truth relations, of propositions. Nothing of any interest would be added, if we called them 'rational' or 'reasonable' or 'justified' in excess to what we can say about them anyway in purely semantic terms. Thus, given the proposed analysis, we should be careful in each individual case to consider what is meant by 'belief', or 'knowledge', or the like: it is *propositional attitudes* to which we properly ought to attribute *rationality*, or *reasonableness*, or *justification*, and it is *propositions* to which we ought to properly attribute *truth*, *falsity*, or any other attributes or relations definable in terms of truth conditions. It means simply committing category mistakes to call the propositional attitudes true or false, or to call the propositions justified, or reasonable, or rational.[11] Thus, a person X's *belief* may be *reasonable* or *unreasonable*, a proposition h may be *true* or *false* — all four resulting combinations are possible (X may believe un/reasonably the true/false proposition h to be true).

There is much more which can be said about this, but I shall restrict my remarks to what will be needed for the understanding of some issues still to be dealt with.[12] My model or paradigmatic case of a propositional attitude is that of a belief. By a *belief* I understand a *disposition* of some person X to 'affirm' some proposition h. An *affirmation* is a *mental event* which occurs (is aroused) *more or less intensely* as an *actualization* of the belief

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[10]Musgrave (2004).

[11]This is, incidentally, my reason for thinking that Alan Musgrave is right in his insistence that it is the believ*ing* and not the believ*ed* for which we can have 'good reasons'. Cp. among others, his (1993).

[12]Cp. below, p. 259. For details see my (1996a) and also my (1996b) and (2002).

disposition under appropriate conditions (for instance, if X considers seriously the proposition h in respect to its truth). Proportional to the intensity of the affirmation, a belief is more or less *firm* ('stronger' or 'weaker'). There are analogous properties for *disbelief*, which is a disposition to '*negate*' some proposition h, and where *negation* is a more or less intense actualization of the disbelief disposition. There are two kinds of unbelief of h, either *disbelief*, where h is negated and $\neg h$ is affirmed, or *being agnostic*, where neither h and $\neg h$ is affirmed nor negated.[13] As both belief and disbelief can be more or less firm, we can use appropriate *metric concepts* (dis/belief of firmness r) for the formulation of rationality principles such as (roughly): a person X has a rational attitude of belief or unbelief concerning a proposition h if and only if X believes h with that firmness r that is proportional to the degree of support that X's empirical evidence $\mathcal{E}$ lends to h (all at time t).

Now, does all this mean subjectivism, or psychologism, or the like? Not at all. It *would*, indeed, be subjectivistic to bind the validity of logical or semantic properties of propositions and their relations in any way to the involved persons or their attitudes. For instance, it would be subjectivistic to demand that, say, the relation of logical consequence ought to 'obey the laws of thought', or, even worse, that the truth value of some proposition depended on the propositional attitudes of eventually involved persons. But nothing of this kind is demanded by the simple distinctions we have drawn above. The objective semantic properties of, and/or relations among, propositions remain untouched by our eventual attitudes towards them.

As an incidental aside we may note that all that talk about the '*rationality of science*', which is so dear to many critical rationalists, is in great danger of subjectivism, or, better, sociologism: *either* 'science' is meant here as a collection of propositions (which ones, by the way, — those from the textbooks, those from the journals, those to which *all* scientists would agree, those to which *at least one* scientist agrees, or what?), then this collection is neither 'justified' nor 'rational' but simply and objectively a (consistent or inconsistent) set of *true* or *false propositions*. Or 'science' refers to a social institution, for which one may define yet another concept of rationality. But this concept will always and unfailingly be of such kind that any talk of the rationality of science becomes sociological, that is, empirical science. The mix-up of these two spheres (collection of propositions and social institution) will then constitute sociologism.

Popper seems to have simply overreacted in his fight against subjectivism: he eliminated (or tried, unsuccessfully in the end, to eliminate) *any* explicit or implicit reference to persons and their propositional attitudes. But, as already observed above, by doing so he lost sight of the very problem which he claimed to have solved. Hume's problem, or the problem of induction, can neither be adequately stated, nor can it be solved, under the restrictions of a Popperian 'objectivist' programme. This does not mean, however, that the *distinction*

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[13]This gives rise to my claim that *beliefs* cannot be (formal) probabilities, because probabilities do not allow us to distinguish between disbelief and agnosticism. Cp. my (1996a).

between objective (logical/semantic) and subject-related (pragmatic) considerations is not important — quite to the contrary, we must remain careful to observe it in all relevant contexts, which can sometimes, due to the ambiguity of the discussed epistemic terms, become a tricky task.

Most of this, however, will be disputed by many contemporary followers of Popper, because in their view I have as yet hardly scratched the surface of the true controversy. So, we may leave this matter here as it stands and turn back to it later.

2 Methodological rules and rational belief

As can be gathered from the foregoing, in particular from sP$_3$, I have come to claim that justification or rationality pertains to propositional attitudes and, thus, to attributes of persons. (I shall argue later that we must further subdivide this into questions of pure or *theoretical rationality* and questions concerning the *rationality of decisions*.) Critical rationalists, however, like to talk about the 'rationality' of *science*, or of scientific progress, procedures, discussions, criticism, preference, methods, and so on. This inflationary proliferation of ascriptions of rationality can be reduced to the focal point of Popper's *methodological* outlook, as it was initiated already in *Logik der Forschung* (Chapter II) and combined with the idea of *objective truth* (which Popper added after it had become respectable) as the *aim of science*. I shall argue that the whole enterprise is seriously flawed.

Let us start by considering the status of methodology and methodological rules.

Whether we regard the rules of scientific method ('methodological rules') as introduced by *decision* (convention) or by *proposal* (convention, again),[14] it is in either case inappropriate to liken them straightforwardly to rules of games, for instance to those of chess, as Popper and, following him, David Miller[15] and others do. Rules of games in the straightforward sense (call them *constitutive rules*) define the framing conditions under which a game is to be played. Constitutive rules are purely conventional; we may change them any time or invent new ones and then play, given such different rules, a different game. In chess, they define the number of squares, kinds and numbers of pieces, and their permitted moves, checking positions, mating the opponent's king as the goal of the game, and so on. But by merely obeying *such* rules one will hardly ever *achieve the goal* of the game, that is, *win it*, and even *if* that should happen nevertheless, it would happen only by mere coincidence. It is not in order to *win* the game that one obeys these rules, but in order to *play* it. If one does not comply with the constitutive rules, one may either be said to play a different game or no game at all, but as long as the rules are obeyed, it is a game of chess by definition (or, rather, by convention).

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[14]This fine distinction was stressed by David Miller (1998), p. 78. Heaven knows what it should be good for.

[15]Cp. especially Miller (1998).

An entirely different matter is whether one plays the game *well* or *badly*. Sometimes strategic and/or tactical *advice* is also based on *a kind of* rules. Such rules, call them *strategic rules*, tell, for instance, whether a move is a 'good' or a 'bad' one *in respect to attaining or approximating the aim of the game*. They are not obligatory in the strict sense like the constitutive rules are, because they fulfil tasks that are analogous to those of so-called rules of prudence: if a player does not comply with them, we will not say that he 'retires from the game', but, instead, that he plays the game badly or foolishly. Strategic rules are based on *hypotheses* about the strengths and weaknesses of moves, positions, and the like, *given* the constitutive rules of the game, in particular its respective aim. They are *not* conventional, but, instead, shaped according to the *underlying hypotheses* about how to increase the chances of success (that is, attaining the aim of the game). The quality of the advice that we can get from strategic rules depends, thus, on the quality of the hypotheses forming their background or basis.

Now, of which kind shall we understand the rules of scientific method to consist of? Are they *constitutive rules* to tell us merely *how* the 'game of science' is to be played, or are they *strategic rules* to tell us how it is to be played *well*?

Popper seems to have not even considered this or any other distinction to this effect and remains thoroughly ambiguous in this respect. His insistence that, for instance, his 'supreme rule'[16] is to be adopted by *decision*, that is, by *convention*, would indicate that it is meant as a *constitutive rule*. This, however, would leave it without any rationale for *why* we should adopt it (we might as well adopt some other rule) and *why* the 'game of science' should be directed by it. More plausibly, and taking into account Popper's explicit reference to the demarcation criterion in this connection (and the further reference to truth or 'getting nearer to the truth' as the aim of science and of 'rational discussion'[17]), we may well assume that he meant it as a *strategic rule*. But then this rule (and all the other 'subsidiary' ones[18]) cannot be reasonably or rationally adopted by a simple decision without backing it up by the *hypothesis* that it is *good* (helpful, supportive, and so on) for doing science *well*, that it is *conducive* (even if fallibly and not unfailingly) *to attaining truth* (or to increasing verisimilitude) as the aim of science. This step, however, leads us full circle back to where we started from: the *hypothesis* that by obeying certain methodological rules we are more likely to approximate the aim of science than we are without obeying them can itself be rationally held only if we have good reasons for believing so, that is, if we are *justified in our (more or less firm) believing* that this hypothesis is true.

This last point may as well be substantiated in the following way:[19] by a

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[16]Popper (1935), § 11: '...that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification'.

[17]Cp. Popper (1972), Chapter 1, § 7. 'The methodological rules ...may be regarded as subject to the general *aim of rational discussion, which is to get nearer to the truth*.'

[18]Jarvie (2001) counts 14 of them.

method we must always understand a method *to some effect* or *for pursuing some aim*.[20] In order to be 'good' for pursuing an aim A, a method M need not be 'safe', that is, lead unfailingly to the realization of A. Let us call a method M 'basically good' if the employment of M raises the tendency for the realization of A as compared to the non-employment of M, and let us call M 'optimal' if M is basically good and there is no other method M' that prompts a stronger tendency for the realization of A than M does; then we may say that a method is 'good' just in case it is either basically good or optimal. This is an *objective* characterization of the 'goodness' of a method: a method M is good, just in case it fulfils these conditions, irrespective of whether or not there exist any persons who believe or know that M, indeed, fulfils them.

Now let us apply this to Popper's *critical method* (CM) *of conjectures and refutations*, and let us, for the sake of the argument, assume that CM were, indeed, objectively good in the explained sense. Let us furthermore assume that scientists were busily occupied with inventing daring hypotheses, testing them severely, and preferring best-corroborated hypotheses for further testing, in short: let it be a critical rationalist's picture book world where scientists take their decisions according to the methodological rules, for example, never to protect their hypotheses against falsification, never to stop testing them severely, never to allow them to drop out without 'good reason', and whatever else CM has in stock for its model scientists. May we then say that these scientists are *rationally pursuing the truth*? Not quite, because *even if* they were, indeed, *objectively* approaching the truth (which nobody could ever find out), they might still be deciding and acting in the described manner because, say, the fortune teller has told them to do so, or for some other insane reason, *or for no reason at all*, thus acting, at best, as the unwitting tools (or fools) of a Hegelian 'cunning of reason'. In other words, neither their actual behaviour nor their *success*, if indeed there is any (objectively speaking), ought to be mistaken for *rationality*.[21]

The same point must apply if one (rashly) were to call it rational if a person *merely believed* that the employed method is a good one, even if it were, indeed, (objectively) good, because such believing might as well be based on a madman's or on no reasoning at all.

There is, thus, no other way left than to postulate that a person must, in order to *pursue an aim rationally*, employ a method of which she *rationally believes* that it is a good method. I have come to call this the *thesis of the primacy of theoretical rationality*: there is, and there can be, *no rational acting or deciding without rational believing*.

This thesis allows for the fallibility of human reason and of human rationality. It is *quite possible* that CM is a good, even an optimal method. But who

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[19] For a more detailed account of this see my (2002).

[20] It would be nonsensical to claim to be in possession of a method without, at the same time, being able to specify what it should be good for.

[21] That the proverbial dog has its day does not make its actions rational.

knows — it may just as well be not better than throwing coins in order to 'decide' which hypothesis to prefer from a given collection of alternative ones. But if we have *good reasons for believing* that CM is good or optimal, that is, if we *rationally believe* that CM is (objectively) good, then we *decide rationally* if we make our decisions in accordance with the rules of CM, *whether or not* CM *is in fact (objectively) a good method*. We *have* nothing better than our justified beliefs to get on with.

We may summarize this section by observing that Popper's methodological view cannot overcome the force of the primacy of *theoretical* rationality, of the rationality of our *beliefs*, without which none of our actions or decisions can gain themselves the status of being reasonable or (*practically*) rational. This does not amount to the claim that CM is not *objectively* a good method. My claim is only that we cannot get on *without* good reasons, *without* justifications for our beliefs, *including* beliefs of such hypotheses that constitute the background of our methodological convictions.

This, however, will be repudiated as a 'subjectivistic' and 'justificationistic' (and, possibly, 'inductivistic') prejudice by all critical rationalists who agree with David Miller's (and William Bartley's) position of *methodological objectivism* (as I call it).

3 Methodological objectivism

Miller's position of methodological objectivism is diametrically opposed to both main points which I have put forward so far: it is *objectivistic* in Popper's sense of objectivism, of which I have argued that it fails even to state the problem of induction adequately, let alone to solve it. And it is *methodological*, of which I have argued that it fails to provide us with an appropriate conception of rationality, let alone to show us why it should be rational to obey the rules of the critical method (or why the critical method should be taken for a 'rational method'[22]). Thus, if my analysis is correct, then Miller's methodological proposals must be useless and his ascriptions of rationality must be empty.

We may skip Miller's reproduction of all the verbal twists and turns that Popper has produced in support of his claim that we may have (good) '*critical*' *reasons* for '*defending*' a *preference* for a theory, in contradistinction to (useless and unavailable) '*positive*' *reasons* for '*justifying*' such a preference (or a theory).[23] Instead, we may deal straight on with Miller's own account of theory preference, or, rather, non-preference, of which he gives a dense ver-

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[22] According to Miller (2002), p. 81, 'rationality' in a primary sense ought to be attributed to procedures or methods.

[23] Popper (1983), Part I, § 2, admits that '[g]iving reasons for one's preferences can of course be *called* a justification (in ordinary language). But [so he continues] it is not a justification in the sense criticized here.' I gather from this that the notion of justification 'criticized here' must be some *technical term* of Popper's private making. I must confess that this specific Popperian concept of justification, as 'criticized here' and at so many other places, is beyond my comprehension.

sion, thereby restricting 'attention to the simplest case, where only truth and falsity are at issue, not comparisons of verisimilitude':[24]

(M_1)   ...it is not hard to give an impeccably deductivist account of what is going on ... A theory T_1 that is refuted is definitely false (given the truth of the test statements involved), while an unrefuted theory T_2 may be true. ... All that may be derived from the empirical report that T_1 is refuted and T_2 is not refuted (together with a statement of our preference for truth over falsehood) is ... that T_1 *should not be preferred to* T_2. No attempt to justify this ... claim is made, but manifestly no justification is needed. Anyone who denies it exposes himself at once to deadly criticism.

I shall risk such deadly criticism and argue that this allegedly 'impeccably deductivist account' is inconclusive and that not even the weak methodological advice

C T_1 should not be preferred to T_2

follows as a conclusion from the proposed premises.
    Obviously C cannot follow from

P$_1$ T_1 is refuted.
P$_2$ T_2 is not refuted.
P$_3$ Truth has preference over falsehood.

because P$_1$ is still compatible with T_1 being true and P$_2$ is compatible with T_2 being false. But we may attempt to add further premises, as contained in the paragraph quoted:

P$_4$ A theory T_1 that is refuted is definitely false (given the truth of the test statements involved).
P$_5$ An unrefuted theory T_2 may be true.

Now, what can we get from P$_1$, ..., P$_5$? Again not C, which Miller claims to deduce, but, instead,

C$'$ T_1 should not be preferred to T_2 (*given the truth of the test statements involved*).

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[24]Miller (2002), p. 99. This is a more detailed version of Miller (1994), pp. 113f.

The (quite sensible) basic idea (though still in 'subjective' terms) can be found already in Popper (1963), Chapter 1, § x: 'Another question sometimes asked is this: why is it reasonable to prefer non-falsified statements to falsified ones? ... The only correct answer is the straightforward one: because we search for truth (even though we can never *be sure* we have found it), and because the falsified theories are *known* or *believed* to be false, while the non-falsified theories may still be true' (my emphases).

Even though Popper tried already in his (1963), Chapter 1 (originally a lecture given in 1953), to correct the true history of his intellectual development (something which later seems to have become his most favoured preoccupation), this piece contains his last sensible (and important) word on induction. However, it was written in a vein that he himself later denounced as 'subjectivistic' and 'inductivistic': in his (1972), in particular Chapters 1 and 3, and in his nearly unbearable (1983), Part I, § 2 (with the footnote: 'partly rewritten in 1979', but really rewritten in 1980 under the harmful influence of W. W. Bartley III) he tried to make us believe that he never meant it that way.

But *such* advice is *no* advice. To advise somebody to do p, given that condition q is fulfilled, is of no use at all for actually making a decision if, at the same time, q is of such kind that its truth *can never be found out*. The decisive point here is that P_4 imposes an *objective* condition under which T_1 is '*definitely*' false. And we cannot skip this condition, because without it the refuted theory T_1 would *not* be 'definitely' false but might (even though 'refuted') still be true, which would render P_4 itself 'definitely false'. Nor can we weaken the condition and put in instead, say, 'given the test statements involved are corroborated', or 'given the test statements involved are provisionally accepted', or similar conditions. They all would allow that the refuted theory T_1 might still be true, in which case an according reformulation of P_4 would, again, be false, in short, T_1 is 'definitely' false *only* if the test statements are *simply and objectively true*. Consequently, it remains for ever unresolved and unascertainable whether the refuted theory T_1 is 'definitely' false. This must carry over to the conclusion, such that from P_1, ..., P_5 no advice in the sense of C (not to prefer T_1 to T_2) can follow, but only C′, the mocking 'advice' not to prefer T_1 to T_2 *if an absolutely unrevealable condition is fulfilled*. But a 'methodology' that does not allow, if needed, to derive *usable* advice is itself useless. Take *any* (contingent) statement q and then give somebody the advice: 'Do p, if q is true — not, if you *believe* or *think* or *provisionally accept* that q is true, but only if q is plainly and objectively true.' Whoever understands the difference between *objective truth* and *belief* or *acceptance of truth* (and is a fallibilist) will also understand that she can *never* find out whether she should do p or not.[25]

From this and the foregoing I draw the conclusion that Miller's and any other objectivistic programme, in the sense of Popper's objectivism, is incapable of solving the problem of induction or even to give sound methodological advice, because it comes to grief over the primacy of theoretical rationality.

But there is still hope left for *critical fallibilism*.

4 Musgrave and good reasons

Alan Musgrave has taken the right step in holding that '[t]hough there are no justified beliefs (belief-contents), there are justified and hence rational believings'.[26] That he proposes an 'act-theory' of belief while I prefer a 'disposition-theory' (as well as some other differences in this respect) is of minor importance here. Suffice to say that he holds that it is the believings, the propositional attitudes (as explained above), that are the proper subject of any attribution of rationality.

But when (under which conditions) can we claim of a belief of some hypothesis to be rational or reasonable? In order to answer this question, Musgrave

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[25]For further remarks on this issue see my (2002).
[26]Musgrave (1991), p. 20. Similarly at various other places.

gives an explanation (with which I agree, subject to some provisos) and an 'epistemic principle' (which I find to be wanting). The explanation is this:[27]

> At the heart of critical rationalism . . . is the positive contention that the failure of our best efforts to show some potential belief or hypothesis to be false *is* a good cognitive reason for us tentatively to adopt that hypothesis as true, that is, to believe it. And if we have a good reason of this kind . . . then our belief . . . is a reasonable belief.

Leaving aside the fact that this formulation might be misunderstood because it mixes up 'potential belief' with 'hypothesis' as possible bearers of truth values, it contains nicely the basic intuition that, for instance, also any software programmer will recognize: in order to validate an item of software it ought to be *tested*, and testing means to try out the most vicious and aggressive tricks in order to let the software produce mistakes. The harder one tries without 'success' (that is, without producing a false or 'unwanted' output) the more one can be satisfied that the software is 'good'. Of course, by this procedure one can never *prove*, that is *verify*, that the software contains no mistakes, but it is the best one can do.

Even though scientific hypotheses are not computer software, we can see that the analogy is telling: produce 'trust' or belief by trying to show a hypothesis to be wrong. If you can't show it wrong then you are right (justified) to trust it *up to a certain point*. This is my first proviso: *how firmly you may believe* a hypothesis in order for this belief to be a *justified* one must be proportional in some way to *how hard you have tried to falsify* it. Thus, it is not a simple matter of belief or not belief, but a matter of belief *of a certain firmness* or *strength* that determines whether it is justified or unjustified, rational or irrational.

My second proviso is, that all this must be combined with what Joseph Agassi has aptly put in a nutshell: 'Empirical support is failed refutation'.[28] In other words, our 'best efforts to show some . . . hypothesis to be false' ought to be attempts at *empirical* refutations as long as we are dealing with contingent, that is, non-analytic (and, of course, consistent) propositions. Metaphysical hypotheses (which are also contingent) do not belong to this category; they are not refutable on an empirical basis. They may clash only with *other* metaphysical propositions. Such efforts towards 'showing' one metaphysical hypothesis to be 'false' by insisting on the truth of another one (and there is no other way of criticizing consistent metaphysical claims), however, cannot lead to any *sound reasons* for believing it. It is true that metaphysical musings, like many other kinds of idle tinkering with thoughts, may sometimes *lead* to, or get *transformed* into, interesting, that is *empirically testable* hypotheses. But then they, or, rather, their empirically testable substitutes, are not metaphysical anymore. *Belief* in metaphysical theses, though of no cognitive value, is open to anybody who has a taste for them, *rational* be-

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[27]Ibidem, p. 21; cp. also Musgrave (2002), p. 30.
[28]See this volume, p. 7.

lief in them is impossible. The appropriate, that is *rational*, attitude towards untested because untestable propositions must always be to *remain agnostic*.

But now for Musgrave's epistemic principle, which in one version (in terms of corroboration) reads:[29]

CR*   It is reasonable to adopt as true or to believe (at time t) that hypothesis from a group of competing hypotheses which has (at time t) been best corroborated.

Before dealing with this, a few procedural remarks should be appropriate.

Musgrave calls this principle 'epistemic' and claims that it is synthetic, which would mean that it expresses a contingent truth (or falsity). But then it ought to be empirically testable, otherwise it is metaphysical.

Hans Albert,[30] Volker Gadenne,[31] and, I believe, also Gunnar Andersson, adopt the policy of treating principles like this as metaphysical. But then I cannot see what they should be good for. As a *justified belief* in metaphysical theses is not possible (as explained above), Albert, Gadenne, and Andersson may believe them or not — it will not add, nor take away, anything from what they may be justified in believing anyway (which latter, however, will be very little indeed for lack of an adequate and *rationally* believed principle as a *presupposition* for so many other beliefs).

Musgrave himself tries to wriggle out of this predicament by simply inventing a third category: '... the things usually called "inductive principles" ... were *metaphysical* principles ..., whereas CR* is an epistemic principle.'[32] Unfortunately he never explains wherein this difference between 'metaphysical' and 'epistemic' should lie. Furthermore, his insistence that CR* is synthetic gets him into a vicious circle which he fails to get rid of in a convincing manner.[33]

My view of this matter is that principles like CR* should be taken as *proposed explications*, and, thus, as *provisionally acceptable 'analytical hypotheses'*. This contention affords some explanation, which I shall try to give by utilizing an accordingly adapted Popperian terminology.

We may understand the task of explicating a concept as a process that can be arranged in analogy to the empirical method of conjectures and refutations in the following way: The conjecture is, that the proposed principle provides an adequate *concept of rationality*. Such a conjecture can be tested against our pre-explicative conceptual intuitions by describing (as 'test statements'), and agreeing upon, particular cases that (to our pre-explicative conceptual intuition) are obvious cases of rational belief or of irrational belief. Then, if such a particular case is, according to our agreed opinion, one of *rational* belief, while the principle does *not* cover it, or if such a particular case is,

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[29]Musgrave (1991), p. 26; cp. also Musgrave (2002), p. 36.
[30]Albert (2002), p. 5 and p. 21.
[31]Gadenne (2002), p. 76 and p. 286.
[32]Musgrave (1991), p. 26; cp. also Musgrave (2002), p. 37.
[33]Cp. the critique in Miller (1994), pp. 121-125.

according to our agreed opinion, one of *irrational* belief, while the principle covers it as rational, the principle is 'refuted', that is, it has been shown to be faulty in respect to our purpose of finding a suitable concept of rationality as an explicatum. Otherwise, as long as no such 'falsifying' example comes up, we may use the concept of rationality in exactly the sense in which it is explicated by the principle.

Before we try this procedure of 'analytic conjectures and refutations' on Musgrave's principle CR*, a possible objection must be dealt with, which runs as follows: CR*, or other principles of such kind, cannot be analytic, because they are 'ampliative' in the sense, as Musgrave claims, '...that they enable you to obtain conclusions which do not follow from ...other premises of ...deductive arguments in which they figure. ...To be ampliative in this sense is simply to be non-analytic.'[34] This he claims to show by the following argument in which CR* occurs as a non-redundant premise:

CR*
Hypothesis h is the best corroborated hypothesis at time t.
Therefore, it is reasonable to adopt hypothesis h as true at time t.

But in my opinion, all that is shown by this is merely that CR* is *inadequate* as a *principle of rational belief* (or of 'reasonable adoption').

Consider a conclusion such as: 'Therefore, Peter Jones is a bachelor.' We may derive this validly from 'Peter Jones is an unmarried male person', which would render the additional *analytic* premise 'All unmarried male persons are bachelors' redundant. But we may as well arrive at the same conclusion from 'Peter Jones is a male person' and an *inadequate and non-analytic* 'principle' to the effect that 'All male persons are bachelors'. Thus, what we should be looking for is a principle on which we can agree that it gives the *meaning* of rational belief such that it would be rendered redundant if we have a premise stating that in some individual case the conditions of that principle are fulfilled. To such purpose we shall apply the method of explication, or of 'analytic conjectures and refutations',[35] as I have called it.

Suppose now that there exists a group of competing hypotheses $\{h_1, \ldots, h_n\}$, of which h_i has (at time t) been best corroborated, and suppose that there is a person X who, at time t, adopts h_i. According to CR* this would be reasonable of X. But suppose further that X doesn't know at all that h_i is best corroborated. In this case, I submit, X's adoption of h_i ought not be rated as reasonable. *Objectively* speaking, it *would* be reasonable for X to adopt h_i, but without knowing this it may as well be by mere coincidence that X has adopted h_i, which should not earn him the epistemic 'praise' of being reasonable. But then CR* is inadequate, since it rates a case as reasonable that obviously ought to be rated as unreasonable. So, let us try to improve a little on CR* and call this CR^1.

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[34]Musgrave (1991), p. 27; cp. also Musgrave (2002), p. 38.

[35]A full account of the true role and value of the methods of explication will be given in my '*Why Philosophical Problems are Genuine — And Why they are Purely Semantical*', in preparation.

CR^1 A person X has a *reasonable* belief in some hypothesis h_i from a collection of hypotheses $\{h_1, \ldots, h_n\}$ just in case h_i is best corroborated, X knows that h_i is best corroborated, and X believes h_i (all at time t).

This does away with the awkward counterexample, but, as can be expected, CR^1 can also easily be refuted.

Let us assume that h_i is best corroborated as before, X knows of this, and X believes h_i, so that, according to CR^1, X would believe h_i reasonably. However, let us furthermore assume that the empirical record (that is, all the empirical evidence gained so far, in particular the evidence gained from all the attempts to falsify the hypotheses under consideration) is still pretty meagre. Even though the evidence corroborates h_i best, everybody working in the field knows that further corroboration will be needed, or that some runner-up h_j is in for corroborations that might even turn the tables. Now suppose further that our person X simply and plainly *believes* h_i, maybe even believes beyond nearly any reasonable doubt. This case would still be covered by CR^1 as a reasonable belief, while I would call it plainly irrational. Obviously we shall have to construct yet another version CR^2, making allowance for beliefs (and disbeliefs) to be graded with respect to their firmness and for *proportioning this firmness of belief according to the degree of corroboration that the involved hypotheses gain from the evidence.*

Whichever formulation of such a principle we shall ever propose as adequate in the end,[36] it should be clear by now, that it conforms with a scheme that can be called the '*Principle of Good Old Induction*' because it served also as the basic idea for both Keynes's[37] and Carnap's[38] theories of induction. And whatever else Carnap changed in later years, there was never any need to change the basic model, as can be seen from a clear restatement of it in his posthumously published 'Inductive Logic and Rational Decisions'.[39]

The decisive point is, that this principle links the *firmness of belief* with the *degree of support* that the believed hypothesis gains from the evidence. It is true that Keynes and Carnap took *probability* for measuring that support, while we take *corroboration*. But this is of minor relevance, because a fitting concept of corroboration that does justice to the basic intuition as contained in Agassi's slogan 'empirical support is failed refutation' is still wanting anyway.

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[36] For a principle that can cope with all the counterexamples to which Musgrave's CR^* falls prey cp. my (2002). For a previous version of it see also my (1996b), which, however, was unfortunately badly mutilated by the printers.

[37] Keynes (1921), p. 17: 'In order that we may have rational belief in p of a lower degree of probability than certainty, it is necessary that we know a set of propositions h, and also know some secondary proposition q asserting a probability-relation between p and h.'

[38] Carnap (1962), p. 181: 'Our conception of the nature of inductive inference ... enables us to regard the inductive method as valid without abandoning empiricism. ... Any inductive statement (that is, not the hypothesis involved, but the statement of the inductive relation between the hypothesis and the evidence) is purely logical. Any statement of probability$_1$ ... is, if true, analytic.'

[39] Cp. Carnap (1971), especially p. 30.

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