Reconstructing "Rational Reconstruction":

A Bayesian Approach to Lakatos' "Methodology of Scientific Research Programmes"(MSRP)

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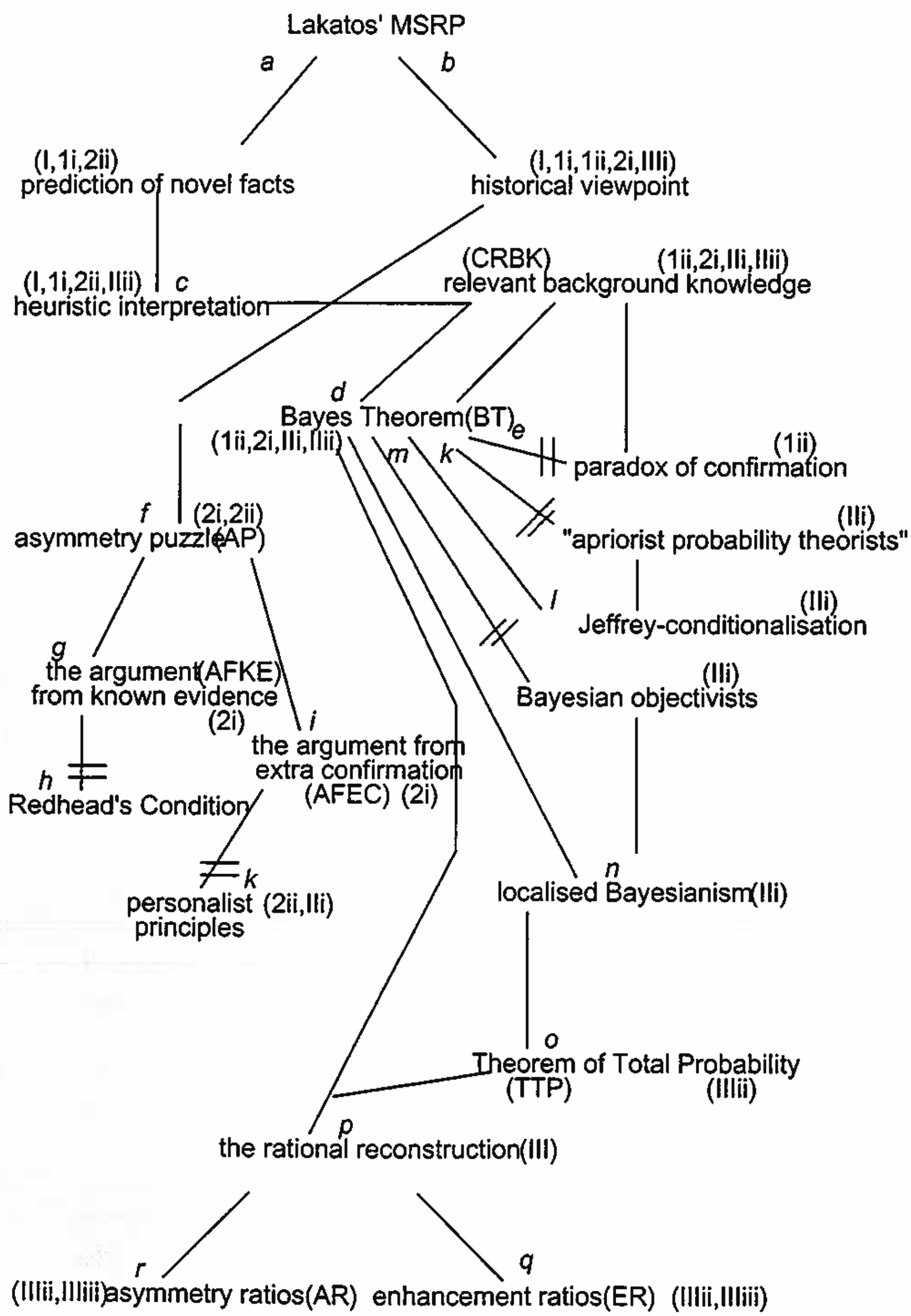

:argument blocked by counterargument

Numbers in brackets relate to the relevant chapters.

Italic letters refer to key overleaf.

Key to Diagrammatic Table of Argument

a The pathway to the reconstruction of scientific discovery stresses the importance of the prediction of novel facts.

b The pathway to the reconstruction of theory appraisal necessitates the temporal viewpoint and leads to the revision of priors.

c The intentional heuristic interpretation presents necessary and sufficient conditions for novelty based on the notion of a problem situation.

d CRBK opens the discussion to BT on the basis of the probabilistic definition of novelty.

e It also avoids the paradox of confirmation in acting as a source for relevance criteria.

f AP tries to block the introduction of the temporal viewpoint as inconsistent with the "conventional view" (CV).

g AFKE states that old evidence cannot support a new theory.

h It is circumvented by introduction of Redhead's personalist condition.

i AFEC states on purely logical grounds that well supported theories cannot be better supported.

j It is rejected by personalist principles, *viz.* that BT calculates only "personal probability functions".

k "Apriorist probability theorists" argue for objective probability attributions and hold Bayesian personalism to be psychologistic.

l Jeffrey-conditionalisation shows that "apriorist probability theorists'" demands cannot be satisfied and argues for "humanised Bayesianism"; thus it avoids relativism through the "requirement of internal coherence" (RIC) for a function of confidence.

m Bayesian objectivists argue that Bayesian personalism is subjectivist and cannot provide a rationale for rational reconstruction.

n Localised Bayesianism rebuts objectivists' ideal demands and provides a "whiff of objectivity" respective to a problem-situation and the principle of stable estimation (PSE).

o Localised Bayesianism allows for the "pragmatic" application of the TTP.

p The rational reconstruction proceeds as a semi-quantitative analysis of belief estimation.

q ER provides criteria for the evaluation of RP's positive methodological guiding rules as well as for theory-comparison.

r AR answers Duhem's problem and explains the negative heuristic.

"Almost everyone interested in confirmation theory today believes that confirmation relations ought to be analysed in terms of *probability* relations. Confirmation theory is the theory of probability plus introductions and appendices."[1]

"In history nothing ever happens in the right place or at the right time - it is the job of the historian to remedy this defect."[2]

O. Introduction

In several influential papers, Imre Lakatos[3] presented a new explanation of rational theory choice which partially retained Popper's falsificationism, but initiated a resurrection of inductive theory-support. Taking Popper's "pernicious view on inductivism"[4] into consideration, Lakatos succeeded in showing how the "rational reconstruction" of a research programme (RP) enables us to arrive at a historical view of theory-confirmation. Subsequently, two questions will be raised in this paper, *viz.* i) whether the project of rational reconstruction of scientific discovery is at all possible and ii) if, when such a possibility obtains, rational reconstruction has any role to play in the appraisal of scientific theories.

Although Lakatos himself thought that his account of heuristics in a RP would escape Bayesian understanding of theory choice[5], I will argue that his own theorising indeed justifies the attempt of its reconstruction as a valuable contribution to the Bayesian approach. There are at least two good reasons why this justification obtains:

The first one focuses on the lengthy debate, between Lakatosians and other theoreticians working on the problem of theory appraisal, of when and why "novel facts" contribute to the confirmation of a theory. Lakatos' approach stands here in opposition to the logical empiricist tradition which holds that merely logical content is to count as confirming a theory and that issues of the time of discoveries are completely irrelevant to any "rational" appraisal of theories.

The second one is about the problem of historical revaluation of theories belonging to a RP. This relates to a similar problem that Bayesians face, *viz.* how can 'prior probability estimates' which an agent had previously assigned to various hypotheses be revised. Such behaviour is regarded as conflicting with the use of "priors" for the significance-evaluation of new evidence in the probability calculus. The question I will raise is whether there are any good reasons in

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[1] Glymour, C. [1980], p. 64.
[2] Mark Twain
[3] See esp. Lakatos, I. [1968], [1970], [1975] with Zahar, E., [1978].
[4] Cf. Newton-Smith, W. H. [1981], p. 52.
[5] Cf. Lakatos, I. [1968a], esp. scts. 2. 2., 3. 4.

Lakatos' theory to view the revision of prior probabilities as a rational undertaking.

Taking a personalist stance towards Bayesianism, I will argue that the notion of "conceptually relevant background knowledge" (CRBK) will pave the way for a "heuristic re-interpretation" of 'old evidence' as 'new facts'. Focusing on individual conceptualisations will reveal why the personalist approach to Bayesianism offers good reasons for the claim that Bayesianism can present a model of rational theory appraisal. Herein, I regard the central doctrine of Bayesianism as follows: if there are any two rival theories in a specific "domain", one ought to choose the one with the highest probability in the light of one's background knowledge. Thus, the probabilities are "conditional", only in so far as they represent the beliefs one *actually* possesses, i. e. they have to be coherent with former decisions and the state of one's knowledge at that time. I will call this "the requirement of internal coherence" (RIC). Although I will hold that probability estimates ought to be regarded as "personalist probability functions", I will reject the claim that they are entirely subjective on grounds of their assessability by rational reconstruction. Personalist reconstruction of the history of science is after all merely concerned with questions of *what* kind of beliefs particular scientists had and *why* exactly those beliefs. It is not interested in the establishment of *a priori* probability distributions[6] as in the systems of Carnap[7] or Hempel[8]. Bayesian personalism accommodates scientist's practical decisions *via semi-quantitative* analyses and provides the best means for the estimation of belief states under uncertainty.

I. The Issue of "Novel Facts" for the "Classical" Historical and the Bayesian Approach

There have been various attempts to provide an adequate account of the notion of a "novel fact", but various as these approaches are, most of them emphasise the importance of the 'novel predictions' for the confirmation of a theory. They stress that these predictions lend stronger support to a theory than the accommodation of "known" facts or of instances the theory was actually designed to account for. Now, one can hardly single out one general statement in the literature of what is to count as "novel evidence", but for the sake of the argument, we may take Zahar's[9] heuristic definition as the one shared more or less explicitly by all subsequent writers.

[6] I will refer to this view as the "apriorist probability theory" later on.
[7] Carnap, R. [1950]
[8] Hempel, C. G. [1965]
[9] Zahar, E. [1973], § 2. 2.

In giving an outline of these approaches, we may fare best in categorising them as viewing 'novel facts' as instances either of "epistemic-problem novelty" or of "use-novelty". The former denotes instances in which a certain fact was *not envisaged* when the hypothesis in question was designed. On the other hand, the latter relates to such cases in which the fact was *not explicitly used* in constructing the hypothesis, i. e. that the fact was not particularly explained by H, as it was put forward by Zahar[10] and elaborated by Worrall[11]. I will focus on the heuristic definition first and pursue this procedure, because of the predominance of "use-interpretations" in the literature[12] and take it thereby as a test-case for the argument against "novel prediction" in general. Later on, I will return to "the problem-novelty interpretation" and my "argument from the subsumption of problem- by use-novelty".

Although various authors have claimed that the notion of "use-novelty" was superior to "problem-novelty" for the explanation of theory choice, the relevance of a specific "domain"[13] for designing a hypothesis has apparently been overlooked. Introducing the concept of a "domain" throws light on the role of the intentionality of a scientist and demands alterations in the 'use-novelty' concept. Now, I will show that the strength of Lakatos' concept of "touchstone theories" has not been exploited and the chance missed to unify Lakatosian methodologies with Bayesian criteria for theory-appraisal.

I 1) The Classical Historical Approach[14]

i) The Notion of 'Novel Facts'

In the attempt to analyse the notion of "the prediction of novel facts" we need to develop an understanding of its relation to the evaluation of theory-supersedence as well. It has long been known that the verification of an extraordinary prediction provides much stronger confirmation than the explanation of a known "fact" or of instances the theory was specifically designed to accommodate. Now,

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10 op. cit., esp. § 1. 1.

11 Worrall, J. [1978a & b]; I regard his main contribution to the question of novel confirmation in taking "empirical support as a three place relation" between i) a theory, ii) a set of factual statements and iii) the set of those factual statements used in constructing the theory, because it allows for regarding heuristic novelty *as* necessary for temporal novelty. Hence, if the *same* theory was arrived at *via* different approaches, then it will not be supported by the *same* fact, because not being used by the scientist constructing the hypothesis is a necessary condition for not being known; a similar position is also held by Musgrave, A. [1974], p. 7.

12 Cf. Nunan, R. [1993], Campbell/Vinci [1982, 1983], Giere, R. [1975, 1984], Redhead, M. [1978] etc.

13 Shapere, D. [1974]; also Garber, M. [1983] and Jeffrey, R. [1992], p. 91.

14 Following Musgrave, A. [1974, p. 7] I will call a theory which takes background knowledge into consideration *a partly historical theory,* because "...once we have decided what *sort* of thing background knowledge is to contain, it will presumably be a historical task to determine its actual contents".

Lakatos has held that one RP[15] will supersede another one only if it is "theoretically progressive", i. e. exceeds its predecessor in content and predicts 'novel' facts, and if it is also "empirically progressive", i. e. reveals confirmation of its predictions:

> Progress is measured by the degree to which a problemshift is progressive, by the degree to which the series of theories leads us to the discovery of novel facts... [which are] improbable or even impossible in the light of previous knowledge[16].

Gardner's[17] discussion of Lakatos original definition points out that Lakatos might have had in mind only that a fact could provide a test of a theory against an older competing one, if the antecedent theory renders the fact *improbable* but the succeeding one does not, i. e. as a quasi "probabilistic crucial test". He rejects such an assumption, because theory appraisal need not involve the comparison of two theories. However, I regard this interpretation as too narrow, because Lakatos has first argued on independent occasions for the importance of theory comparison in competitive situations[18] and secondly shown how theory appraisal from within a RP is possible[19]. Instead, the real problem with Lakatos' MSRP consists in the fact that "rational reconstruction" proceeds only *ex post facto*[20], whereas we want to know how rational decisions and predictions are possible with respect to the state of our knowledge at any given time. Thus, we have to look for another approach to theory confirmation, if we want to include such a-historical requirements and might take Gardner's probabilistic interpretation as a convenient vantage point, insofar as it opens the Lakatosian approach to Bayesian interpretations.

But, before we can proceed this way we should commence with the outline of the "historical approach" in some more detail. Zahar[21] has subsequently suggested a weaker definition of 'novel facts' than the original one provided by Lakatos, which read "facts previously unknown to the scientific world". This definition would rule out evidence which we intuitively count in favour of a theory[22], in that

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[15] A research programme (RP) can be taken to be a sequence of developing theories in the history of science. Such a program consists of methodological rules, of which one kind tells which paths of research to avoid (*"the negative heuristic"*) and the other one what paths of research especially to pursue (*"the positive heuristic"*). Herein, a RP is characterised by its enduring hard core of theories, which are essentially prevented from thorough falsification by "the negative heuristic". Instead, a protective belt of "auxiliary theories" is established, which has to allow for partial falsification.

[16] Lakatos, I. [1970], p. 118.

[17] Gardner, M. [1982], p. 8.

[18] Lakatos, I. [1978], p. 23 & p. 69ff.; with Zahar, E. [1975], p. 184ff.; [1968a], *passim*.

[19] Lakatos, I. [1978].

[20] As most of his critics have remarked; e. g. Hacking, I. [1983], p. 118f., Newton-Smith, W. H. [1981], chpt. 4.

[21] Zahar, E. [1973], § 1. 1.

[22] Gardner, M. [1982], p. 3 explains this adaptation thus, that Lakatos and Zahar, E. [1975] have realised how absurd it is to deny that a theory's explaining an anomaly is to count in its favour, i. e. that an anomaly can be reconstructed as a genuine problem and it's solution renders the RP progressive.

we count previously "known" facts (e. g. the Michelson-Morley experiment) as confirming later theories (such as Special Relativity). Zahar proposed a definition of "heuristic novelty", which demands that a fact should not be *used* in constructing the hypothesis at hand:

> "A fact will be considered novel with respect to a given hypothesis if it did not belong to the problem-situation which governed the construction of the hypothesis".[23]

From here, various authors have worked on the notion of a "problem-situation" of which Gardner's analysis[24] seems to be the most straightforward one: he holds that the notion falls apart into two uses: it is either i) referring to a set of phenomena for which the theory is thought to deliver some explanations (the setting of a problem) or it signifies ii) a set of phenomena which was actually used in designing the theory (the solution of a problem). Although he allows for the subjective conceptualisation of the scientist constructing a theory, Gardner makes the mistake of conflating the particular background information, which was actually used by the scientist in constructing a "*working*-hypothesis", with the full background knowledge available, which obtains in the scientific community[25]. His notion of a 'problem situation' is too vague to answer the question whether a scientist in constructing her hypothesis *actually* possessed a fact or not, because of the conflation of the conceptualisation of a problem with its objectivity.

We should insert Zahar's emphasis here, that reconstructing "personal" heuristics necessitates discovering a specific set of factual statements *actually* used[26] in the design of a theory from auto-biographies and other hardly assessable historic material[27]. Taking the discussion of heuristic novelty from here, I will hold against Gardner that problem novelty can be "subsumed" under use novelty, because we want to regard the problem anticipated by the scientist while the actual construction of a 'working hypothesis' takes place. "Subsumption" here is not to be identified with "logical entailment" and the term will be used to denote a conceptual approach to understanding *what* the problem is. Entailment was demanded by Gardner to get an account of 'subjective use' and 'objective fit', but as I will later argue in relation to the notion of a "domain" of a theory, this linkage depends on the interpretative framework, whether we regard the individual scientist "at work", the scientific community in its "actual possession of a specific state of knowledge at a given time" or whether we talk about some "objective field of reference corresponding" to the theories which are applied.

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[23] Zahar, E. [1973], p. 103.
[24] Gardner, M. op. cit., p. 3.
[25] ibid., p. 3.
[26] See also J. Worrall's "actual use"-definition [1978a], p. 48.
[27] Zahar, E. op. cit., p. 103f.

A better definition of "problem-situation" than Gardner's, is given by Shapere's notion of a "theory-domain" which is "the total body of information for which, ideally, an answer to [a] problem is expected to account"[28]. I will hold that despite the objections raised against it[29] the concept of a "domain" provides us with consistent necessary *and* sufficient conditions for 'novelty' regarding the individual scientist's knowledge in designing a theory. Herein Shapere's notion offers a more adequate explanation than Zahar's, because: i) a 'domain' defined as above provides objective grounds for comparing the particular scientist's knowledge to a theory's body of statements and connects theoretical domains, thus offering the possibility of content-comparison; and ii) the explicit statement of information regards 'facts' not only as plain states of affairs but as subjectively used ones in the interest of theory-design or as objectively incorporated ones in a theory's body of statements.

Construed as above, the concept of 'facts' presents *sui generis* Lakatos' and Zahar's "temporal viewpoint" in understanding the heuristic approach as an *intentional* one. That is to say, that a human interest[30] is reflected in the path to the construction of a hypothesis, which aims at the solution of a specific scientific problem. Providing solutions to it necessarily involves giving an explanation of how *formerly* unrelated facts are *now* connected, i. e. how they are explained in the light of a *new* theory. The temporal viewpoint accommodates this situation, because we must distinguish the actual *explanandum* of a theory from previously "known" facts, its *explanans*, as seen in a new light. From here, both approaches are likewise important: we ought to look for i) the way a theory was built, i. e. *which* facts were actually used for this purpose and ii) *what* actually these facts were at time t in the light of a certain hypothesis H.

Now, accepting this view for the question of novel confirmation will involve regarding both approaches as interrelated and problem novelty as 'subsumed' to use novelty in the sense above. On the other hand, we do not have to dispense with either 'heuristic' or 'problem novelty', because both approaches are compatible when 'heuristic novelty' is construed as the prior concept. Nevertheless, they have different meanings as explicated and can be treated separately when reconstructing the design-situation of a theory. Herein interpretation of the theory occurs, which is seemingly a personal affair: the essential interrelation of i) and ii) yields 'temporal novelty' as a necessary *and* sufficient condition for 'novelty', because it cannot be separated from the

[28] Shapere, D. op. cit., p. 528.

[29] E. g. Gardner, M. op. cit., p. 9.

[30] In opposition to authors in the sociology of knowledge such as Habermas, J. [1972] and Bloor, D. [1976], I take the very notion of "human interest" only as a principle for the rational reconstruction of the pathway to the design of a theory. Thus, this "interest" has to be panned out in the form of a rational aim, i. e. the resolution of a specific scientific problem. Essentially, I am rejecting Habermas thesis that natural sciences and the humanities have *per se* different interests. If their interests can at all be taken to be different, then it is only with respect to the different scientific problems in various "special sciences".

interpretation of a fact f by a theory T, which explains *what* the fact in question is a fact *for*. A fact of the form "that p" will only obtain within the conceptual framework of the theory we hold while asserting it. On the other hand, in order to maintain an objective basis for these facts to obtain, we need to take "worldly" states of affairs as primitive, *viz.* as existing "out there" in a world independent of us and our venture of discovering them.

Having seen that 'facts' cannot be construed without their relation to a (background) theory, we can return to the discussion of 'novel confirmation'. What should be clear by now is that the 'data' used in the construction of a hypothesis need not in themselves be new, but rather the 'agreement' or connection between T and these 'data' as 'facts' is "unforeseen". The characteristic common feature these situations present is that *it was unknown to the scientist in question that the theory actually explains the facts* and thus the discovery of this new explanation gives support to the theory. Here, I am anticipating my position as a Bayesian personalist, holding that necessary and sufficient conditions for 'novelty' *can* be given with respect to the individual scientist's decisions, because as Gardner puts it:" One cannot be using or trying to account for something of which he [the scientist] is ignorant"[31]. Now, before I can further argue for this view, I have to introduce an analysis of the very notion of "background knowledge".

ii) Background Knowledge or a Background Theory?[32]

In proposing the notion of "epistemic novelty", Popper made use of its relation to a certain stack of 'background knowledge', to avoid the notion of inductive theory support, which he thought to be unwarranted. Instead, he worked out "the requirement of the independent testability" of theories and proposed to regard a class of facts as problem-novel in such a way that a prediction which was initially implausible could be rendered highly plausible given that a hypothesis was found to explain the phenomena[33]. These predictions must be unexpected and "risky", relative to our prior background knowledge, whereby *b* for Popper "includes all our available background knowledge at the time in question":

(1) pr (E/~H&b) is low.

Watkins'[34] has further shown that the introduction of background knowledge also avoids "the paradox of confirmation"[35] and indicated that hardly any positive

[31] Gardner, M. ibid., p. 11.
[32] Cf. Musgrave, A. [1974].
[33] Popper, K. [1965], p. 36 and [1963], p. 36.
[34] Watkins, J. W. N. [1964].
[35] The paradox arises in the 'purely logical' approaches to confirmation, as in Carnap, R. [1950] and Hempel, C. G. [1965] which made it necessary for them to develop auxiliary strategies to commence with

instance of a hypothesis yields support for it. He proposed to regard background knowledge as a non-trivial source of 'relevance criteria'[36] for the identification of facts that count as evidence *for* the theory. Accepting 'background knowledge' as a source of such informations necessitates the view of "conceptual relevance" as opposed to objective "probabilistic relevance", which, as "chances", would be a claim about some objective state of the world. Regarding CRBK thus as a personal affair presupposes some entity deciding what the fact is relevant *for* [37], be it the scientist herself or the scientific community in general.

A more sophisticated account of the involvement of background knowledge than Popper's original one has been presented by Giere[38], who presents two conditions for what is to count as a 'good scientific test' of a hypothesis: i) the hypothesis in conjunction with initial conditions and essential auxiliary assumptions entails the predicted outcome and ii) that the predicted event would be improbable given these particular initial conditions and auxiliary assumptions, but the negation of the hypothesis ~H. We can formulate Giere's second condition as such:

(2) pr (E/~H & a) is low,

here, *a* represents the combination of auxiliary assumptions and initial conditions, which "surround" the finitely closed language of the background knowledge[39]. Such an interpretation *a* of background knowledge as the sum of conjuncts which is actually used for the design of a hypothesis provides us with means against the argument that "*known evidence*" (AFKE) does not provide new confirmation[40].

Now, Campbell/Vinci have criticised Giere's approach, saying it is "trivially satisfied"[41] with the exception that E has a high probability to obtain solely in *a*. On the basis of Popper's requirement of severe testing, they object to Giere's rationale for eliminating those cases, where the predicted evidence was likely on the basis of some rival hypotheses in the field. Here their critique amounts to the

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the initial project of giving a logical foundation for confirmation. As I am not primarily concerned with it here, I have to refer the reader to Mackie, J. [1963], p. 265f.

36 Cf. Mackie, J. [1969], where he holds a similar position based on Keynes' notion of 'favourable relevance'. In particular, Mackie views the relevance criterion basically characterised as: a hypothesis H is to be confirmed by an observation E in relation to some 'body of background knowledge b, iff. it is more likely to occur on the basis of the conjunction of H&b alone. He calls this the "*inverse principle*": pr (E/b&H) > pr (E/b).

37 Shapere, D. [op. cit., p. 526]: "'significance' [relevance] is a function of what [is] called 'background knowledge'.

38 Giere, R. [1984], pp. 88-94 and [1983], *passim*. It seems to be in return a development on J. W. N. Watkins as in [1984], pp. 101-3.

39 Giere, R. ibid.; arguing for the exclusion of the hypotheses from background knowledge ("everything else known at the time") Giere states: "if we *included* the hypothesis in our calculations, then the prediction would have to be judged very likely, because the hypothesis... implies the prediction. But the hypothesis being tested should not count as part of what we already know", p. 104.

40 Campbell/Vinci [1983], p. 324.

41 op. cit., p. 321.

claim, that given that some H→E, the conjunction of H with a series of other *a*'s, a_1, ..., a_n, will always amount to new theories which have to be "tested independently", whereas Giere, in their opinion, allows for any *a* being conjoined to H in such a way that the resulting hypotheses are empirically equivalent and, hence indistinguishable by testing. Their claim is similar to the "tacking paradox" which is that an infinite amount of false theories can be attached to some observational statements which has not yet been falsified. Campbell/Vinci require that these additional false statements should be weeded out, hence their requirement for substituting *b* for *a*, in order to get the scientific community's overall knowledge in as a *corrigens*.

But, on this interpretation Campbell/Vinci are obviously attacking a strawman and I will hold that Giere's position[42] can be explicated in a much more subtle way. The paradoxical situation in "the tacking paradox" just amounts to the recognition that the theories at hand *are* all "empirically adequate", i. e. indistinguishable on the basis of an *a* for the scientist in question. Campbell/Vinci's requirement, then, contains two flaws: a) it is begging the question at the level of *b* - even within the scientific community there are historical cases of the empirical adequacy of different theories -[43] and b) they are blurring the difference between *b* and *a*, which is needed to derive the "heuristic" interpretation of novelty. Arguing their case, Campbell/Vinci subsequently employ Zahar's counterexample[44] that the anomalous precession of Mercury's perihelion confirmed Einstein's Special Relativity hypothesis, although the evidence was known, i. e. contained in *b* on their interpretation, long before Einstein developed his theory. Postulating that the relevant background information should not include E, although it was known when the theory was devised, they construe the background knowledge *b'* deprived of E, so that:

(3) pr (E/~H&b') is low,

Campbell/Vinci hold that an interpretation of *a* as Gardener's *b'* - the person's background knowledge - would not be available to Giere, because Popper and Giere have laid down their requirement as a necessary condition for confirmation in general and not merely for novel confirmation. However, we can take the Popper - Giere requirement not to conflict with the 'personal' interpretation of background knowledge. Thus, I will argue that Campbell/Vinci's strategy is unsound, because it does not capture the whole thrust of Zahar's argument. Their formalisation is of no help, because it includes in the evidence E what was not contained in it. As we have already seen, the problematic concept is that of "knowledge", but if we take E as denoting *certain* knowledge, which is only

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[42] It can be construed as an anticipation of M. Redhead's position [Campbell/Vinci, op. cit., p. 322f.], but, as I will show, this need not damage its content as the personalist position can withstand Campbell/Vinci's attacks.

[43] See W. V. O. Quine's thesis of the "indeterminacy of reference" [1969].

[44] Zahar, E. [1973], p. 101.

available to some Laplacian Demon, then we can easily construe E' as *personal evidence*:

(3a) pr (E'/~H&a) is low[45].

This formalisation does not have to conflict with i) Giere's & Popper's requirement of objective evidence, because if the person has an adequate grasp of objective evidence in her *a*, then the subjective evidence 'fits' the objective: E' = E[46] and their requirement can be satisfied as non-'person relative' with respect to the objective basis of actual states of the world; and ii) Gardner's account of personalistic background knowledge *b'*, because only in the case that a scientist knows all the background knowledge of her scientific community will $b' = b$[47]; and iii) the heuristic requirement, because only if all available evidence is used, i. e. included in *a*, then $a \rightarrow$E; otherwise if only some evidence is used, only $a \rightarrow$E' obtains[48].

We can reconstruct Zahar's argument now, while holding that "knowing a fact" is not identical with "evidence". That is to say, that by application of Zahar's criterion knowledge of the fact of Mercury's perihelion is by no means to be taken as evidence for Einstein's theory, because only an explanatory account of the 'mechanism' why such a fact has come about can present us with evidence in favour of the adequacy of the proposed theory. What becomes obvious within this view is that facts contained by one theory can change their information or content from one theory to another[49] through "reinterpretation". We have already introduced the "requirement of interpretation" above in (3a), insofar that under the condition that we are not logically omniscient agents, E' ≠ E obtains. If a new

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45 I will prefer Giere's position denoted as *a* to Gardner's *b'*, in order to keep some "whiff of objectivity". We can take Giere's *a* to represent the objective parts of information, whereby the probability-statement can be seen as giving the degree of belief in E's belonging to the conjuncts obtaining in the denominator; anyway, problems of knowing or not knowing the evidence are reflected in my distinction of E from E'. In the long run, appropriate historical research hopefully tells us what had actually been in a scientist's mind.

46 As far as I can see, this situation seems to reflect C. Howson's [1984, p. 246 and 1985, p. 7] notation of 'K-{E}', wherein K denotes some stack of background knowledge and for the case that E is not "known" it is subtracted from K. Thus my denotation seems not to be essentially different from Howson's, but it should suffice to clear up some misunderstandings in the literature. The introduction of E and E' simply tries to avoid the 'evidence'/'knowledge' conflation. As I have argued that evidence is always evidence *for* some hypothesis, I do not think we should include this contentious part into the used background knowledge *a*. Hence, we can distinguish exactly *which* "evidence" was used in the design of *which* theory with respect to *what* temporal state of background knowledge and *relative* to the scientific community's overall knowledge at that time.

47 Notice: Gardner's notion is one of the inventor's conceptualised knowledge only, whereby E is not in conjunction with *b*, but is "inquired about" or "thought upon".

48 In opposition to Howson, I will take only E as to include everything in *a* dependent on it, whereas E' will be taken as to include only the *relevant* statements in *a*, in order to avoid problems of the kind as "the tacking paradox".

49 I will not hold that their meanings change altogether, e. g. one might say that the items of a "domain" may be preserved from one situation to another. In writing this paper I hold to realist inclinations and dismiss the strong claim for "meaning variance"as it has been held by Quine.

theory has actually superseded an older one by containing the relevant parts for its domain, then that part of the old theory "became part of a larger body of information which called for a fuller *deeper explanation* [emphasis added]"[50]. With respect to the temporal view, which I regard as 'implicit' on this account, Lakatos held that before a new theory in the field is proposed, 'refuting instances' of it are classified as 'unexplained anomalies' and their importance for crucial experiments becomes obvious with "hindsight", i. e. the "*new discovery*"[51] that both theories at hand were actually competing *in* or *about* the same "domain" for which they have been designed to give answers. Hence, the historical approach presents us with the means to discover those decisive bits of information for theory evaluation.

Furthermore, as Lakatos held that scientists can operate with (in) competing RPs and use empirical data as evidence to choose between them, we should not compare a new theory with our background knowledge alone. Instead, we should *also* compare it with the older theory which it is apt to supersede, for the objective specification of the "domain" that both are about. Hereby, Lakatos calls the old theory the 'background theory' or 'touchstone theory'[52] and 'novelty' is defined in terms of additional "potential falsifiers". Lakatos' account turns out to be a recommendation of Popper's testifiability criterion[53], *viz.* that the superseding theory should predict new facts such that they can be in principle independently falsified and therefore increase in content over the old one. From here, there can be predictions which i) conflict with the predictions made by the old theory (which can be decided in "crucial experiments") and those ii) predictions delivered by the new theory alone (to be tested independently). Now, Lakatos made strong use of the idea that 'background knowledge' is to be replaced by a 'background theory' to improve on Popper's definition of the severity of tests[54]. For clarification we have to distinguish two meanings of 'background knowledge' here: the one refers to the conceptual relevance of items "contained" in the background knowledge, whereas the other one denotes the stack of information that is provided by it. Neither meaning is decisively analysed by Lakatos. Nevertheless, the main difference between Lakatos and Popper with regard to the role of scientific background knowledge can be presented as:

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[50] Shapere, D. op. cit., p. 520.; likewise Giere, R. [1983, p. 292.] arguing that such a "domain" can also provide good reasons for the idea of accumulating evidence for a theory and the growth of science in general.

[51] also Garber, D. [1983, p. 120]: "If old evidence can be used to raise the probability of a new hypothesis, then it must be by way of the *discovery* of previously unknown logical relations".

[52] Lakatos, I. [1968], pp. 375-390.

[53] Especially Popper, K. [1957] and [1963] and I. Lakatos' observations on Popper's development [1978, p. 93ff.].

[54] Popper decided, as late as 1963, that "the severity of tests can be objectively compared, and... we can define a measure of their severity" [p. 388ff.]. Here, he defined it as the difference between the likelihood of the predicted effect in the light of the theory under test in conjunction with the background knowledge and on the other hand of the likelihood in the light of the background theory alone. Thus, were Lakatos speaks about the background knowledge as background theory, Popper takes it as the "unproblematic knowledge" we assume while testing [1959, p. 375, n. 2].

"But whereas Popper acknowledged the *influence* of metaphysics *upon* science, I see metaphysics as an integral part of science. For Popper... metaphysics is *merely* 'influential'; I specify concrete patterns of appraisal. And these conflict with Poppers earlier appraisals of 'falsifiable' theories ..."[55].

While Popper regarded "unproblematic background knowledge" as knowledge about the content of the theory alone, Lakatos goes so far as to include "methodological concepts", which on Popper's account are strictly external to science and have nothing whatever to do with the content of the background knowledge. However, Lakatos accounts for the evaluation of evidence by scientists beforehand and thus allows for a personalist interpretation of test-severity. On top of that, a definition of novelty which accounts for 'use novelty' *and* the 'heuristic ingredient' has to regard the scientific "guiding rules" as well. Such a definition was given by Frankel, which also accords well with Lakatos' original intention:

"A fact is novel with respect to a given hypothesis and its research programme, if it is not similar to a fact which already has been used by members of the same research programme to support an hypothesis designed to solve the same problems as the hypothesis in question."[56]

I cannot see why it is necessary to mention other members of a RP, because, albeit such a move might provide us with hunches for historical research of what was in a researcher's mind, what essentially matters in the heuristic definition is the path to the construction of the particular hypothesis in question. This path seems to be either hidden in an individual researcher's head or in the communication within a team working on exactly the same sub-problem. But, what is such a path to the construction of a theory and what counts as *using* a fact in obtaining a theory?[57] The answer to this question was already given: priority lies in the personal preference of a particular bit of background knowledge *a* and a specific human use for a certain hypothesis: it is thus relative to the scientist we want to look at. Admittedly, these scientists might have applied methodology inherent in the broader RP, but this can still be *ad hoc* in one or the other sense and we want a newly designed theory to convince us of its performance and not of its ability to incorporate every known fact.

Objecting to Frankel's definition, as Nunan does that the model will account for any solved anomaly to count as a "new fact",[58], clearly misses the point: predictions (here rather confused with explanations) are nice to have for the

[55] Lakatos, I. [1974], p. 148, n. 2.

[56] Frankel, H. [1979], p. 25.

[57] E. g. Glymour, C. [1980, p. 99] criticises Lakatosians for not having clarified what it means to *use* a fact.

[58] Nunan, R. [1984], p. 278.; also I can't share Nunan's insistence [p. 279] on novelty only if a fact has been "entertained in some rival research program", for two reasons: i) a fact could have been used in some prior theoretical framework of the RP and was *newly interpreted* and ii) members of the RP have seen members of another RP working with some data in a certain way, while when working with the raw data as well, they extended their theory to this area of the putative research domain and *used* the data *in a new way*, which cannot simply be grafted backwards onto the rival programme.

appraisal of a singular theory if they can be accommodated into one theory. There, they do the job of resolving anomalies, solving "normal" problems or being simply *ad hoc*. But what is to be regarded as Lakatos' main point is his insistence on the comparison with a "touchstone theory", i.e. if both have the same explanatory content, but one of them predicts additional outcomes, then it is clearly to be favoured. This requirement holds particularly in what Lakatosians and Bayesians want to explain, *viz.* how scientists are convincible if they still subscribe to an old research programme by following their "negative heuristic". Taking the Lakatosian discussion of "novel confirmation" from here, the way is now open for a Bayesian personalist interpretation of theory appraisal based on the heuristic interpretation of novel facts.

I 2) The Bayesian Approach

i) The Temporal Relation Between Hypothesis and Evidence

As presented above, the focus on the temporal order of discovery and acceptance of theories in Bayesian approaches was seen as entirely irrelevant to the logic of rational theory choice by logical empiricists[59]. In this section I will hold that it isn't just an irrelevant matter of psychology, but reveals a very important feature about the approximation of scientific truths and the question of theory-supersedence. Before I will give a positive outline of the Bayesian position on 'novel confirmation', I shall refer to Campbell/Vinci's approach again, presenting it as a paradigm-example for the misconception of Bayesian principles.

Although they also agree on the relevance of the temporal relationship between hypotheses and their evidence, they deny that such an interpretation can be given in what they call a "conventional Bayesian position", because it would misconceive the significance of predictive novelty in ignoring the special kind of 'heuristic novelty'. I object to their exposition of the "conventional view" (CV)[60] and will argue that Bayesianism has *always been* a theory of learning from evidence. As such it focuses on the personal conceptualisation of scientists when estimating the probabilities of hypotheses. In their "asymmetry puzzle"[61] they state that CV is inconsistent, because it could not explain the variation of

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59 Cf. Popper, K. [1959], chpt. 1, sct. 2.

60 I will refer to CV as Campbell/Vinci's "Standard Bayesian Vindication" [op. cit., p. 324.], *viz.* that the explanation of why predictive novelty confirms a theory is given by appeal to BT alone and that predictive novelty merely varies inversely with its prior probability. I will hold throughout my argument, that this view of the "standard vindication" is simplistic and inadequate.

61 Herein Campbell/Vinci stand in the tradition of the logical empiricist's attack on Bayesianism while arguing that CV cannot solve the "asymmetry puzzle" in appealing to the logic of confirmation, because that would involve focusing on the theory's content.

confirmation-values presented by the probability calculus without a parallel change in the evidence. I will argue against their presupposition that Bayesians necessarily hold to the *dogma of conditional probabilities* and unrevisable priors and show that the problem is present in their interpretation of 'evidence' as logically connected to "conditional probabilities" right from the start[62].

Bayesian personalism instead conceives 'conditional probability functions' of the form pr (p/q) as relativised to some state of background information, which we have introduced as *a*. Now, pr (p/q) measures, *a* not taken to contain q, what the agent's degree of belief would be in p, were she to come to know q[63]. Now, assume *a* would already include q, then pr (p/q) would be equal to pr (p) if p→q. Were these two quantities subsequently substituted into the support measure, then its value would be 'zero'. This seems to be completely undesirable and that is where Campbell/Vinci insert their counterarguments against personalism.

Their AFKE is presented as pr (E/H&b) = 1 and E being known so that pr (E/b) = 1, by Bayes' theorem (BT) pr (H/E&b) = pr (H/b) would obtain. In the aftermath even they hold this result to be counterintuitive. Now, on the basis of a personalist approach to Bayesianism, Michael Redhead[64] offers a striking answer to Campbell/Vinci's "asymmetry puzzle": "given that theory T was designed to explain some phenomenon e..., then the likelihood of its being true given e and background knowledge b' (excluding e) gets no enhancement over the likelihood of T's being true given only b':

Writing pr (T/b') = x, pr (e/~T&b') = ε and taking pr (e/T&b') = 1 and using pr (~T/b') = 1-x

$$\text{pr}\,(T/b'\&e) = \frac{x}{x + \varepsilon\,(1-x)} \qquad \text{(iv)}$$

We define an enhancement ratio y by

$$y = \frac{\text{pr}\,(T/b'\&e)}{\text{pr}\,(T/b')}$$

whence using (iv) we obtain the simple result

$$y = \frac{1}{x + \varepsilon\,(1-x)} \qquad \text{(v)}$$

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62 As R. Jeffrey glossed this situation: "The sentences are not telegraph lines on which the external world sends observation sentences for us to condition upon", [1992], p. 78.

63 The standard argument for the measurement of degrees of belief in terms of a conditionalised probability function is explicated by de Finetti, B. [1937], pp. 108-110.

64 Redhead, M. [1978], p. 357.

We can now explain that if a theory T is *ad hoc*... with respect to the experiment e then $\varepsilon = 1$, i. e., the explanation of e by T in no way depends on the truth or falsehood of T, both of which eventualities lead with certainty to the result e. This is just what a scientist means when he says T was an *ad hoc* explanation of e... To show the consistency of our analysis if we put $\varepsilon = 1$ in (v) we get $y = 1$, so the posterior and prior probabilities of T are equal (there is no enhancement ...)".

That is to say, that "knowing" *and* "using" a fact in the design of a theory does not push the "*enhancement ratios*" up further, hence, there is no sense in conducting a prediction-experiment with that theory. On the other hand, we have to focus on the status of *b'* in order to find out what the designer of a theory *actually* new in her background knowledge and will find that the conceptualisation of facts has changed whereas Campbell/Vinci treat "knowledge" as a *certain* and unvarying affair. Mistakenly identifying personalist with objectivist interpretations of the probability calculus, Campbell/Vinci state that in the

> "hypothetical case of Einstein's explanation (T) of Brownian motion in a 'novel' liquid (E), pr (E/T&b') = 1 and pr (E/~T&b') is high, and yet T is explanatorily relevant to E given b'. Indeed, without T there would have been no (satisfactory) explanation for E. *Redhead's contrary view seems to rest on a confusion of explanatory with probabilistic relevance.*"[65]

Apparently, the most important confusion has taken place in Campbell/Vinci's refusal to acknowledge CV as a personalist approach. I will hold against their argument that they misconceive the conceptual role of background knowledge in theory design as it is presented by "Redhead's condition"[66], as formula (3) was dubbed by them. In their argument Campbell/Vinci criticize the condition as unsound and launch two counterexamples against it: i) "the case of evidence not known but probable" and ii) "the copycat case".

i) They demand that when Einstein came to explain Brownian molecular motion in applying statistical thermodynamics, he should have known that Brownian motion was discovered in a plenitude of other liquids. Hence, this outcome should have been in his background knowledge *a*.

ii) Dr. Original, proponent of one of two competing hypotheses H, is able at time t1 to show mathematically that H implies E, some experimental result, whereas the competing hypothesis H* implies ~E. In his calculation E is taken to be unlikely given his background information at t1. Now, Dr. Copycat, a proponent of H*, comes to notice the result and revises H* so that it entails E too: H**. His

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[65] op. cit., p. 331.

[66] It states that E is an outcome which can be derived from H together with appropriate additional assumptions preserved in *b'*; Redhead, M. [1978], p. 356f.

reason for this adaptation is simply that he does not want to be "scooped" by his college if E turns out to be true.

Now, in the first case Campbell/Vinci hold that Redhead's condition fails, although significant confirmation of H results, whereas in the latter case the condition is satisfied, but no confirmation occurs. However, Redhead's subsequent objection fits well with my earlier counterargument, *viz.* that Campbell/Vinci illegitimately identified facts and evidence:

> "[Redhead's condition] was supposed to be a criterion for evidence e *not* to be *ad hoc* in relation to the hypothesis h, where non-ad hocness was equated, following Zahar [1973], with heuristic novelty in the sense that e did not belong to the problem-situation h was constructed to deal with... The condition... was supposed to be an explication in Bayesian terms of the notion of *heuristic novelty*. It was not concerned with explicating some *modified* notion of *epistemic novelty* as Campbell and Vinci assume ..."[67]

Redhead has repeated Worrall's[68] warning to avoid the serious confusion of the meaning of background knowledge in the sense of those auxiliary assumptions and initial conditions needed to derive E from H&*b'* and the broader sense of everything we hold unproblematic at the time H is proposed. Accordingly, a more adequate formalisation of Redhead's condition would be :

(4) $\quad pr\,(E/{\sim}H\&a) << 1$[69],

With Redhead we can reject Campbell/Vinci's claim that this condition could be trivially satisfied:

> "Suppose e is used as a heuristic ingredient in the construction of h in the sense of a 'filter' on all the various alternatives to h which the investigator will entertain with a non- vanishing prior probability, i. e. the *only* alternatives to h will be hypotheses which also explain e. In such a situation we would clearly arrive at the result
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> [2] $\quad pr\,(e/{\sim}h\&a) = 1$ "[70].

Given that H was designed with the intention of explaining E, it is replaced by the above condition, which contests "trivial satisfaction", i. e. the *ad hoc*-adjustment in this situation is rendered obvious, because under the "*filter condition*" of equation [2] we get pr (E/a)=1 by implication. Thus arriving from (4) at [2] in the case of ad hocness or from [2] at (4) in a case of genuine prediction is a result that stems not from "knowledge" comprised in *a* alone, but

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[67] Redhead, M. [1986], p. 116.

[68] Worrall, J. [1978a], n. 6, p. 66.

[69] Corresponding to my earlier argument *a* should be used instead of Campbell/Vinci's previous presentation of *b'*.

[70] Redhead, M. ibid.

from the function of the "heuristic ingredient" itself, i. e. whether $a \rightarrow E$ or $a \rightarrow E'$ obtains.

Given a personalist interpretation as in Redhead's argument, we can reject Campbell/Vinci's counterexamples: First, Brownian motion designed to explain molecular behaviour does indeed confirm thermodynamics on the grounds that the latter theory was not established to explain Brownian motion, i. e. especially in those liquids which are "novel" with respect to our heuristic assumption[71]. The second case of "Dr. Copycat" is likewise dismissable, because it does not account for the actual belief in his hypothesis, but for some hidden psychological strategy of a scientist fearing to loose his reputation. Instead, we require from rational reconstruction that if something is to be lost at all then it must be the belief in the theory's truth. Given that we are interested in *rational* theory choice we want a commitment from scientists' decisions instead of fluctuating beliefs depending on "scientific modes". Accordingly, the counterexamples proposed by Campbell/Vinci break down and we have seen that the temporal order of the discovery and acceptance of theories can play an important role in a Bayesian personalist approach to theory appraisal, i. e. it does not lead into the paradoxical situations proposed by "apriorist probability theorist's" arguments.

ii) Novel Facts, Old Evidence and Bayesian Personalism:

Our intuitions and the history of science tell us that there is additional evidence, given that old facts have been re-interpreted. But what exactly is the *mechanism* that entitles them to be new evidence for a hypothesis? I concur with Glymour[72] that confirmation of a hypothesis obtains not only through the realisation of epistemic novelty, i. e. that some evidence is predicted by the hypothesis, but also through the *"novel discovery"* that some old evidence is entailed by it. Holding that an "old fact", some old piece of evidence, can confirm a hypothesis which was not originally designed to explain this fact, we are implying that we have learned something about the relation of the fact to our theory at hand and call this process of *"learning something new about E"* its "new interpretation". In this context, facts can be evidence only insofar as they contribute to a hypothesis or

[71] We can take this argument when formalised as: assume that H was actually not designed to explain E, as yet unknown, but nevertheless is later on discovered as entailing E. Suppose also that prior to the experiment which yielded E, pr (E) is thought to be fairly high. Now, $E \notin a$ and the Bayesian support of H by E is therefore small. From here we take the initial pr (E) actually to be pr (E'). Identifying them misses the point, because Brownian motion as used in Campbell/Vinci's example was explained for different liquids. Then, $E' \in a$, but its probability is regarded as small. Hence, the support of H by E' is significant since probability-values are computed relative to a-E' and not relative to a-E, which is the experimental result obtained.

[72] Glymour, C. [1980], pp. 91ff.

can be said to be instances of that hypothesis, i. e. "evidence" is *intrinsically a relation of facts to a hypothesis*[73].

Thus the reinterpretation of facts through "novel discoveries" yields a "deeper mechanism" within scientific explanation which is responsible for our belief-change. As Lakatos has pointed out[74]:

> "...what had previously seemed a speculative reinterpretation of old facts... turned out to be a discovery of novel facts... *And we should certainly regard a newly interpreted fact as a new fact, ignoring the insolent priority claims of amateur fact collectors".*

Campbell/Vinci[75] object that the account of "novel discovery" could not deliver necessary conditions for genuine 'enhancement', especially in cases where we "know" that some particular piece of evidence is entailed by the hypothesis but not whether it is true. Now, I will claim that we can have a belief about the proposition's being true without being forced to abandon our probability statement altogether, even, when the proposition turns out later to be false. The question for the agent is how far she would change her odds given new, better, knowledge, which renders the view defended here a theory of *dispositional* properties of an agent's belief-structure and I will hold with Glymour[76] that 'old evidence' can confirm a new theory, if

(5) $\quad pr\,(H/b\&E\&(H\rightarrow E)) > pr\,(H/b\&E)$[77] .

Campbell/Vinci's argument from "*extra confirmation*" (AFEC) (that new evidence can raise the probability of (H/E) bigger than 'one') hinges upon the independence of the prior probabilities of E and H given an assignment to pr (E/H&b). They argue that this independence is required by the epistemic interpretation of novelty, i. e. the ability to determine novelty independent of the truth of the hypothesis. But, given that Bayesianism is a theory of degrees of belief in the "truthlikeliness"[78] of a theory, we are not compelled to regard our

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[73] A similar position is hold by Howson, C. [1984], p. 250.

[74] There is a lengthy discussion of the relation of interpretation, facts and novelty in: Lakatos, I. [1970], pp. 156-7.

[75] Campbell/Vinci, op. cit., p. 324.

[76] Glymour, C. op. cit., p. 92.

[77] Although I realise that Glymour's formalisation is non-standard in Bayesian literature, I will make use of it for conceptual clarification; nevertheless, Glymour's unconventional view had its influence on Garber, M. [1983] and Jeffrey, R. [1992]. Standard Bayesian accounts will treat (H→E) as implicit knowledge contained in E. But , as I have shown in (3a) such a standard view has always given rise to confusions about the status of 'evidence' and of 'background knowledge'. Thus, when E' is fully acknowledged in its significance, as excluding that H→E, then and only then will the above inequation be superfluous. But, as long as the confusions about the meanings of "logical entailment" and of the "knowledge of ..." this consequence prevail, so long will (5) provide a block to further misconceptions.

[78] For the concept of "truthlikeliness" see: Field, H. [1973] and Niiniluoto, I. [1979].

Taking historical evidence into account, we may say that a hypothesis can only be approximately true at a given time t. Likewise, it can be said to be >partially true< if either only a part of the hypothesis is true or that it is true only in some particular frame of reference, i. e. a "domain" of application.

"probability-assignments" as *being* actually true, instead, they are the most likely outcome[79] or the best bet for the moment[80]. That is to say, that *via* new interpretations the probability estimation in relation to a specific theoretical framework starts anew, when interpretation of facts has rendered their role in the theory significantly different, i. e. there is no enhancement of theory support "bigger than one". Having analysed Campbell/Vinci's objections to CV, the AFEC and the AFKE, I now reject their thesis "that no Bayesian conception of confirmation can be acceptable"[81]. I have demonstrated that their attack is based on a false apprehension of Bayesianism when construed *personalist*.

II. "Heuristic Novelty" Provides an Adequate Understanding of Bayesian Personalist Principles on Conditionalisation

i) Bayesian Personalist Principles

So far, I have rejected Campbell/Vinci's presentation of CV and will now commence my critique in pointing out that their account of the relevance of the 'temporal relationship between hypothesis and evidence' is only marginally different from Zahar's previous definition. Further on, I will show how a personalist Bayesian analysis provides a more appropriate criterion regarding decision-making on the basis of given, limited amounts of evidence when uncertainty prevails.

Three amendments are made by Campbell/Vinci[82] on Zahar's notion of 'heuristic novelty' which seem prima facie plausible, but are implicit in Zahar's argument:

i) 'temporal novelty' does not entail 'heuristic novelty' [as I have argued earlier, it is rather the other way round: temporal novelty can be subsumed to use/heuristic novelty when regarded as essentially interrelated with the intention to solve a scientific problem],

ii) the element of novelty should be more than simply the absence of a fixed design to include E [the very notion of evidence can only be understood as 'being evidence for some hypothesis', thus the notion of a "novel fact" necessitates some

[79] This position can be broadened to the overall knowledge possessed by a scientific community to represent an inter-subjective view on the matter. In any case, our knowledge is restricted and history presents us with the fallibility of most of our theories.
[80] Jeffrey, R. [1992, chpt. 5] refers to these as "Bayesian solutions" given by "Wald's Rule" [in: 1950], p. 78.
[81] Campbell/Vinci, op. cit., p. 326.
[82] op. cit., p. 333ff.

process of interpretation which *per se* is to be regarded as expanding the notion of "a fixed design to include e" by involving cognitive concepts]

iii) evidential novelty has to be connected to "selection novelty" [Campbell/Vinci regard Zahar's definition as "E is novel with respect to H iff the selection of H is not *ad hoc*"; again, I see no improvement in this step: applying the notion of interpretation yields that a fact is novel in the light of some new theory by giving a new explanation for the role of the fact in its theory].

From here, we recognise why Campbell/Vinci were unable to appreciate the merits Redhead's approach presents: *viz.*, it yields an unambiguous definition of ad hocness and an explanation of what it is for a scientist to discover that an 'old fact' gives support to a "new" theory. Instead, Campbell/Vinci's account of "explanation"[83] falls short of capturing an important part of scientific practice: that hypotheses are designed to explain phenomena *via* laws, which establish an 'intrinsic relationship' between them.

Despite their initial critique, Campbell/Vinci subsequently argue for a Bayesian explanation of heuristic novelty on the basis of increasing confidence in some hypothesis that generates successful predictions. On Bayesian conditionalisation the probability calculus is interpreted as a deductive consequence which *must* hold (by RIC as will be later shown) between pr_0 (H) (the prior probability assigned to H before the acquisition of evidence) and pr_1 (H) (after such acquisition) whenever an agent is confronted with some new evidence. The newly revised probability estimation is based on previous probability estimates as prescribed by the personalist interpretation of BT:

$$(6) \qquad pr_1(H) = pr_0(H/E) = \frac{pr_0(H) \cdot pr_0(E/H)}{pr_0(E)}$$ [84]

An objection repeatedly raised against Bayesian conditionalisation is whether priors have to be regarded as posterior probabilities themselves, such that conditionalisation enters into an infinite regress. I will regard such foundationalist challenges as irrelevant to the task of determining the specific background knowledge, held by a scientist at a certain time t when designing a theory, and of specifying the new information-input inserted into such a "closed calculating system". Hence the brand of Bayesian personalism I am advocating here is "*localised*" *qua* a given problem situation.

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[83] also R. Jeffrey: "But something is missing here, namely the supportive effect of belief in E. Nothing in the equivalence [of Garber's]... depends on the supposition that E is a 'known fact', or on the supposition that p (E)...is close to 1. It is such suppositions that make it appropriate to speak of 'explanation' of E by H instead of mere implication"; op. cit., p. 92.

[84] Here pr_0 (H/E) represents the prior probability which the agent assigned to H given E, whereas pr_0 (E/H) yields the estimated probability of E when H is assumed to be "true".

Localisation also occurs when considering the time-interval for conditionalisation: e. g. Campbell/Vinci's coherence-requirement for probability assignments leaves it entirely unspecified of what is to be regarded as an appropriate time-interval for "coherence" to obtain. We either have to assume that they hold an implicit foundationalist view on determinism or presuppose some long range Bayesian position. Both positions seem to me untenable, because they are too vague for an appropriate account of theory choice. I will propose that a "localised" personalist position requires coherence only for the problem relevant time-interval, i. e. the time in which a scientist devises her hypothesis". This account, then, gives sufficient conditions for the overall project of specifying rational theory choice, in that the time relevant to the design of a theory turns out to be dependent on the scientific context. E. g. it is easy to decide whether Einstein made actually use of the Michelson - Morley experiment when designing the theory of Special Relativity, once we have got the appropriate data. Although, this does not imply that it might not be a nearly impossible amount of work for a historian to discover the data, it does specify the set up of the historical question clearly from the beginning of the investigation.

By raising the prior probability of H, the Bayesian conditionalisation technique provides means to explain why the verification of an initially unlikely prediction can provide greater support for a theory than a likely one. The degree of belief in a hypothesis, given the particular bit of evidence thus has to fall within a determinate *range* of values, a position endorsed by the later R. Carnap and by R. C. Jeffrey[85]. Comparing both approaches, I regard Jeffrey's conception of "probability spaces" as more adequate to the personalist interpretation[86], because Jeffrey's model demands only (structural) consistency of probability assignments than specific value description and identity of probabilities.

Jeffrey saw that "strict Bayesian conditionalisation"[87] is too restrictive if construed, such, that revisions of prior probabilities have to accord solely with the actual body of evidence inserted. Jeffrey's conditionalisation technique can be regarded as a genuine answer to the problems posed by "strict

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[85] Jeffrey, R. [1965], chpt. 11 and [1975].

[86] Accommodating the objections that Bayesian theory seems to take data input E as certain [Keynes, C. I. (1921) and C. I. Lewis (1946)], Jeffrey's approach takes E only as "given", i. e. as a provisional acceptance of data relevant for a specific problem. Hereby "personal degrees of certainty" can be panned out in terms of assigning high probabilities to "starter-hypotheses" for the actual calculation. Notice, that probability assignments here are personal degrees of belief on the appropriate characterisation of observational data and the "truthlikeliness" of a hypothesis.

[87] The term is used by Teller, P. [1973, p. 244ff.] in pointing out that "strict conditionalisation" is only a "special case" of what he calls "generalised conditionalisation". I take this to accord with "Bayesian personalism", the position which I am arguing for. The main point on which I agree with Teller is the emphasis on the "qualitative condition on changes of belief", which goes well beyond the purely logical interpretations. Putting forward his position, Teller offers a genuine proof of how qualitative instances can be inserted into probability functions, while assuming that rational agents are capable of weighing their confidence in certain beliefs on some subjective "internal scale"and estimate the relation of two beliefs held. Therefore this approach bears a strong relation to Redhead's *semi-quantitative* rationale.

conditionalisation", because he admits that it is impossible for human agents to anticipate all changes which could affect their current background beliefs. His improvement on the conventional formalisation of BT regards agents' statements on evidential support as attached with some degree of probability only.

If 'pr_1 (E)' represents a revised probability of E and '~E' the statement that E is false, then Jeffrey's principle of conditionalisation delivers that:

$$(7) \quad pr_1 (H) = pr_1 (E) \cdot pr_0 (H/E) + [1 - pr_1 (E)] \cdot pr_0 (H/{\sim}E)$$ [88],

whereby "conventional conditionalisation" will present as a "limiting case of the present more general method of assimilating uncertain evidence, and the case of conditionalisation is approximated more and more closely as the probability [of E] approaches 1"[89], such that if pr_1 (E) $\approx$ 1, then the conventional rule of conditionalisation $pr_1 (H) = pr_0 (H/E)$ will be obtained.

A problem with this account of conditionalisation occurs since it revises prior probabilities different if they appear in another order. They would accidentally be taken as conjoint probabilities conflicting with the demand of their independence. Solving the problem of asymmetric estimation, pr_0 (E) has to be regarded as a personal reflection (at a certain time t) according to one's background knowledge, as Field[90] has pointed out. He has revised Jeffrey's principle that it is able to cope with the succession of probability assignments in rendering pr_1 (E) a function of pr_0 (E) together with an additional "input-parameter" that represents the degree of which "new sensory stimulation" affects E[91] .

Thus, BT is merely "based" *on* or calculating "personal probability functions" while following the lines given by the axioms of the mathematical theory of probability. This view yields one restriction to revision only: RIC[92]. Still, our demand is that revision should be rationally reconstructable when numerical values assigned are projected onto a coherent "*function of confidence*". What we then attain is a theory-dependent confidence level, such that applying and working in one theory will present the accumulation of evidential support as a process of increasing probability. Now, this process can only be specified from within the theoretical framework, such that well supported theories cannot be significantly more supported by new evidence, as demanded by the AFNE. In the case when the heuristic definition of novelty makes it necessary that we conceive our theory as essentially re-interpreted, then our probability estimation has to start "from scratch". The RIC will only support the stability of "confidence-

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88 Cf. Jeffrey, R. [1965], chpt. 11.

89 op. cit., p. 160.

90 Field, H. [1978].

91 Sensory stimulation then has to accord to certain "input laws", which yield their connection to a finite set of observational statements; op. cit., p. 361f.

92 Nunan, R. [1984] has called this "the conversion technique", p. 270.

relations", given by the distribution of the "confidence-function", if the constraints of the theoretical framework are not violated.

Even though we might "feel" a similar inclination to the new theory's being true as for the old one, confidence can only be evaluated appropriately if we take conceptual considerations into account. On pain of "internal incoherence", we have to change the distribution of "confidence relations", just because we have learned that the new theory is the better one, i. e. we realise that Bayes' rule "grasps us by the throat" and forces us to move on to a new confidence distribution. Now, even if this model may be seen as strictly accounting for ideal rational agents only, we should assume that it is in principle possible to approximate rational beliefs "from outside", whereby "approximation" means that we can compare rational behaviour with our belief estimations. Hence, the main presupposition I adhere to in my argument is that I envisage actions of rational agents as dependent on their beliefs[93]. This is not to say that the Bayesian model cannot in principle account for a general model of rationality, but I regard Bayesian personalism as a sufficient means to demarcate rational from irrational choices, given the following conditions: a) a certain "known" amount of observational data and b) a distinct state of background knowledge for the rational agent in question.

Now, before I step further and apply Bayesian personalist principles to rational theory choice and the reconstruction of scientific methodology, I will finally reject the thesis that Bayesian personalism is subjectivist. This is frequently held by non-Bayesians and Bayesian objectivists (e. g. H. Jeffreys, E. T. Jaynes and R. Rosenkrantz). We can take R. A. Fisher's classical statement to represent their attack:

> "Advocates of inverse probability [the traditional name for generating posterior probabilities via Bayes' rule] seem forced to regard mathematical probability ...as measuring merely psychological tendencies, theorems respecting which are useless for scientific purposes"[94].

The critics of personalism hold that science is objective with respect to scientific inferences. If those inferences turned out to be "simply" personal beliefs and belief change only restricted by RIC, then the inductive conclusions obtained will merely reflect "personal opinions". On the other hand, objectivists like E. T. Jaynes require that statistical analysis should not make use of "personal opinions", but of "the specific factual data on which those opinions are based"[95]. The main claim I have been making for personalism focuses especially on the epistemological problem hidden in this objectivist position, *viz.* how are we to

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[93] That knowledge of probability relations is mainly important for it's bearing on action is captured by de Finetti's "tendency to act as if..." [1937, p. 111] or in Peirce, C. S. [C. P. 5. 12]: "[a belief is] that upon which a man is prepared to act".

[94] Fisher, R. A. [1947], p. 6f.

[95] Jaynes, E. T. [1968], pp. 227-41.

obtain unambiguous knowledge of "factual data" in the first place, which is independent of conceptual preconditions? I do not intend to roll out the whole debate about "theory-ladeness of observation", but I emphasise that the personalist position can accommodate the concepts of "truth-approximation" and "truthlikeliness", whereas objectivism cannot and instead presupposes "logical omniscience". Although I am in general sympathetic with the objectivists' claim about the generality of their model, it is not clear to me how they live up to their own ideal. As Howson/Urbach put it:

> "*No prior probability or probability-density distribution expresses merely the available factual data; it inevitably expresses some sort of opinion about the possibilities consistent with the data.* Even a uniform prior distribution is defined only relatively to some partition of these probabilities..."[96].

Now, what is left in the personalist's arsenal? Besides the problems of identifying empirical evidence and the attachment of actual values to the calculus, one would at least in a constructivist framework have to admit, that once agreement on norms of appraisal is established then such evaluation of the relationship between different kinds of evidential support for a theory holds in an unambiguous manner. The question accordingly is what is such a "constructivist framework" and what is to be coined as "appropriate circumstances" for Bayesian explanation? At least two answers are necessary: first, empirical testing starts after some amount of would-be discoveries have been made, which are to be taken as worth testing: i. e. several incompatible hypotheses H, H',... with reasonably high probability have to be recognised as "starters" before further testing and learning can go ahead.

Second, the hypothesis at hand, i. e. our model with its parameters filled in by observed data, should yield a causal explanation in the end. If we accept these assumptions, then BT does not deliver an outcome independent of the given evidence or will necessarily turn out ambiguous if we allow for the revision of prior probabilities in the process of science.

However, the gap that separates Bayesian personalism and objectivism is given by the epistemological problem of arriving at "secure" knowledge, which I claim although announced by objectivists cannot be delivered by them. In defending Bayesian personalism I will follow D. Garber's account of "localised Bayesianism"[97]. It will be clear that there is quite a middle ground on which objectivists' boldness and subjectivists' security can meet.

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[96] Howson, C./Urbach, P. [1989], p. 289.

[97] Garber, D. [1983], op. cit.: "The goal is to build a hand-held calculator,as it were, a tool to help the scientist or decision-maker with particular inferential problems... In order to apply it in some particular situation, we enter in only what we need to deal with in the context of the problem at hand", p. 111.

Garber suggests we define "personal probability functions" not over the maximally specific language of the whole scientific system L, but only for a modest problem-relative language L* for the duration of our interest in a specific problem. Starting off with a particular problem at hand (and there the approach is widely compatible with Shapere's "domain" and Gardner's "problem-situation"), scientists are only interested in a *relevant* group of hypotheses H_i and what they could learn about them by acquiring some evidence E_i. The problem relative language L* would be, given this scientific setting, just presented as the "truth-functional closure" of the H_i and the E_i. The epistemological problem for objective Bayesianism could be bypassed in treating the H_i and E_i as atomic sentences which get their meanings by their structure, insofar an L* can be provided in given observational circumstances, i. e. "as being approximately true" of some states of the world.

Summing up, the move indicated here "amounts to replacing the *logically* possible worlds of the global language with more modest *epistemologically* possible worlds, specified in accordance with our immediate interests"[98]. Thus, "localised Bayesianism" can retain a "whiff of objectivity" without running into the dangerous waters of those idealisations in the "objective Bayesian" position.

ii) Heuristic Novelty in its New Definition:

So far I have shown why 'novelty' has to be defined with respect to the specific 'initial background knowledge-situation' of the designer of a theory. The important ingredient of such a definition is to reveal whether the agent actually made use of the fact, or whether she was ignorant of the matter that the theory *actually* explained the data[99]. Now, if this conception offers the only weak point, which formalistic challengers can score against Bayesians, *viz.* that it is a "restricted" model of rationality, then it will be an easy task to reconstruct the theory against this charge. Herein I will supply BT with a "*meta-principle*" based on a "temporal belief function of probability statements", which I take to be an implicit component of Bayesianism, given the personalist standpoint. Further on, I want to outline the argument in a more formal and concise manner and highlight the main points of significance.

We may go ahead by distinguishing three different classes of facts in relation to a hypothesis H[100]. First, there is a class of facts I(H) which contains 'phenomena'

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98 Garber, D. op. cit., p. 113.

99 Although critics of this approach [e. g. A. Musgrave] have pointed out that it will render ignorance a virtue, I will turn the tables on them: it will indeed render ignorance a virtue, but only insofar as the scientist's knowledge is up to date with the best knowledge available. If the critics were right, there could be no ignorance whatever, because human kind was omniscient and science an absurd and redundant venture.

100 As originally proposed by Niiniluoto, I. [1983]

which the theory was explicitly designed to explain. Secondly, and this is likewise the main line for our argument for the relevance of the *temporal order* of 'evidence', up to the time t the theory H has predicted previously 'unknown facts' which are included in a class F_t (H). Herein, the 'data' referred to in F_t (H) can be of the same kind as the data in class I(H), but in case of F_t (H) = I(H) there is no "enhancement" in support, because we simply do not know whether there is *ad hoc*-explanation or 'genuine accommodation' of facts *via* the explanatory power of the theory itself. Thirdly, there might come a time t, where H is able to explain also old facts in a new light, i. e. the facts are not new as "raw" data , but their 're-interpretation' renders them as such. Such facts are said to obtain in class E_t (H).

Now, we can take the explanatory power of theory H at the time t to be represented by the union I(H) $\cup$ E_t (H), and its predictive power to be F_t (H). The main point here is that both growing classes, F_t (H) and E_t (H), contain 'novel facts' but of *two sorts*. This point has been repeatedly overlooked in the discussion of novel confirmation. In case a rival theory H_1 is introduced into the domain at time t_1, where $t_1 > t$, a sufficient condition for H_1 being a better theory than H is given by:

$$I(H) \cup F_t(H) \cup E_t(H) \subseteq I(H_1)$$

and

$$(F_{t1}(H) - F_t(H)) \cup (E_{t1}(H) - E_t(H)) \subseteq F_{t1}(H_1) \cup E_{t1}(H_1).$$

That is to say, that H_1 explains the previous successes of H, but by the time of t_1 it entails more novel facts than H[101]. We have already said that for our interpretation of the Bayesian theory of confirmation, evidence E supports a theory H iff E increases the probability of H relative to the initial 'background knowledge-situation' of the designer, *a*, i. e.

H is additionally confirmed iff pr (H/E&a) > pr (H/a). This was shown with regard to Giere's and Worrall's improvement on Popper's conception of background knowledge. For new predictions in F_t (H) new confirmation is given by

$$(8) \qquad pr(H/E\&a) = \frac{pr(H/a)\, pr(E/H\&a)}{pr(E/a)} \text{, such that}$$

(9) (i) $0 < pr(H/a) < 1$

(ii) $pr(E/H\&a) = 1$

(iii) $pr(E/a) < 1$.

[101] This condition, again, reflects Lakatos' requirement for theory supersedence, [1970], ibid., p. 116.

If a scientist has deduced E from H&a, and E stands for a previously unknown fact, then data contained in F_t (H) will support the hypothesis H. From here it is possible to demonstrate the effects of varying prior probabilities and of the re-interpretation of these facts. By BT:

$$(10) \qquad \text{pr (H/Ea) - pr (H/a) = pr (H/a)} \left(\frac{1}{\text{pr (E/a)}} - 1 \right)$$

and on above conditions (i-iii), facts E give more confirming support for H if they are less probable on *a*, i. e. the more "surprising" they are relative to it as Popper originally demanded for the severity of tests. Also, a theory which makes use of a lot of *ad hoc* assumptions does not receive as much support from the data as a competing theory which straightforwardly explains the data.

However, the situation with class E_t (H) is different and contains the bone of contention in the previous debate. The problem is that some interpretations regard E_t (H) as containing previously "known" facts, which are said to obtain in the overall background knowledge *b* (held by the scientific community), such that E cannot support H because:

(11) if pr (E/b) = 1, then pr (H/E&b) = pr (H/b).

Because of this result, Campbell/Vinci ("the standard Bayesian vindication"[102]), Glymour ("Bayesian kinematics"[103]) and Gardner ("Bayesian assumptions"[104]) have argued that 'old evidence' cannot confirm a 'new theory' according to CV. This understanding rests on a misconception depending on the dogma conceiving scientists as perfect logicians, who by knowledge of H→E must conclude that pr (E/H) = 1. If we take pr_y, now, to represent a probability measure for such a perfect logician Y, given that pr_y (H/E) is defined[105], and if H→E, then pr_y (H/E) > pr_y (H).

But holding to our Bayesian personalist position, we instead regard agent X not as logically omniscient, because there are always consequences of the hypothesis at hand that we do not know to be its consequences. Thus we imagine such a "semi-rational" Bayesian to change her belief (in H) only in relation to the

102 Campbell/Vinci, op. cit., p. 324.

103 Glymour, C. [1980], p. 86.

104 Gardner, M. [1982], p. 13.

105 As it will be, if pr_y (E) > 0 and pr_y (E) < 1,and pr_y (H) > 0; since H→E, pr_y (E) > 0 if pr_y (H) > 0.

"conditional degree of belief"[106] (in H given E, such that $H \rightarrow E$). In the worst case, it might turn out that really $H \rightarrow E$ but X erroneously takes H to be completely irrelevant to E. So, we would have to replace the above condition with:

If X *really* knows that $H \rightarrow E$, then and only then $pr_X(E/H) = 1$.

If our imperfect logician X further knows that $H \rightarrow E$, then and only then

$$pr_X(H/E) < pr_X(H).$$

After all, our assumptions can be summarised as follows: the personal probability function pr_X of an imperfect logician X, such as some historical scientist, will be taken to satisfy

$$pr_X(H) = pr_X(H/E) < pr_X(H/E \,\&\, (H \rightarrow E))$$[107].

We may highlight these results for the discussion of rational theory choice: here, we are clearly confronted with fallible, historical persons and aim at the rational reconstruction of the way they evaluated the theories they worked with. Clearly, these agents are imperfect logicians of the same kind as agent X and cannot be taken to be agent Y, as the above mentioned approaches want to see them. Therefore, applying rational reconstruction, we have to regard historical decisions as resting on meta-statements, i. e. thoughts and beliefs that scientists actually had about the truth of their theories.

Summing up, *a* represents the 'initial background knowledge situation' of the agent X, and *i* can be taken to be X's (meta-) statement, representing the facts of I(H) when H is known to X as explaining them. Therefore it turns out that $a \rightarrow E$, such that the initial plausibility of H relative to *a* is:

(12) $pr(H/a) = pr(H/i) > pr(H)$.

Now, if E expresses the facts included in *a* but not known to be explained by the hypothesis then the following obtains:

(13) $pr(H/b\&(H \rightarrow E)) = pr(H/i\&E\&(H \rightarrow E)) > pr(H/i\&E) = pr(H/a)$.

Hence, we realise that the discovery of facts in the class $E_t(H)$ will indeed give support to the hypothesis H in question.

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[106] As Garber, D. [1983] has put it: "The basic concept for the Bayesian is that of *a degree of belief* [emphasis added]. The degree of belief that a person S has in a sentence p is a numerical measure of S's confidence in the truth of p, and is manifested in the choices S makes among bets, actions, etc.", p. 100.
[107] see: Glymour, C. [1980], p. 92.

III. The Bayesian Interpretation of Lakatos' MSRP

i) Lakatos' Historical Method and The Revision of Prior Probabilities

As I have already said, Lakatos' contribution to rational theory appraisal can be regarded as a response to the problem of revising prior probabilities in Bayesian theory. It might thereby give answers for the intra-RP choice of constituent "theoretical devices" as well as for the external choice between different RPs in general. Although it has been stated that external decisions fall short of sharing a common body of accepted beliefs[108], I will argue that this does not pose a serious problem for Lakatosians, because their account of background knowledge will allow them to work in different RP's[109]. For the Bayesian, this will not be a problem either: given that the prior probabilities are neither zero nor one, L. Savage has pointed out that Bayesian conditionalisation can force the beliefs of two agents to convergence about the subsequent probability estimations[110] even if they started out with very different initial degrees of belief. His argument is based on "the principle of stable estimation" (PSE)[111], which states that there are such cases in which the actual estimations are very insensitive to variations that occur in the prior distributions, i. e. the posterior distribution will be approximately similar, as it would have been if the priors were uniformly distributed[112].

Although criticism might linger about such "pragmatic procedure", I have already stressed the shortcomings of the Bayesian objectivist approach and its incapability of identifying "uniform prior probabilities". Again, what justifies scientists in such procedure is their confidence that a problem relevant language will cover their presuppositions and will make approximately true assertions about the "domain" they want to cover. "Localised Bayesianism" will thus reveal such a pragmatic attitude a thoroughly rational procedure, because it can give ready answers for what is to count as "appropriate" prior probabilities, as "sensitive" hypotheses (as starters), and as approximately "true" assertions.

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108 see: Nunan, R. [1984], p. 271.

109 Cf. Lakatos, I. [1968], in: Lakatos, I. /Musgrave, A. [1970], esp. chpt. 2, sct. C, pp. 116-32.

110 Savage, L. [1954], "In certain contexts any two opinions... are almost sure to be brought very close to one another by a sufficiently large body of evidence... The conclusion... is not that evidence brings holders of different opinions to the same opinions, but rather to similar opinions...", p. 68.

111 which is also due to Edwards, W. [1968] and Lindman, H. [1963, joint paper with both other authors], p. 201ff.

112 These authors show that PSE can be applied when three conditions are met: i) a 99% or higher interval should be proposed assuming that the prior distribution in it is uniform, whereby both ends of the interval have to be (pragmatically) specified; ii) the actual priors have to be accepted as stable; iii) we have to "verify" that outside of the proposed interval there will be no priors horrendously high in comparison with the ones we "caught in our interval-net".

Lakatos, providing a solution to the "problem of belief variation", on his account, focused on the evaluation of successions of theories within one RP rather than on the comparison of singular theories. He emphasised their common link to a problem area which stands at their beginning allowing for the unique determination of reference. The historical analysis of these theory-chains was assumed as to indicate them as either "progressive" or "degenerative"[113] with respect to the problem area. After all, the pragmatic acceptation of hypotheses would be rendered rational iff the RP originating in some "domain" turned out progressive on average values.

The point that these analyses can only proceed *ex post facto* and cannot be used for the estimation of predictions could be overcome by adopting the Bayesian view, i. e. we can give the historical analysis its right to provide us with useful information for the estimation of prior probabilities, whereas incoming data can be compared and rational choices made regarding future states of experiments. Further, intra-RP decisions are unproblematic for Bayesian analysis, because scientists working in that RP share a homogenous conception of what they hold to be relevant background knowledge and agree intersubjectively on how they estimate new evidence. The Bayesian account provides also an explanation for what P. Urbach has called the "strength" of a heuristic[114], which characterises the "internal belief" that working in the RP will lead to fertile results. That is to say, that a RP will be "internally" defined as progressive and accordingly change the scientific community's views only after "new" evidence has been predicted by it. Otherwise, if there is no new evidence coming in, there will be no revision of probabilities and the beliefs held by the scientific community stay the same.

Let me shortly touch on the debate about the role of "guiding rules" in methodology again: what is obvious from above consideration is that the confidence of scientists in their RP is reconstructable as an overall rational affair, insofar as the heuristic is evaluated on the basis of objective experimental outcomes, i. e. with respect to *the world*. On the other hand, "cross-examinations" between the RP and a "touchstone theory" will be important for scrutiny of the objectivity of the guiding rule, i. e. with respect to *another theory*. As J. Worrall has pointed out, one RP may be progressive but have a weak heuristic, whereas the other is degenerating but has a strong heuristic[115]. "Cross-examinations" can tell us whether the initial beliefs attached to the RPs are justified or stand in need of further revision, i. e. they are by no means a redundant affair, but contribute essentially to the project of estimating the objectivity of "metaphysical guiding rules" in science.

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[113] Cf. Lakatos, I. [1971a].

[114] Urbach, P. [1978], pp. 99-113.

[115] Worrall, J. [1978a], p. 64.

As will be shown in the last section, Bayesian computation yields a "great asymmetry", in that refutations of hypotheses present a comparatively slower decrease in probability whereas confirming instances lead to a steep increase. Consequently, a theory is able to withstand a long succession of refutations if it is punctuated only occasionally by confirming instances. Thus, the theory's subjective probability is steadily increasing on average values. Now, the Bayesian can straightforwardly explain such changes of belief, while the testing of the hypotheses takes place, whereas other approaches lack such an ability. I will hold that intra-RP decisions, as well as external decisions of theory choice, can be accounted for and that the RIC is to be seen as a necessary and a sufficient condition for rational decision making. It demands the accordance of the confidence-status, as a meta-criterion, with the temporal belief function on probability statements and will allow us to construct Bayesian conditionalisation without "logical omniscience"[116], i. e. we can construct "Bayesianism with a human face"[117]. We have seen that "strict conditionalisation" is too strong an idealisation, whereas "internal conditionalisation" is epistemologically easier to achieve when "locally circumscribed".

Now, I have said a great deal about the importance of the prediction of "novel facts" for both traditions, the Lakatosian and the Bayesian, but their position in relation to likewise unsuccessful predictions has been neglected so far. Hardline Popperians have held that such negative predictions are to be regarded as falsifying the whole theory, which in their understanding would have to be abolished altogether. On the other hand, Lakatosians have claimed that scientist's clinging to their theories when facing anomalies would be thoroughly rational behaviour[118]. If the Popperian criterion were applied, then most of those theories which are thought of as science's outstanding achievements would be regarded as completely unscientific[119]. Instead, my claim here will be that Lakatosians are justified in their evaluation of such situations in the history of science, because the Bayesian personalist approach to scientific inference offers a reasonable explanation for it.

Nevertheless, Lakatos himself has not readily provided a rationale for why a RP's occasional predictive capacity could compensate for all the failures produced and why some theories were dubbed noble enough to be incorporated

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[116] Garber, D. [1983].

[117] Jeffrey, R. [1992], chpt. 5.

[118] "The sophisticated falsificationist... sees nothing wrong with a group of brilliant scientists conspiring to pack everything they can into their favourite research programme... As long as their genius-and luck-enables them to expand their programme *'progressively'*, while sticking to its hard core, they are allowed to do it": Lakatos, I. [1979], p. 187.

[119] This point was already held by Duhem, P. [1905], where he claimed that most of these remarkable theories are not conclusively falsifiable by purely observational statements in a sufficient way. Historically, certain parts of highly successful theories have been made responsible for the false predictions derived and have been excised and replaced. But, scientists are left with the question which part to get rid of, when the possibility obtains that even distant parts of a theory can be blamed.

into the "hard core" of a RP. Scientific rationality could only be successfully reconstructed when considered with "hindsight". The determination of rational beliefs "on the spot", so to speak was either impossible or left to scientist's "methodological fiat". For the Bayesian personalist on the other hand rationality conveys that beliefs accord with the principle of ordinary probabilistic inference. However, if Bayesians reject "wholesale falsificationism" as Lakatosians do, then they have to face the so-called "Duhem problem" as well.

ii) The Duhem Problem and its Bayesian Solution[120]

The original problem reads as this: a given theory T (corresponding to some part of the "hard core" of a RP) in conjunction with an auxiliary hypothesis A implies an empirical consequence which is shown to be false by the observation of some outcome E. Rejecting wholesale falsificationism we suppose that both components of the conjunction, T and A are by themselves not refuted. Furthermore, we wish to determine the separate estimates of the probabilities of T and A given E. Assuming "conditional probabilities"[121], then BT shows:

$$(14) \quad pr\,(T/E) = \frac{pr\,(E/T)\,pr\,(T)}{pr\,(E)} \qquad pr\,(A/E) = \frac{pr\,(E/A)\,pr\,(A)}{pr\,(E)}\ .$$

For the evaluation of the posterior probabilities of T and A, we ought to find values for the terms on the right-hand side. Given that there is no symmetrical effect on the separate probabilities, then the terms allow for the estimation of which hypothesis suffers most due to the refutation. Now, for the evaluation of the posterior probabilities of T and A, we need to keep the values of pr (E/T), pr (E/A) and pr (E) fixed. This can be explicated in using the "Theorem of Total Probability" (TTP) to establish the relative likelihoods with respect to the problem-situation. As I have shown in relation to "localised Bayesianism" and the introduction of the PSE, we are (pragmatically) justified in putting TTP to work, because they allow us to construe TP relativised to a localised language, which is based on prior distributions consisting of continuous parameters accepted as approximately "true":

[120] Cf. Howson, C. /Urbach, P. [1989], chpt. 4, sct. 3.

[121] Which we will treat here only as conditional on (coherent with) our present state of beliefs (as the minimum-requirement of the personalist approach).

$$
\begin{aligned}
(15)\quad pr\,(E) &= pr\,(E/T)\,pr\,(T) + pr\,(E/{\sim}T)\,pr\,({\sim}T)\\
pr\,(E/T) &= pr\,(E\&A/T) + pr\,(E\&{\sim}A/T)\\
&= pr\,(E/T\&A)\,pr\,(A/T) + pr\,(E/T\&\,{\sim}A)\,pr\,({\sim}A/T)\\
&= pr\,(E/T\&A)\,pr\,(A) + pr\,(E/T\&\,{\sim}A)\,pr\,({\sim}A)\,.
\end{aligned}
$$

Through refutation of T&A in conjunction, the term pr (E/T&A) yields 'zero' as the result. Therefore:

$$(16)\quad pr\,(E/T) = pr\,(E/T\&{\sim}A)\,pr\,({\sim}A)\,.$$

In analogy to the above argument we will obtain also the following results:

$$
\begin{aligned}
(17)\quad pr\,(E/A) &= pr\,(E/{\sim}T\&A)\,pr\,({\sim}T)\\
pr\,(E/{\sim}T) &= pr\,(E/{\sim}T\&A)\,pr\,(A) + pr\,(E/{\sim}T\&\,{\sim}A)\,pr\,({\sim}A).
\end{aligned}
$$

[16+17, can both be taken as having the same value when considered as a sufficient approximation for some specific scientific purpose.]

Given that the terms are fixed by above procedure, then the posterior probabilities can be tentatively determined, if we apply a personalist account of probabilities. What matters are not the particular values which one obtains using those three probability terms, but rather their *relative* values with respect to each other. That is to say, that our approach is a *semi-quantitative* one, which will not depend on the precise numbers inserted, because it yields merely qualitative interpretations. The final results of the analysis are only reliable, if the calculation is insensitive to precise numbers inserted, i. e. when the probability terms are re-interpreted in the original qualitative way. In the end, by insertion of real numbers, BT yields the posterior probabilities in which we are interested[122]. These numbers are simply ones which a Bayesian personalist has assigned as an approximation of the belief states of some scientist in question who is or was about to act, *viz.* to choose between theories or to appraise the theory he is working with.

Some striking results are obtained by Dorling and Redhead, which show that observational outcomes can have clear asymmetric effects on the probabilities of T and A[123]. However, in general A will turn out to be a conjunction of auxiliary

[122] Cf. Dorling, J. [1977]; he has demonstrated in a specific example that an asymmetry between the effect of a refutation on the posterior probabilities of a theory T and auxiliary hypothesis A will obtain. Hence, this result would justify scientists in retaining T and abandoning A.

[123] As Howson, C./Urbach, P. state, those results obtained by the insertion of real numbers to the probability calculus are actually relatively insensitive to changes in the assumptions we made, i. e. "their accuracy is not a vital matter as far as our explanation is concerned", op. cit., p. 101.

hypotheses, whose combined subjective probability is to be estimated less than T, the "hard core". As Dorling[124] points out, to obtain the same qualitative results, we will only have to consider that T starts off more probable than not, also more probable than A and that E should not be readily explainable by some other rival theory in the field. Now, these are demands which can be easily accommodated by most of the common scientific theories, but even then the increase in the probability of T as a result of some refuting instance will be negligibly small. In reliance on the *actual historical* probability assignments used by those authors, BT as a personalist reconstruction can explain why scientists are more or less undisturbed by refuting instances and why they continue to work in their RP as was described by Lakatos' "negative heuristic". The Bayesian rationale requires merely that we classify the rival hypotheses in accordance with their scientific use. This does not amount to unrestricted or relativistic choice *per se*, but envisages the prior demands of scientific purposes at hand. Hence, the Bayesian approach is essentially concerned with the approximation of best solutions under uncertainty, thus, giving an adequate representation of scientific rationality as a problem solving activity. Hereby, Bayesian personalism accords not only with distinctive scientific behaviour, but conforms well with ordinary ways of human reasoning as well.

Now, something is still to be said about scientists' attitudes towards refuting instances: although we have shown that refutations need not have the devastating effect on scientific theories in general as proposed by "wholesale falsificationists", that doesn't mean that just ignoring refutations or patching-up theories in an *ad hoc*-manner is permitted. In order that personalist Bayesianism can be taken as a model of scientific rationality, we have to fit all available data into our calculus that counts in favour and against the theory in question. If there turns out to be quantitatively more evidence for the theory, so much the better: if there is less and the refuting instances accumulate, then it's high time to look for alternative theories in the field. For scientific rationality to count as such, we have to demand that all *relevant* data has to be computed and we have already shown that the notion of a "domain" will provide us with an instrument where to look for it. So far as there are rival theories within that "domain" so good, because it turns out to be easier to circumscribe the area of interest, as is implicit in Lakatos' notion of "touchstone theories".

Bayesianism as such is a means of scientific rationality not its end: it allows us to cash out Lakatos' heuristic inventory as to define what exactly scientists do, when they consider "progressiveness" or "problemshifts" in theories. But, without allowing for Lakatos' "metaphysical guidelines" to come in, we would probably not know where to start our search. Popper swayed all the time to allow "metaphysical guidelines" their rationality and was mostly alert to discount them as mere instances of the psychology of discovery. With Lakatos to the contrary,

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124 Dorling, J. op. cit., p. 184.

we might answer that they indeed have an essential role to play in the process of science. But, in order to avoid vagueness, they ought to be reconstructed in Bayesian terms to allow for the evaluation of their actual role in scientific thinking. Thus, in the last section I will show how "*enhancement ratios*" represent Lakatos' assumption of the notions of "degenerative" and "progressive problemshifts".

iii) Enhancement Ratios, Crossover Points and Problem Shifts

In further advancing the reconstruction of Lakatos' MSRPs I will mainly focus on the argument given by Michael Redhead[125]. It is based on the introduction of "*enhancement ratios*" which represent the positive change in beliefs of how well a theory is supported by available data. The reason of this venture is to model the "progress" or the "degeneration" of RPs with respect to "problem shifts". Calling E the result of an experimental test of T&A, five parameters are introduced:

$$\begin{aligned} (18) \qquad x &= pr(T) \\ y &= pr(A) \\ k_1 &= pr(E/T\&{\sim}A) \\ k_2 &= pr(E/{\sim}T\&A) \\ k_3 &= pr(E/{\sim}T\&{\sim}A) \end{aligned}$$

Here, T and A are supposed to be probabilistically independent. Subsequently, enhancement ratios are defined as $y_T = pr(T/E) / pr(T)$, $y_A = pr(A/E) / pr(A)$ and used as a representation of Dorling's personalistic interpretation of BT:

for confirmation, when T&A→E, we get

$$(19) \qquad y_T = \frac{y + k_1(1-y)}{xy + k_1 x(1-y) + k_2 y(1-x) + k_3(1-x)(1-y)}$$

$$(20) \qquad y_A = \frac{x + k_2(1-x)}{xy + k_1 x(1-y) + k_2 y(1-x) + k_3(1-x)(1-y)}$$

[125] Redhead, M. [1980].

in the refutation case

$$(21) \qquad y_T = \frac{k_1 (1-y)}{k_1 x (1-y) + k_2 y (1-x) + k_3 (1-x)(1-y)}$$

$$(22) \qquad y_A = \frac{k_2 (1-x)}{k_1 x (1-y) + k_2 y (1-x) + k_3 (1-x)(1-y)}$$

Now, an "*asymmetry ratio*" can be defined such as $AR = y_T/y_A$,
which yields:

for confirmation:

$$(23) \qquad AR = \frac{y + k_1 (1-y)}{x + k_2 (1-x)}$$

and for refutation:

$$(24) \qquad AR = \frac{k_1}{k_2} \cdot \frac{(1-y)}{(1-x)} .$$

The asymmetry ratio AR does not depend on k_3, because as defined above, it solely represents the probability of E given that T and A are both false. By application of Redhead's formalisation to Dorling's argument $k_1 = k_3 << 1$: *the requirement for partial falsification* as in Duhem's problem, i. e. E is neither in T nor in the conjunction of T and A, and $k_2 << k_1, k_3$, which shows that the probability that E is in A is even smaller than its not being in T. Further on, this presents a large asymmetry ratio for refutations,

viz. $\frac{k_1}{k_2} >> 1$, i. e. E is in T but not in A.

Also, it is possible to obtain a large value for this ratio if x is very close to unity in relation to y. As Redhead points out, the simplest model of a RP is produced by assuming that $k_1 = k_2 = k_3 = k$, whereby

$k << 1$ and $\frac{(1-y)}{(1-x)} >> 1$, i. e. the model represents Lakatos thesis that theories within the hardcore are more likely to be "true" than auxiliary assumptions, whence assuming that pr (T) > pr (A). In this case, the denominators in (19) and (20) reduce to xy + k (1- xy), the k-parameters in the numerators and denominators of (21) and (22) cancel, also in (24) and AR for confirmation (23) yields

$$\text{(25)} \qquad AR = \frac{y + k\,(1-y)}{x + k\,(1-x)} \;.$$

The result shows that for the case of refutation the enhancement and asymmetry ratios do not depend on the value of k, whereas for k << xy, $y_T \approx \frac{1}{x}$ and $y_A \approx \frac{1}{y}$.

In other words pr (T/E) ≈ 1 and pr (A/E) ≈ 1, as was expected. Now, the alterations in (19), (20), (21) and (22) allow for the modelling of the "degeneration" or the "progress" in "problemshifts", i. e. the analysis of the effects of unsuccessful and successful predictions. In the following example, accompanied by figure 1, Redhead presents how the rational choice between the hard core of a theory and some auxiliary hypotheses is to be understood:

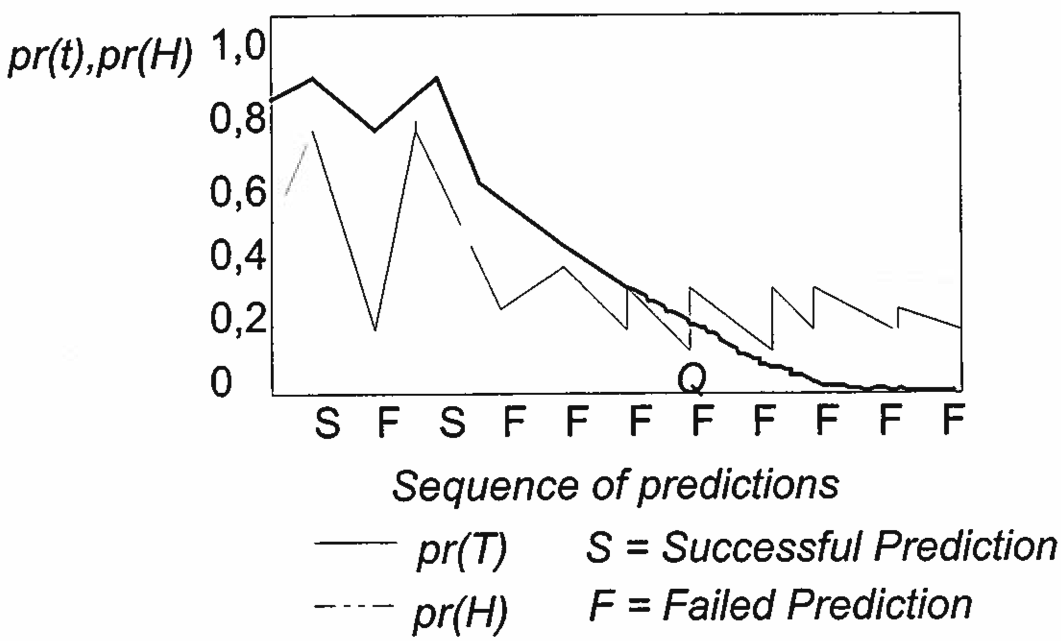

Figure 1.

We take two sequences of predictions, one for T and one for A, wherein initially two alternating successful and failing predictions were made. Both are followed by a run of failures, where *semi-quantitative* parameter values where assumed. Then, after each refutation, pr (A) is (again) taken to be 0.5 for a new auxiliary hypothesis introduced to account for the anomaly, simply assuming the new articulation of the protective belt to be triggered by such an anomaly. Although at any time pr (T) can be lifted quite high by new confirming instances, at the "*crossover point*" Q, where pr (T) and pr (A) intersect, it would become rational to make adjustments in the hard core of the theory rather than in its auxiliary belt. Hence, we now have an internal criterion for the abolition of an RP, independent of any competing RP.

What can be said about the asymmetry effect of failure on pr (T) and pr (A) is, that it is preserved independently of the quantity of successful predictions preceding the failure: Following *n* successful predictions, pr (T) increases from pr_0 (T) to pr_n (T) and pr (A) from pr_0 (A) to pr_n (A). The effect of the refutation on pr (T) and pr (A) after a succession of *n* successful predictions will be shown by y_n (T) and y_n (A). We can make use of the transformation of (19), given the conditions used by Dorling as above and obtain:

$$(26) \quad pr_{n+1}(T) = \frac{pr_n(A) + k(1 - pr_n(A))}{pr_n(T)\, pr_n(A) + k(1 - pr_n(T)\, pr_n(A))} \cdot pr_n(T) \,.$$

We can derive a similar result for pr_{n+1} (A) and transform both equations to get:

$$(27) \quad \frac{1 - pr_{n+1}(A)}{1 - pr_{n+1}(T)} = \frac{1 - pr_n(A)}{1 - pr_n(T)} \,.$$

Then:

$$(28) \quad \frac{y_n(T)}{y_n(A)} = \frac{y_0(T)}{y_0(A)} = \frac{1 - pr_0(A)}{1 - pr_0(T)} = \frac{1 - pr(A)}{1 - pr(T)} = AR \,,$$

which yields a result that is independent of *n*. Also, in the old notation, likewise independent of *n*, we obtain an asymmetry ratio for refutation after *n* successes of:

$$(29) \quad \frac{y_n(T)}{y_n(A)} = \frac{1-y}{1-x} = AR \,.$$

Depending on the real numbers inserted, we can show that pr_n (A) and pr_n (T) tend quickly towards unity with increasing *n.* Within this model of the asymmetric effect of refutations in the auxiliary belt the stronger belief in pr(T) is accommodated by taking its initial value to be closer to unity than that of pr(A). Thus, the actual behaviour of scientists to cling to the hard core of the theory they are working in is demonstrated to be a thoroughly rational one. On the other hand the abolition of T (as T_1) for A or for another RP T_2 which is demanded by the "cavalier attitude to refutation"[126] has to take place as soon as the "crossover point" is reached and our theory at hand has lost its momentum, i. e. it is degenerating with respect to A or T_2.

IV. Conclusion

In the attempt to rationally reconstruct Lakatos' MSRP, I have raised two major questions, *viz.* whether it is possible to give a rational account of the process of scientific discovery and whether it will have any importance for the appraisal of given theories. I have shown that the attempted rational reconstruction is justified on the basis of two similar approaches shared by Lakatosians and Bayesians: first, the historical revaluation of theories in a RP relates to the revision of prior probability estimates in BT and second, temporal considerations play a major role in the heuristic account of "novel confirmation" as well as in the personalist Bayesian interpretation of the probability calculus.

Focusing on the 'fact'/'evidence'-confusion in the debate about "novel confirmation", I have shown how the heuristic account can deliver necessary and sufficient conditions for novelty, as "the facts that were actually used by the scientist devising a theory", when it is based on her intentions to solve a specific scientific problem. Thus, I have given an account of 'personal evidence' with respect to the CRBK of the scientist. Further, I have shown how the personalist interpretation of BT can justify even old 'evidence' becoming a 'new fact' by its re-interpretation in a new theoretical framework, which was based on a meta-principle on temporal belief functions.

I have rejected the argument of Bayesian objectivists that Bayesian personalism is psychologist or inconsistent on grounds of the RIC and have stressed in turn that their ideal demands cannot be met by real agents. Thus, I opted for a "humanised Bayesianism" in accordance with Jeffrey's principle of conditionalisation and have presented Bayesianism further as localised on the basis of a problem relevant language, such that the objective nature of the problem can be retained, approximative solutions to the problem area be given, but the overall problem for objectivists circumvented, *viz.* how privileged prior distributions could be found.

Finally, I have shown how Bayesian personalist principles can be applied to complete Lakatos' MSRP with its main problem that it could only deliver a rational account of scientific decision making *via* "hindsight". By introduction of "asymmetry" and "enhancement ratios" it was demonstrated why Lakatos' insistence on partial falsificationism is justified, how the Duhem problem can be

[126] Dorling, J. [1977], p. 184f.

solved and that the role of metaphysical "guiding rules" can be viewed as a thoroughly rational belief in a theory's (objective) progressiveness.

As I have stated in the introduction, Lakatos has taken the edge off Popper's neo-Humean view on inductive theory support, but at the end of the day, we seek a positive solution of the problem of induction and theory appraisal, which I tried to propose on the basis of Bayesian personalism.

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