On Novel Facts

A Discussion of Criteria for Non-ad-hoc-ness in the Methodology of Scientific Research Programmes

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Zusammenfassung

Das Problem, unter welchen Bedingungen eine Hypothese oder Theorienmodifikation als methodologisch akzeptabel gilt, wird in der wissenschaftstheoretischen Tradition als die Frage des Ad-Hoc-Charakters von Hypothesen diskutiert. Das gleichartige Problem tritt aber auch in Lakatos' Methodologie wissenschaftlicher Forschungsprogramme auf, welche von methodologisch zulässigen Theorienänderungen die Vorhersage ‚neuer Tatsachen' verlangt. Über diesen Begriff der neuen Tatsache und damit der Adäquatheitsbedingungen für wissenschaftliche Erklärungen hat sich eine weitgefächerte Debatte entsponnen. In diesem Papier wird der Versuch unternommen, die Forderung der unabhängigen Testbarkeit einer Hypothese, welche im Rahmen der Diskussion des Ad-hoc-Charakters von Hypothesen eine wichtige Rolle spielt, auch für die Frage der Spezifizierung von ‚neuen Tatsachen' fruchtbar zu machen. Ich argumentiere zugunsten der Bedingung, daß eine Hypothese als methodologisch akzeptabel gelten sollte, wenn sie zumindest zwei unabhängige Tatsachen erklärt. Ein derartiger Ansatz verlangt die Kennzeichnung dessen, was als ‚eine Tatsache' zu gelten hat. Die Schwierigkeit einer derartigen Kennzeichnung ist ein notorisches Problem jedes Kriteriums, das auf unabhängige Testbarkeit zielt. Eine Klärung dieses Problems wird über das Konzept der empirischen Generalisierung versucht. Als ‚eine Tatsache' im methodologischen Sinne gilt demnach ein gesetzmäßiger Zusammenhang zwischen zwei Meßgrößen. Dies erlaubt weiterführend eine Klärung des Problems, was methodologisch als ‚ein Experiment' zu werten ist, d. h. was als Reproduktion desselben und was als andersartiges Experiment gelten soll. Mit Hilfe dieser Klärungen wird unter anderem der Ad-hoc-Charakter der Lorentzschen Kontraktionshypothese sowie das Problem der Gleich- oder Verschiedenartigkeit von Michelson-Morley- und Kennedy-Thorndike-Experiment untersucht.

I. THE PROBLEM

Hempel's paradox of confirmation proceeds from two very plausible premises to an absurd conclusion. If it is agreed that (1) any positive empirical instance of a hypothesis confirms this hypothesis and that (2) if such an empirical instance confirms a hypothesis it also confirms a hypothesis logically equivalent to the first one, one has to admit the conclusion that facts which apparently have nothing to do with a hypothesis actually confirm this hypothesis. Let 'All ravens are black' be our hypothesis. A logically equivalent form of it is: 'What is not black, is not a raven'. A pink elephant is an empirical instance of this hypothesis. We have to conclude, therefore, that a pink elephant constitutes empirical support for the claim that all ravens are black.[1] The usual reaction to this rather odd way of reasoning consists in giving a stricter sense to the notion of 'confirmation'. Not just any fact explained by a

[1] cf. Hempel 1965, 14–15; Gethmann 1984.

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theory confirms or supports this theory; on the contrary, the explanation has to meet certain methodological requirements. Conformity to the facts is only a necessary but not a sufficient condition for the acceptance of a hypothesis. In the tradition of methodological theory the need for the further qualification of a hypothesis has been discussed under the rubric of avoiding ad hoc hypotheses. What is essentially at stake, therefore, is the problem of what is constitutive for an ad hoc explanation. I want to discuss this problem within the framework of Lakatos's methodology of scientific research programmes (MSRP). Lakatos requires that the successive versions of a research programme must constitute a series of 'progressive problemshifts'. Each such version, emerging from its predecessor by adding or otherwise adjusting auxiliary hypotheses, must predict 'novel facts'.[2] But what is a 'novel fact'? In other words, what kind of evidence really supports a theoretical modification? In the last decade, an extended debate has arisen among MSRP proponents concerning the problem of giving exact criteria for the novelty of a fact. This problem roughly coincides with the traditional problem of the ad-hoc-ness of an explanation.[3] In Lakatos's methodology the concept of 'novelty' or 'non-ad-hoc-ness' lies at the heart of the problem of assessing theories. Most of the key MSRP notions such as 'progressive' or 'degenerating programmes' remain vague unless we can determine what constitutes confirming evidence. Clarifying this problem is central to elucidating the assessment problem.

What I want to propose in this paper is a novel criterion of novelty. I will argue for the following agreement about what should be considered as a non-ad-hoc explanation:

A hypothesis explains a fact in a non-ad-hoc manner, if it simultaneously explains at least one additional independent fact that either constitutes an anomaly for the rival theory or that falls beyond its realm of application, i. e. that is neither derivable from nor inconsistent with the competing approach.

In order to make this criterion plausible it is necessary first to review the discussion within the MSRP concerning the problem of novelty. In order to avoid becoming too scholastic in tone this review will have to be rather sketchy. But I will try in any event to present all the essential features of the debate.

Lakatos originally considered a fact as novel which was "hitherto unexpected".[4] This is a very strong requirement. It demands not only that a 'novel' fact was previously 'not known', but that it was also improbable in the light of accepted knowledge. As a result, this strong requirement was 'watered down' by a second condition. In addition, the 'old facts' that a programme explains in a new way should also count as 'novel' and therefore as supporting evidence.[5]

[2] cf. Lakatos 1970, 32, 48.

[3] This ist only a rough coincidence because one of the usual characteristics of an ad hoc hypothesis is that it is introduced for the sake of anomaly repair. The novel fact debate, however, is concerned with the legitimacy of *any* theoretical modification, regardless of the reason for its being introduced.

[4] Lakatos 1970, 33.

[5] 1.c., 70.

In 1973, Elie Zahar rightly analyzed Lakatos's approach to the issue as an irresolute wavering between a strict predictivism and the partly successful attempt to count new explanations of known results in favour of a research programme. Relying on the theoretical context account of meaning, Zahar pointed out that because any novel theory entails a change in the connection between some of its theoretical concepts and the aspects of reality attached to them, almost every statement of the theory can be considered as offering a new explanation. And thus it becomes a novel fact in Lakatos's weak sense.[6] This reveals the dilemma. On the one hand, our epistemological intuition requires that we credit Newton's deduction of Kepler's laws, Bohr's explanation of the Balmer series or Einstein's solution of the Mercury perihelion anomaly to the corresponding theory, although all these facts were already well-known at the time the explanatory theory was developed. On the other hand, one cannot say that any reproduction of a familiar fact by a programme constitutes a success of that programme. This would allow for the most phantastic ad hoc explanations.

Under what conditions can a fact be said to support a theory? Zahar attempts to provide an answer. In the case of a theory explicitly designed to account for certain facts, we certainly do not hold that the theory is genuinely supported by these facts. This implies that the way in which a theory is devised becomes an important part of its methodological assessment. Facts merely accomodated by the theory do not count as supporting evidence. Zahar concludes:

> This suggests the following redefinition of the notion of 'novel fact'. A fact will be considered novel with respect to a given hypothesis if it did not belong to the problemsituation which governed the construction of the hypothesis.[7]

John Worrall later summarized this criterion this way:

> one can't use the same fact twice: once in the construction of a theory and then again in its support.[8]

In general, this criterion is no obstacle to predictions. If a fact was not previously known, it will generally not have been used in constructing the explanatory theory.[9] According to Zahar, however, this criterion also implies that Einstein's theory is supported by the explanation of the Mercury

[6] Zahar 1973, 102. Noretta Koertge also criticizes Lakatos's "lapse into the we-live-in-a-different-world-after-a-revolution syndrome. If he wants to count a research programme as progressive when it is found to predict a known result, he should say just so" (Koertge 1971, 171).

[7] Zahar 1973, 103; partially italicized in the original. Zahar's criterion was partly anticipated by Whewell who stated that "the evidence in favour of our induction is of much higher and more forcible character when it enables us to explain and determine cases of *kind different* from those which were contemplated in the formation of our hypothesis" (Whewell, in: Butts 1968, 153; italics in the original).

[8] Worrall 1978a, 48.

[9] One might suspect that this is not only 'generally' but universally so, but Campbell and Vinci presented a counter-example. They consider the case of a modification of a theory which was intended to be a different explanation of a fact predicted by a rival theory which had been undetected up to that point; cf. Campbell/Vinci 1983, 327.

perihelion anomaly precisely because Einstein did not try to explain it. The solution to the problem was rather an unexpected present.[10] In addition, Zahar's approach leads to the assessment that the explanation of the retrograde planetrary movements and other immediate consequences of the crude Copernican model already supported the model and not only the first confirmed prediction, the actual observation of the phases of Venus in 1616.[11]

Zahar's approach is fraught with difficulties. In 1974, Alan Musgrave pointed out that according to Zahar's notion of support, the way in which a scientist arrived at his theory is of decisive importance in appraising this theory's methodological merits. Moreover, methodological assessment becomes an exceedingly thorny endeavour because of the role biographical details play in tracking down the steps by which the scientist arrived at his theory. This means that identical achievements by two different scientists can be appraised very differently depending upon how they reached their results.[12]

Musgrave's point can be made even stronger. His objection presupposes that at the very least the problem situation of a single scientist can be adequately reconstructed. Uncertainty in the methodological assessment would thus only emerge if several scientists formulated the same theory but were motivated by a different set of problems. The situation, however, is worse. History of science is in many cases incapable of unambiguously reconstructing a scientist's problem situation. Let me illustrate this by examining a typical dispute among historians of science concerning the origin of an important idea, say, the genesis and early development of Lavoisier's oxygen theory. Meldrum has suggested that one of Cigna's articles, published in May 1772, probably provoked Lavoisier's series of experiments in the same year since Lavoisier's first experiments on combustion dealt with the very same substances that Cigna had investigated: sulphur and phosphorus.[13] Others have proposed that the origins of the Chemical Revolution are to be traced back to the contemporary debate on the mysterious disappearance of diamond under high temperatures. At the beginning of the 1770s some scientists concluded that the phenomenon ought to be interpreted as the actual combustion of diamond. It seems plausible, therefore, that Lavoisier thought combustion processes to merit special attention.[14] Guerlac dismantles these assertions[15] and replaces them with his own account of the leading role effervescence played in Lavoisier's experiments. Lavoisier considered effervescence to be the release of air previously fixed in boddies. In pursuing this idea

[10] Zahar 1973, 257.

[11] Lakatos/Zahar 1976; according to this approach, the Copernican programme was progressive from the beginning. But it seems doubtful whether this account is well suited to the historical data. There was actually no mass conversion of astronomers to the Copernican programme (and therefore no basic value judgement favouring this programme) prior to the discovery of the phases of Venus (see Nunan 1984, 285–286).

[12] Musgrave 1974, 12–14.

[13] cf. Guerlac 1961, 77.

[14] op.cit., 78; Guerlac mentions McKie, Duveen, and Klickstein as advocates of this view.

[15] op.cit., 79–90.

further, Lavoisier arrived at a new theory of air, regarding it as a compound of an aerial base and caloric, the matter of heat. These freshly formed ideas directed Lavoisier's attention to the reduction of metallic calces where air was likewise set free. Since it was very natural to view this process as an effervescence, the next step for Lavoisier was to examine the nature of combustion and calcination. Guerlac concludes that it was

> the phenomenon of effervescence observed in the reduction of metals which pointed the way to his great series of experiments.[16]

Morris shifts the accent. He takes the genesis of Lavoisier's ideas to be the genesis of a novel theory of the states of matter. Thus, he argues that Lavoisier regarded the release of combined air and vaporization as analogous processes. The result in both cases is a compound with caloric, the matter of heat being responsible for the elastic properties. Lavoisier then developed this account into a general theory of the gaseous state, supplementing it with similar ideas concerning liquids. Morris holds that the germ of Lavoisier's early concept can be traced to the doctrine that

> fluid elasticity is not a property unique to air but is simply a *state* which other substances can assume solely on the quantity of fire matter combined with them.[17]

In other words, Morris declares the theory about the states of matter to be the primary subject of Lavoisier's endeavours, whereas Guerlac takes these ideas as a mere by-product, designed to explain the heat from combustion as the release of caloric.[18] I might also mention in passing that in Kohler's opinion it was the question of the constitution of acids that provoked Lavoisier to address the problem of combustion.[19] The traditional view, moreover, regards the problem of weight increase in the calcination of metals as the main incentive for the formation of Lavoisier's theory.[20] Hankins, at last, refuses to label one problem as the origin of the oxygen theory. After listing the problems the theory was possibly intended to tackle, he confesses significantly:

> We will never know exactly what combination of ideas and circumstances caused Lavoisier to undertake his research on combustion . . .[21]

If, following Zahar, one assumes that reconstructing the context of discovery is a necessary prerequisite for any methodological appraisal, then the problem of which data and experiments actually support Lavoisier's theory can only be solved after all doubt has been removed about the role each played in the genesis of the theory. Any account of the early stages in the development of Lavoisier's system provides its own set of supporting evidence. If one follows Guerlac and takes effervescence as the starting point,

[16] op.cit. , 106.

[17] Morris 1969, 376; italics in the original.

[18] Guerlac 1969, 382; this view is also held by Gough 1969, 267.

[19] Kohler 1972, 349–355.

[20] This traditional interpretation is advocated by Berthelot 1890, for example; cf. Morris 1969 , 377.

[21] Hankins 1985, 98.

the release of a gas in metallic reductions cannot be accepted as supporting evidence. If the problem of weight is given precedence, the assessment is reversed. Any new approach to the origins of a theory causes a shift in methodological assessment. Zahar's combination of genetic and epistemological factors runs into problems because the origins of complex and comprehensive theories are extremely difficult to unveil. Yet it hardly seems appropriate to dispense with the methodological assessment until agreement has been reached about the origins of important ideas.

The difficulties arising over the applicability of Zahar's heuristic criterion recently provoked Michael Gardner to a *coup de force*. After discussing the pitfalls of the criterion he concludes:

> And in the end it seems to me that we are forced to adopt what is perhaps the simplest and most obvious criterion of novelty with respect to a given theory: not known to the person who constructed the theory at the time he did so.[22]

Gardner, however, is not very successful in his attempt to cut the Gordian knot. As far as personal novelty is concerned, one generally has to rely on the theory constructor's own word. His contemporaries certainly have hardly any other means at their disposal. Gardner tries to get around the problem of deception or general credibility by arguing that a scientist's professional ethos not only prevents him from fabricating experimental data but also from pretending that he was ignorant of a certain fact.[23] This analogy, however, is misleading. Whereas experiments can be repeated as a means of verifying their results, we generally have to rely on the scientist himself in order to assess what he knows. Reconstructing historically a scientist's general level of knowledge means using techniques at least as esoteric as those needed to determine whether a certain fact has been used in theory-construction. Because Gardner's criterion makes methodological appraisal dependent on the knowledge of a scientist, it seems to imply that in the case of identical hypotheses made on the basis of same evidence, the less a scientist knows, the more praiseworthy his theory is. If one bears in mind that Lakatos's methodological theory is intended to be part of Poppers's world 3 and that it therefore deals with problems and theorems, and not with their status in the mind of the scientist, then the implications of Gardner's model should make it clear that things simply don't work this way.

In 1978, John Worrall presented a more sophisticated version of Zahar's notion of a novel fact. He grants that methodological appraisal does depend on the approach to a theory, but he divests this approach of all personal factors. For Worrall the objective heuristic of a research programme, i. e. the research strategies associated with a programme, is what is important. What merits praise are no longer the origins of a hypothesis or the way an individual

[22] Gardner 1982, 10.
[23] op.cit., 11.

scientist arrives at his theory but the agreement between a theory and the heuristic of the respective research programme.[24]

In general it seems that recent work on the development of the MSRP is characterized by two general tendencies: (1) the attempt to develop an objective interpretation of the heuristic criterion for novel facts and (2) a greater degree of emphasis than in Lakatos on the positive heuristic. While Zahar's early writing occasionally conveyed the impression that an individual scientist's private associations and motives determine the value of his work, both he and Worrall subsequently try to eliminate all psychological overtones from the heuristic criterion. At stake is whether there are independent theoretical reasons for developing a hypothesis or whether it can be simply read off from the results. Worrall and Zahar, in other words, seek to establish clear-cut, i. e. objective and precise criteria for judging whether a fact has been 'used' in constructing a theory. This issue, however, has not yet been satisfactorily settled. A detailed account has only been developed for some individual cases such as the adjustment of parameters.[25] Those cases where the methodological assessment could be guided by this definition, however, are not very common in the history of science. Nevertheless, the general direction is clear. The question is less the actual construction of a theory than its logical constructibility. What is important are not the facts which may have actually played a role in theory construction, but those which are indispensible for its construction. Gerard Radnitzky has taken this final step towards an exclusively logical version of the heuristic concept of support. He scrupulously avoids mixing the contexts of discovery and justification and re-establishes a strict separation between them. A fact cannot be used in support of a theory, if it is also required for the design of that theory.[26]

This logically neat variant of the heuristic criterion suffers from a lack of applicability in practice by allowing for methodological appraisal only in limited cases. It seems rather plausible, on the one hand, that predicted, i. e. hitherto unknown facts are not necessary for theory construction. On the other hand, the adjustment of parameters indicates the inevitable reference to experimental results. The really interesting intermediate realm, however, remains untouched, leaving the problem of non-ad-hoc explanation of known facts unresolved. In advocating such a sterile logical version based on increasingly elaborate analysis of increasingly idealized cases the MSRP runs the risk of losing touch with scientific practice. It risks abdicating its role as the key to methodological appraisal. Ranging from psychological relativism to

[24] Worrall 1978a, 51, 59–60; 1978b, 325; this criterion is also foreshadowed in Whewell's writings. The adequacy of auxiliary hypotheses should be judged by considering the progress of theories. In true theories "all additional suppositions *tend to simplicity* and harmony . . . we have thus a constant convergence to unity. In false theories, the contrary is the case. The new suppositions are something altogether additional; – not suggested by the original scheme; perhaps difficult to reconcile with it" (Whewell in Butts 1968, 155 italics in the original).

[25] Worrall 1978b, 325; cf. also Zahar's explanation of Planck's analysis of Kaufmann's experiment; Zahar 1978, 71–97.

[26] Radnitzky 1979, 242–244.

inapplicable logicism, all versions of the heuristic criterion appear equally unattractive.

The second tendency peculiar to the development of the MSRP was the increasing emphasis placed on the heuristic power of a programme. In 1971, in response to Noretta Koertge's objections[27], Lakatos conceded that greater heuristic power may constitute the a-priori superiority of a programme prior to all empirical tests.[28] Similarly, Zahar sought the rationale of Planck's and Minkowski's early conversion to Einstein's relativity programme in relativity theory's superior heuristic power, although up to that point the theory's potential had not led to confirmed predictions or to novel facts in Zahar's sense.[29] Worrall goes even farther by placing the criterion of heuristic power on the same level with the progress or degeneracy of a programme.[30] In the face of such developments, however, I cannot refrain from advancing some reservations. Kuhn has pointed out that the possible conflict between several epistemological standards of equal status rules out an unequivocal assessment of a proposed hypothesis. Thus a theory can exhibit high conformity to the facts while at the same time contradicting other well-approved and experimentally confirmed views. On the other hand, a theory may display great fertility but suffer from conceptual inconsistencies. Kuhn regards this problem as one source of the possible incommensurability between two theories or paradigms.[31] In my opinion, one of the primary merits of Lakatos's methodology is his replacement of the traditional multitude of contrasting or even, conflicting methodological demands with a single, precise, and sophisticated criterion of fertility, supplemented by empirical demands. This avoids Kuhn's problem. Reintroducing a number of possibly conflicting yardsticks for methodological appraisal means that none of the competing theories can be unambiguously assessed methodologically, not even in retrospect. Methodology can do nothing but shrug its shoulders and suspend judgement. This is certainly no recommendation for philosophy of science.

Unfortunately, the criterion of heuristic power also appears rather unprecise. Worrall appraises the heuristic of Ptolemy's programme as weak, because one had to wait for the occurrence of anomalies and was not able to foresee them[32]; Musgrave thinks it was strong because it almost provided an algorithm for the solution of observed anomalies.[33] But even assuming that the concept of heuristic power could be rendered precise, it seems insufficient to use the promise of theoretically progressive problem shifts (and nothing else is offered by a powerful programme heuristic) as the acceptance criterion of a programme. The promise of success, even if it is well-founded, is alone not a sufficient criterion for the adoption of a theory.

[27] Koertge 1971, 160–173.
[28] Lakatos 1971b, 176.
[29] Zahar 1973, 241.
[30] Worrall 1978a, 63–64.
[31] Kuhn 1977, 320–339.
[32] Worrall 1978a, 60.
[33] Musgrave 1978, Note 35, [illegible]

A more promising approach is suggested by Larry Laudan's distinction between criteria of acceptance and criteria of pursuit. In modifying Laudan's approach, I propose to take empirical progress as the rationale for accepting a programme, or at least one of its stages, and the power of heuristic as a good reason for pursuing it further. This distinction helps to clarify the kind of competition between phenomenological and atomic-kinetic thermodynamics. According to Peter Clark, the heuristic was weak in the former but strong in the latter. On the other hand, whereas phenomenological theory made advances in the last decades of the 19th century, statistical theory degenerated.[34] One would expect in such a situation that the phenomenological theory was being accepted and the other abandoned. But at the same time, attempts should have been made to overcome the phenomenological theory and further kinetics. This is precisely what happened. Heuristic power, taken as criterion of pursuit, rationalizes the attempts to transcend phenomenological thermodynamics and explains why the kinetic tradition survived as an undercurrent rather than being rejected outright. Interpreting events in this way, however, leads to very different conclusions than those reached by Laudan. Laudan considers that the rate of increase of the theory's 'problems-solving effectiveness' establishes the rationality of pursuit[35], whereas I hold the development of fruitful lines of research to be of decisive importance in this matter. The implication here is that a programme is worth pursuing when it raises many new and interesting problems and thus promises novel insights. Laudan's criterion, on the contrary, disregards such a phenomenon because it diminishes problem solving effectiveness.

II. THE PROPOSAL

Is it all possible to develop a viable criterion of acceptance, if we reject a heuristic criterion of acceptance because it seems to over-estimate the importance of conceptual guidelines in methodological assessment or because, as in the versions we have examined, it is caught between the scylla of psychological relativism and the charybdis of logically disinfected inapplicability? In 1974, Alan Musgrave suggested that since Lakatos's methodology deals with competing theories, it would be reasonable to count as supporting evidence the explanation of those facts which are prohibited or at least improbable in the light of the best rival theory available.[36] 'Novel', in Musgrave's sense, no longer implies that a fact is prohibited by the entire background knowledge or that it was previously unknown to science. His notion of novelty refers instead to facts which could not have been expected from the best rival theory available. Such a criterion exalts the explanation of facts which conflict with the competing account or which cannot be otherwise

[34] Clark 1976, 75–91.

[35] Laudan 1977. 111; for Laudan "the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates" (op.cit., 68).

[36] Musgrave 1974, 15–23; 1978, 182–186, Note 17.

explained. Furthermore, it permits the extension of a theory into realms of reality about which the adversary theory had nothing to say. Musgrave's approach thus rewards the explanatory achievements of a theory and considers the explanation of already known results to be such an intricate task that it is difficult to complete it satisfactorily. A strict predictivism regards accounting for known facts as a mere prerequisite to taking part in the essential 'chase after predictions'. By focusing on anomalies, Musgrave redirects the MSRP into a more Popperian track. Anomalies should be taken more seriously than they are by Lakatos. Lakatos's scientist has such nonchalant confidence in his positive heuristic that he refuses to indulge in observations, and "will lie down on his couch, shut his eyes and forget about the data".[37] According to Musgrave's approach, a theory is supported by all the facts that it explains, whereas the best rival theory available fails to account for them, regardless of whether these facts were previously known or the theory was designed for their explanation.

Musgrave's theoretical concept of support seems to be slightly ambiguous. The notion of the 'best rival theory available' may refer on the one hand to the immediate precursor within the same programme. In this case, the support of the current version of a programme is compared to the most recent one. On the other hand, Musgrave's approach may compare the respective achievements of two rival programmes, thus expressing the support of the entire programme compared to its competitor. I will speak of intra-programme support in the first case and of inter-programme support in the second one.

Worall criticised Musgrave's view by claiming that it fails to solve the problem of ad hoc explanations. Let a fact e be discovered independently of two rival theories T_1 and T_2 and let them both be incapable of explaining e. The protagonists of T_1 and T_2 may now easily produce a slightly modified version of their respective approaches so as to account for the difficulty. Which theory is actually supported by e depends solely on which one first succeeded in explaining the intricacy. If, for example, the adherents of T_1 propose hastily T_1' which is in fact nothing else but a conjunction of T_1 and e, then e supports T_1' and henceforth cannot support any version of T_2.

> Any theory no matter how 'cooked-up' can, on this theoretical view, gain support from the facts so long as it is the first to explain them.[38]

Worrall's objection holds. The theoretical criterion can only be maintained if one requires qualified anomaly solutions, i. e. if one sets forth criteria to exclude ad hoc explanations. One should neither dispute the methodological importance of anomaly solutions nor admit anomaly solutions indiscriminately. The neglect of the second aspect constitutes the weakness in Musgrave's version of the theoretical criterion.

The same fault is present in Richard Nunan's approach. Nunan recently

[37] Lakatos 1970, 50.

[38] Worrall 1978b, 331; Worrall's objection virtually relies on Watkins's antitrivialization principle: "any philosophical account of scientific progress must be inadequate if it has the (no doubt unintended) implication that it is always trivially easy to make theoretical progress in science" (Watkins 1984, 166; partly italicized in the original).

'warmed up' the inter-programme version of Musgrave's criterion. According to him, a fact

> is novel with respect to a given hypothesis if it has not already been used in support of, or cannot readily be explained in terms of, a hypothesis entertained in some rival research program.[39]

But Nunan, like Musgrave, does not sufficiently consider the problem of ad hoc explanations. He seems to associate ad-hoc-ness in some way with heuristic incoherencies, but gives no details.[40] Likewise, if the reference to 'readily explained facts' is intended to exclude ad hoc accounts, some less cryptic hints are required. If scientists could reach an inter-programme agreement concerning the methodological requirements an explanation has to meet in order to be regarded as scientific or acceptable, Nunan's failure to tackle that problem would be insignificant. But this is hardly the case. Structural chemistry sought to explain the cohesion of bodies by assuming the existence of particular particle figures which, by fitting together, cause particles stick to each other mechanically. This explanation is no longer accepted by Newton. He states:

> And for explaining how [cohesion] may be some have invented hooked atoms, which is begging the Question.[41]

On the other hand, the Leibniz-Clarke-correspondence provides ample evidence that Leibniz regarded the Newtonian explanation in terms of universal attraction as no explanation at all. We are at a loss how to apply Nunan's criterion in such cases of programme dependent and therefore variant standards for explanations, cases where the protagonists of one programme claim to have readily explained a fact whereas the opponents dispute this claim. This failure is due to Nunan's neglect to specify methodological requirements for a scientific explanation. Only by establishing such requirements would it be possible to weigh and to judge with hindsight the variant claims entertained by both sides.

What could such methodological standards be like? Lakatos seeks to preclude ad hoc adaptations by stating that: "A given fact is explained scientifically only if a new fact is also explained with it."[42] In another passage he indicates that those facts are to be considered novel which are improbable in the light of or even forbidden by a rival theory.[43] We cannot ignore that Lakatos sometimes advocated a strict predictivism and that he was occasionally inclined to take a mere novel explanation of known facts as supporting evidence. Nevertheless, Lakatos's writings also seem to provide a foundation for the further development of the theoretical criterion. I would like to argue that we should consider a hypothesis as explaining a fact in a non-ad-hoc manner if the very same hypothesis explains at least another independent fact which either constitutes an anomaly for the rival theory or falls beyond its

[39] Nunan 1984, 279.
[40] see ibid., 285, 291.
[41] Newton 1730, 388.
[42] Lakatos 1970, 34.
[43] ibid., 32.

realm of application, i. e. which is neither derivable from nor inconsistent with the competing approach. The value of genuine predictions is an immediate consequence of this version of the theoretical criterion. Successfully predicted facts generally were not expected on the basis of the competitor. Naturally, the competitor is also invited to account for the difficulty thus created, but the advantage achieved by the rival can only be compensated for if the competitor explains a second anomaly or extends its realm of application. In other words, a theory derives support from its excess content, i. e. the explanation of those facts the rival theory cannot explain regardless of whether they were previously known.

Newton's mechanical theory, for example, explained Galileo's and Kepler's laws. Bohr's atomic model not only solved the stability problem of the hydrogen atom, but also accounted for the hydrogen spectrum. The prediction of Neptune's existence not only reconciled Newtonian theory with the apparently irregular movement of the other planets, but also allowed for the prediction of the regular movement of a new astronomical body (i. e. Neptune). Einsteins's theory of General Relativity not only accounted for the anomalous rotation of the Mercury perihelion but predicted in addition a value for the bending of light rays in the sun's gravitational field. On the other hand, when Maxwell replaced the atomic model of elastic spheres with a conception of atoms as centres of force, he cleared up the unexpected proportionality of viscosity and temperature but nothing else.[44] In other words, Maxwell's explanation was ad hoc.

Of course, ad-hoc-ness does not necessarily have to be a lasting or, as it were, fateful property of a hypothesis. It may well be the case that the full implications of a theory for experimental research are not recognized when the theory is formulated but only at a later stage. In addition, the refinement of other theories may entail new inter-theoretic relations and laws which allow for the deduction of observable implications of the hypothesis in question. For these reasons, I suggest to distinguish between the two following cases:

No independently testable implications are *known* (ad hoc_1). No such implications have been *actually confirmed* (ad hoc_2). Characterizing a hypothesis as being ad hoc_1 or ad hoc_2 is, therefore, time-dependent.[45]

[44] See Clark 1976, 54.

[45] Methodological literature provides us with a vast number of conditions for the ad-hoc-ness of a hypothesis. Apart from the concepts already discussed (i.e. apart from the predictivistic and the post-Lakatos versions of the heuristic criterion) I am aware of no less than eight different notions of ad-hoc-ness (and I do not, of course, pretend to know them all).

(1) No excess empirical content or no independent testable consequences actually *exist* (Lakatos 1971a, 112; Grünbaum 1976, 337).

(2) No such consequences are *known* (Lakatos 1971a, 112; Grünbaum 1976, 336).

(3) No such consequences are *confirmed* (Zahar 1973, 101; Grünbaum 1976, 334).

(4) All such consequences are empirically *refuted* (Lakatos 1971a, 112).

(5) It is assumed that a hypothesis is independently testable, *but* it is further assumed (motivated by the claims of some rival theory) that the hypothesis will fail in subsequent experimental tests (Grünbaum 1973, 717).

(6) The hypothesis is empirically ad hoc in the senses (1) or (2) or (3) *and* has furthermore no

What we now need is a criterion for determining single facts. This separation problem constitutes a notorious difficulty for any account which relies on some sort of 'independent testability' for characterizing non-ad-hoc explanations. Hempel suspects that a logically sharp criterion for distinguishing between different facts does not exist at all. To clarify the notion of an 'independent consequence' it is necessary instead to refer to pragmatic notions such as 'significantly' or 'interestingly' different observational consequences.[46] I want to propose a solution to the separation problem based on the concept of low-level empirical generalization. As described above, a 'fact' in the methodologically relevant sense is not to be considered as a singular statement about something happening at a certain place and time. A fact should be regarded instead as an empirical regularity which occurs repeatedly if certain circumstances are realized. If one bears in mind that the assessment criterion presented requires that a hypothesis solves at least two anomalies, then the empirical generalizations should be identified with the 'facts' to be explained. One only has to ask oneself what could possibly constitute an anomaly for or an extension of a theory. Certainly not a singular statement about a curious event. Only if such a curiosity is reproducible does it become more than a *feu follet*.

If we agree that facts are to be understood as empirical generalizations, the next question is: What constitutes a 'single' empirical generalization? A promising answer seems to be: A 'single fact' is a law-like relation between two variables that are observationally or experimentally detectable. Boyle's law (pV = constant), for example, expresses such a relation between pressure and volume, viz. between two experimentally susceptible parameters. In other words, Boyle's law constitutes a single fact. It is important to realize that an empirical generalization of such a kind in addition to the explicitly stated regularity also (tacitly) contains a ceteris-paribus-clause in the form: either there do not exist any further variables which might influence the experimental outcome or such variables are kept constant. In the case of Boyle's law the ceteris-paribus-clause is only valid if no temperature changes occur. In other words, an empirical law-like generalization entails the claim that unless explicitly stated otherwise no further variables are influential on the experimental outcome. Without such a ceteris-paribus-clause an empirical law does in fact not state anything, because every unexpected, anomalous result can be attributed to the presence of an unknown influence. In order to make testing at all possible, it is necessary to understand an empirical law as the conjunction of the explicitly stated relation and a ceteris-paribus-clause. The empirical

theoretical plausibility or sanction (Grünbaum 1976, 333–337).

(7) There are confirmed consequences *but* the hypothesis is not in accordance with the heuristic of a research programme (Lakatos's 'ad hoc$_3$') (Lakatos 1971a, 112). In other words, concept (6) holds theoretical plausibility to be a *sufficient* condition for non-ad-hoc-ness, concept (7) views it as a *necessary* one.

(8) A necessary condition for an ad hoc hypothesis is its 'non-fundamentality'; it 'fails to go to the heart of the matter' (Leplin 1982, 237).

[46] cf. Grünbaum 1976, 350.

detection of an additional parameter that influences the experimental result (provided that this influence is of law-like fashion) therefore constitutes an additional fact. The ideal gas law $p \cdot V = R \cdot T$, for example, entails Boyle's law if the temperature T is constant, and introduces an additional temperature-dependance. That is, it implies in addition a law-like relation between pressure and temperature and between volume and temperature. Each of these two-parameter-relations is (with the proviso of the ceteris-paribus-clause) experimentally testable and indeed valid. The ideal gas law therefore embodies three facts.[47]

In order to clarify the separation criterion let me apply it to the empirical support of Torricelli's 'air-pressure hypothesis'. Torricelli's new idea was that we all live at the bottom of a vast 'sea of air'. Because of its weight the air exerts a pressure upon all things on the surface of the earth. This new conception eventually supplanted the old horror-vacui theory, according to which nature abhors the void. By means of this hypothesis Torricelli was able not only to explain the notorious suction pump anomaly, that is, the formerly mysterious fact that such devices failed to pump water higher than about ten meters, but also to predict a relation between the specific weight of the liquid used in a 'barometer' and the height to which it rised. He tested his prediction by replacing water with mercury and indeed confirmed that the ratio of the height of climb of the mercury column to the height of a water column was equivalent to the ratio of their specific weights. A second prediction is entailed by Torricelli's hypothesis. There should exist a relation between the height of climb of a liquid column and the height above sea-level. On high mountains, for example, the weight of the air above, which balances the weight of the liquid, is smaller than at sea-level. This results in a reduced height of climb of the liquid column. This second prediction was verified in the famous Puy-de-Dôme experiment induced by Pascal.

According to the proposed criterion for the separation of facts, all the three regularities constitute different facts. First, there was an explanation of an enigmatic failure in the functioning of pumping devices which had not been previously accounted for. In other words, Torricelli explained how the formerly mysterious failure in the regular functioning of suction pumps depended on the height to which the water was to be raised. Secondly he predicted a relation between the height of climb and the specific weight of the liquid used. Thirdly, his hypothesis implied a causal relationship between the height to which a liquid rised and the height above sea-level.

It is important to distinguish clearly between the empirical problem of the verification of a two-parameter-relation, that is the problem of establishing the facts, and the methodological problem of assessing the quality of an explanation of these facts. One can indeed question whether the relation Torricelli established between height of climb and specific weights is really

[47] Matters would be different if a set of two-parameter relations implied a further two-parameter relation. The logical consequences of experimental laws must not count themselves as separate laws.

sufficiently tested by an experiment involving only two substances. Further experiments with other liquids, however, do not constitute separate facts themselves, but only test whether one fact really proves correct. The same holds for the Puy-de-Dôme case. The repetition of this experiment on other mountains or even in areas below sea-level serves only to establish one fact. These considerations indicate that we have to distinguish between the methodological problems involved in the concept of the 'sufficient' empirical confirmation of facts (i. e. low-level empirical generalizations) and the notion of 'sufficient' explanatory power of higher level hypotheses.[48] In this paper, only the second aspect is treated. For the methodological point of view adopted here, an empirical regularity is not an explanans, but an explanadum.

III. THE APPLICATION

Since a theory is best understood when it is put into practice, I want to discuss two examples of applying the assessment and separation criteria. In order to illustrate the two criteria I have chosen affinity theory, a research programme in 18th century chemistry, and the notoriously problematic ad hoc character of Lorentz's contraction hypothesis.

Let me first sketch L. B. Guyton de Morveau's contributions to the affinity programme so as to appraise them subsequently in the light of the criteria presented above.

In the 18th century the dominant view about the origins of the chemical behaviour of substances assumed the existence of interparticulate forces or affinities. These forces were supposed to act electively, i. e. to be specific to each pair of substances, and to remain constant regardless of factors such as temperature or the quantity of the substances employed. The order in which substances precipitate each other out of solutions indicates the order of affinity. If, for example, terra ponderosa pura ($BA(OH)_2$) is added to a solution of vitriolated tartar ($K_2(SO_4)$), heavy spar ($BaSO_4$) precipitates, leaving a residue of caustic potash (KOH) [$Ba(OH)_2 + 2\,K_2SO_4 \rightarrow BaSO_4 + 2\,KOH$]. The common interpretation of this reaction at the time was that terra ponderosa pura displays a greater affinity to vitriolic acid than to caustic potash.[49] Beginning in the 1770s one of the foremost aims of the affinity theory was to express the basic concept of elective attractions numerically, i. e. to transform the qualitative order, known through the precipitation reactions, into a quantitative series indicating the strength of the interparticulate forces. In 1775, Guyton began a series of experiments designed to achieve this aim. He tried to base the measurement of affinities upon the measurement of adhesions. He then determined the adhesive strength by measuring the force required to detach little metallic discs from the surface of liquids after controlling for their weight.

[48] Hempel makes a roughly analogous distinction. He distinguishes between 'explanation-seeking questions' and 'reason-seeking or epistemic' questions. Whereas the first type of question concerns the reasons *why* something happened, the second type refers to the reasons *that* something happened (Hempel 1965, 334–335).

[49] Bergman 1775, 28–32.

Guyton began his set of experiments by attacking the view that adhesion was not an effect of chemical forces but was instead brought about by atmospheric pressure. He determined the forces necessary to detach a glass disc from the surfaces of different liquids at constant pressure and found the nature of the liquid to be a significant factor. Moreover, he also demonstrated that a disc still cleaved to mercury in a vacuum. After having established the chemical origin of adhesion in this way, Guyton extended his experiments to discs of tallow. Since their adhesion to several aqueous solutions proved to be stronger than that of glass discs, Guyton assumed that an attractive force existed between fat and water and not, as was generally assumed, a repulsive one. In a similar way he also demonstrated the existence of an attraction between glass and mercury.[50]

After these preliminary examinations Guyton began the second phase of his experiments: measuring affinities in terms of adhesion. He placed different metallic discs of equal size, i. e. of equal active area, on mercury and measured the force required to separate them. The order of affinity turned out to be equivalent to the order of adhesive strength. In other words, the succession of substances in the apparently physical property of adhesion coincided with the order in chemical attraction, i. e. with the order in which the different metals precipitated and replaced each other in solutions in mercury. For Guyton these results proved beyond any doubt that adhesion and affinity rested on a common foundation.[51] After successfully reproducing the qualitative affinity series via a quantitative determination of adhesion, Guyton concluded that the force of adhesion was a quantitative measure of affinity strength.

How should we evaluate Guyton's conclusions? First, it should be mentioned that Guyton was a fervent Buffonian. Buffon, in his mammoth *Histoire naturelle,* had advocated the view that all forces of inanimate nature, affinities explicitly included, can be traced back to gravitation. The seemingly increased strength of chemical as compared to gravitational attraction is due in reality to the small interparticulate distances. Furthermore, the dependance of chemical action on the nature of the chemical substances can be explained by taking into account the different shapes or forms of the ultimate particles.[52]

Guyton's procedure for measuring affinities appears to be an MSRP success story for Buffonianism. From his basic Buffonian conviction Guyton deduced that

a) only one single force is active in all chemical processes and that
b) this force is attractive.

He concluded that if adhesive forces are not the result of physical circumstances, i. e. atmospheric pressure, but of chemical origin, then these forces must be identical to the forces responsible for solution, i. e. affinities. This conclusion led to the first claim: the order of the strength of adhesive forces is identical to the order of affinities. His second claim was closely

[50] cf. Smeaton 1963, 61–62.
[51] Guyton 1786, 467.
[52] Buffon 1770, 1–10; 1774a, 1–36; 1774b, 57–64.

related to the first: all facts which seemingly militate against the operation of attractive forces, e.g. the insolubility of fat in water or the downward bent surface of mercury in glass-tubes, facts that were generally explained on the assumption of repulsive forces, must on closer inspection turn out to be the result of the action of attractive forces. Guyton succeeded in confirming both claims.

If the proposed assessment criterion is taken as the basis of judgement, Guyton's experiments can be regarded as supporting a Buffonian position. Guyton arrived at two novel explanations of phenomena that were not previously explained within the Buffonian programme but rather constituted anomalies for it. One suspects, however, that Guyton needed two hypotheses to arrive at two explanations so that each hypothesis falls short of the requirements of the assessment criterion. I will treat the problem of the individuation of hypotheses more systematically in the next section, but in this case matters seem rather clear. If we agree that empirical regularities contain a ceteris-paribus-clause, we can also admit the same in the case of explaining hypotheses. In other words, an explaining hypothesis tacitly entails the claim that unless otherwise stated the relation between the empirical or theoretical quantities it expresses is the only relevant relation between these quantities. Put this way Guyton's hypothesis (a) is just the explicitly stated ceteris-paribus-clause of hypothesis (b). This leads to the conclusion that both assumptions actually constitute one hypothesis.

But the story does not end here. At this point Guyton had to ask himself whether precipitations can be explained if the action of repulsive forces is excluded? He proposed a theory of solutions[53] according to which a substance is soluble in a solvent if the particle density of the solvent and the dissolved substance are approximately equal. If this condition of equiponderance is violated, precipitation occurs as a direct consequence of the difference in specific weight. The apparent repulsion in precipitation processes is in reality just a result of gravitational attraction. Thus oil does not dissolve in water because of the unequal densities of the two materials. This leads to the following claim: if one employs a solvent less dense than water, spirit of wine (alcohol) for example, oil ought to dissolve in it. Since this proved to be the case, it constituted the first intra-programme extension effected by the theory of solutions. A second consequence was that the precipitation of a previously dissolved substance occurs when the density of the solvent is diminished. In fact, adding spirit of wine to an aqueous saline solution or water to a solution of bismuth in nitric acid had precisely the expected effect.

Applying the separation criterion shows, however, that both facts cannot be labeled as 'independent'. They are both empirical instances of the very same empirical regularity, namely the claimed relation between the specific weights of solving and solved substances. It is wholly irrelevant for this issue that in one case the product is a solution and in the other one a precipitation. This diversity only concerns the credibility of Guyton's empirical claim and does

[53] For Guyton's theory of solutions see Smeaton 1963, 58.

not express separate facts. In other words, Guyton's theory of solution is not endorsed by the assessment criterion.

A much debated problem in the philosophy of science is the ad-hoc-ness of Lorentz's contraction hypothesis. This problem is generally discussed in terms of the difference between the Michelson-Morley-experiment (MME), which led to the formulation of the hypothesis, and the Kennedy-Thorndike-experiment (KTE). It is usually asked whether MME and KTE are different in kind, so that a positive outcome of the KTE would have supported Lorentz's electron theory. Grünbaum advocates the view that both experiments *are* of different kind and that because of this the KTE *is* an independent test for Lorentz's contraction hypothesis.[54] Leplin, on the other hand, is more cautious on this point.

> Was the Kennedy-Thorndike experiment, for example, sufficiently different from the Michelson-Morley experiment to constitute an independent test of CH [contraction hypothesis]. . .? No general principle of individuation for experimental types is available to decide such cases.[55]

Leplin, therefore, suspends his judgement. Let us look where the criterion proposed here leads us to.

As is well known, in the year 1881 Michelson (together with changing collaborators) began a series of experiments to verify the existence of an 'aether drift' caused by the relative motion of the earth through the aether. According to the aether theory, the speed of light is constant relative to the aether, i. e. to the system at absolute rest. Relative to an observer moving with respect to the aether, however, the measured velocity of light should depend on the observer's state of motion, according to Galileo's law of the addition of velocities. The (two-way) speed of light can be measured by reflecting a light signal off a mirror (at known distance) back to the origin and recording the elapsed time between emission and return. According to the aether theory, such a light signal should have different velocities if it is emitted in the direction of the earth's motion through the aether or perpendicular to it. Michelson devised his experiment to detect this effect. He used an interferometer with two perpendicular arms of equal length. If two light signals are emitted at the same time, one in the direction of the earth's motion and one perpendicular to it, both should need a different time for travelling along a path of equal length. This time difference should be traceable by interference effects.[56] As no such effects occurred, Lorentz designed his contraction hypothesis, according to which the motion of a body through the aether leads to a contraction of this body in the direction of its motion. The contraction factor $(1 - v^2/c^2)^{1/2}$ was such that the shortening of the one arm of the interferometer exactly compensated for the difference in the travel time of both light rays. Lorentz's theory led to the following general expression for the time difference in such interference experiments:

[54] Grünbaum 1976, 349–350.
[55] Leplin 1982, 244.
[56] For the details compare French 1971, 49–55; Born 1920, 185–188.

$$\Delta t = \frac{2\,(l_{10} - l_{20})}{c}\left(1 - \frac{v^2}{c^2}\right)^{-1/2} \qquad (*)^{57}$$

l_{10} and l_{20} are the respective lengths of the interferometer arms when unaffected by the motion through the aether, c is the speed of light in the aether system, v the velocity of the earth relative to the aether. In the case $l_{10} = l_{20}$ (MME), Δt reduces to zero, in accordance to the Michelson-Morley result. In the case of the interferometer arms having different lengths, however, there should, on the Lorentz account, occur a time difference with corresponding interference effects. The KTE of 1932 was undertaken to test whether these effects actually occurred. Again, the answer was negative.[58]

Beginning, probably, with Poincaré's acid-tongued remarks on the poor appearance of Lorentz's electron theory[59], the methodological assessment of the contraction hypothesis has been subject to extensive debate in the philosophy of science. On the basis of the *heuristic* criterion for the novelty of facts, Zahar claims that the Lorentz contraction hypothesis was *not ad hoc* because it was suggested by the heuristic of Lorentz's programme. Lorentz, according to Zahar, derived the contraction hypothesis from a more fundamental theory, the 'Molecular Forces Hypothesis' (Zahar's term), which states that molecular forces behave and transform like electromagnetic forces. The origin of this hypothesis had nothing to do with the MME. Zahar concludes:

> My claim is that the [contraction hypothesis] is non ad hoc, and that Michelson's result . . . supported [Lorentz's programme].[60]

The validity of Zahr's claim is denied by Kenneth Schaffner. He concedes that the molecular forces hypothesis actually suggested the contraction hypothesis, but argues that this hypothesis was itself ad hoc in the heuristic sense.[61] This discussion indicates once more the validity of the inapplicability objection made in the first section with respect to the heuristic criterion.

Relying on the independent testability criterion Grünbaum claims that the contraction hypothesis was not ad hoc because the KTE provides an independent test of this hypothesis. The contraction hypothesis, therefore, actually possessed independent falsifiable consequences and it was in fact

[57] French 1971, 62.

[58] In fact, apart from the different arm lengths, a second distinction exists between MME und KTE. The MME only tested the isotropy of the velocity of light, within *one* inertial system, whereas the KTE compared the velocities of light between several inertial system, moving with different velocities with respect to the aether (cf. French 1971, 71). In the following discussion, this second aspect is left out of consideration because it introduces no novel methodological aspects.

[59] Poincaré 1902, 182.

[60] Zahar 1973, 107.

[61] Schaffner 1974, 49–53.

falsified by the negative outcome of the KTE.[62] Popper[63] and Zahar[64] agree. According to this account, MME and KTE were of different kind, so that a positive result of the latter would have supported Lorentz's theory.

How can we appraise this situation in terms of the proposed assessment and separation criteria? We first have to ask whether the contraction hypothesis was ad hoc. A look at equation (*) reveals that *it was not*. The contraction hypothesis predicts *two* experimentally susceptible two-parameter relations. One between the transit time difference of the light rays and the relative velocity of the earth through the aether and a second one between the same time difference and the difference in the interferometer arm lengths. To put it more precisely, the application of the separation and assessment criterion taken together leads to the conclusion that Lorentz's hypothesis was never ad hoc_1. This implies the appraisal that the contraction hypothesis had the chance of becoming an acceptable explanation. Matters ran different and so it failed (i. e. it never lost its ad hoc_2 status). But it is important to realize that it failed for empirical and not for methodological reasons.

This appraisal is quite independent of any claim about a possible difference between MME and KTE. In fact, no such criterion for the individuation of types of experiments had been proposed up to this point. But it is appropriate to regard those experiments as being of the same kind which serve to establish the same fact, i. e. that test the same empirical relation(s). On this proposal, MME and KTE are of *same* kind, because both were designed to test equation (*). It is of no relevance whatever in this context that the KTE was much better suited for this purpose than the MME because the range of data recorded was much greater. In fact, the latter was rather poorly designed as a test of equation (*) because it only examined the degenerate case $l_{10} = l_{20}$. But considerations of that kind only refer to the first level of methodological assessment mentioned above. They deal with the question how reliably the facts are established. But they do not influence the methodological virtues of the explanatory hypotheses itself.

Concerning the problem of singling out one type of experiment, an additional difficulty emerges. What about an experiment of MM- or KT-type that is designed to test a possible material-dependance of the contraction.[65] And what about the difference between the Miller experiment, which was exactly analogous to the MME apart from that it employed a light path of 64 meters, and the original MME, where the light path had the length of only 11 meters.[66] To clarify such cases one has to bear in mind that any empirical generalization entails a creteris-paribus-clause. In other words, it is assumed that all relevant parameters are under experimental control, i. e. that there exist

[62] Grünbaum 1976, 388; Grünbaum emphasizes that a falsification of the contraction hypothesis *itself* could always be avoided by shifting the blame to other auxiliary assumptions; cf. ibid. and also Grünbaum 1973, 715–725.

[63] Popper 1934, 51.

[64] Zahar 1973, 104.

[65] Such an experiment was actually conducted by Michelson; cf. Lakatos 1970, 76.

[66] This question puzzles Grünbaum; cf. Grünbaum 1976, 349.

no additional relevant influences. Obviously, experiments as those described above test this ceteris-paribus-clause. That is to say, if they lead to expected results, i. e. if they confirm the clause, they do not give rise to a separate fact, since – as stated above – the ceteris-paribus-clause is to be considered as part of the pertinent fact. Matters are quite different, however, if it is discovered that the experimental outcome is unexpectedly altered by the variation of the quantity under consideration. This means the introduction of a new empirical relation and consequently the establishment of a new fact. In other words, as long as the variations in the experimental conditions do not lead to the detection of empirical regularities, (i. e. if they only serve to increase, for example, the accuracy of the experiment) it remains the same experiment.

To conclude, Lorentz's theory cannot be held responsible for explaining the MME in an ad hoc manner. At most, Lorentzians can be blamed for not having sufficiently tested the explaining hypothesis. This conclusion indicates that the superiority of Einstein's special theory should not be grounded by reference to the ad-hoc-ness of the contraction hypothesis (or similar methodological blunders).[67] The methodological justification of Einstein's approach must be achieved by different means. I think that the methodological criterion of maximum explanatory power, i. e. the requirement that a theory should use a minimum set of fundamental postulates so as to explain a maximum range of phenomena with maximum precision, might be the adequate means for this purpose.

IV. SOME FURTHER CLARIFICATIONS

Obviously, a criterion for determining a 'single fact' does only half the job. What is also needed is a criterion for the individuation of hypotheses. Often it is possible to express one hypothesis as a conjunction of several ones or to join

[67] It is appropriate to underline this conclusion by two further remarks.

It should be noted, first, that even the negative outcome of the KTE could have well been digested within Lorentz's theory. If one adds a time dilation hypothesis to Lorentz's theory, this doubly amended aether account implies the Kennedy-Thorndike null result. Furthermore, the conjunction of contraction and time dilation hypotheses – besides rescuing the theory from the Kennedy-Thorndike attack – led to the prediction of a special 'quadratic' Doppler effect (cf. Grünbaum 1973, 723–724). In other words, the auxiliary dilation hypothesis is *not ad hoc*$_1$, too.

Secondly, the ad-hoc-ness of the contraction hypothesis is discussed in the methodological literature to the best of my knowledge exclusively in terms of the MME-KTE-case. This is rather strange, because there exists a *second* experimental test of the contraction hypothesis which is without doubt independent of the KTE. This is the experiment of Wood, Tomlinson and Essen (WTE), published in 1937 (cf. Wood/Tomlinson/Essen 1937). WTE use *sound* waves in a solid rod (instead of *light* waves) to detect the contraction. A rotating rod, that is during one revolution sometimes oriented *in* the direction of the earth's orbital motion and sometimes *perpendicular* to it, should undergo, on the contraction hypothesis, a change of length. This should result in a corresponding change of frequency of the rod's longitudinal vibration. No such effect occurred. In other words, as in the Kennedy-Thorndike-case, the contraction hypothesis is in fact independently testable and fails in this independent test.

It is noteworthy that also the WTE experiment is not a 'waterproof' falsification of the contraction hypothesis. Lorentz's account could have been saved by assuming a change of the rod's elasticity to the extent that the change of length would be exactly compensated (a suggestion WTE indeed put forth). Yet I am not aware of any possibility to test *this* rescuing hypothesis.

vice versa several assumptions into a single one. Furthermore, in addition to this 'language dependance effect', one hypothesis might not suffice to solve an anomaly,[68] so that several auxiliary assumptions may be needed instead. Maxwell's first gas theory of 1860 only achieved its baffling novel predictions with the aid of the following three hypotheses: (1) molecules are to be regarded as elastic spheres, (2) after a collision all directions of movement are equally probable, and (3) the distribution of any velocity component is independent of the distribution of the other components.[69] Obviously, we cannot expect each auxiliary assumption (together with the preceding theory) to clear up two anomalies. Generally, such a cut-up version will explain nothing at all. In such a case, however, we can apply the assessment criterion to the conjunction of all pertinent assumptions. In other words, we can view Maxwell's three tenets methodologically as constituting one hypothesis. The situation is quite different, however, if the conjunction of the theory T with the hypothesis H_1 clears up anomaly e_1, and the conjunction of T with H_2 implies the solution of e_2. In this case, we should rule out the treatment of H_1 and H_2 as a methodological unit. Putting this more systematically, we can say that a hypothesis in the methodological sense is the minimum set of claims which allows for the derivation of an empirically testable relation.[70] (And it is required that this minimum set actually allows for the derivation of at least two such relations.) In addition, as discussed above every hypothesis should be seen as containing a ceteris-paribus-clause. This means that the explicit enunciation of this clause does not constitute a separate hypothesis. This proposal, of course, is far removed from a logically neat criterion of individuation. Its actual application relies to a great extent on what appears intuitively plausible in a certain case of theory alteration.

Up to now I have only considered the development within a programme and specified the rules for intra-programme progress. What is lacking is a definition of the conditions under which one programme overtakes its rival. I would be willing to claim that a programme is superior if it is supported by a greater number of non-ad-hocly explained facts, taking 'non-ad-hoc' in the *intra*-programme sense. In comparing programmes, too, it is excess content that counts. But simply transferring the criterion for non-ad-hoc explanations from the intra-programme level to the inter-programme level appears inadequate because this would imply that in order to count an explanation as supporting one programme as opposed to another one, this explanation has to account for at least two of the rival's anomalies. This requirement, however, is certainly too strict. There is hardly any hypothesis put forward in the history of science that could meet such a requirement. Accordingly, a theory should be viewed as successful if it solves its own problems decently. If, on the other hand, a programme indeed succeeds in explaining anomalies in the

[68] In the following I will only mention anomalies. But programme extensions are always connoted.

[69] Clark 1976, 50.

[70] This criterion circumvents the language dependance problem. It is wholly irrelevant how many sentences such a hypothetical proposition actually contains.

rival programme, such an explanation generally also constitutes an extension of this programme's realm of application and consequently (if brought about non-ad-hocly) intra-programme progress. This holds true, for example, in the case of Torricelli's hypothesis (as described on p. 218). This hypothesis explained the suction pump anomaly, i. e. an anomaly of the rival horror vacui programme. And at the same time this explanation is supported in the intra-programme sense because it enlarged the scope of the pertinent programme in a non-ad-hoc fashion (being accompanied by two additional predictions).

One problem, however, deserves particular attention. How does one assess an auxiliary hypothesis which resolves a sufficient number of anomalies, but in turn also creates further difficulties. Boltzmann's arbitrary exclusion of the vibrational and the third rotational degrees of freedom for diatomic molecules by his model of two rigidly connected spheres correctly explained the anomalous values for the ratio of the specific heats of diatomic gases. It also correctly predicted the occurrence of the third rotational degree of freedom in the polyatomic molecules of heavy gases with the correct value for the ratio of the specific heats. If Boltzmann's solution been taken seriously from a dynamical point of view, however, the result would have been an infinite specific heat of gases.[71] The model thus solved two anomalies, but suffered from an empirical difficulty the preceding programme version had not encountered. According to the assessment criterion, both an empirical problem which the precursor had not exhibited as well as the reemergence of a previously resolved anomaly counts as support for the *preceding* variant. Such a case thus has a negative influence on the net support for the theoretical modification. Since Boltzmann's proposal is ad hoc_1[72], it has to be rejected on methodological grounds.[73]

The criterion I have presented requires that a programme be supported by more facts than its rival if it is to be considered as superior. I am thus rejecting Lakatos's requirement that a programme which supersedes its rival must explain the entire unrefuted content of this rival.[74] It is legitimate for a programme to replace an older one even if the acceptance of the new programme is accompanied by a certain reduction in explanatory power, i. e. by a 'Kuhn loss', provided that it manages to overcompensate the defect at another place. Geocentric theory, for example, is an often quoted example of a Kuhn loss. Together with Aristotelian dynamics, it explained why bodies are not carried away from the earth's surface or why a stone thrown perpendicular to the earth's surface returns to its original position. The Copernican programme, on the other hand, was at a loss to account for these events until a

[71] Clark 1976, 84.

[72] 'Ad hoc_1' is used here in a slightly altered sense. It refers not only to the number of known independent empirical consequences of a hypothesis but also to the number of known empirical problems created by this hypothesis.

[73] The situation changed completely some decades later when Boltzmann's auxiliary hypothesis became a *theorem* of quantum mechanics. This shows how risky an enterprise science (and, of course, methodology) actually is. We can never be sure when we dismiss a hypothesis as ad hoc whether science some day will prove us wrong.

[74] Lakatos 1970, 32.

new dynamics was developed. A similar case occurred around 1830 when the wave programme gained general acceptance at the expense of the excellent corpuscular explanation of dispersion, which it was not able to replace with a satisfactory equivalent. Not until 1886 did the wave programme succeed in overcoming this difficulty.[75] In all these cases, however, the theory as a whole provided more new non-ad-hoc explanations than were lost in its adoption. The gains, in other words, outnumbered the losses. The assessment criterion I am proposing no longer requires that all debts be settled first in such cases. It balances gains and losses. The excess number of non-ad-hoc explanations is the critical factor. The emphasis on non-ad-hoc-ness also represents an answer to Worrall's criticisms. Worrall objected that the initial version of the theoretical criterion over-emphasized temporal priority when a programme is expanded into an area whose problems the competitor had not yet tackled (see p. 214). On the basis of the approach sketched here, however, it is not only temporal priority but additional methodological qualifications that make a hypothesis acceptable. If a programme succeeds in bringing about non-ad-hoc penetration into an area previously neglected by the adversary programme, what it explains that the rival does not explain counts as excess content and hence as support. This, however, holds true only as long as the competitor offers no non-ad-hoc explanation of his own. If he manages to neutralize the excess content, the new programme loses its advantage. Although Worrall's objection that a fact used as supporting evidence for one programme cannot be used to support a rival programme is valid, this peculiarity does not question the adequacy of the assessment criterion because it no longer refers to just any explanation but to qualified accounts.

There are some remarks I would like to add on the role of the programme heuristic. According to the view presented here, agreement or disagreement with the programme heuristic has no influence on the acceptance or repudiation of a hypothesis. A hypothesis which develops organically out of a programme heuristic has no initial advantage over an assumption which runs counter to the general trend of the programme. The empirical hurdles are the same for both. This does not mean, however, that the programme heuristic is a negligeable quantity or that the vicissitudes of a programme should be as highly regarded as a sophisticated and long-term research strategy. On the contrary, Lakatos's concept of positive heuristic provides an excellent means of characterizing the continuity peculiar to mature science. My objection is that recent changes in the MSRP transformed the heuristic into a veritable *deus ex machina*.

The heuristic criterion judges methodological quality in terms of a theory's 'descent' by making assessment dependent on the genesis of a theory. Popper calls such a procedure a 'pedigree theory of validity'. In his view any theory regardless of its origin has to legitimize itself by expounding its own empirical achievements.[76] Agassi modifies Popper's version of the pedigree theory, which takes descent as the hallmark of quality, by regarding descent as an

[75] cf. Worrall 1978a, 63–64.
[76] Popper 1963, 21–27.

explanation of quality.[77] The snob, Agassi argues, would say that someone, who is an aristocrat, is good. One needs no other verification of his superiority. The reasonable conservative, on the other hand, tests someone's qualities and explains his good results as a function of aristocratic birth. In contrast to the snob, he asserts that there is a high correlation between descent and quality, but does not renounce independent tests.[78] In my opinion, this second variant of the pedigree theory places heuristic factors in the proper role in the MSRP. Programmes with a coherent development, whose single stages develop out of an articulate heuristic generally have a better chance of achieving excess empirical content than a patched-up series of unrelated hypotheses. Precisely because a strong heuristic favours empirical success it is not necessary to introduce it a second time into the acceptance rules. Programmes with powerful heuristic guidelines generally manage to clear the empirical hurdles. If agreement with the programme heuristic is not able to support an idea, this agreement can still motivate the scientist to continue in the same direction. The conflict with the programme heuristic, on the other hand, may point to difficulties independent of the known anomalies. Since the heuristic establishes a rationality of pursuit in the sense mentioned above, it hints not at the acceptance of hypotheses but at their improvement.

The assessment criterion, as I have presented it, approves not only the confirmed prediction of genuinely novel facts, but also the inclusion of previously known, but as yet unrelated facts in one theory. Scientific progress often means integrating disparate facts. What is most important, however, is to distinguish between genuinely novel relationships and the linguistically disguised reuse of well-known facts. This is what Watkins' antitrivialization principle requires[79] and this, it seems, is what Lakatos failed to do when he liberalized his initially strict predictivism.

Appealing to the Lakatos's true intentions, however, does not constitute a justification. The criterion needs more than just a noble descent. It has to bear fruit; and this fruit should be empirical. Arguing for methodological criteria should not rely exclusively on an appeal to intuitions, guided by a few examples. These criteria have to be checked systematically against the history of science. The approach I have outlined must be judged by its ability to explain and justify a greater number of events in the history of science than the other MSRP versions.[80]

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[77] Agassi 1977, 34.
[78] Ibid.
[79] cf. note 38.
[80] For this procedure for appraising the adequacy of a methodology compare Lakatos 1971; cf. also Carrier 1986.

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