theories. It is a succession of theories and not one given theory which is appraised as scientific or pseudo-scientific. But the members of such series of theories are usually connected by a remarkable *continuity* which welds them into *research programmes.* This *continuity* – reminiscent of Kuhnian 'normal science' – plays a vital role in the history of science; the main problems of the logic of discovery cannot be satisfactorily discussed except in the framework of a *methodology of research programmes.*

3 A METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES

I have discussed the problem of objective appraisal of scientific growth in terms of progressive and degenerating problemshifts in series of scientific theories. The most important such series in the growth of science are characterized by a certain *continuity* which connects their members. This continuity evolves from a genuine research programme adumbrated at the start. The programme consists of methodological rules: some tell us what paths of research to avoid (*negative heuristic*), and others what paths to pursue (*positive heuristic*).[1]

Even science as a whole can be regarded as a huge research programme with Popper's supreme heuristic rule: 'devise conjectures which have more empirical content than their predecessors.' Such methodological rules may be formulated, as Popper pointed out, as metaphysical principles.[2] For instance, the *universal* anti-conventionalist rule against exception-barring may be stated as the metaphysical principle: 'Nature does not allow exceptions.' This is why Watkins called such rules 'influential metaphysics'.[3]

But what I have primarily in mind is not science as a whole, but rather *particular* research programmes, such as the one known as 'Cartesian metaphysics'. Cartesian metaphysics, that is, the mechanistic theory of the universe – according to which the universe is a huge clockwork (and system of vortices) with push as the only cause of motion – functioned as a powerful heuristic principle. It discouraged work on scientific theories – like (the 'essentialist' version of) Newton's theory of action at a distance – which were inconsistent with it (*negative heuristic*). On the other hand, it encouraged work on auxiliary hypo-

[1] One may point out that the negative and positive heuristic gives a rough (implicit) definition of the 'conceptual framework' (and consequently of the language). The recognition that the history of science is the history of research programmes rather than of theories may therefore be seen as a partial vindication of the view that the history of science is the history of conceptual frameworks or of scientific languages.

[2] Popper [1934], sections 11 and 70. I use 'metaphysical' as a technical term of naive falsificationism: a contingent proposition is 'metaphysical' if it has no 'potential falsifiers'.

[3] Watkins [1958]. Watkins cautions that 'the logical gap between statements and prescriptions in the metaphysical-methodological field is illustrated by the fact that a person may reject a [metaphysical] doctrine in its fact-stating form while subscribing to the prescriptive version of it' (*Ibid.*, pp. 356–7).

theses which might have saved it from apparent counterevidence – like Keplerian ellipses (*positive heuristic*).[1]

(a) *Negative heuristic: the 'hard core' of the programme*

All scientific research programmes may be characterized by their '*hard core*'. The negative heuristic of the programme forbids us to direct the *modus tollens* at this 'hard core'. Instead, we must use our ingenuity to articulate or even invent 'auxiliary hypotheses', which form a *protective belt* around this core, and we must redirect the *modus tollens* to *these*. It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core. A research programme is successful if all this leads to a progressive problemshift; unsuccessful if it leads to a degenerating problemshift.

The classical example of a successful research programme is Newton's gravitational theory: possibly the most successful research programme ever. When it was first produced, it was submerged in an ocean of 'anomalies' (or, if you wish, 'counterexamples'[2]), and opposed by the observational theories supporting these anomalies. But Newtonians turned, with brilliant tenacity and ingenuity, one counter-instance after another into corroborating instances, primarily by overthrowing the original observational theories in the light of which this 'contrary evidence' was established. In the process they themselves produced new counter-examples which they again resolved. They 'turned each new difficulty into a new victory of their programme'.[3]

In Newton's programme the negative heuristic bids us to divert the *modus tollens* from Newton's three laws of dynamics and his law of gravitation. This 'core' is 'irrefutable' by the methodological decision of its proponents: anomalies must lead to changes only in the 'protective' belt of auxiliary, 'observational' hypotheses and initial conditions.[4]

I have given a contrived micro-example of a progressive Newtonian problemshift.[5] If we analyse it, it turns out that each successive link in this exercise predicts some new fact; each step represents an increase in empirical content: the example constitutes a *consistently progressive theoretical shift.* Also, each prediction is in the end verified; although on three subsequent occasions they may have seemed momentarily to

[1] For this Cartesian research programme, cf. Popper [1960*b*] and Watkins [1958], pp. 350–1.

[2] For the clarification of the concepts of 'counterexample' and 'anomaly' cf. *above*, p. 26, and especially *below*, p. 72, text to n. 3.

[3] Laplace [1824], livre IV, chapter 11.

[4] The actual hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error. In this paper this process is not discussed.

[5] Cf. *above*, pp. 16–17.

be 'refuted'.[1] While 'theoretical progress' (in the sense here described) may be verified immediately,[2] 'empirical progress' cannot, and in a research programme we may be frustrated by a long series of 'refutations' before ingenious and lucky content-increasing auxiliary hypotheses turn a chain of defeats – *with hindsight* – into a resounding success story, either by revising some false 'facts' or by adding novel auxiliary hypotheses. We may then say that we must require that each step of a research programme be consistently content-increasing: that each step constitute a *consistently progressive theoretical problemshift.* All we need in addition to this is that at least every now and then the increase in content should be seen to be retrospectively corroborated: the programme as a whole should also display an *intermittently progressive empirical shift.* We do not demand that each step produce *immediately* an *observed* new fact. Our term '*intermittently*' gives sufficient *rational* scope for dogmatic adherence to a programme in face of *prima facie* 'refutations'.

The idea of 'negative heuristic' of a scientific research programme rationalizes classical conventionalism to a considerable extent. We may rationally decide not to allow 'refutations' to transmit falsity to the hard core as long as the corroborated empirical content of the protecting belt of auxiliary hypotheses increases. But our approach differs from Poincaré's justificationist conventionalism in the sense that, unlike Poincaré, we maintain that if and when the programme ceases to anticipate novel facts, its hard core might have to be abandoned: that is, *our* hard core, unlike Poincaré's, may crumble under certain conditions. In this sense we side with Duhem who thought that such a possibility must be allowed for;[3] but for Duhem the reason for such crumbling is purely *aesthetic*,[4] while for us it is mainly *logical and empirical.*

(*b*) *Positive heuristic: the construction of the 'protective belt' and the relative autonomy of theoretical science*

Research programmes, besides their negative heuristic, are also characterized by their positive heuristic.

Even the most rapidly and consistently progressive research programmes can digest their 'counter-evidence' only piecemeal: anomalies are never completely exhausted. But it should not be thought that yet unexplained anomalies – 'puzzles' as Kuhn might call them – are taken in random order, and the protective belt built up in an eclectic fashion, without any preconceived order. The order is usually decided in the theoretician's cabinet, independently of the *known* anomalies.

[1] The 'refutation' was each time successfully diverted to 'hidden lemmas'; that is, to lemmas emerging, as it were, from the *ceteris paribus* clause.

[2] But cf. *below*, pp. 69–71. [3] Cf. *above*, p. 22.

[4] *Ibid.*

Few theoretical scientists engaged in a research programme pay undue attention to 'refutations'. They have a long-term research policy which anticipates these refutations. This research policy, or order of research, is set out – in more or less detail – in the *positive heuristic* of the research programme. The negative heuristic specifies the 'hard core' of the programme which is 'irrefutable' by the methodological decision of its proponents; the positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-programme, how to modify, sophisticate, the 'refutable' protective belt.

The positive heuristic of the programme saves the scientist from becoming confused by the ocean of anomalies. The positive heuristic sets out a programme which lists a chain of ever more complicated *models* simulating reality: the scientist's attention is riveted on building his models following instructions which are laid down in the positive part of his programme. He ignores the *actual* counterexamples, the available '*data*'.[1] Newton first worked out his programme for a planetary system with a fixed point-like sun and one single point-like planet. It was in this model that he derived his inverse square law for Kepler's ellipse. But this model was forbidden by Newton's own third law of dynamics, therefore the model had to be replaced by one in which both sun and planet revolved round their common centre of gravity. This change was not motivated by any observation (the data did not suggest an 'anomaly' here) but by a theoretical difficulty in developing the programme. Then he worked out the programme for more planets as if there were only heliocentric but no interplanetary forces. Then he worked out the case where the sun and planets were not mass-points but mass-*balls*. Again, for this change he did not *need* the observation of an anomaly; infinite density was forbidden by an (inarticulated) touchstone theory, therefore planets *had* to be extended. This change involved considerable mathematical difficulties, held up Newton's work – and delayed the publication of the *Principia* by more than a decade. Having solved this 'puzzle', he started work on *spinning balls* and their wobbles. Then he admitted interplanetary forces and started work on *perturbations*. At this point he started to look more anxiously at the facts. Many of them were beautifully explained (qualitatively) by this model, many were not. It was then that he started to work on *bulging* planets, rather than round planets, etc.

Newton despised people who, like Hooke, stumbled on a first naive model but did not have the tenacity and ability to develop it into a research programme, and who thought that a first version, a mere

[1] If a scientist (or mathematician) has a positive heuristic, he refuses to be drawn into observation. He will 'lie down on his couch, shut his eyes and forget about the data'. (Cf. my [1963–4], especially pp. 300 ff, where there is a detailed case study of such a programme.) Occasionally, of course, he will ask Nature a shrewd question: he will then be encouraged by Nature's YES, but not discouraged by its NO.

aside, constituted a 'discovery'. He held up publication until his programme had achieved a remarkable progressive shift.[1]

Most, if not all, Newtonian 'puzzles', leading to a series of new variants superseding each other, were forseeable at the time of Newton's first naive model and no doubt Newton and his colleagues *did* forsee them: Newton must have been fully aware of the blatant falsity of his first variants. Nothing shows the existence of a positive heuristic of a research programme clearer than this fact: this is why one speaks of 'models' in research programmes. A '*model*' is a set of initial conditions (possibly together with some of the observational theories) which one knows is *bound* to be replaced during the further development of the programme, and one even knows, more or less, how. This shows once more how irrelevant 'refutations' of any specific variant are in a research programme: their existence is fully expected, the positive heuristic is there as the strategy both for predicting (producing) and digesting them. Indeed, if the positive heuristic is clearly spelt out, the difficulties of the programme are mathematical rather than empirical.[2]

One may formulate the 'positive heuristic' of a research programme as a 'metaphysical' principle. For instance one may formulate Newton's programme like this: 'the planets are essentially gravitating spinning-tops of roughly spherical shape'. This idea was never *rigidly* maintained: the planets are not *just* gravitational, they have also, for example, electromagnetic characteristics which may influence their motion. Positive heuristic is thus in general more flexible than negative heuristic. Moreover, it occasionally happens that when a research programme gets into a degenerating phase, a little revolution or a *creative shift* in its positive heuristic may push it forward again.[3] It is better therefore to separate the 'hard core' from the more flexible metaphysical principles expressing the positive heuristic.

Our considerations show that the positive heuristic forges ahead with almost complete disregard of 'refutations': it may seem that it is the '*verifications*'[4] rather than the refutations which provide the

[1] Reichenbach, following Cajori, gives a different explanation of what delayed Newton in the publication of his *Principia*; 'To his disappointment he found that the observational results disagreed with his calculations. Rather than set any theory, however beautiful, before the facts, Newton put the manuscript of this theory into his drawer. Some twenty years later, after new measurements of the circumference of the earth had been made by a French expedition, Newton saw that the figures on which he had based his test were false and that the improved figures agreed with his theoretical calculation. It was only after this test that he published his law...The story of Newton is one of the most striking illustrations of the method of modern science' (Reichenbach [1951], pp. 101–2). Feyerabend criticizes Reichenbach's account (Feyerabend [1965], p. 229), but does not give an alternative *rationale*.

[2] For this point cf. Truesdell [1960].

[3] Soddy's contribution to Prout's programme or Pauli's to Bohr's (old quantum theory) programme are typical examples of such creative shifts.

[4] A 'verification' is a corroboration of excess content in the expanding programme. But, of course, a 'verification' does not *verify* a programme: it shows only its heuristic power.

contact points with reality. Although one must point out that any 'verification' of the $(n+1)$th version of the programme is a refutation of the nth version, we cannot deny that *some* defeats of the subsequent versions are always foreseen: it is the 'verifications' which keep the programming going, recalcitrant instances notwithstanding.

We may appraise research programmes, even after their 'elimination', for their *heuristic power*: how many new facts did they produce, how great was 'their capacity to explain their refutations in the course of their growth'?[1]

(We may also appraise them for the stimulus they gave to mathematics. The real difficulties for the theoretical scientist arise rather from the *mathematical difficulties* of the programme than from anomalies. The greatness of the Newtonian programme comes partly from the development – by Newtonians – of classical infinitesimal analysis which was a crucial precondition of its success.)

Thus the methodology of scientific research programmes accounts for the *relative autonomy of theoretical science*: a historical fact whose rationality cannot be explained by the earlier falsificationists. Which problems scientists working in powerful research programmes rationally choose, is determined by the positive heuristic of the programme rather than by psychologically worrying (or technologically urgent) anomalies. The anomalies are listed but shoved aside in the hope that they will turn, in due course, into corroborations of the programme. Only those scientists have to rivet their attention on anomalies who are either engaged in trial and error exercises[2] or who work in a degenerating phase of a research programme when the positive heuristic ran out of steam. (All this, of course, must sound repugnant to naive falsificationists who hold that once a theory is 'refuted' by experiment (by *their* rule book), it is irrational (and dishonest) to develop it further: one has to replace the old 'refuted' theory by a new, unrefuted one.)

(*c*) *Two illustrations: Prout and Bohr*

The dialectic of positive and negative heuristic in a research programme can best be illuminated by examples. Therefore I am now going to sketch a few aspects of two spectacularly successful research programmes: Prout's programme[3] based on the idea that all atoms are compounded of hydrogen atoms and Bohr's programme based on the idea that light-emission is due to electrons jumping from one orbit to another within the atoms.

(In writing a historical case study, one should, I think, adopt the following

[1] Cf. my [1963–4], pp. 324–30. Unfortunately in 1963–4 I had not yet made a clear terminological distinction between theories and research programmes, and this impaired my exposition of a research programme in informal, quasi-empirical mathematics.

[2] Cf. *below*, p. 88.

[3] Already mentioned *above*, pp. 43–4.

procedure: (1) one gives a rational reconstruction; (2) one tries to compare this rational reconstruction with actual history and to criticize both one's rational reconstruction for lack of historicity and the actual history for lack of rationality. Thus any historical study must be preceded by a heuristic study: history of science without philosophy of science is blind. In this paper it is not my purpose to go on seriously to the second stage.)

(c 1) *Prout: a research programme progressing in an ocean of anomalies*

Prout, in an anonymous paper of 1815, claimed that the atomic weights of all pure chemical elements were whole numbers. He knew very well that anomalies abounded, but said that these arose because chemical substances as they ordinarily occurred were *impure*: that is, the relevant 'experimental techniques' of the time were unreliable, or, to put it in our terms, the contemporary 'observational' theories in the light of which the truth values of the basic statements of his theory were established, were false.[1] The champions of Prout's theory therefore embarked on a major venture: to overthrow those theories which supplied the counter-evidence to their thesis. For this they had to revolutionize the established analytical chemistry of the time and correspondingly revise the experimental techniques with which pure elements were to be separated.[2] Prout's theory, as a matter of fact, defeated the theories previously applied in purification of chemical substances one after the other. Even so, some chemists became tired of the research programme and gave it up, since the successes were still far from adding up to a final victory. For instance, Stas, frustrated by some stubborn, recalcitrant instances, concluded in 1860 that Prout's theory was 'without foundations'.[3] But others were more encouraged by the progress than discouraged by the lack of complete success. For instance, Marignac immediately retorted that 'although [he is satisfied that] the experiments of Monsieur Stas are perfectly exact, [there is no proof] that the differences observed between his results and those required by Prout's law cannot be explained by the imperfect character

[1] Alas, all this is rational reconstruction rather than actual history. Prout denied the existence of any anomalies. For instance, he claimed that the atomic weight of chlorine was exactly 36.

[2] Prout was aware of some of the basic methodological features of his programme. Let us quote the first lines of his [1815]: 'The author of the following essay submits it to the public with the greatest diffidence... He trusts, however, that its importance will be seen, and that some one will undertake to examine it, and thus verify or refute its conclusions. If these should be proved erroneous, still new facts may be brought to light, or old ones better established, by the investigation; but if they should be verified, a new and interesting light will be thrown upon the whole science of chemistry.'

[3] Clerk Maxwell was on Stas's side: he thought it was impossible that there should be two kinds of hydrogen, 'for if some [molecules] were of slightly greater mass than others, we have the means of producing a separation between molecules of different masses, one of which would be somewhat denser than the other. As this cannot be done, we must admit [that all are alike]' (Maxwell [1871]).

of experimental methods'.[1] As Crookes put it in 1886: 'Not a few chemists of admitted eminence consider that we have here [in Prout's theory] an expression of the truth, masked by some residual or collateral phenomena which we have not yet succeeded in eliminating.'[2] That is, there had to be some *further* false hidden assumption in the 'observational' theories on which 'experimental techniques' for chemical purification were based and with the help of which atomic weights were calculated: in Crookes's view even in 1886 'some present atomic weights merely represented a mean value'.[3] Indeed, Crookes went on to put this idea in a scientific (content-increasing) form: he proposed concrete new theories of 'fractionation', a new 'sorting Demon'.[4] But, alas, his new observational theories turned out to be as false as they were bold and, being unable to anticipate any new fact, they were eliminated from the (rationally reconstructed) history of science. As it turned out a generation later, there was a very basic hidden assumption which failed the researchers: that two pure elements must be separable by *chemical* methods. The idea that two different pure elements may behave identically in all *chemical* reactions but can be separated by *physical* methods, required a change, a '*stretching*', of the concept of 'pure element' which constituted a change – a *concept-stretching expansion* – of the research programme itself.[5] This revolutionary highly *creative shift* was taken only by Rutherford's school;[6] and then 'after many vicissitudes and the most convincing apparent disproofs, the hypothesis thrown out so lightly by Prout, an Edinburgh physician, in 1815, has, a century later, become the cornerstone of modern theories of the structure of atoms'.[7] However, this creative step was in fact only a side-result of progress in a different, indeed, distant research programme; Proutians, lacking this *external* stimulus, never dreamt of trying, for instance, to build powerful centrifugal machines to separate elements.

(When an 'observational' or 'interpretative' theory finally gets eliminated, the 'precise' measurements carried out within the discarded framework may look – with hindsight – rather foolish. Soddy made fun of 'experimental precision' for its own sake: 'There is something surely akin to if not transcending tragedy in the fate that has overtaken the life work of that distinguished galaxy of nineteenth-century chemists, rightly revered by their contemporaries as representing the crown and perfection of accurate scientific measurement. Their hard won results, for the moment at least, appears as of as little interest and

[1] Marignac [1860].
[2] Crookes [1886].
[3] *Ibid.*
[4] Crookes [1886], p. 491.
[5] For 'concept-stretching', cf. my [1963–4], part IV.
[6] The shift is anticipated in Crookes's fascinating [1888] where he indicates that the solution should be sought in a new demarcation between 'physical' and 'chemical'. But the anticipation remained philosophical; it was left to Rutherford and Soddy to develop it, after 1910, into a scientific theory.
[7] Soddy [1932], p. 50.

significance as the determination of the average weight of a collection of bottles, some of them full and some of them more or less empty.'[1])

Let us stress that in the light of the methodology of research programmes here proposed there never was any rational reason to *eliminate* Prout's programme. Indeed, the programme produced a beautiful, progressive shift, even if, in between, there were considerable hitches.[2] Our sketch shows how a research programme can challenge a considerable bulk of accepted scientific knowledge: it is planted, as it were, in an inimical environment which, step by step, it can override and transform.

Also, the actual history of Prout's programme illustrates only too well how much the progress of science was hindered and slowed down by justificationism and by naive falsificationism. (The opposition to atomic theory in the nineteenth century was fostered by both.) An elaboration of this particular influence of bad methodology on science may be a rewarding research programme for the historian of science.

(*c 2*) *Bohr: a research programme progressing on inconsistent foundations*

A brief sketch of Bohr's research programme of light emission (in *early* quantum physics) will illustrate further – and even expand – our thesis.[3]

The story of Bohr's research programme can be characterized by: (1) its initial problem; (2) its negative and positive heuristic; (3) the problems which it attempted to solve in the course of its development; and (4) its degeneration point (or, if you wish, 'saturation point') and, finally, (5) the programme by which it was superseded.

The background problem was the riddle of how Rutherford atoms (that is, minute planetary systems with electrons orbiting round a positive nucleus) can remain stable; for, according to the well-corroborated Maxwell–Lorentz theory of electromagnetism they should collapse. But Rutherford's theory was well corroborated too. Bohr's suggestion was to ignore for the time being the inconsistency and consciously develop a research programme whose 'refutable' versions were inconsistent with the Maxwell–Lorentz theory.[4] He proposed five postulates as the *hard core* of his programme: '(1) that

[1] *Ibid.*

[2] These hitches inevitably induce many individual scientists to shelve or altogether jettison the programme and join other research programmes where the positive heuristic happens to offer at the time cheaper successes: the history of science cannot be *fully* understood without mob-psychology. (Cf. *below*, pp. 90–93.)

[3] This section may again strike the historian as more a caricature than a sketch; but I hope it serves its purpose. (Cf. *above*, p. 52.) Some statements are to be taken not with a grain, but with tons, of salt.

[4] This, of course, is a further argument against J. O. Wisdom's thesis that metaphysical theories can be refuted by a conflicting well-corroborated scientific theory (Wisdom [1963].) Also, cf. *above*, p. 27, text to n. 7, and p. 42.

energy radiation [within the atom] is not emitted (or absorbed) in the continuous way assumed in the ordinary electrodynamics, but only during the passing of the systems between different "stationary" states. (2) That the dynamical equilibrium of the systems in the stationary states is governed by the ordinary laws of mechanics, while these laws do not hold for the passing of the systems between the different states. (3) That the radiation emitted during the transition of a system between two stationary states is homogeneous, and that the relation between the frequency ν and the total amount of energy emitted E is given by $E = h\nu$, where h is Planck's constant. (4) That the different stationary states of a simple system consisting of an electron rotating round a positive nucleus are determined by the condition that the ratio between the total energy, emitted during the formation of the configuration, and the frequency of revolution of the electron is an entire multiple of $\frac{1}{2}h$. Assuming that the orbit of the electron is circular, this assumption is equivalent with the assumption that the angular momentum of the electron round the nucleus is equal to an entire multiple of $h/2\pi$. (5) That the "permanent" state of any atomic system, i.e. the state in which the energy emitted is maximum, is determined by the condition that the angular momentum of every electron round the centre of its orbit is equal to $h/2\pi$.'[1]

We have to appreciate the crucial methodological difference between the inconsistency introduced by Prout's programme and that introduced by Bohr's. Prout's research programme declared war on the analytical chemistry of his time: its positive heuristic was designed to overthrow it and replace it. But Bohr's research programme contained no analogous design: its positive heuristic, even if it had been completely successful, would have left the inconsistency with the Maxwell–Lorentz theory unresolved.[2] To suggest such an idea required even greater courage than Prout's; the idea crossed Einstein's mind but he found it unacceptable, and rejected it.[3] Indeed, *some of the most important research programmes in the history of science were grafted on to older programmes with which they were blatantly inconsistent.* For instance, Copernican astronomy was 'grafted' on to Aristotelian physics, Bohr's programme on to Maxwell's. Such 'grafts' are irrational for the justificationist and for the naive falsificationist, neither of whom can countenance growth on inconsistent foundations. Therefore they are usually concealed by *ad hoc* stratagems – like Galileo's theory of circular inertia or Bohr's correspondence, and, later, complementarity principle – the only purpose of which is to hide the 'deficiency'.[4] As

[1] Bohr [1913*a*], p. 874.

[2] Bohr held at this time that the Maxwell–Lorentz theory would *eventually* have to be replaced. (Einstein's photon theory had already indicated this need.)

[3] Hevesy [1913]; cf. also *above*, p. 50, text to n. 1.

[4] In our methodology there is no need for such protective *ad hoc* stratagems. But, on the other hand, they are harmless as long as they are clearly seen as problems, not as solutions.

the young grafted programme strengthens, the peaceful co-existence comes to an end, the symbiosis becomes competitive and the champions of the new programme try to replace the old programme altogether.

It may well have been the success of his 'grafted programme' which later misled Bohr into believing that such fundamental inconsistencies in research programmes can and should be put up with *in principle*, that they do not present any serious problem and one merely has to get used to them. Bohr tried in 1922 to lower the standards of scientific criticism; he argued that '*the most* that one can demand of a theory [i.e. programme] is that the classification [it establishes] can be pushed so far that it can contribute to the development of the field of observation by the prediction of *new* phenomena.'[1]

(This statement by Bohr is similar to d'Alembert's when faced with the inconsistency in the foundations of infinitesimal theory: '*Allez en avant et la foi vous viendra.*' According to Margenau, 'it is understandable that, in the excitement over its success, men overlooked a malformation in the theory's architecture; for Bohr's atom sat like a baroque tower upon the Gothic base of classical electrodynamics.'[2] But as a matter of fact, the 'malformation' was not 'overlooked': everybody was aware of it, only they ignored it – more or less – during the progressive phase of the programme.[3] Our methodology of research programmes shows the rationality of this attitude but it also shows the irrationality of the defence of such 'malformations' once the progressive phase is over.

It should be said here that in the thirties and forties Bohr abandoned his demand for 'new phenomena' and was prepared to 'proceed with the immediate task of co-ordinating the multifarious evidence regarding atomic phenomena, which accumulated from day to day in the exploration of this new field of knowledge'.[4] This indicates that Bohr, by this time, had fallen back on 'saving the phenomena', while Einstein sarcastically insisted that 'every theory is true provided that one suitably associates its symbols with observed quantities'.[5])

But *consistency* – in a strong sense of the term[6] – *must remain an*

[1] Bohr [1922], my italics. [2] Margenau [1950], p. 311.

[3] Sommerfeld ignored it more than Bohr: cf. *below*, p. 63, n. 7.

[4] Bohr [1949], p. 206. [5] Quoted in Schrödinger [1958], p. 170.

[6] Two propositions are inconsistent if their conjunction has no model, that is, there is no interpretation of their descriptive terms in which the conjunction is true. But in informal discourse we use more formative terms than in formal discourse: some descriptive terms are given a fixed interpretation. In this informal sense two propositions may be (weakly) inconsistent given the standard interpretations of some characteristic terms even if formally, in some unintended interpretation, they may be consistent. For instance, the first theories of electron spin were inconsistent with the special theory of relativity if 'spin' was given its ('strong') standard interpretation and thereby treated as a formative term; but the inconsistency disappears if 'spin' is treated as an uninterpreted descriptive term. The reason why we should not give up standard interpretations too easily is that such emasculation of meanings may emasculate the positive heuristic of the programme. (On the other hand, such

important regulative principle (over and above the requirement of progressive problemshift); and inconsistencies (including anomalies) *must* be seen as problems. The reason is simple. If science aims at truth, it must aim at consistency; if it resigns consistency, it resigns truth. To claim that 'we must be modest in our demands',[1] that we must resign ourselves to – weak or strong – inconsistencies, remains a methodological vice. On the other hand, this does not mean that the discovery of an inconsistency – or of an anomaly – must *immediately* stop the development of a programme: it may be rational to put the inconsistency into some temporary, *ad hoc* quarantine, and carry on with the positive heuristic of the programme. This has been done even in mathematics, as the examples of the early infinitesimal calculus and of naive set theory show.[2]

(From this point of view, Bohr's 'correspondence principle' played an interesting double role in his programme. On the one hand it functioned as an important heuristic principle which suggested many new scientific hypotheses which, in turn, led to novel facts, especially in the field of the intensity of spectrum lines.[3] On the other hand it functioned also as a defence mechanism, which 'endeavoured to utilize to the utmost extent the concepts of the classical theories of mechanics and electrodynamics, in spite of the contrast between these theories and the quantum of action',[4] instead of emphasizing the urgency of a unified programme. In this second role it reduced the degree of problematicality of the programme.[5])

Of course, the research programme of quantum theory as a whole was a 'grafted programme' and therefore repugnant to physicists with deeply conservative views like Planck. There are two extreme and equally irrational positions with regard to a grafted programme.

meaning shifts may be in some cases progressive: cf. *above*, p. 41.)

For the shifting demarcation between formative and descriptive terms in informal discourse, cf. my [1963–4], 9(*b*), especially p. 335, n. 1.

1 Bohr [1922], last paragraph.

2 Naive falsificationists tend to regard this liberalism as a *crime against reason*. Their main argument runs like this: 'If one were to accept contradictions, then one would have to give up any kind of scientific activity: it would mean a complete breakdown of science. This can be shown by proving that *if two contradictory statements are admitted, any statement whatever must be admitted*; for from a couple of contradictory statements any statement whatever can be validly inferred...A theory which involves a contradiction is therefore entirely useless *as a theory*' (Popper [1940]). In fairness to Popper, one has to stress that he is here arguing against Hegelian dialectic, in which inconsistency becomes a *virtue*; and he is absolutely right when he points out its dangers. But Popper never analysed patterns of empirical (or non-empirical) progress on inconsistent foundations; indeed, in section 24 of his [1934] he makes consistency and falsifiability mandatory requirements for any scientific theory. I discuss this problem in more detail in chapter 3.

3 Cf. e.g. Kramers [1923].

4 Bohr [1923].

5 Born, in his [1954], gives a vivid account of the correspondence principle which strongly supports this double appraisal: 'The art of guessing correct formulae, which deviate from the classical ones, yet contain them as a limiting case...was brought to a high degree of perfection.'

The *conservative position* is to halt the new programme until the basic inconsistency with the old programme is somehow repaired: it is irrational to work on inconsistent foundations. The 'conservatives' will concentrate on eliminating the inconsistency by explaining (approximately) the postulates of the new programme in terms of the old programme: they find it irrational to go on with the new programme without a successful *reduction* of the kind mentioned. Planck himself chose this way. He did not succeed, in spite of the decade of hard work he invested in it.[1] Therefore Laue's remark that his lecture on 14 December 1900, was the 'birthday of the quantum theory' is not quite true: that day was the birthday of Planck's reduction programme. The decision to go *ahead* with temporarily inconsistent foundations was taken by Einstein in 1905, but even he wavered in 1913, when Bohr forged forward again.

The *anarchist position* concerning grafted programmes is to extol anarchy in the foundations as a virtue and regard [weak] inconsistency either as some basic property of nature or as an ultimate limitation of human knowledge, as some of Bohr's followers did.

The *rational position* is best characterized by Newton's, who faced a situation which was to a certain extent similar to the one discussed. Cartesian push-mechanics, on which Newton's programme was originally grafted, was (weakly) inconsistent with Newton's theory of gravitation. Newton worked both on his positive heuristic (successfully) *and* on a reductionist programme (unsuccessfully), and disapproved both of Cartesians who, like Huyghens, thought that it was not worth wasting time on an 'unintelligible' programme and of some of his rash disciples who, like Cotes, thought that the inconsistency presented no problem.[2]

The rational position with regard to 'grafted' programmes is then to exploit their heuristic power without resigning oneself to the fundamental chaos on which it is growing. On the whole, this attitude dominated old, pre-1925 quantum theory. In the new, post-1925 quantum theory the 'anarchist' position became dominant and modern quantum physics, in its 'Copenhagen interpretation', became one of the main standard bearers of philosophical obscurantism. In the *new* theory Bohr's notorious 'complementarity principle' enthroned

[1] For the fascinating story of this long series of frustrating failures, cf. Whittaker, [1953], pp. 103–4. Planck himself gives a dramatic description of these years: 'My futile attempts to fit the elementary quantum of action into the classical theory continued for a number of years, and they cost me a great deal of effort. Many of my colleagues saw in this something bordering on a tragedy' (Planck [1947]).

[2] Of course, a reductionist programme is scientific only if it explains more than it has set out to explain; otherwise the reduction is *not* scientific (cf. Popper [1969]). If the reduction does not produce new empirical content, let alone novel facts, then the reduction represents a degenerating problemshift – it is a mere linguistic exercise. The Cartesian efforts to bolster up their metaphysics in order to be able to interpret Newtonian gravitation in its terms, is an outstanding example for such merely linguistic reduction. Cf. *above*, p. 41, n. 3.

[weak] inconsistency as a basic ultimate feature of nature, and merged subjectivist positivism and antilogical dialectic and even ordinary language philosophy into one unholy alliance. After 1925 Bohr and his associates introduced a new and unprecedented lowering of critical standards for scientific theories. This led to a defeat of reason within modern physics and to an anarchist cult of incomprehensible chaos. Einstein protested: 'The Heisenberg–Bohr tranquillizing philosophy – or religion? – is so delicately contrived that, for the time being, it provides a gentle pillow for the true believer'.[1] On the other hand, Einstein's *too* high standards may well have been the reason that prevented him from discovering (or perhaps only from publishing) the Bohr model and wave mechanics.

Einstein and his allies have not won the battle. Physics textbooks are nowadays full of statements like this: 'The two viewpoints, quanta and electromagnetic field strengths, are complementary in the sense of Bohr. This complementarity is one of the great achievements of natural philosophy in which the Copenhagen interpretation of the epistemology of quantum theory has resolved the age-old conflict between the corpuscular and the wave theories of light. From the reflection and rectilinear propagation properties of Hero of Alexandria in the first century A.D., right through to the interference and wave properties of Young and Maxwell in the nineteenth century, this controversy raged. The quantum theory of radiation during the past half century, in a striking Hegelian manner, has *completely* resolved the dichotomy'.[2]

Let us now return to the logic of discovery of *old* quantum theory and, in particular, concentrate on its *positive heuristic*. Bohr's plan was to work out first the theory of the hydrogen atom. His first model was to be based on a fixed proton-nucleus with an electron in a circular orbit; in his second model he wanted to calculate an elliptical orbit in a fixed plane; then he intended to remove the clearly artificial restrictions of the fixed nucleus and fixed plane; after this he thought

[1] Einstein [1928]. Among the critics of the Copenhagen 'anarchism' we should mention – besides Einstein – Popper, Landé, Schrödinger, Margenau, Blokhinzev, Bohm, Fényes and Jánossy. For a defence of the Copenhagen interpretation, cf. Heisenberg [1955]; for a hard-hitting recent criticism, cf. Popper [1967]. Feyerabend in his [1968–9], makes use of some inconsistencies and waverings in Bohr's position for a crude apologetic falsification of Bohr's philosophy. Feyerabend misrepresents Popper's, Landé's and Margenau's critical attitude to Bohr, gives insufficient emphasis to Einstein's opposition, and seems to have forgotten completely that in some of his earlier papers he was more Popperian than Popper on this issue.

[2] Power [1964], p. 31 (my italics). '*Completely*' is meant here literally. As we read in *Nature* (**222**, 1969, pp. 1034–5): 'It is absurd to think that any fundamental element of [quantum] theory can be false...The arguments that *scientific* results are always temporary, cannot hold. It is the *philosophers'* conceptions of modern physics that are temporary, because they have not yet realized how profoundly the discoveries of quantum physics affect the whole of epistemology...The assertion that ordinary language is the ultimate source of the unambiguousness of physical description is verified most convincingly by the observational conditions in quantum physics.'

of taking the possible spin of the electron into account,[1] and then he hoped to extend his programme to the structure of complicated atoms and molecules and to the effect of electromagnetic fields on them, etc., etc. All this was planned right at the start: the idea that atoms are analogous to planetary systems adumbrated a long, difficult but optimistic programme and clearly indicated the policy of research.[2] 'It looked at this time – in the year 1913 – as if the authentic key to the spectra had at last been found, as if only time and patience would be needed to resolve their riddles completely.'[3]

Bohr's celebrated first paper of 1913 contained the initial step in the research programme. It contained his first model (I shall call it M_1) which already predicted facts hitherto unpredicted by any previous theory: the wavelengths of hydrogen's line emission spectrum. Though some of these wavelengths were known before 1913 – the Balmer series (1885) and the Paschen series (1908) – Bohr's theory predicted much more than these two known series. And tests soon corroborated its novel content: one additional Bohr series was discovered by Lyman in 1914, another by Brackett in 1922, and yet another by Pfund in 1924.

Since the Balmer and the Paschen series were known before 1913, some historians present the story as an example of a Baconian 'inductive ascent': (1) the chaos of spectrum lines, (2) an 'empirical law' (Balmer), (3) the theoretical explanation (Bohr). This certainly looks like the three 'floors' of Whewell. But the progress of science would hardly have been delayed had we lacked the laudable trials and errors of the ingenious Swiss school-teacher: the speculative mainline of science, carried forward by the bold speculations of Planck, Rutherford, Einstein and Bohr would have produced Balmer's results deductively, as test-statements of their theories, without Balmer's so-called 'pioneering'. In the rational reconstruction of science there is little reward for the pains of the discoverers of 'naive conjectures'.[4]

[1] This is rational reconstruction. As a matter of fact, Bohr accepted this idea only in his [1926].

[2] Besides this analogy, there was another basic idea in Bohr's positive heuristic: the 'correspondence principle'. This was indicated by him as early as 1913 (cf. the second of his five postulates quoted above on p. 56), but he developed it only later when he used it as a guiding principle in solving some problems of the later, sophisticated models (like the intensities and states of polarization). The peculiarity of this second part of his positive heuristic was that Bohr did not believe its metaphysical version: he thought it was a temporary rule until the replacement of classical electromagnetics (and possibly mechanics).

[3] Davisson [1937]. A similar euphoria was experienced by MacLaurin in 1748 over Newton's programme: Newton's 'philosophy being founded on experiment and demonstration, cannot fail till reason or the nature of things are changed... [Newton] left to posterity little more to do, but observe the heavens, and compute after his models' (MacLaurin [1748], p. 8).

[4] I use here 'naive conjecture' as a technical term in the sense of my [1963–4]. For a case study and detailed criticism of the myth of the 'inductive basis' of science (natural or mathematical) cf. *ibid.*, section 7, especially pp. 298–307. There I show that

As a matter of fact, Bohr's problem was not to explain Balmer's and Paschen's series, but to explain the paradoxical stability of the Rutherford atom. Moreover, Bohr had not even heard of these formulae before he wrote the first version of his paper.[1]

Not all the novel content of Bohr's first model M_1 was corroborated. For instance, Bohr's M_1 claimed to predict all the lines in the hydrogen emission spectrum. But there was experimental evidence for a hydrogen series where according to Bohr's M_1 there should have been none. The anomalous series was the Pickering–Fowler ultraviolet series.

Pickering discovered this series in 1896 in the spectrum of the star ζ Puppis. Fowler, after having discovered its first line also in the sun in 1898, produced the whole series in a discharge tube containing hydrogen and helium. True, it could be argued that the monster-line had nothing to do with the hydrogen – after all, the sun and ζ Puppis contain many gases and the discharge tube also contained helium. Indeed, the line could *not* be produced in a pure hydrogen tube. But Pickering's and Fowler's 'experimental technique', that led to a falsifying hypothesis of Balmer's law, had a plausible, although never severely tested, theoretical background: (*a*) their series had the same convergence number as the Balmer series and therefore was taken to be a hydrogen series and (*b*) Fowler gave a plausible explanation why helium could not possibly be responsible for producing the series.[2]

Bohr was not, however, very impressed by the 'authoritative' experimental physicists. He did not question their 'experimental precision' or the 'reliability of their observations', but questioned their observational theory. Indeed, he proposed an alternative. He first elaborated a new model (M_2) of his research programme: the model of ionized helium, with a double proton orbited by an electron. Now this model predicts an ultra-violet series in the spectrum of ionized

Descartes's and Euler's 'naive conjecture' that for all polyhedra $V-E+F=2$ was irrelevant and superfluous for the later development; as further examples one may mention that Boyle's and his successor's labours to establish $pv=RT$ was irrelevant for the later theoretical development (except for developing some experimental techniques), as Kepler's three laws may have been superfluous for the Newtonian theory of gravitation.

For further discussion of this point cf. *below*, p. 88.

[1] Cf. Jammer [1966], pp. 77 ff.

[2] Fowler [1912]. Incidentally his 'observational' theory was provided by 'Rydberg's theoretical investigations' which 'in the absence of strict experimental proof [he] regarded as justifying [his experimental] conclusion' (p. 65). But his theoretician colleague, Professor Nicholson, referred three months later to Fowler's findings as 'laboratory confirmations of Rydberg's theoretical deduction' (Nicholson [1913]). This little story, I think, bears out my pet thesis that most scientists tend to understand little more *about* science than fish about hydrodynamics.

In the Report of the Council to the Ninety-third Annual General Meeting of the Royal Astronomical Society, Fowler's 'observation in laboratory experiments' of new 'hydrogen lines which have so long eluded the efforts of the physicists' is described as 'an advance of great interest' and as 'a triumph of well-directed experimental work'.

helium which coincides with the Pickering–Fowler series. This constituted a rival theory. Then he suggested a 'crucial experiment': he predicted that Fowler's series can be produced, possibly with even stronger lines, in a tube which is filled with a mixture of helium and chlorine. Moreover, Bohr explained to the experimentalists, without even looking at their apparatus, the catalytic role of the hydrogen in Fowler's experiment and of chlorine in the experiment he suggested.[1] Indeed, he was right.[2] Thus the first apparent defeat of the research programme was turned into a resounding victory.

The victory, however, was immediately questioned. Fowler acknowledged that his series was not a hydrogen, but a helium series. But he pointed out that Bohr's monster-adjustment[3] still failed: the wavelengths in the Fowler series differ significantly from the values predicted by Bohr's M_2. Thus the series, although it does not refute M_1, still refutes M_2, and because of the close connection between M_1 and M_2, it undermines M_1![4]

Bohr brushed off Fowler's argument: *of course* he never meant M_2 to be taken too seriously. His values were based on a crude calculation based on the electron orbiting round a fixed nucleus; but *of course* it orbits round the common centre of gravity; *of course*, as is done when treating two-body problems, one has to substitute reduced mass for mass: $m_{e'} = m_e/[1+(m_e/m_n)]$.[5] This modified model was Bohr's M_3. And Fowler himself had to admit that Bohr was again right.[6]

The apparent refutation of M_2 turned into a victory for M_3; and it was clear that M_2 and M_3 would have been developed within the research programme – perhaps even M_{17} or M_{20} – without *any* stimulus from observation or experiment. It was at this stage that Einstein said of Bohr's theory: 'It is one of the greatest discoveries.'[7]

Bohr's research programme then went on as planned. The next step was to calculate elliptical orbits. This was done by Sommerfeld in 1915, but with the (unexpected) result that the increased number of possible

1 Bohr [1913*b*].

2 Evans [1913]. For a similar example of a theoretical physicist teaching a refutation-keen experimentalist what he – the experimentalist – had really observed, cf. *above*, p. 45, n. 5.

3 Monster-adjustment: turning a counterexample, in the light of some new theory, into an example. Cf. my [1963–4], pp. 127 ff. But Bohr's 'monster-adjustment' was empirically 'progressive': it predicted a new fact (the appearance of the 4686 line in tubes containing no hydrogen).

4 Fowler [1913*a*].

5 Bohr [1913*c*]. This monster-adjustment was also 'progressive': Bohr predicted that Fowler's observations must be slightly imprecise and the Rydberg 'constant' must have a fine structure.

6 Fowler [1913*b*]. But he sceptically noted that Bohr's programme had not yet explained the spectrum lines of *un-ionized*, ordinary helium. However, he soon abandoned his scepticism and joined Bohr's research programme (Fowler [1914]).

7 Cf. Hevesy [1913]: 'When I told him of the Fowler spectrum, the big eyes of Einstein looked still bigger and he told me: "Then it is one of the greatest discoveries."'

steady orbits did *not* increase the number of possible energy levels, so there seemed to be no possibility of a crucial experiment between the elliptical and circular theory. However, electrons orbit the nucleus with very high velocity so that when they accelerate their mass should change noticeably if Einsteinian mechanics is true. Indeed, calculating such relativistic corrections, Sommerfeld got a new array of energy levels and thus the 'fine-structure' of the spectrum.

The switch to this new relativistic model required much more mathematical skill and talent than the development of the first few models. Sommerfeld's achievement was primarily mathematical.[1]

Curiously, the doublets of the hydrogen spectrum had already been discovered in 1891 by Michelson.[2] Moseley pointed out immediately after Bohr's first publication that 'it fails to account for the second weaker line found in each spectrum'.[3] Bohr was not upset: he was convinced that the positive heuristic of his research programme would, *in due course*, explain and even correct Michelson's observations.[4] And so it did. Sommerfeld's theory was, of course, inconsistent with Bohr's first versions; the fine structure experiments – with the old observations corrected! – provided the crucial evidence in its favour. Many defeats of Bohr's first models were turned by Sommerfeld and his Munich school into victories for Bohr's research programme.

It is interesting that just as Einstein got worried and slowed down in the middle of the spectacular progress of quantum physics by 1913, Bohr got worried and slowed down by 1916; and just as Bohr had, by 1913 taken the initiative from Einstein, Sommerfeld had taken the initiative from Bohr by 1916. The difference between the atmosphere of Bohr's Copenhagen school and Sommerfeld's Munich school was conspicuous: 'In Munich one used more concrete formulations and was therefore more easily understood; one had been successful in the systematization of spectra and in the use of the vector model. In Copenhagen, however, one believed that an adequate language for the new [phenomena] had not yet been found, one was reticent in the face of too definite formulations, one expressed oneself more cautiously and more in general terms, and was therefore much more difficult to understand.'[5]

Our sketch shows how a progressive shift may lend credibility – and a *rationale* – to an inconsistent programme. Born, in his obituary of

[1] For the vital mathematical aspects of research programmes, cf. *above*, p. 52.
[2] Michelson [1891–2], especially pp. 287–9. Michelson does not even mention Balmer.
[3] Moseley [1914]. [4] Sommerfeld [1916], p. 68.
[5] Hund [1961]. This is discussed at some length in Feyerabend [1968–9], pp. 83–7. But Feyerabend's paper is heavily biased. The main aim of his paper is to play down Bohr's methodological anarchism and show that Bohr *opposed* the Copenhagen interpretation of the *new* (post-1925) quantum programme. In order to do so, Feyerabend, on the one hand, overemphasizes Bohr's unhappiness about the inconsistency of the *old* (pre-1925) quantum programme and, on the other hand, makes too much of the fact that Sommerfeld cared less for the problematicality of the inconsistent foundations of the *old* programme than Bohr.

Planck, describes this process forcefully: 'Of course the mere introduction of the quantum of action does not yet mean that a *true* Quantum Theory has been established...The difficulties which the introduction of the quantum of action into the well-established classical theory has encountered from the outset have already been indicated. They have gradually increased rather than diminished; and although research in its forward march has in the meantime passed over some of them, the remaining gaps in the theory are the more distressing to the conscientious theoretical physicist. In fact, what in Bohr's theory served as the basis of the laws of action consists of certain hypotheses which a generation ago would doubtless have been flatly rejected by every physicist. That within the atom certain quantized orbits (i.e. picked out on the quantum principle) should play a special role could well be granted; somewhat less easy to accept is the further assumption that the electrons moving on these curvilinear orbits, and therefore accelerated, radiate no energy. But that the sharply defined frequency of an emitted light quantum should be different from the frequency of the emitting electron would be regarded by a theoretician who had grown up in the classical school as monstrous and almost inconceivable. But numbers [or, rather, *progressive problemshifts*] decide, and in consequence the tables have been turned. While originally it was a question of fitting in with as little strain as possible a new and strange element into an existing system which was generally regarded as settled, *the intruder, after having won an assured position, now has assumed the offensive*; and it now appears certain that it is about to blow up the old system at some point. The only question now is, at what point and to what extent this will happen.'[1]

One of the most important points one learns from studying research programmes is that relatively few experiments are really important. The heuristic guidance the theoretical physicist receives from tests and 'refutations' is usually so trivial that large-scale testing – or even bothering too much with the data already available – may well be a waste of time. In most cases we need no refutations to tell us that the theory is in urgent need of replacement: the positive heuristic of the programme drives us forward anyway. Also, to give a stern 'refutable interpretation' to a fledgling version of a programme is dangerous methodological cruelty. The first versions may even 'apply' only to non-existing 'ideal' cases; it may take decades of theoretical work to arrive at the first novel facts and still more time to arrive at *interestingly testable* versions of the research programmes, at the stage when refutations are no longer foreseeable in the light of the programme itself.

The dialectic of research programmes is then not necessarily an alternating series of speculative conjectures and empirical refutations. The interaction between the development of the programme and the empirical checks may be very varied – which pattern is actually realized

[1] Born [1948], p. 180, my italics.

depends only on historical accident. Let us mention three typical variants.

(1) Let us imagine that each of the first three consecutive versions, H_1, H_2, H_3 predict some new facts successfully but others unsuccessfully, that is each version is both corroborated *and* refuted in turn. Finally H_4 is proposed which predicts some novel facts but stands up to the severest tests. The problemshift is progressive, and also we have a beautiful Popperian alternation of conjectures and refutations.[1] People will admire this as a classical example of theoretical and experimental work going hand in hand.

(2) Another pattern could have been a lone Bohr (possibly without Balmer preceding him), working out H_1, H_2, H_3, H_4 but self-critically withholding publication until H_4. Then H_4 is tested: all the evidence will turn up as corroborations of H_4, the first (and only) published hypothesis. The theoretician – at his desk – is here seen to work far ahead of the experimenter: we have a period of relative autonomy of theoretical progress.

(3) Let us now imagine that *all* the empirical evidence mentioned in these three patterns is already there at the time of the invention of H_1, H_2, H_3, H_4. In this case H_1, H_2, H_3, H_4 will not represent an empirically progressive problemshift and therefore, although all the evidence supports his theories, the scientist has to work on further in order to prove the scientific value of his programme.[2] Such a state of affairs may be brought about either by the fact that an older research programme (which has been challenged by the one leading to H_1, H_2, H_3, H_4) had already produced all these facts – or by the fact that too much government money lay around for collecting data about spectrum lines and hacks stumbled upon all the data. However, the latter case is extremely unlikely, for, as Cullen used to say, 'the number of false facts, afloat in the world, infinitely exceeds that of the false theories;'[3] in most such cases the research programme will clash with the available 'facts', the theoretician will look into the 'experimental techniques' of the experimentalist, and having overthrown and replaced his observational theories will correct his facts thereby producing *novel* ones.[4]

[1] In the first three patterns we do not involve complications like successful appeals against the verdict of the experimental scientists.

[2] This shows that if exactly the same theories and the same evidence is rationally reconstructed in different time orders, they may constitute either a progressive or a degenerative shift. Also cf. volume 2, chapter 8, p. 178.

[3] Cf. McCulloch [1825], p. 19. For a strong argument on how extremely unlikely such a pattern is, see *below*, p. 70.

[4] Perhaps it should be mentioned that manic data collection – and 'too much' precision – prevents even the formation of naive 'empirical' hypotheses like Balmer's. Had Balmer known of Michelson's fine-spectra, would he have ever found his formula? Or, had Tycho Brahe's data been more precise, would Kepler's elliptical law ever have been put forward? The same applies to the naive first version of the general gas law, etc. The Descartes–Euler conjecture on polyhedra might never have been made but for the scarcity of data; cf. my [1963–4], pp. 298 ff.

After this methodological excursion, let us return to Bohr's programme. Not all developments in the programme were foreseen and planned when the positive heuristic was first sketched. When some curious gaps appeared in Sommerfeld's sophisticated models (some predicted lines never did appear), Pauli proposed a deep auxiliary hypothesis (his 'exclusion principle') which accounted not only for the known gaps but reshaped the shell theory of the periodic system of elements and anticipated facts then unknown.

I do not wish to give here an elaborate account of the development of Bohr's programme. But its detailed study from the methodological viewpoint is a veritable goldmine: its marvellously fast progress – on inconsistent foundations! – was breathtaking, the beauty, originality and empirical success of its auxiliary hypotheses, put forward by scientists of brilliance and even genius, was unprecedented in the history of physics.[1] Occasionally the next version of the programme required only a trivial improvement, like the replacement of mass by reduced mass. Occasionally, however, to arrive at the next version required new sophisticated mathematics, like the mathematics of the many-body problem, or new sophisticated physical auxiliary theories. The additional mathematics or physics was either dragged in from some part of extant knowledge (like relativity theory) or invented (like Pauli's exclusion principle). In the latter case we have a 'creative shift' in the positive heuristic.

But even this great programme came to a point where its heuristic power petered out. *Ad hoc* hypotheses multiplied and could not be replaced by content-increasing explanations. For instance, Bohr's theory of molecular (band) spectra predicted the following formula for diatomic molecules:

$$\nu = \frac{h}{8\pi^2 I}[(m+1)^2 - m^2]$$

But the formula was refuted. Bohrians replaced the term m^2 by $m(m+1)$: this fitted the facts but was sadly *ad hoc*.

Then came the problem of some unexplained doublets in alkali spectra. Landé explained them in 1924 by an *ad hoc* 'relativistic splitting rule', Goudsmit and Uhlenbeck in 1925 by electron spin. If Landé's explanation was *ad hoc*, Goudsmit's and Uhlenbeck's was also inconsistent with special relativity theory: surface points on the largish electron had to travel faster than light, and the electron had even to be bigger than the whole atom.[2] Considerable courage was needed to

[1] 'Between the appearance of Bohr's great trilogy in 1913 and the advent of wave mechanics in 1925, a large number of papers appeared developing Bohr's ideas into an impressive theory of atomic phenomena. It was a collective effort and the names of the physicists contributing to it make up an imposing roll-call: Bohr, Born, Klein, Rosseland, Kramers, Pauli, Sommerfeld, Planck, Einstein, Ehrenfest, Epstein, Debye, Schwarzschild, Wilson' (Ter Haar [1967], p. 43).

[2] A footnote in their paper reads: 'It should be observed that [according to our theory] the peripheral velocity of the electron would considerably exceed the velocity of light' (Uhlenbeck and Goudsmit [1925]).

propose it. (Kronig got the idea earlier but refrained from publishing it because he thought it was inadmissible.[1])

But temerity in proposing wild inconsistencies did not reap any more rewards. The programme lagged behind the discovery of 'facts'. Undigested anomalies swamped the field. With ever more sterile inconsistencies and ever more *ad hoc* hypotheses, the degenerating phase of the research programme had set in: it started – to use one of Popper's favourite phrases – 'to lose its empirical character'.[2] Also many problems, like the theory of perturbations, could not even be expected to be solved within it. A rival research programme soon appeared: wave mechanics. Not only did the new programme, even in its first version (de Broglie, 1924), explain Planck's and Bohr's quantum conditions; it also led to an exciting new fact, to the Davisson–Germer experiment. In its later, ever more sophisticated versions it offered solutions to problems which had been completely out of the reach of Bohr's research programme, and explained the *ad hoc* later theories of Bohr's programme by theories satisfying high methodological standards. Wave mechanics soon caught up with, vanquished and replaced Bohr's programme.

De Broglie's paper came at the time when Bohr's programme was degenerating. But this was mere coincidence. One wonders what would have happened if de Broglie had written and published his paper in 1914 instead of 1924.

(*d*) *A new look at crucial experiments: the end of instant rationality*

It would be wrong to assume that one must stay with a research programme until it has exhausted all its heuristic power, that one must not introduce a rival programme before everybody agrees that the point of degeneration has probably been reached. (Although one can understand the irritation of a physicist when, in the middle of the progressive phase of a research programme, he is confronted by a proliferation of vague metaphysical theories stimulating no empirical progress.[3]) One must never allow a research programme to become a *Weltanschauung*, or a sort of *scientific rigour*, setting itself up as an arbiter between explanation and non-explanation, as mathematical rigour sets itself up as an arbiter between proof and non-proof. Unfortunately this is the position which Kuhn tends to advocate:

[1] Jammer [1966], pp. 146–8 and 151.

[2] For a vivid description of this degenerating phase of Bohr's programme, cf. Margenau [1950], pp. 311–13.

In the progressive phase of a programme the main heuristic stimulus comes from the positive heuristic: anomalies are largely ignored. In the degenerating phase the heuristic power of the programme peters out. In the absence of a rival programme this situation may be reflected in the psychology of the scientists by an unusual hypersensitivity to anomalies and by a feeling of a Kuhnian 'crisis'.

[3] This is what must have irritated Newton most in the 'sceptical proliferation of theories' by Cartesians.