[INCOMPLETE – Written for the Adolfest Conference at Pittsburgh in April 2003 to celebrate Adolf Grünbaum's 80th birthday. Most references missing; not for quotation in its present form]

Adhocness and Content-Increase: is there life after Grünbaum?

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1. *Introduction: Grünbaum's attack on Popper's notion of increase of content as the hallmark of scientific advance*

Most of us believe that theory-change in science has been a rationally analysable process. We believe, that is, that when one theory, Newton's for example, is replaced as the accepted theory in science by a rival, Einstein's in the same example, it is because the newer theory turns out to be better than the old in some objective sense and a sense, moreover, crucially related to the experimental evidence. Even those who have abjectly surrendered (at any rate on Mondays, Wednesdays and Fridays) to an overly subjectivist form of Bayesianism, believe this really – hence their desperate pointing in the direction of (in my view, unavailing) results concerning the 'washing out of priors'. The problem has always been to construct a set of agreed criteria of theory-appraisal that would convincingly yield this generally agreed result. (Of course by 'most of us' here I mean 'most *philosophers* of science'; nothing I say will cut much ice with some of our benighted sociologist colleagues.)

Let's, in accordance with usual practice, separate two types of theory-change: those 'theory-modifications' associated with Kuhnian 'normal science' or Lakatosian intra-research programme change – let's call them "mini-changes" - on the one hand, and the sorts of (apparently) radical theory change associated with Kuhnian 'revolutions' or switches of Lakatosian research programme – let's call them "mega-changes" - on the other. A much-analysed "mini-change" is that which led to the prediction of the existence of the planet Neptune. Here the initial theoretical system consisted of Newton's laws of motion plus the principle of universal gravitation together with the auxiliary that there are 7 planets of which Uranus is the furthest from the sun; and the change was to a revised framework involving those same general laws but now conjoined with Adams's and Leverrier's new auxiliary assumption that there is a further trans-Uranian planet, Neptune. (Of course a number of other auxiliary assumptions such as that the only non-negligible forces on any planet in the solar system are its gravitational interactions with the sun and other planets were common to both the old and new frameworks.) Examples of "mega-changes" include the shift from the corpuscular to the wave theory of light in the early 19th century or that from Newtonian to relativistic physics in the early 20th.

All the instances I just mentioned are of changes that were accepted within science and that seem intuitively like success stories. Other proposed changes – we will meet some examples later - have, on the contrary, *not* been generally accepted. If science is the rational process that most of us believe it to be there must, it seems, be some clear-cut criterion that separates the sheep from the goats – some general characteristic that marks out the rationally or scientifically justified changes from the rest. Karl Popper several times argued for the claim (attractive for its unity, at any rate) that *both* mini- *and* mega-changes, when rational or scientific, always in fact involve an *increase in content*.[1] As we shall see in a moment, Popper attempted various different (semi-) formal analyses of the idea of content-increase, but the intuitive idea seems, at first glance, clear at any rate in the case of mini-changes. Take again the celebrated case of Adams and Leverrier: their postulation of the existence of the trans-Uranian planet, Neptune, was certainly *ad hoc* in the straightforward sense that it was specifically introduced as an attempted solution of the problem posed by the inconsistency of the data on Uranus's orbit with the conjunction of Newton's theory and the then accepted auxiliaries; it was, however, also "content-increasing", and therefore potentially a genuine scientific advance, in that the new theoretical framework including Adams' and Leverrier's revised auxiliary *not only* had the correct consequences about Uranus's orbit, but also of course predicted the existence of the new planet and, at least roughly, its mass and orbit.

The Adams and Leverrier shift, then, produced a theoretical framework that seems to have had extra content, extra content which, moreover, lent itself to independent

[1] As usual, more complicated than this. Popper sometimes appeals to the 'dimension' of a theory – intuitively a measure of the simplicity of its falsifiers –as a measure of falsifiability and therefore, in his terms, acceptability ahead of the evidence. However he never gets close to satisfactorily analysing this notion, and his preferred account does seem to be in terms of content-increase.

empirical test. This is to be contrasted with cases in which the modified theoretical framework (modified by the inclusion of a new auxiliary assumption at the cost of another) allegedly *merely* solves the problem it was introduced to solve – that is, the modified framework gets right the phenomena that were inconsistent with the unmodified framework, but seems to have no further content and hence cannot be tested *independently* – independently, that is, of the phenomena that already tested (and in at least one case clashed with) its predecessor.

Instances that have often been cited as examples of *failure* of content-increase are the invocation of a major epicycle in Ptolemaic astronomy to explain planetary stations and retrogressions and (incorrectly as Grünbaum showed)[2] the postulation of the Lorentz-Fitzgerald contraction hypothesis within classical physics to explain the anomalous 'negative' result of the Michelson-Morley experiment. Taking the simpler Ptolemaic case: the initial model of a planet, Mars, say, travelling on a single circular orbit around a stationary earth, predicts that we will observe constant eastward motion of the planet around the sky (superimposed, of course, on a constant apparent diurnal westward rotation with the fixed stars); this is directly refuted by the observation that Mars's generally eastward motion is periodically interrupted by times at which its observed motion slows until it halts momentarily and then begins briefly to move backwards in a westward direction before again slowing and turning back towards the east. The introduction of an epicycle of suitable size and the assumption that Mars moves around the centre of that epicycle at a suitable velocity while the whole epicycle itself is carried around the main circular orbit (now called the deferent) leads to the correct prediction that Mars will exhibit these stations and retrogressions. However, according to the often cited story, this is *all* that the epicycle does: it merely restores consistency with the previously observed data but the resulting system has no further content and hence cannot be independently tested. This case is also often cited as an example of a move which is *ad hoc* 'in the pejorative sense' – there is of course nothing wrong with introducing a new hypothesis or theory specifically to solve a certain empirical problem, but, so it has been claimed, such solutions are cheap if they do not admit of tests independent of the phenomena they were introduced to explain and they cannot, it seems, admit of independent test, if the

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theoretical systems of which they are part fail to exhibit content-increase over the predecessor theoretical system. A potential *mega*-change proposed at any rate by some thinkers – I use the term lightly – would be the replacement of Darwinian by Creationist theory. But this would clearly fail the content-increase test - a failure that is part of the justification of the fact that this proposed theory-shift has not been made within proper science.

So, allowing for a certain amount of rational reconstruction, and using T to stand for a 'central' or 'core' theory and A to stand for an appropriate conjunction of auxiliary assumptions, Popper had two main claims:[3]

1. It is at least a necessary condition for the scientific legitimacy of the mini-shift from T&A to T& A' that it be content-increasing.
2. It is at least a necessary condition for the scientific legitimacy of the 'revolutionary' mega-shift from a theoretical system built around T to one built around T' (inconsistent with T) that it be content-increasing.

And of course he tied this notion of content-increase to his ideas about falsifiability being the hallmark of science and increased falsifiability being the hallmark of progress in science. (In line with his – exaggerated - emphasis on falsifiability, Popper usually - though not always[4] - fails to stress the surely intuitive further requirement that at least some of these content-increases be successful, that is lead to further empirically-checkable predictions that are actually correct. We will return to this later, let's for the moment just concentrate on content-increase as an alleged *necessary* condition for scientific legitimacy.)

Popper offered no less than six different more detailed analyses of the content of a theory that might be used to underwrite these two theses involving the intuitive idea of content-increase. In a series of three important articles in the *British Journal for the Philosophy of Science* in 1976,[5] Adolf Grünbaum demonstrated in uncompromising detail just why five of Popper's analyses of the notion of the content

[3] refs
[4]
[5] refs

of a theory (and when one theory exhibits increased content relative to another) cannot in fact coherently underwrite either of the theses 1 and 2. The Popperian analyses that Grünbaum attacked are those that do not attempt to introduce a notion of the *measure* of a theory's content. These non-metrical construals of content-increase seem initially to have the better chance of underwriting theses 1 and 2 within a Popperian framework at least. This is because Popper's favoured metrical notion ct(T), given by ct(T) = 1- Prob(T), seems pretty obviously hopeless. In fact this way of measuring content, when conjoined with Popper's favourite thesis that all universal theories have probability zero, trivially yields the result that all such universal theories have ct = 1. Hence on Popper's favoured metrical notion, there can be no increase in content in going from Newton's to Einstein's theory or from the theoretical framework of classical physics pre-Adams and Leverrier to that framework after the postulation of the existence of Neptune. This is because both theories and both frameworks would, according to Popper, have the same - maximal - content.

The natural *non*-metrical construal of the content of any theory is as its *consequence class* – the deductively closed set of the theory's logical consequences. Since all such classes are infinite (denumerably infinite in any sensible language), the idea of using cardinality considerations to distinguish theories with different amounts of content is a clear non-starter and the only ordering relation that seems to offer itself is the subset relation. However, as Grünbaum quickly shows, the idea that we could use this notion of content to underwrite the claim that Einstein's theory has greater content than Newton's will not remotely work either. The basic reason is that the two theories are *logically inconsistent* and hence each entails the negation of the other. Assuming that the individual theories N and E taken singly are consistent, then of course their mutual inconsistency implies that it cannot be the case that $C(N) \subseteq C(E)$, let alone $C(N) \subset C(E)$, since, for one thing, $N \varepsilon C(N)$ but not $\varepsilon C(E)$.

Grünbaum showed that essentially the same difficulty afflicts Popper's notion of the *informative content* of a theory (just the set of sentences inconsistent with the theory) and the *empirical content* of a theory (on Popper's idiosyncratic formulation the intersection of the informative content with the set of sentences expressible in purely observational terms – that is, the set of observation sentences that *contradict* the theory). Obviously since E and N contradict one another, E ε IC(N) and N ε IC(E) so again on the assumption that each is internally consistent neither IC(E) $\subset$ IC (N) nor, more importantly for Popper's concerns, IC(N) $\subset$ IC (E) holds. Similarly, since the inconsistency between the two theories cannot be restricted to the purely theoretical level, there are empirical consequences that Newton has that are not also entailed by Einstein (indeed ones whose negations are entailed by Einstein's theory) and so again the subset relation required by Popper trivially fails to hold.

What Popper seems to mean by the "problem content" of a theory T can be constructed by simply taking every logical consequence a of T, and appending a question mark to it – that is PC (T) = {is it the case that a?/a ε C(T)}. As he himself points out, this characterisation supplies a 1-1 correspondence between PC(T) and C(T); and so it follows that the same inability to underwrite his theses 1 and 2 that affects his characterisation C(T) applies equally well to PC(T).

This leaves the idea that the content of a theory should be thought of as the set of questions to which it gives answers. Popper talks of this as a 'generalisation' of the Tarskian content C(T) and certainly the notion of the question-answering content of a theory is altogether vaguer and hence less susceptible to clear-cut negative results. However, Grünbaum argued that Popper's attempt to show that Einstein's theory has greater content than Newton's fails on this question-answering construal of content just as badly as it does on the other construals.[6]

It might seem at first sight that Popper had much better prospects of underwriting his claim about content increase when it comes to cases of 'mini-changes' – such as the Adams and Leverrier case. But Grünbaum demonstrated that there too none of Popper's accounts of content can possibly succeed. The basic problem is exactly the same. Collapsing Newton's theory together with those auxiliaries that remained constant in this episode into T, letting A be the pre-Adams-and-Leverrier statement of

[6] I agree with Adolf's conclusion here, though I have some qualms about some of the assumptions. I guess how exactly (more formally) we construe 'ordinary language' questions is always bound to be somewhat arbitrary – but I am one of those who favour a two-valued approach avoiding 'obviation' (so I am one of those who thinks that the right answer for me to the famous question 'Have you stopped beating your wife?' is 'I haven't' (ie no)). But this just pushes Popper's 'question content' toward his 'problem content' to which Adolf's strictures clearly apply (this is why I agree with the conclusion)…

the number, positions and masses of the planets, and A’ the corresponding statement post-Adams and Leverrier (now including the assertion of the existence of Jupiter), then A’ is of course inconsistent with A, and so *a fortiori* is T&A’ with T&A. Hence as before since $\neg$ (T&A ε T&A’) (assuming the latter consistent) it cannot be the case that T&A $\subset$ T&A’. And the IC, EC and PC construals all fail for essentially the same reason. Finally Grünbaum goes on to argue that it is at best unclear that Popper’s question content QC can do any better.

Popper’s aim in the case of such “mini-changes” was of course, as noted earlier, to use the idea of content increase to differentiate the Adams and Leverrier case and others of its scientifically creditable ilk from allegedly *ad hoc* shifts such as that supposedly involved in the invocation of the Lorentz-Fitzgerald hypothesis or Ptolemaic epicycles. The idea was that those theory-shifts that many have identified as *ad hoc* in the pejorative sense are exactly those in which there is *no* increase of content. Clearly, then, Grünbaum’s demonstration that Popper has produced no sense in which the change of an auxiliary assumption from A to an A’ inconsistent with it *ever* produces increased content has an equally negative impact on Popper’s thesis about what characterises *ad hoc* hypotheses.

There is surely no way to save Popper’s particular analyses of content-increase and of *ad hoc* hypotheses from Grünbaum’s onslaught. Nonetheless it is difficult – for me at least – to relinquish entirely the idea that there are important intuitions here which, when properly analysed, will supply notions that play major roles in the account of rational theory-change in science. In the remainder of this talk, I will attempt to move towards what I hope will prove more defensible analyses of these notions (of course whether in the end they should count as analyses of ‘content increase’ or ‘adhocness’ rather than as analyses of *replacements* for these notions is up for grabs). I should say that in doing this, I am by no means attempting to reopen a possibility that Grünbaum sees himself as having closed off. To the contrary, and with his invariable precision and clarity, Grünbaum is explicit that his strictures apply, directly at least, only to Popper’s own attempted definitions and characterisations. Indeed he himself mentions in passing one of the crucial ideas behind what I think is the right approach to the issue of content-increase in the case of “mega-changes”;[7] and he of course developed his own hierarchy of types of *adhoc*ness (albeit while inclining to agree with Hempel that there may in the end be no precise, logical account of the notion to be had). A more accurate, but altogether clumsier, title for this talk would thus have been ‘*Adhoc*ness and content-increase: is there life after Grünbaum’s demolition of Popper’s analyses?’

2. ‘Scientific Revolutions’ and Content-increase: life after Grünbaum?

It is not clear that even Popper’s starting-point when it comes to grand theory-change (“mega-change”) was sensible. Do we really have anything like a clear-cut intuition that Relativity theory has greater content than Newton’s theory? Certainly the former is empirically accurate over a greater range; but that seems an entirely different matter – a question of having more correct (empirical) content rather than having more content *simpliciter*. Let’s start then instead with a case where we do surely have clear-cut intuitions.

Consider the shift to Newton’s theory from the union of Galilean physics and Kepler’s laws. There seems here to be a clear intuitive sense in which this is a shift to a theory of greater content – while Galileo’s laws cover the motion of freely falling bodies close to the earth’s surface and terrestrial projectiles, and Kepler’s laws cover the motions of the planets, Newton’s theory is a truly universal theory covering all motions of *all* bodies, terrestrial bodies, planets and all other bits of matter both in the solar system and beyond; moreover, while Galileo’s and Kepler’s laws tell us only *how* projectiles and planets move, Newton’s theory goes on to tell us *why* they move as they do.

Notice however that, as decades of criticism of Nagel’s account of ‘homogeneous reductions’ has revealed, and as was already seen clearly in advance by Duhem and later by Popper,[8] even in this case matters are not as straightforward as they might initially appear. Newton’s theory is strictly inconsistent with Galileo’s laws and with

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[7] Reference to Havas. Of course G is right that Popper cannot help himself to Havas’s ideas to explicate his notion of content-increase. Section 2 of my paper can be viewed simply as an elaboration of this point.
[8] refs

Kepler's laws – for example, the acceleration of a body falling from the top of the tower of Pisa, according to Newton's theory is, even ignoring its gravitational interaction with any body in the universe except the earth, not constant as it falls as Galileo's law of free fall asserts it to be, but is rather a function of the body's (of course changing) distance from the centre of mass of the earth; similarly, the planets do not move in the strict ellipses required by Kepler's first law according to Newton's theory because of the planet's gravitational interaction with bodies other than the sun.

As a result of this inconsistency, Grünbaum's argument contra-Popper again applies to show that the content of Galileo's laws or the content of Kepler's laws (taking the natural consequence class notion of content) cannot form proper subsets of the content of Newton's theory. What is it then that underlies this – here, I think, firm – intuition that Newton's theory constitutes content-increase over its predecessors? Basically, surely, the fact that we *can* derive as special cases of Newton's theory, not Galileo's laws or Kepler's laws themselves, but rather *replacements* for them which, although strictly inconsistent with the laws they replace, are either empirically indistinguishable from them, or distinguishable only within some very small margin.[9]

This is clearest in the case of Galileo's law of free fall. As we already saw, Newton's theory (again making the approximating assumption that the only gravitational effect on the falling body is that of the earth and ignoring air resistance) entails that the body's acceleration is not constant but rather satisfies

$$a = G.m_E/r^2$$

where G and the mass of the earth m_E are both constant alright, but r is the – of course changing – distance between the centre of mass of the body and the centre of mass of the Earth. However, setting r = r' + R, where R is the radius of the Earth and r' the distance from the earth's surface, we see that, even when the body is at the top of the Tower, R is massive in comparison with r', and so r in fact changes very little – in fact by far too little to be observationally detectable - during the fall. We have then a replacement for Galileo's law of free fall (call it G') which is (a) observationally

[9] ref to John Watkins

indistinguishable from G and (b) unlike G itself *is* a consequence of Newton's theory. Letting N be Newton's theory, the judgement of content-increase is based, properly based, not on the non-fact that $C(G) \subset C(N)$ but rather on the genuine fact that $C(G') \subset C(N)$. Notice moreover that this relationship is asymmetric: neither N itself, nor any observationally indistinguishable modification of it N', follows as a special case of G.

Another theory-shift that intuitively seems clearly content-increasing is the one from Fresnel's elastic solid wave theory of light to Maxwell's electromagnetic theory. Here the intuitive judgement is that Maxwell's theory does everything that Fresnel's did, saving just as well all the known phenomena of visible light, but it also revealed that the visible spectrum is but a meagre portion of the whole electromagnetic spectrum, and of course Maxwell's theory had much to say about the non-visible part of the spectrum. Maxwell's theory swallows whole the content of Fresnel's theory and goes on to add much distinctive content of its own.

Again however this loose description suggests a much closer agreement with the Nagel reduction model than is strictly justified. Fresnel's theory is *not* a special case, a sub-theory, of Maxwell's. How could it be when it centrally presupposes an all-pervading elastic solid ether, vibrations in which constitute light, while Maxwell's theory – at any rate in what might be called its definitive or mature form – rejects such an elastic ether and attributes light instead to a displacement current in a *sui generis* electromagnetic field? (It is well known that Maxwell himself and some of his followers, such as Kelvin, hoped to show that the electromagnetic field could be, in turn, explained as resulting from the contortions of some underlying mechanical medium of the Fresnel type. However the repeated failure to produce a 'mechanical model' that was independently testable led eventually to the acceptance that there is no such medium and that the field is a separate, independent entity.)

But although Fresnel's theory itself is not a sub-theory of Maxwell's, a structurally identical facsimile of it is. If we concentrate on the mathematical equations entailed by Fresnel - for example, his equations for the relative intensities of the light polarised in the plane of reflection and in the plane orthogonal to it, of the reflected and refracted beams when a beam of light is incident at the interface between two optically different media - these equations are also derivable from Maxwell's theory. Once again, although these are the exact same equations, it is not Fresnel's theory that is thus derived within Maxwell – the terms in the equations representing the amplitudes of the waves refer for Fresnel to the extent to which real particles of the ether are moved away from their equilibrium positions during the passage of the light, while for Maxwell those same terms, standing in the same relations, refer to forced vibrations of the electric and magnetic field vectors.

So, as in the Galileo to Newton case, the judgement that there is content-increase in the Fresnel to Maxwell case is based, properly based, not on the fact that $C(F) \subset C(M)$, but rather on the fact that there is an F', in this case involving exactly the same equations as F, and for which $C(F') \subset C(M)$ – in the straightforward and uniquely legitimate sense that M entails F'. As before, this is asymmetric: there clearly is no "facsimile" M' of M such that it is a sub-theory of Fresnel's. Basically because Fresnel's theory is silent about any connections between optical and electro-magnetic phenomena

Returning then to the case of the switch from Newton to Einstein, in dispute between Popper and Grünbaum, this seems to me best thought of as an amalgam of the two cases we just considered. As before there is, I think, no clearcut sense in which Einstein's theory has greater content than Newton's theory itself; instead there is a replacement N' for Newton's theory which is (a) such that its empirically testable predictions are, over a significant range of phenomena, observationally indistinguishable from those of Newton's theory itself and (b) is, unlike Newton's theory, a genuine sub-theory of Einstein's. The logical shift from Newton's theory, N, itself to N' involves however both of the manoeuvres we saw exemplified in these two earlier cases. As in the Fresnel-Maxwell case, although we finish up with equations which do much of the same predictive work, the terms involved are interpreted quite differently within Newton's and Einstein's theory – we may still have the m and the t, for example, but now they mean quite different things. And as in the Galileo-Newton case (though unusually, indeed seemingly uniquely, not in the Fresnel-Maxwell one), N' is not identical to N (even laying aside interpretative issues) but only observationally equivalent by virtue of 'tending to it' under a certain limiting process. (The only reason we don't quite talk this way in the Galileo-Newton case is that we know we can't build towers that have heights comparable to the radius of the earth so that the restriction to 'ordinary' falls can be left implicit.)

Once again the relationship of content-increase involved here is, I take it, asymmetric: there is no 'replacement' E' for Einstein's theory E which stands in the same relationship to N as our N' does to the whole of E.

Some of you will not be surprised to hear that I think that all this reflects, and reflects credit on, a position called structural realism[10] – though this doesn't of course mean that it is false!

In sum, then, I think that there is a sense in which the progress of science – *mature* science – has been, despite so-called revolutions, progressive because 'essentially' cumulative: the newer theory in a complex but significant sense retains the older one and adds extra stuff. Popper tried to capture this sense in which science is 'essentially' cumulative in a way that Grünbaum demonstrated was naïve. But the judgment of *essential* cumulativity (at least for "mature science" – i.e. physics) can be salvaged *via* a more sophisticated analysis.

3. ***Ad hoc*** **versus *Content-increasing moves*** **within *research programmes: life after Grünbaum?***

Let's now turn to the case of "mini-changes" – change of 'auxiliary' rather than 'central' or 'core' theory. Here again, remember, Popper claimed that what differentiates scientifically acceptable changes of auxiliary assumption such as the Adams and Leverrier postulation of Neptune from cases like the Ptolemaists' invocation of epicycles is that the scientifically acceptable shifts are content increasing. Those shifts that are not content increasing are *ad hoc* (in the pejorative sense). Again, Grünbaum showed that Popper's attempts to underwrite these

[10] ref

judgments totally fail: the good shifts just cannot exhibit overall content increase in any serious sense, because the theoretical system shifted to is inconsistent with the one shifted from. Again however there seems to be an important differentiating characteristic at play here – something to do with the good shifts exhibiting increased testability - a characteristic that Popper perhaps sensed but certainly failed accurately to describe. Can we do better?

It's best, I think, to begin by considering a few illustrative examples.

3(a) ***Pure Adhoccery: Velikovsky***

Most cases from real science that have attracted claims of (pejorative) *adhoc*ness turn out, on inspection, to be less than clear-cut. As Adolf himself pointed out, the LFC – often regarded as a purely *ad hoc* response to the negative result of the Michelson-Morley experiment - is in fact independently testable by the Kennedy-Thorndike experiment. Moreover, although I rounded up one of the usual suspects in citing Ptolemaic epicycles to suggest the idea of an *ad hoc* hypothesis (indeed 'adding an epicycle' is sometimes used as effectively synonymous with indulging in adhockery), and although major epicycles were undoubtedly introduced *ad hoc* by Ptolemaic astronomers explicitly to account for planetary stations and retrogressions (amongst other things), their introduction was in fact independently testable. The epicycle construction entails, when conjoined with the independently plausible assumption that apparent brightness of the planets is correlated with their closeness to us, the correct observational result that any planet will be at its brightest when in the middle of its retrogressive phase. It is best, then, to look for clear-cut (or more nearly clear-cut) cases in the realms of pseudoscience.

Immanuel Velikovsky developed a theory about a giant chunk of material that somehow broke away from Jupiter and took up a promising career as a comet that made two separate series of orbits around the earth before settling down to a quieter life as the planet Venus. The "close encounters" between the comet and the earth were, according to the theory, responsible for such remarkable (alleged) phenomena as the falling of the walls of Jericho and the parting of the Red Sea. Velikovsky accepted that such cataclysms could hardly have been restricted to selected parts of the Middle East and looked for records of similar natural pyrotechnics on the same scale in other contemporary record-keeping cultures. The search revealed some loose corroborations but also some much sharper and embarrassing gaps. Velikovsky was, however, far from stumped: he postulated that, for the scribes in *some* cultures, the events associated with the close encounters with the comet had proved so traumatic that "collective amnesia" had set in. Which precise cultures had suffered from this regrettable complaint? Why, exactly those for which we have reasonably reliable and extensive records , which, however, fail to mention any cataclysms on the appropriate scale: collective amnesia afflicted precisely those cultures C_1..... C_n for which no suitable records of cataclysms exist.

This really does get close at least to a pure case. We have, I take it, good reason to believe that we have all the records that there are to be had from cultures that were keeping fairly extensive records at the time of, say, the exodus from Egypt. Hence *all* that the collective amnesia hypothesis does is reconcile Velikovsky's basic cometary hypothesis with the known records, some of which had been at odds with that theory when combined with the initial natural auxiliary that record-keeping cultures would have recorded events on the scale of the parting of the Red Sea or the fall of the walls of Jericho (worth a line in anyone's diary one might have thought!) There is no way of further testing Velikovsky's modified theory – at least not with this sort of historical data. We have an initial theoretical system consisting (at least) of the basic Velikovsky cometary hypothesis V and the 'natural' auxiliary N about record-keeping cultures, the conjunction V& N is inconsistent with some data e, e is then used to construct a modified version N' of the auxiliary (the collective amnesia version), the conjunction V & N' then automatically yields e – it was bound to do so by the manner of its construction; but it yields absolutely no further empirically checkable prediction that was not already yielded by V & N.

3(b) "*Degrees of adhocness": Ptolemy vs Copernicus on planetary stations and retrogressions*

The usual story about Ptolemaic epicycles is, as we have already noted, incorrect. But this does not mean that the underlying intuition that planetary stations and retrogressions give stronger empirical support to Copernican theory than to Ptolemaic theory is also incorrect. As we saw, once the major epicycle has been introduced to explain the observed phenomenon of stations and retrogressions of, say, Mars, it proceeds to make the independently testable prediction (*modulo* plausible background assumptions) that Mars will seem brightest when in the middle of its retrogressive phases.

But compare this to the Copernican account. This of course postulates that we are on a moving observatory. This postulate, together with the observationally based thesis that Mars and the Earth have different orbital periods, entails that the Earth will periodically overtake Mars as they both move eastward round the sun. This directly entails that Mars, as it *in fact* follows an uninterruptedly eastward path will *appear* to stand still and briefly retrogress. And this Copernican account in turn directly entails, no less than the Ptolemaic account did, that Mars is at its nearest point to the Earth (and hence at its apparent brightest) when in the middle of its (now apparent) retrogression.

Speaking intuitively but I believe fundamentally correctly, Copernican theory supplies a direct reason for *both* the stations and retrogressions *and* the fact that planets seem brightest when retrogressing. On the other hand, the basic Ptolemaic geostatic theory gives no reason at all why there should be observable stations and retrogressions – major epicycles have to be introduced and tailored specifically to yield them. Once introduced, they have an independently testable consequence, one that turns out moreover to be empirically correct. But the overall empirical support remains greater for Copernican theory: it gains full support from both phenomena, the Ptolemaic theory from only one of the two. This is because Ptolemaic theory needs the first phenomenon to justify the introduction of a feature into the theory whose parameters are then fixed on the basis of that phenomenon. Copernicus is, if you like, 'less *ad hoc*' than Ptolemy in this respect; though fundamentally the judgment is one about empirical support and can be expressed without using the notion of *adhoc*ness at all.

3 c) ***'Ad hoc' is not a four-letter word: Neptune again (and 'Vulcan')***

Allan Franklin once gave a talk at LSE entitled '*Ad hoc* is not a four letter word'. His title was of course undeniably literally correct, but his message was less trivial though still true: namely, that so-called *ad hoc* manoeuvres are very frequent in science and are often methodologically entirely kosher. Although he took it that he was arguing against those – like myself – who had emphasised the negative aspect of some manoeuvres often called ‘*ad hoc*’, there is in fact no contest here at all.

Many have of course emphasised the need to demarcate pejorative from non-pejorative charges of *adhoc*ness, but nonetheless this whole issue has often seemed obscure (sometimes because it has *been* obscured!). In terms of ordinary usage *ad hoc* means something like ‘introduced for, or addressed to, some specific end’ (*Shorter Oxford English Dictionary*: ‘ for this or the particular purpose’). And clearly there can be no general condemnation of a scientist for introducing a theory for the specific purpose of dealing with some initially anomalous result – such a condemnation would make no sense in general and would in any case convict many of the most celebrated successes in the history of science.

Taking the paradigm example yet again, it is of course quite clear that the postulate of a trans-Uranian planet was specifically introduced to deal with the fact that the orbit of Uranus proved stubbornly anomalous: that is, that the predictions made on the basis of Newton’s theory and the then accepted auxiliaries were wide of the observational mark. Moreover, since obviously some specific anomalous phenomenon might *provide the occasion* for some theoretician to start in earnest to look for a new theory, whether central or auxiliary, without any observable features of that phenomenon being used in constructing the new theory, we should add that of course in this case the anomalous orbit of Uranus not only provided the impetus for Adams and Leverrier to don their thinking caps, the details of the orbit were crucial in constructing the particular replacement assumption that they came up with. To a first approximation, Adams and Leverrier assumed that Newtonian theory was correct and freed up some initially fixed parameters – ones specifying the number, masses and orbits of the planets in the solar system – and then finally worked out what value those now free parameters had to have in order to account for the observed details of Uranus’s orbit.

The postulate was *ad hoc* in the straightforward literal sense, it was *ad hoc* in the stronger sense that observed data (those concerning Uranus’s orbit) were actually used in the construction of the postulate (that is, the data played an indispensable heuristic role in the process that led to the postulate) and yet the successful prediction of the existence of Neptune, based on that postulate, is of course regarded as one of Newtonian theory's – and indeed theoretical science's – greatest successes.

Naturally it is important here that Neptune was actually observed. Popper's obsession with falsifiability meant that he overemphasized the importance of a theoretical framework's making extra predictions and underemphasized the importance of some of these predictions turning out to be observably or experimentally correct. This is vividly underlined by the Vulcan episode. As is again well known, prior to his attempt to explain the anomalous motion of Uranus, Leverrier had worked on the anomalous advance of Mercury's perihelion. In what proved an unsuccessful dry run for the successful Neptune project, in the attempt to account properly for Mercury's orbit, Leverrier postulated an intra-Mercurial planet, tentatively named Vulcan. This proposed switch was exactly on a par with the later successful Neptune case – both were in the ordinary sense *ad hoc* responses to anomalous observations; both involved working backwards from the assumption that Newton's theory had to be correct in order to deduce what further auxiliary assumptions needed to be made to give the right account of those observations; and both led to new theoretical systems that not only accounted for the anomalous data they were based on, but also made extra testable predictions. The difference between the two, and the reason why the Vulcan move is not counted as a great scientific success, is of course the simple fact that the predictions made in the Vulcan case turned out to be observationally incorrect – despite a good deal of assiduous inspection of the heavens close to the sun during eclipses no evidence of any moving object misidentified as a fixed star was found; put loosely, while Neptune was discovered observationally after having been predicted, there turned out to be no such thing as the planet Vulcan.

3(d) ***Steps toward an improved analysis***

The first couple of steps toward improving on Popper's analysis seem, given these examples, fairly straightforward.

Suppose that we have a 'mini'-change from T&A to T&A', what makes such a change one of the good guys?

Well, first of all nothing to do with overall content increase. As Adolf suggests at one point, Popper seems to have been seduced by the idea that what happens in these cases is that an extra auxiliary is *added* – as if Adams and Leverrier simply added the assumption that Neptune exists. In fact of course they "modified" (i.e. contradicted) a previous auxiliary. The pre-Adams and Leverrier theoretical system indeed has a very definite, falsifiable, bold implication about Neptune – that it doesn't exist and therefore that you won't see it anywhere in the sky at any time!

What *did* increase in this episode is *correct observational content.* The post Adams and Leverrier system makes at least one prediction which is different from anything predicted by the corresponding pre-system and different from the observational data – in this case the initially anomalous data on Uranus's orbit - that not only clashed with the original system but, in my view altogether more importantly, was used in the construction of the Adams and Leverrier modifying hypothesis. Notice that this qualification about the new observationally testable predictions being different from the data used in the construction of the new system is necessary to prevent Velikovksy's switch involving the collective amnesia hypothesis counting as a 'good guy'. Intuitively of course this switch is, to say the least, somewhat questionable from a scientific point of view – yet clearly Velikovsky's modified system has correct entailments not shared with its predecessor: namely that no records of suitable cataclysms will be found in cultures C_1..... C_n (the ones specified to have suffered from 'collective amnesia').

This seems, then, to be an important difference between the Velikovsky shift and the Leverrier one. However, reflection on our third example, that of Ptolemy and Copernicus, shows that our initial question was a little naïve. When, as is invariably the case, we are adjudicating between two different rivals, it is not a question of demarcating the 'good guys' – the good shifts from frameworks of the form T & A to T & A' – from the bad guys, but more generally telling the better guys from the less good. The introduction of a major epicycle within Ptolemaic theory to account for planetary stations and retrogressions is, as we saw, both independently testable and independently confirmed. Hence this shift - while *ad hoc* in the ordinary sense - did more than simply accommodate the initially anomalous phenomenon it was constructed to accommodate: it made an extra prediction, one that turns out to be empirically correct. Hence, on our original classification, this shift should count as one of the good guys – and indeed, despite the bad publicity epicycles have achieved, I think that this is correct so far as it goes. But it doesn't of course necessarily mean that, when it comes - centuries later - to comparing the general Ptolemaic framework to the rival Copernican one, stations and retrogressions supply no empirical reason for preferring the newer framework. On the contrary and despite the fact that many commentators have seen the issues here as essentially pragmatic, concerned with increased harmony and the like, it seems to me that the right judgement is that although Ptolemy gets some empirical support here, Copernicus gets more. Having used one phenomenon (stations and retrogressions) to fix the value of a parameter, Ptolemaic theory goes on to predict and get support from a further phenomenon (time of maximum brightness). Copernican theory on the other hand gets *both* phenomena right straight off the bat – there is no relevant free parameter to fix; the very fact that we are, according to Copernican theory, on a moving observatory yields both the stations and retrogressions and the fact that the planet will be at its brightest while retrogressing straight off. Ptolemy gets some empirical support, Copernicus gets more.

In sum, although Popper raised the question of when it was or was not "legitimate" to modify an auxiliary assumption while retaining the 'central' theory of some theoretical framework, this was always the wrong question to ask. The real question is simply about empirical support (anathema to Popper, of course): modifications of auxiliaries are to be assessed simply in terms of how much extra empirical support the overall theoretical system complete with modified auxiliary achieves compared to the initial unmodified system.

The fact that such modifications will standardly be made *ad hoc* – that is, will be addressed to some known experimental difficulty – is in itself neither here nor there. However, when the data that constitutes the difficulty is actually used in the construction of the modified auxiliary (standardly by fixing the value of some initially free parameter) that data itself cannot supply support for the overall theory (even though *given the basic theory* it may supply conclusive support for the particular modified auxiliary).

There are two types of case where phenomena *do* supply empirical support for the overall theory from which they follow (as opposed to supplying support for some particular specific or auxiliary assumption, on the supposition that the basic overall theory (the 'core' theory) is to be taken for granted). The first case – exemplified by stations and retrogressions within Copernican theory – is where some phenomenon follows 'naturally' from the basic theory without the need for any special assumption based on data. The second and more common case – exemplified by the impact of the discovery of Neptune on Newtonian theory – is where some particular version of an overall theory is developed on the basis of a particular set of data (the details of Uranus's orbit in this case) but that particular version of the theory turns out to entail independently checkable further data.

This all needs more detailed analysis that I can provide here, though I am confident that these basic judgments survive such an analysis.[11] Amongst the many issues that would be involved in this further analysis are two raised by Adolf himself. *First* whether we need to talk in terms of *known* independent testability rather than in terms of independent testability in the logical, platonic sense. And *secondly* whether the notion of independent testability (whether known or not) is capable of a precise logical definition devoid of any 'pragmatic' element.

As this indicates there is certainly still life in these issues – just as there is, as I argued earlier, in the issue about content-increase through so-called scientific revolutions. Life after Grünbaum – certainly; but it's life informed, and improved, by Grünbaum.

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[11] For an elaboration of the claims made in the last two paragraphs and for at least the beginnings of the more detailed analysis promised here see my (2002) – actually written for the IUHPS conference in Krakow August 1999.