

The Antikythera Mechanism: a Review of the Evidence,
and the Case for Reconstruction as a Planetarium.

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In January 1974 Derek Price wrote to a correspondent: "... I have just completed a book-length monographic study of the Antikythera mechanism ... as far as I am concerned [this] wraps the whole thing up".

Price seems to have had enough; and I can sympathize. I have studied this artefact for about as long as he did, and whatever pressure he experienced, I can match it. My employer forbade my research, so I have conducted it in my own time and at my own cost, in the face of professional and personal difficulties: intrigue; betrayal; bullying; injury; illness; loss for years of all my data (some still not recovered); the long illness and death of my collaborator; and more. Even so, *I am still here*.

Price was referring to his important paper *Gears from the Greeks*. His account of the artefact always troubled me; but it was in 1983, when working on a later fragmentary Greek geared instrument, that I became convinced that I must take up the challenge of the Antikythera Mechanism. Price's paper provided important comparison material, and re-reading it brought the problems with his treatment clearly into focus.

I pass over the detail. There is little point in attacking one's predecessors' work; it shows meanness of spirit and denies the fact that those who come after are certain to profit by all that has been done before. It is enough to say that, having a background of expertise in mechanism and in workshop technique, and in the history of these subjects, I saw the value of approaching the Antikythera Mechanism essentially as a mechanical problem. My interests have broadened, but my aim has remained to resolve, as far as possible, the evident mechanical problems of the instrument, and to develop a more plausible reconstruction.

The Group gained access to the artefact while my own project was still in progress, and I was forced to suspend further work and concentrate on publishing, in haste, what I could. I gave up waiting for results from the epigraphist who had studied the inscriptions for me, and, in spite of a generous offer of financial support, deferred a collaboration over image-enhancement and other manipulation of my radiographs. At the Conference

on Early Greek Technology here in Athens, last October, I discussed my first, provisional attempt at a complete reconstruction of the instrument, and displayed a working model illustrating it.

I am gratified that the Group have accepted and adopted many of my findings: the Moon-phase display on the front dial; most of the gearing scheme, including the train to, and the function displayed on, the upper back dial; the spiral design of the scales and associated slots of the back dial; and perhaps the evidence for the loss of epicyclic mechanism from below the front dial, together with my suggestion for its purpose. Some other features of that reconstruction were merely provisional; I expressed doubt about the function displayed on the lower back dial and drew attention to unresolved problems associated with the epicyclic gear in the train leading to it. It is satisfactory that these points are now all resolved, at a stroke, by a small modification.

I have a great body of observations to publish; but it is extremely difficult to do so without adequate illustration, and the loss of quality in publishing my radiographs, in their natural state, would make them unintelligible. Besides, it is best to describe detail within an overall scheme. Therefore I published features of my reconstruction in advance of the data on which they are based. The Research Group have taken a similar course, though perhaps for other reasons, and they and I are now in much the same position, of asking others to accept results on trust. No doubt the Group will publish more fully in due course, and so will I. The value of my observations does not depend on them being the most recent.

It is desirable to be able to subject research to independent verification. My primary research material comprises sketches, notes, photographs and radiographic plates; as secondary material there are computer files using simple, widely-available software, and (so far) a single experimental model. These materials, deposited in a suitable archive, will in due course be accessible for reappraisal. Reassessment of the Research Group's material appears likely to remain a more intractable problem, due to their primary reliance on the manipulation of digitized data by advanced computer software. One might say: the higher the technology, the greater the credibility gap! But for now, we may use the two independent sets of results – the Group's and my own – to corroborate one another.

Tooth Counts

We begin with the gear counts. I worked from digitized versions of plain radiographs, viewing the images on-screen under magnification, and applying simple computer tools to their geometrical analysis. Many wheels are severely mutilated, often having only rather short runs of teeth. In some cases the traces of the teeth are distinctly non-circular, and often their spacing – whether measured at the tips, the roots, or at intermediate points on the flanks – is alarmingly irregular, making interpolation very unreliable. I decided against a statistical treatment because in the easier cases there was nothing to be gained and in the harder ones the data-sets were too small and too poor; instead, I relied on the eye of experience to assess the geometry – tooth-spacing, roundness and centring – in determining what would constitute a workable wheel.

In general, though, my tooth-counts and the Group's agree closely. One case stands out, in which they record a "definite" count that differs markedly from mine: that of the small contrate wheel in the Moon-phase apparatus. Here I was hampered by Bromley's loss of a suite of radiographs. In publishing a "rough estimate" I remarked that the actual number is of little importance because we know that it and the lost wheel that it engaged simply formed a pair with equal numbers of teeth.

The Group record thirteen other tooth-counts as "definite". I too recorded thirteen counts as "certain": eleven, in full agreement, are common to the two sets. Otherwise our error-limits are mostly similar, and where our preferred counts are not identical they differ by only one. We might compare our detailed working, but in fact there is little to be gained; it is clear that the Group have, like myself, allowed context to dictate, within the acceptable range, just what tooth-count was probable.

Gearing Scheme

I established the arrangement of the wheels with the help of images prepared using my home-made linear-tomography apparatus. My gearing scheme remains unchanged, except for the addition of wheel *m3* to axis *m*, the separation of two small wheels (*e1* and *e2*, in the Group's nomenclature) on axis *e*, and the removal of the conjectural idle wheel from the epicyclic platform. My published observations support these modifications.

Although the teeth on the rim of the epicyclic platform were redundant in my scheme, I stated that my estimate of 223 teeth for the complete wheel suggested that, together with the gear ring pinned to it, this wheel could have formed a wheel-pair in a train embodying the Saros period-relation, connecting the upper and lower back dials. The Group's modification makes use of these two wheels in exactly that way.

This is done by adding a wheel to axis *m*, to a vacant square wheel-seat that the Group report finding in their CT scans. This feature is actually visible to the naked eye, as my sketch made on 23rd January 1990 shows; and it was clear that a wheel planted here might engage the teeth on the epicyclic platform. I was still undecided on the point when I built this part of my model, and I included the square wheel-seat as a talking-point, with the intention of exploring the possibility further.

Adding this wheel calls for some disengagement of the train elsewhere if the gearing is not to be locked. The separation between the small wheels e1 and e2, which achieves this, can be found in my radiographs, once one is alerted to it; but it is far from obvious in the unprocessed images.

In any case, the necessity for these changes, so as to yield the correct output function at the lower back dial, became *obvious* once the meaning of the dial markings was understood. With hindsight, this may be deduced from fragment A alone; but it is presumably made clearer and more certain by what is found in fragment F. Ever since my first visit, when Dr Kalligas told me of his discovery of fragment E, I have urged the desirability of looking for further unrecognized fragments. I am glad that my suggestion has been proved right, but I am sorry not to have had the benefit of it.

I published the observation that both the upper and the lower pairs of wheels on the epicyclic platform appear to engage one another, leaving no place for an idle wheel; but within my assumed constraints the epicyclic gear was unworkable without one, and so I ascribed the appearance to distortion of the fragment.

In identifying the slot-and-pin device I indicated its use for modelling the lunar anomaly, but stated that I saw it in a context in which its period was wrong for Hipparchean lunar theory. The Group's modification, which I have just described, removes that objection by making the platform rotate at

the rate of the Moon's apsidal line, and allowing the epicyclic wheels to rotate with the Moon's tropical period. I also noted that the device's amplitude seemed wrong, and – as the Group themselves acknowledge – that objection still stands.

So, by the end of last year I had published all the observations necessary for correcting the gearing scheme, needing perhaps only quiet reflection to let them to fall into place. Provisionally, though, I explained these details, correctly identified but not integrated into the mechanical scheme, as traces left after some alteration to the instrument. While I now withdraw that suggestion, I do not withdraw the argument that led me to it; the artefactual evidence supports the strange form of the wooden case as I have reconstructed it, and this strange form is best explained by supposing that the Antikythera Mechanism was altered in some way.

In passing, it will be noticed that I do not indicate places for the so-called *parápegma* plates. There are several places where they might be fixed but, as with the “door plates”, we have no evidence that the *parápegma* plates were actually jointed to the case at all. The jumbled state of the fragments may perhaps best be explained by the supposition that they were actually detached.

In any case, it is satisfactory that my gearing scheme has been corrected, yielding what we now know to be the right output period at the lower back dial and resolving the several puzzles that I have noted, *each in exactly the way that I envisaged*. The small scale of the modification may be judged by the fact that I made the necessary alterations to my model in a single afternoon.

Back Dials

My description of the spiral arrangement of the scales of the back dials, together with the slots alongside them, was based on careful observation of the artefact and on geometrical analysis of digitized radiographs of the sections of these dials preserved in fragments A and B. The Group confirm my findings. It is perhaps evidence from fragment F that leads them to invert my four-turn spiral for the lower back dial. I have put off the considerable work of that alteration until I know more about the Group's analysis of the geometry.

In any case, my suggestion that the spiral slots might have run together now seems irrelevant, seeing that their true purpose – guiding riders that show which turn of the scales should be read – is confirmed by the Group’s observation of the rider embedded in fragment B. I considered this function, and I acknowledge that others urged it on me; but I rejected it because of the obvious risk of damage if the user were to have continued to turn the knob after a rider had run to the end of its slot. In an instrument that seems otherwise to be remarkably well thought out, this seems an oddly awkward feature. However, although I previously missed seeing the rider in my unprocessed images, I can confirm that it is there. I support the Group’s reconstruction in broad outline, but suggest some small differences of detail.

In my tentative scheme, I divided the four-turn spiral of the lower back dial into 218 parts. The function actually displayed calls for division into 223 parts. Both are equally compatible with my data taken from fragment A, the difference being well under 0.2 mm in the width of each divisions, even at the greatest radius. I have erased the 218 divisions from my model and replaced them with 223.

Front Dial

We now see that enough of the original survives to support a confident reconstruction of the whole of the back part of the instrument, at least in its essentials. At the front, we face a different class of problem; here we have evidence that something is lost but not *enough* evidence to be certain just what it was. We cannot ignore this evidence, but must try to interpret it, if we wish to discuss the function of the instrument as a whole.

The Moon’s place on the front dial was modified according to a lunar theory, arguably that of Hipparchos. Solar theory should also have been modelled. Heuristically, solar theory comes first. Besides, although the solar anomaly is smaller than the lunar anomaly, its exclusion would be a significant defect in an instrument intended – at least in part – for eclipse prediction. We therefore consider how solar theory might have been included.

The lunar theory is modelled remarkably neatly and economically in its eccentric version, by an eccentric slot-and-pin device carried on a platform rotating at the rate of the Moon’s apsidal line. The simplest way of modelling the Hipparchean solar anomaly, with its fixed apsidal line, would

have been to plant a similar slot-and pin assembly, for the corresponding eccentric solar theory, on the front of the frame plate.

This device would have been easier to fit if the wheel, from which it would have been driven, were smaller. We must therefore consider this large wheel for a moment. Price called it the “main drive wheel”, but his term implies a spurious rationale for its size. There is no advantage in applying the driving torque to a large wheel, when – as here – it is immediately transmitted through the much smaller wheel fixed directly underneath it; and if the need were simply to provide a reduction gear to drive the instrument slowly, there are far easier ways in which it might have been arranged. So far, we have no sound mechanical reason for making this wheel so large, and the cutting, by hand, of a wheel of over 200 teeth is tedious. No reconstruction can be regarded as satisfactory, if it does not explain why the wheel is there.

My observation, that this wheel carried epicyclic mechanism, is *crucial*. The wheel certainly carried elaborate structure; some traces are visible to the naked eye and others are seen clearly by radiography. At its centre we see a boss, separate from the wheel and therefore fixed to the frame plate, with a squared upper end. This is most easily explained as the seat for a stationary central wheel giving motion to one or more epicyclic wheels.

I call this wheel the Mean Sun Wheel, because it turns at the rate of the Mean Sun: one revolution representing the passage of a year. The purpose of epicyclic mechanism mounted on it, underneath a dial marked with the signs of the Zodiac, is closely defined by its context. Firstly, it could have served to introduce the solar anomaly according to the epicyclic model; but that, requiring only a simple train of three wheels leading to a very small epicycle, still offers no explanation for the wheel’s large size and its extensive structure. Besides, we have seen that the *eccentric* form of the theory might have been modelled even more easily, and the way in which lunar theory was modelled shows that the designer must have been aware of the possibility.

Otherwise, the lost mechanism could have served to model the motion of one or both of the inferior planets, according to an epicyclic theory. The geometry of each planetary epicycle is fixed by astronomical observation, and it is the need to accommodate the epicycle for Venus that finally offers a

rationale for the wheel's presence. The radius of this epicycle is large. The disc modelling it has to have a radius about three-quarters of the distance at which it is set from the centre of the platform that carries it. In other words, the scale on which the mechanism can be made depends on the size of any central obstruction that the disc must clear. Besides, there is a high angular acceleration of the planet relative to the Sun during the retrograde episode, so high loads are developed in the linkage driving its pointer. We find that the Mean Sun Wheel is large enough to model the motion of Venus on a scale that is both mechanically satisfactory and commensurate with that of other parts of the instrument.

Note that my argument for the restoration of at least this one planetary motion is driven *explicitly* by the need to account for artefactual evidence which cannot be ignored, and which has not been explained in any other way. This distinguishes it from other conjectural restorations of planetary indications to the Antikythera Mechanism, which have been mere speculations, clearly at odds with the artefactual evidence.

I now appeal to the principle of consistency. In Hellenistic astronomy no one planet seems to have been regarded as more important than any of the others, and, having contrived the means of modelling the motion of one, the designer would surely have been interested in including as many as he could. I have shown that corresponding mechanism for Mercury and for the solar anomaly can be combined with that for Venus, and have pointed out that only such a combination can account for the extensive traces of structure on the Mean Sun Wheel.

The same argument for consistency leads me to suggest that the superior planets – Mars, Jupiter and Saturn – should also have been included. I have shown how this, too, may be achieved in a way that is entirely consistent with what remains, using only machine elements and ensembles based on precedents found in the original fragments. Each superior planet requires its own epicyclic platform, because it is the rotation of the platform that models the planet's specific Zodiacal motion. The three corresponding assemblies call for a moderate increase in the depth of the instrument; but since there is no physical join between fragment C (containing the dial fragment) and fragment A (containing the frame plate and most of the gearing), we do not know how deep the original wooden case was.

I made these conjectural additions with compound trains to demonstrate that there is no difficulty in realizing excellent period-relations. On the other hand the simple epicyclic theory commonly associated with Apollonios of Perga, which I adopted throughout as being historically unexceptionable, leads to short-term, fluctuating errors in the positions of the planets; but this is no more objectionable in a geocentric planetarium than is the uniform motion in concentric circular orbits of the planets in most orreries. The model can easily be altered to realize planetary theory with an eccentric deferent, or even – certainly for the superior planets – equant theory, if some rewriting of the accepted history of astronomy should make it seem appropriate.

Much of the detail of any reconstruction of the Antikythera Mechanism is necessarily conjectural; but I conclude these remarks by drawing attention to further features which support the essential correctness of my restoration of its front dial display.

Firstly there is my discovery, in fragment C, of the sliding bolt for fastening the front dial plate. This quite elaborate provision, avoiding the need to use any tools in taking the dial out and replacing it, suggests strongly that the *user* needed access to the space below it. In a reconstruction with no planetary motions there is no good reason for this provision, because there would be nothing under the dial needing attention. Planetary mechanism, on the other hand, has to be set to some chosen epoch; and if the gearing actually chosen yielded poor approximations to the astronomical period-relations, the user might have needed to adjust it quite frequently.

Secondly, in devising mechanism for the superior planets I carried motion to it in the simplest way possible, by adding a side arbor with four equal wheels. The lowest is driven by the Mean Sun Wheel; and each of the others, in turn, drives the first wheel of one of the three stages. I planted this arbor in an arbitrary position, I now interpret a previously-unexplained detail as evidence that just such an arbor was originally planted elsewhere.

On the frame plate are two small rectangular bars, roughly square in section, each with a cut-out notch underneath. They are poorly aligned with one another, but I think that originally they lay parallel, symmetrically placed on either side of a line radial to the Mean Sun Wheel, and that one became displaced before being fixed in its present place by corrosion. I envisage a rectangular bearing block fitted between them. A notch across its

lower face, corresponding to those in the bars, would allow one to drive a cotter through the aligned notches to restrain it endways. A pivot-hole in the block would have received the lower pivot of the arbor, which would have been shouldered to prevent the block from rising. On withdrawing the cotter one could have shifted the arbor to draw its lower wheel out of gear with the Mean Sun Wheel, or perhaps could even have withdrawn it completely without further disassembly. This would have eased assembly and disassembly, and would have facilitated setting the planetary mechanism.

Then there is fragment D. The Group and I agree that it contains a wheel of 63 teeth, but it has no place in the Group's partial reconstruction. If it was part of this instrument at all then it is most simply explained as part of a lost planetary train.

Others may wish to discuss whether a reconstruction of the instrument simply as an eclipse-predictor seems plausible in the context of what we know of the astronomy of its time. I argue solely on the basis of what I find in the artefact: there is far more evidence to account for than we can comfortably ignore; and the most straightforward explanation, *which seems to explain it all*, is the hypothesis that the instrument included planetary indications. If that is right, then the Antikythera Mechanism was a planetarium with the additional function of eclipse-prediction, a surprisingly close parallel for one of the planetaria described by Cicero.

Finally, though, it is healthy to adopt a broad vision. The detail of this one instrument is relatively unimportant. The great point is that it exhibits a high levels of competence on the part of both designer and maker, and that these can only be acquired by experience. However remarkable it may seem to us, the Antikythera Mechanism represents a class of work which cannot have been far outside the normal experience of these men. Let the historians work on that.