# ON THE IMPORTANCE OF CORRECT MECHANICS IN PARANORMAL RESEARCH

by R. H. Wood

Paranormal metal bending and table-levitation have one thing in common, namely that the increased possibility of instrumental control of the mechanics, statics and dynamics involved implies that these two subjects might be the first to lead to a 'break-through' in theory. For that reason Hasted and Robertson[1,2] and Colin Brookes-Smith[3] are to be congratulated on their choice. However, the mechanics of moving objects, and the implied forces, is a much more difficult subject than many investigators imagine, even when we are not concerned with the object breaking, but only with external forces. Still more difficulty is encountered when the object is expected to break under its internal stresses and strains. Whereas we might have to look at basic physics to 'explain' paranormal forces, and even if we regard it as some natural extension of physics, any investigator is involved in advanced mechanics in his *reporting* of experiments. If that is wrongly reported than speculative physics will not rescue it. Here I will demonstrate the kind of mistakes that can easily be made, with reference to the above papers, and afterwards discuss how to avoid such erros in future. Levitation is considered first since only applied forces are involved.

## 1. Table Lifting and its Instrumentation

Even with a simple cube, Figure 1, there are three independent forces $X_1$, $Y_1$, $Z_1$ and three couples (moments) $MX_1$, $MY_1$, $MZ_1$, which can be applied on any one of the six faces. This is equivalent to saying that there are three forces and three eccentricities of such forces on any one of three planar projections of a lifted

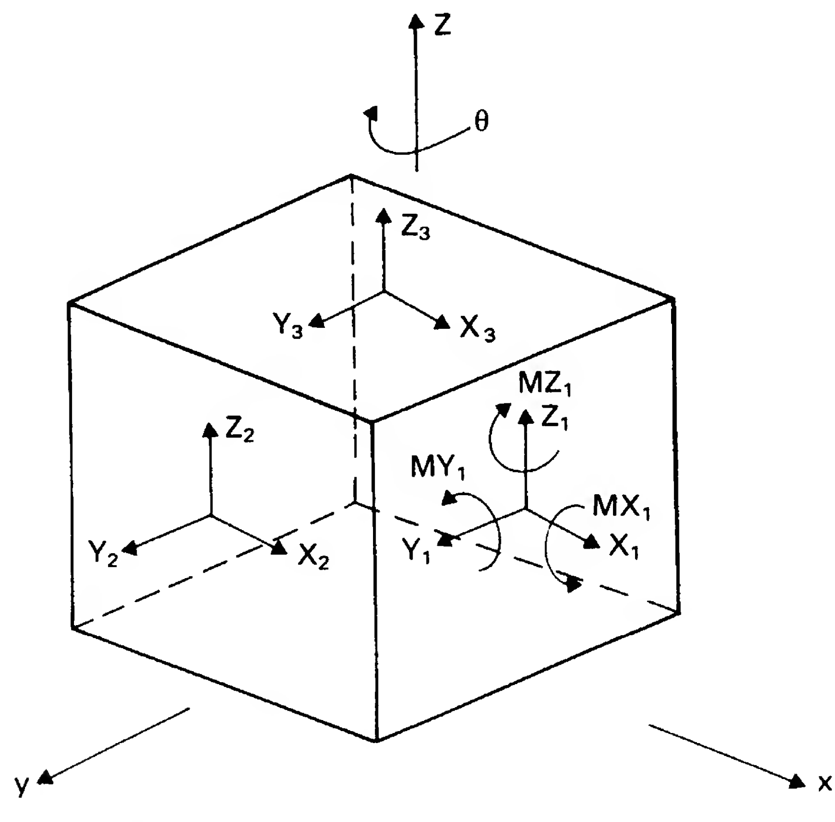


Figure 1. Forces and moments on a cube.

table. The instrumentation to cover all these forces would be enormous, unless movement is limited to one direction only—say the vertical z direction only, without rotation $\theta$ about z. This Brookes-Smith wisely did on his last 'Vertical Slide Dynamometer Table' but most of the discussion concerns 'hinged hand panels', Figure 2, for each sitter, where the slope of the panel decides the recorded force. Since we do not know *where* the upward force F is to be applied, suppose that is at distance b from the hinge. Let the spring force $F_s$ act at a distance a from the hinge, together with a downward reaction R at the hinge. Then for equilibrium of the hand panel (extra to its own weight as datum) we have:

Vertical forces: $R + F_s = F$
Moments about hinge: $F.b = F_s.a$

Whence $F_s - Fb/a$, which means that the correct force in the spring is not recorded unless, fortuitously, $b = a$. If any force were applied over the hinge ($b = 0$) then no force would be recorded at all. However, note that the actual upward force on the table is still given by

$$R + F_s = (F - F_s) + F_s = F$$

as expected. This paradox is caused because, with a hinged panel, Brookes-Smith was not measuring a force, rather a bending moment. It is never possible to ignore moment equilibrium alongside force equilibrium. Thus any book, board, or panel can be lifted about one edge manually, keeping it absolutely horizontal, the trick being to apply a bending moment as well as an upward force. This fallacy could be avoided by separating the complete table-top, Figure 3, from its support by three instrumented cantilevers. This will yield three independent items of information; the total force F and eccentricities $e_x$ and $e_y$, in short *where* the force F acts on the table top, i.e. one force and two moments. With more sophisticated mountings horizontal forces and their centres of application could also be recorded without difficulty. Only with great care can any statements about the magnitudes of paranormal forces be made.

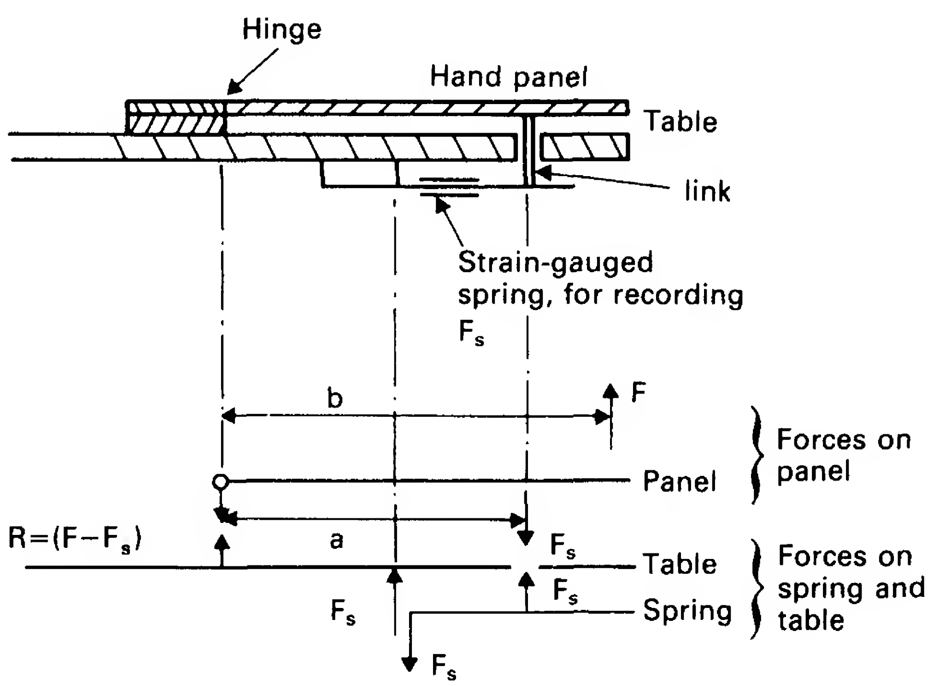


Figure 2. Hinged hand panels.

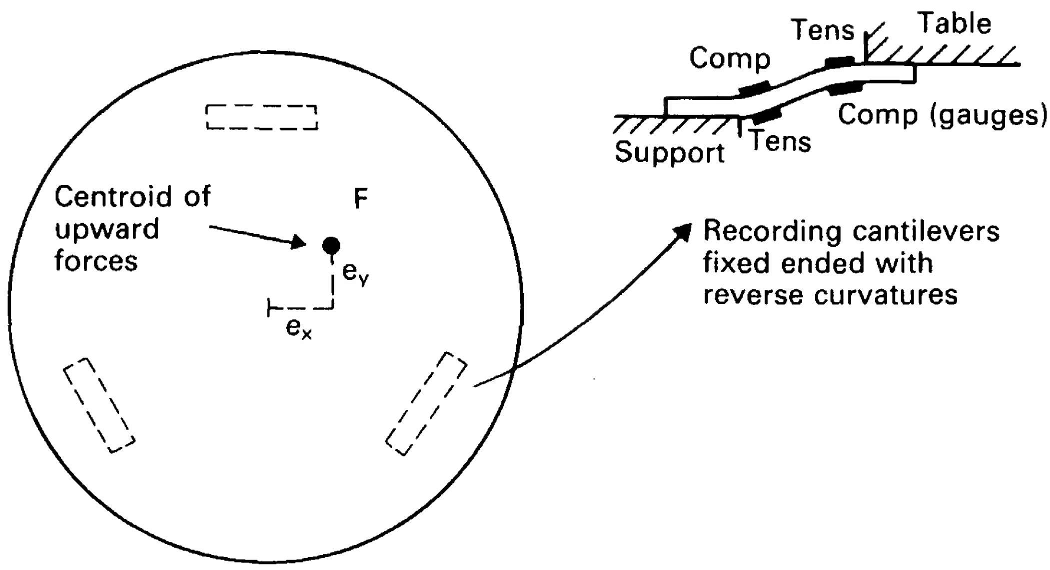


Figure 3. Three supporting cantilevers for a table top.

## 2. Stresses and Strains in Metal Bending

Resistance gauges are excellent, but only if the magnitude of strains, relative to the yield strain, are kept in mind. Unfortunately all Hasted's strains are published as millivolt signals, with nowhere a strain (extension divided by original length) mentioned at all. Those signals purporting to show 'metal churning' (Figure 2, Ref. 1) are about ±3 mV. On enquiry I was kindly given the calibration 1 mV = $1{\cdot}6 \times 10^{-7}$ strain, and since yield strains are commonly of the order $1{\cdot}5 \times 10^{-3}$, we are here recording about 1/3000 of the yield strain. Having spent a lifetime testing and analysing real metal structures, I would expect an elastic response, and would not believe any such erratic strain-diagrams unless some deliberate bending could first demonstrate linear strain bending diagrams at such small strains. (Quite recently a Professor of Civil Engineering told me that recorded strains of less than order $10^{-6}$ can not be trusted.) Moreover statements such as 'signals correspond to quasi-forces of about 20 gm weight' mean nothing to a stress-man, and simply mislead the reader.

Theoretical interpretation of tests leads to severe difficulties. The first example concerns the principle strains allegedly produced by Stephen North on circular discs where it is stated[1] 'for . . . a single radial stress vector we would expect corresponding signals [principal strains] to be approximately equal and of opposite sign'. Such a push-pull system is known only for pure shearing action, and the authors clearly did not mean that. There is a well known solution for forces P, acting on the diameter D of a disc of thickness t, giving principal stresses at the centre of $2P/\pi tD$ and $-6P/\pi tD$. If Hasted and Robertson meant that then they were in error by a factor of 3. In fact nothing at all can be said unless complete stress fields are clearly specified, implying that investigators of the paranormal should beware of plunging into the field of stress analysis.

One might think that twisting a spoon by one complete turn[2] would not be difficult to interpret. However the same authors argue that such large strains could not happen normally (and therefore must be paranormal), since they derive a formula (Ref. 2, p. 394) for *extensional* strain $\varepsilon_0$ which exceeds normal tensional limits. This formula is quite false. Any student of mechanics is taught that twisting is caused by *shear* strains, Figure 4, which were never mentioned;

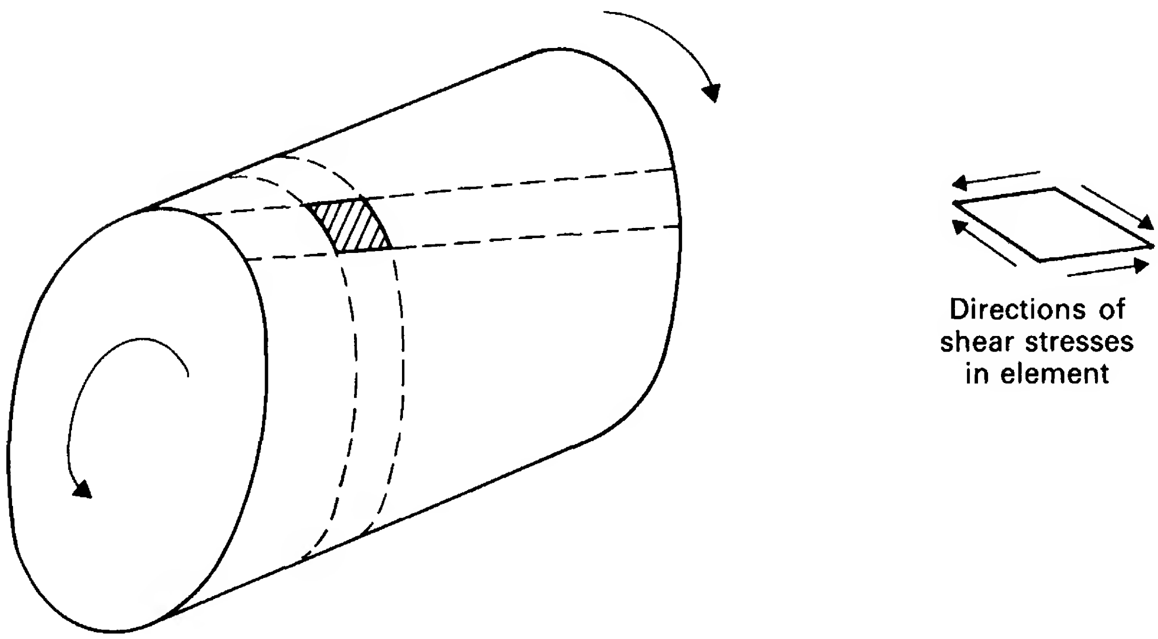


Figure 4. Shear strains in torsion (of a spoon or rods).

and that specimens in a testing machine may sometimes make several revolutions before fracture, perhaps with necking. Investigators of metal bending must know elementary stress analysis.

## Recommendations Arising Out of the Criticisms

It is only fair to add, sympathetically, that all investigators are here involved in multi-disciplinary sciences; mechanics is only one aspect, but it is central.

The main point here is that, to make progress, the S.P.R. needs to set up

(a) A scientific advisory panel, from physicists at one end to psychologists at the other, to help intending-investigators, not forgetting engineers in the middle.

(b) Above all, an advisory editorial board, to scrutinise intending-publications. All other professional societies are so equipped. Publications in the paranormal will fail in their objectives unless the standard is high throughout.

## REFERENCES

1. Hasted and Robertson. 'The detail of paranormal metal bending'. *J.S.P.R.*, March 1979.
2. Hasted and Robertson. 'Paranormal action on metal and its surroundings'. *J.S.P.R.*, June 1980.
3. Colin Brookes Smith. 'Data-tape recorded experimental PK phenomena'. *J.S.P.R.*, June 1973.

## Comment by John Hasted

*Section 1—Table-lifting*

These criticisms I believe to be substantially correct. The sophistication necessary for a full instrumentation of table-lifting is considerable; this is why many of us have avoided entering the field.

*Section 2—Metal-bending*

It is perhaps unfair to claim that Hasted's strain records are published without mention of units, since these can readily be deduced from the information given, viz.

Vol. 48, p. 365, caption to Figure 2:
'b Signal produced by 50 gm weight dropped from 2 cm height onto end of cantilevered latchkey.
'c. Signal produced by 175 gm weight dropped from 10 cm height onto end of cantilevered key.
'e, f. Transients produced by subjects without touching key.'

Vol. 50, p. 9 (Ref. 1), caption to Figure 2:
'Signals correspond to quasi-forces of about 20 gm weight.'
Caption to Figure 4:
'Calibrations show that the largest signals correspond to a quasi-force of about 100 gm weight.'

It is true that these are not strains, but stresses, given in terms of weights, for the reason that these might be more easily understood by JSPR readers. The strain calibrations have been published elsewhere (*The Metal-Benders,* Routledge & Kegan Paul, 1981, p. 51) as follows:

Part of Table 4.1:
Strain sensitivity $\Delta l/l = \varepsilon = 3{\cdot}33 \times 10^{-6}$/mV
Moment sensitivity of aluminium strip 12 mm wide, 0·75 mm thick

$$\sigma l = 2 \times 10^{-4} \text{ Nm/mV}$$

Professor Wood is correct in calculating that the Figure 2, Ref. 1 signals produced an unusually small strain, ~1/3000 of the yield strain; being 6 mm thick, the specimen was relatively tough, but linear strain bending diagrams could be experimentally demonstrated down to about 1/1000 of the yield strain.

The authors are pleased that professional stress analysis criticism has become available to them and have been aided by useful discussions with Professor Wood. Although we are satisfied that the great majority of our paranormal strain signals (although possibly not including those of Figure 2, Ref. 1) are not electrical in origin, there is nevertheless the obligation to develop equipment in which the noise level is mechanical (i.e. microphonic) rather than electrical, as is the cause for the resistive strain gauge. This we have now done by the use of piezoelectric ceramic sensors, made of lead zirconate titanate; these produce a much larger electrical signal than resistive strain gauges, and will readily respond to atmospheric shock waves; signals which we believe to be paranormal have been recorded on these detectors during 1981; these signals were not acoustic in origin, since they were not simultaneously recorded either on the human ear or on audio microphone. An analysis of these data will be presented in due course.

Whereas in civil engineering, strains of less than $10^{-6}$ may not be trustworthy, physical apparatus measuring strains as small as $10^{-12}$ has been used in the detection of possible gravitational waves.

Our statement in Ref. 1 that 'we would expect corresponding signals (principal strains) to be approximately equal and of opposite sign' was indeed misleading, since it referred to a pure shear, or 'pure bend', if there were such a thing. The authors actually meant just that, having been accustomed to think in terms of 'metal-bending'; but we erroneously claimed that this corresponded to a single radial stress vector.

We agree that the formula derived on page 394 of Ref. 2 is inapplicable in that it was derived in terms of extension rather than shear (although the extension and shear moduli bear a constant ratio to each other). Nevertheless, by applying torque to a batch of similar stainless steel spoons, it has been possible to show experimentally that they fractured substantially before they twisted as tightly as the paranormal examples. A treatment should now be given in terms of pure shear, since the shear modulus yield point for stainless steel must also be known.

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COMMENT by Colin Brookes-Smith

Dr. Wood refers to my June 1973 JSPR paper on data-tape recording during the table levitation experiments in the Daventry PK sittings of 1971–72 and it may clear the air a bit if I explain as briefly as possible what I was aiming at in those experiments. I wanted to know whether strain-gauge instrumentation using an amplitude-modulated audio-frequency carrier system was practicable where total darkness was essential (to avoid witness inhibition); where complex recording apparatus might be psychologically inhibitory (until the group members became habituated); and where all kinds of unexpected PK displays might occur spontaneously. After a few initial sittings without any recording apparatus, we were extremely fortunate in obtaining comparatively steady commanded (or 'suggested') table movements which after twenty or thirty sittings gradually developed into complete with-hand-contact all-four-table-legs-off-the-floor levitations to heights of three or four feet (i.e. not just hops or tilts). The recording apparatus (all home made) functioned adequately in regard to force effects but inadequately in regard to expected light-occultation and air-pressure effects. As an exploration of force effects seemed the most promising, I aimed at trying to discover the extent to which PK was admixed with UMA during different phases of a table levitation. I made four strain-gauged flat-spring 'dynamometers' built into hinged panels set round the edge of the octagonal table. These panels had handles which the sitters could easily grasp or (later) small and intentionally awkward wooden ribs which the sitters could only just grasp between their fingers to apply an upward UMA force. The four strain-gauges and their temperature compensating counterparts that formed the 'bridge circuit' were connected electrically in series so as to summate the forces contributed by the four sitters. Prior to the sittings, a cruciform test-rig with slings attached to the force handles on the table was fitted up in my home workshop with a large spring balance and an overhead windlass so that the table could be lifted gradually and a calibration made relating the lifting force in pounds and the amplitude of the carrier signal in volts. When all the apparatus was duly assembled at Daventry ready for the sitting, there was nothing left to do but to tape-record the audio-frequency carrier's voltage variations while the table was subjected to UMA and PK forces singly or in combination. Up to three carriers of slightly different frequencies could be recorded simultaneously on the tape so that other variables such as levitation height and forces acting between the table top and pedestal as well as speech and sounds would subsequently be available for study. The next day the tape record was transcribed using selective frequency amplifiers and a multi-pen strip-chart recorder so that the various forces, movements and sounds or commands by the spokesman could be studied in detail and at leisure.

These strip-chart records were not expected to approach high-accuracy (perhaps departures of 10% or 20% of true value) because at this stage of PK exploration one is only too thankful to get any quantified information. To summarise the chief findings, (1) there was a continually varying mixture of UMA and PK force depending presumably on the ever fluctuating balance between favourable and unfavourable psychological attitudes in the sitter group. (2) Unusual or awkward methods of applying UMA (e.g. finger-tip gripping wood ribs on the strain-gauged panels) seemed to invoke more PK than more usual methods (grasping handles). In other words, PK was more likely to be induced as a means of augmenting an inadequate normality. (3) The paranormal agency that produced the levitation was evidently applied to the table top not to the pedestal. (4) an artefact secretly applied (i.e. Batcheldor's 'deliberate deception technique') appeared to trigger PK but had little further effect once PK had been induced (see Figure 6, p.87). Considering the difficulties (a 25 mile journey each way with the apparatus) the experience gained was well worth the labour of making all the apparatus and carrying out the experiments. Despite inevitable innacuracies in the tape-to-chart transcriptions of PK signals and amplifier calibrations, quantitative information of PK activity was obtained that had rarely if ever been achieved before. The strain-gauged panels, as intended, recorded the fluctuating proportion of table weight supported by UMA and by inference, the amount of table weight supported by PK.

Dr. Wood has expressed concern 'that publications in the paranormal will fail in their objectives unless the standard is high'. Alas, I am sure we are still a long way from achieving high standards in any enquiry into the paranormal especially if instrumentation is employed. Many disciplines will increasingly be brought to bear on the problems and it may not matter much if there are short-comings that would be unacceptable in orthodox fields. But the real difficulties—the 'central' aspects—are psychological, surely not 'mechanics' as he avers. Parapsychologists are struggling with the problems but do not (and I think dare not) claim to be making much headway in understanding paranormality. If I may quote from my other writings, the path through the maze is not only strewn with thorns but with deep-dug booby-traps for the unwary investigator.

I do not know whether Dr. Wood has formed or attended a successful PK group but I am confident that if that is not the case, he can undoubtedly induce group-PK if he follows K. J. Batcheldor's recommendations. Then direct experience will highlight for him the psychological difficulties and subtleties of PK phenomena. There is no mystery—only massive ignorance and how welcome every new worker is in this important and largely neglected scientific field.

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