Systematic errors in "Big G"?

The numerical scatter in classical measurements of the gravitational constant may, after all, be explicable; the elastic properties of the wires from which torsion balances are suspended may be frequency-dependent.

THE methods of measurement of Newton's gravitational constant have hardly changed since Henry Cavendish's first successful attempt in 1789. Take a torsion balance, perhaps a metal bar shaped like a dumbbell, suspended through its centre of gravity by a fibre of some kind. Then place equal (and very large) masses symmetrically on either side of the axis of the fibre and with their centres of gravity in the same horizontal plane as the centre of gravity of the bar. Then let the dumbbell oscillate through a small angle, and measure the period or the frequency of the oscillation.

A little thought will show that there are two stable orientations of the torsion bar between the gravitating masses: one in which the bar lies along the line joining the two masses and the other, which is metastable, in which the bar is perpendicular to that line. So the measurement of the frequency of oscillation in each of these two positions is a measure of the gravitational force-couple acting on the bar in terms of the force in the fibre that resists twisting, the torsion force. Two measurements give two equations, from which the supposedly constant force in the fibre can be eliminated. The outcome is a measurement of the gravitational constant G as defined in Newton's equation for the force between two masses, GmM/r^2 (with obvious notation).

There is general admiration of Cavendish (after whom the Cambridge physics laboratory is named) for having won a result of any kind with the techniques available at the end of the Eighteenth Century. In practice, much of the credit for that goes to a country parson, Michell, who moved to London to help the great man, who designed the experimental arrangement and who died before Cavendish's measurement was made.

The passage of two centuries has notoriously failed to bring as much precision to the measurement of G as would be expected for such an important quantity. So much became apparent during the great hunt for the supposed 'fifth force' a decade ago. Typically, individual measurements have estimated errors of one part in a thousand. Disconcertingly, different measurements yield values that differ among themselves by several parts in a thousand, as if there were some systematic error in the measurements or their interpretation.

Now, it seems, a Japanese physicist at the University of Tokyo, Kazuaki Kuroda, has identified a recondite cause of systematic error in the estimation of G by means of the time-of-swing of a torsion balance (*Phys. Rev. Lett.* **75**, 2796–2798; 9 October 1995). In essence, his explanation is that the torsion force in the suspending fibre may not be a fixed quantity, but may be dependent on the frequency with which the torsion bar is oscillating. Plainly that would vitiate the assumption that the behaviour of the suspension of the oscillating bar is a constant in the two measurements of the frequency in the parallel and perpendicular configurations respectively. The matter is all the more disconcerting because the variability of the elastic constants is particularly marked at low frequencies (as must be the frequencies of oscillating torsion balances used for the laboratory measurement of G).

By Kuroda's account, the frequency dependence of elastic constants has come to light because of current interest in the design of oscillating masses for use in the detection of gravitational waves when that becomes a reality. Plainly those who hope to practice that trade need to understand the sources of noise in their detectors, which among other things include the dissipative processes that may go on in the material suspending the pendulum bob.

That internal damping in the suspension of a torsion pendulum should influence the dynamics of the material is not surprising. Standard suspensions include such materials as fused quartz and tungsten wires which, with the best will in the world, are made of crystallites of some kind or another and in which dislocations are bound to appear when the materials are stressed, and then at least partially disappear after an appropriate interval. The experimental data about this phenomenon so far gathered appear to relate to esoteric materials such as an alloy of copper and beryllium (see T. J. Quinn *et al.*, *Phys. Lett. A* **197**, 197–208; 1995), but Kuroda reasonably supposes they will be found in all materials used for suspending torsion balances.

To construct a model for the behaviour of a suspending wire, Kuroda simply supposes that the torsional elastic constant will be a complex quantity, the real part of which has its usual meaning while the complex part represents the internal damping. (The torsional constant is simply connected with what is called the shear modulus.) But the concept of damping depends crucially on the existence of a relaxation time, which in turn implies that the behaviour of a suspension wire will vary with the degree of mismatch between the oscillation period of the torsion pendulum and the relaxation time. To complicate matters further, Kuroda takes over from the experiments with Cu–Be the notion of a spectrum of relaxation times, ranging from less than 10 seconds to more than 5,000 seconds.

In plain language, what this means is that, for any material, there will be a difference between the elastic constants measured at high frequency and those measured in systems oscillating so slowly that distortions have time in which to relax into their original condition. By analogy with materials such as the Cu–Be alloy, Kuroda concludes that the fractional difference between the high-frequency and low-frequency values of the shear modulus will be of the order of 0.02, and will be positive in the sense that the high-frequency value will be greater than that under relaxed conditions. And that in turn implies that the values of G measured by the torsion balance will be greater than the true value of the constant.

There is some supporting evidence for this assertion. One of the classic torsion balance measurements of G is that by P. R. Hey and P. Chrzanowski in 1942. They supported their torsion bar by tungsten wires differently prepared. In one experiment, the tungsten wire was hard-drawn, in the other a wire of the same dimensions was annealed. The two measurements differed by more than one part in a thousand, or by two standard deviations at each measurement. That discrepancy by itself should have suggested, at the time, that the explanation lay in the way the wire had been prepared.

There are two conclusions to be drawn from this argument, one of which is that the torsion balance may not be the best way in which to measure G. It is particularly troubling that the effect of damping in the suspension of the torsion bar seems to have the unilateral effect of biasing the value of G upwards. Kuroda is appropriately modest in refraining from a call to throw all those carefully constructed instruments on the rubbish heap.

The opportunity to which Kuroda points is that of understanding why the measured values of G since the time of Cavendish have failed to converge on a consensus value. With a little luck, it may yet be possible to go back to previous measurements and remove the bias. The result may yet be a dissipationless suspension for use in precision measurements. **John Maddox**