Comment on Moffat's Disk

Andy Ruina
Theoretical and Applied Mechanics and Mechanical Engineering
Cornell University, Ithaca, NY 14850
ruina@cornell.edu

June, 2000, expanded March 31, 2002

[This is an expanded version of a letter sent to Nature and also sent directly to Moffatt in June 2000. It was not accepted. Moffatt's response to a different letter makes a passing reference to some of this letter in its last paragraph (Nature **408** 30 November 2000, pg 540).]

Moffatt (Nature **404**, April 2000, pp 833 - 834) discusses the ever-intriguing motions of a rolling disk, as it whirrs and shudders to a horizontal stop. Everyone has watched coins do this and people who like science toys have seen 'Euler's Disk', a commercial toy, do it much longer. Although a superficial reading might make one think the paper addresses the question of why the coin wobbles faster as it gets lower, this is not the topic. The issue the paper addresses is how the tip angle changes with time.

First let me review the contents and claims of the paper. Without the word limits of a Nature 'Brief Communication' I can explain at more leisure than could Moffatt.

Summary of Moffatt's paper

The paper starts with a rederivation, using angular momentum balance, of the classical relations for one set of solutions of the rigid-body dynamics equations for a uniform rigid disk that rolls without slip. This is the special family for which the center of mass is stationary (doesn't move).

$$\Omega^2 = \frac{4g}{a\alpha} \qquad \text{(for } \alpha \ll 1\text{)} \tag{1}$$

$$\begin{aligned} \text{where } g &= \text{the gravity constant,} \\ a &= \text{the radius of the thin disk,} \\ \alpha &= \text{the disk's angle of tip as it wobbles around (assumed} \\ &\quad\ \text{to be small with 0 corresponding to flat), and} \\ \Omega &= \text{the angular velocity of the ground contact point,} \\ &= \text{the precessional angular velocity.} \end{aligned}$$

Also, uncontroversially, the energy (kinetic +potential) of a disk in the motions as described by Eqn. (1) is

$$E = \frac{3}{2} M g \alpha. \tag{2}$$

The key observation is that this equation says that the wobbling rate (Ω) goes to infinity as the disk gets close to flat (α close to zero). This feature of this classical formula corresponds well to what is observed with coins and the Euler Disk toy where the shuddering gets fast as the disk falls.

Moffatt then assumes that at all times the motion is close to this steady precession, so that Eqn. (1) can be used at all times. As energy is lost at a rate

$$\Phi = \text{ the energy dissipated per unit time,}$$

α gets smaller and, by equation (1), Ω gets bigger.

Moffatt assumes that the only energy loss is from the flow of a viscous fluid squished around between nearly parallel flat surfaces (the bottom of the disk and the support). By an argument that is close to dimensional analysis Moffatt estimates the rate of energy dissipation in terms of the parameters above as well as

$$\mu = \text{ the air viscosity,}$$

keeping terms of order 1 (e.g., π) as he finds convenient. He gets

$$\Phi = C \cdot \pi \mu g a^2 / \alpha^2 = C \cdot \left(\frac{\pi \mu a^4}{16 g} \right) \cdot \Omega^4 \tag{2.5}$$

where C is a proportionality constant which Moffatt takes as 1 for quantitative purposes. A property of Moffatt's assumed viscous solution is that it applies a torque, but no net force, to the disk. Assuming that all energies and dissipations have been accounted for, the loss of energy per unit time is the dissipation, so

$$\frac{dE}{dt} = -\Phi. \tag{3}$$

Because all quantities in Eqn. (3) can be written in terms of, say, α, Eqn. (3) is then a first order differential equation in α. Moffatt observes that the solution of this equation predicts that in some finite amount of time

a) α gets to zero, and
b) $d\alpha/dt$ goes to infinity as α goes to zero. Thus, justifying the title 'Euler's disk and its finite-time singularity', he notes that his model predicts that there is a singularity that occurs in finite time.

The paradox of such a singularity is resolved, he says, by looking at linear momentum balance in the vertical direction. As α goes to zero, in his calculated solution, the disk's downwards acceleration goes to infinity. Linear momentum balance thus predicts that a tension force is needed from the ground (gravity is constant and the fluid applies no net force) at a critical time. At that time, says Moffatt, the calculation then ceases to be valid and the singularity is avoided.

He notes confidently in the final technical sentence of the paper that "the adiabatic approximation is still well satisfied" at this time.

Using a listed value for the viscosity of air, and assuming his proportionality constant $C = 1$ (keeping the π) he predicts a total shuddering time of 100 seconds, close to what is observed with the toy.

Critique

I am critical of the paper in a number of ways at a number of levels.

But, to be clear, let me first say what I agree with in the paper.

The special classical solutions Moffatt uses for a rolling disk, Eqns. (1,2), are probably good approximations for many rolling coins and the Euler Disk toy once the angle of tip is small, but not too small. The people who wrote the side of the toy's box thought so also, before Moffatt had ever seen the toy (In older versions of the toy, Eqn. 1 was printed on the box). In this regime, the energy balance Eqn. (3) can be usefully used.

Any theory, such as Moffatt's linear viscous dissipation, that uses Eqns. (1-3) and which has a non-zero dissipation Φ predicts a singularity in finite time. That is, the system only has finite energy, so with a non-zero dissipation rate that energy must go to zero in finite time. According to Eqns (1,2) this requires that Ω go to infinity in finite time. For one reason or another, all such theories must become invalid before this singularity and Moffatt was right to try to 'resolve' it.

Further, I could imagine a disk that was flat, smooth, and round enough rolling on a surface that was flat, smooth and hard enough, so that Moffatt's theory would be a good approximation for some range of small angles.

But here the agreement ends. Some of the criticism below may be regarded as unfair in that it criticizes claims which Moffatt did not make. But I believe that some of these claims are implied by Moffatt, how else could the existence of the paper be justified.

Applicability of Moffatt's theory

First, as was mentionned in the popular press by some critics and also by a letter to Nature (**408** pg 540) Moffatt's theory cannot apply to all disks that are observed to shudder rapidly as they come to a stop on a flat surface. The clearest example is a wedding band which has nothing close to the needed flat layer of air under it.

Moffatt's calculation is not good for coins either. Most coins are hardly flat compared to the assumed layer beneath them. And, should this be doubted, one can look at the predicted scaling (comparing a US penny with a US quarter, say) and see that the scaling from Moffatt's theory is not predicted for coins. Finally, experiments in a vacuated bell-jar (Nature **408** pg 540) show no significant increase in settling time as would be predicted if air friction were dominant.

Finally, I don't believe Moffatt's theory is good for the Euler disk toy either, as I infer a few ways. First, the toy comes with a parabolic bowl. Thus the gap between the disk and the toy does not have the needed shape nor scaling as α gets small. Second, most disks the size of that toy are not nearly as good as that toy. To assume that all the toy's dissipation is fluid loss is to assume that the toy manufacturer has done a perfect job of getting rid of all the other common losses. It seems more natural to assume that there is room for improvement in the toy design. Third, the toy, as purchased gives vastly different settling times depending on what surface the support surface is placed on. On a carpet the toy only shudders for a few seconds. One would have to assume that the surface Moffatt used in his experiments was the best of all possible surfaces. Fourth, experiments in a vacuum only show a modest increase in settling time, even for the toy. Fifth, experiments of McDonald using the toy show a dissipation rate that is not proportional to Ω^4 as Moffatt calculates, but something much less.

In play with various nominally flat round disks on various nominally flat round tables one finds that a given disk has settling times that vary by factors of two or so, while the disk parameters and air viscosity surely vary by far less than 1% between observations. Thus Moffatt's viscous mechanism can't be the primary damping for most disks most of the time.

How flat is flat?

To assume smooth steady rolling one needs flat contacting surfaces. How flat? Presumably flat enough so that the accelerations are less than g. Assuming n undulations per radian (about $6n$ undulations per circumference) with roughness d, the associated bound on accelerations are

$$g > n^2\Omega^2 d \Rightarrow d < \frac{g}{n^2\Omega^2}$$

Setting $g = 10\text{m/s}^2$, $\Omega = 500/\text{s}$ at the end (Moffatt's paper misprints this as 500 Hz), $n = 4$ (about one undulation per cm of circumference), we get $d < 2.5 * 10^{-6}$m. That is, to keep smooth contact to the predicted liftoff time would require surface undulations less than 2.5 μm over a cm and less than 25nm roughness per mm. This is highly stringent machining.

Electrical contact measurements for final shuddering of a nominally flat round steel disk on a nominally flat steel plate showed intermittent contact as the above roughness calculation shows should be likely (Simha, private communication 2000) thus the smooth rolling assumption is questionable for many disks near the end of their shuddering motion.

Altogether there is no evidence that I know of that indicates that Moffatt's Ω^4 fluid dissipation dominates for any disk on any surface, although it is concievable that such a disk could be made.

Moffatt's math is a little wrong.

Moffatt's algebra and calculus manipulations are good. But the math around the algebra is not all there.

Assume that some disk is sufficiently flat and round and does roll on a sufficiently stiff, flat surface that Moffatt's fluid dissipation mechanism is always dominant. How then is the singularity 'resolved'?

The singularity is resolved by the breakdown of the accuracy of Eqns. (1,2). Moffatt claims that the 'adiabatic approximation' is good all the way up to tensional contact. Moffatt (personal communication 5/22/00) was unwilling to bet on the correctness of this statement "I hold by the statement that I made about this in the paper. I don't like to bet on a certainty–it wouldn't be fair on you–".

Moffatt checks the validity of its approximate solution at the time of predicted contact loss by the consistency check of slow variation. But this is not sufficient. At the predicted termination time, no-longer-negligible torque from the air pressure has already changed the unperturbed dynamics (eq. 1 therein) by order 1.

That is, an integration of the full equations for a rolling disk with fluid drag (not restricted to Eqns. 1,2) would differ markedly from Eqns. 1,2 before tensional contact would be predicted. That this has to be the case is made clear by the observation that the fluid mechanism has no net force, and thus no means to pull the disk down with the accelerations that the restricted theory predicts.

It turns out from integration of more full equations, that the breakdown of the adiabatic approximation does occur before tensional contact, but that the time of this breakdown is very close to the time predicted by Moffatt. That is, Moffatt's mistake was real, but this does not substantially affect his predictions. I only harp on it because, once one discards the physical applicability of his theory all that is left is the model, and even that is not done correctly.

Competing Theories

Moffatt is correct to refute macroscopic frictional slip as a loss mechanism. But there are other possible contact losses.

One loss mechanism of which one can be sure is radiation through the support surface. This could be from collisions (non-flatness) or from deformation of the support structure. There is no need for plastic contact to invoke this mechanism. In effect, the disk could be rolling up hill all the time. Primary evidence in favor of this mechanism is that the Euler Disk toy has settling times that depend on the surface its base is placed on. Also, in our own experiments with metal disks on thick metal plates, the settling time depended on what surface the lower plate was supported by.

Another possible loss mechanism is from collisions, as noted in the roughness calculations above.

Standard rolling friction due to plastic deformation of the contacting surfaces and micro-slip there is another possible mechanism.

Assume, as one common class of rolling loss approximations, dissipation proportional to (contact speed)$^\beta$, and thus

$$\Phi = C\Omega^\beta.$$

Classic rate-independent rolling friction would give $\beta = 1$. A viscous contact term would give $\beta = 2$, and a quadratic collisional loss mechanism might give $\beta = 3$.

In a slow-varying small-angle theory like in the paper, all of these give a finite-time singularity; and the slow-varying singular approximation will be inaccurate near stoppage for many reasons (one possibility being neglected fluid effects).

Are Finite-time singularities any big deal? The title of Moffatt's paper leads one to think that there is a big surprise in having a mechanics problem where an approximate theory predicts a singularity (and thus the breakdown of the theory). Although always intriguing, finite-time singularities within approximate theories are not so rare in classical mechanics.

1) Almost as soon as calculus and mechanics were invented Newton worked out a finite-time singularity. Newton's Principia describes the finite-time singularity as two gravitationally attracted particles fall in to one another.

2) The "Painleve paradox" describes a smoothly sliding rod that eventually jambs with infinite acceleration.

3) The rolling of a french curve (with zero curvature at the tail) on a flat surface leads to infinite forces as a singular configuration is approached

4) A very well-known simple bouncing ball model predicts an infinite number of bounces in finite time.

Why the special family of solutions? A final open question concerns the classical" solution quoted for a rolling disk (Eqns 1,2). Really there is a three-parameter family of classical solutions, and even a two-parameter family of simply-expressed circular rolling motions. Without supporting comments, Moffatt selects for perturbation a one-parameter subset of rolling solutions. These solutions also are solutions for a disk freely sliding on a frictionless plane. Play with coins, toys, etc., indeed shows that these seem to be about the right subset for final shuddering during real rolling of many disks. The possible dynamical attraction to this special one-parameter family of solutions seems likely (to me) to follow from dissipative side-slip effects in the rolling contact, a significant contact mechanism neglected in the paper.

But without considering the possibility of slip there is no a-priori reason to expect that even a slowly damped perfect-rolling ideal disk should start at, or stay near, Moffatt's assumed one-parameter family of solutions.

Summary

Moffatt's theory adds next to nothing to the understanding of rolling coins or even of the beautiful Euler Disk toy. It does describe the behavior of some ideal, perhaps never seen, disks for some time, but even then he incorrectly gives the reason for the termination of his theory.