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A Pi Test

by Miles Mathis

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I got an interesting email from a reader yesterday. He had rerun my pi test with ball bearings, in answer to Steven Oostdijk's now famous test at youtube. His results failed to confirm pi=4, being much closer to 3.14. His results didn't land right on 3.14, as Steven's had on 4, but they were close enough to be troubling to me for about half a minute. Until I studied the set-up.

This reader Peter sent me a video, which you can view here. His set-up is similar to Steven's, being at about the same scale and using steel bearings. The diameter of the circle is 10” and the diameter of the balls is 3/4”. But instead of using plastic tubing to guide the balls, he used a flat metal track, 3/8” in width. So, exactly half the width of the balls. The track was square cut, not round, so the balls touched at two points, inside and out. Everything was professionally milled, so as you do your analysis, you may assume no error crept in there. He told me his experiment was “more precise” due to the steel cutting tools used, and in some ways that is true. As a matter of cutting precision, his experiment was indeed more advanced. But as a matter of a test of theory, it failed to perform for some reason.

The question is, why? I want to give you the opportunity to figure it out for yourself, before I tell you. I have given you enough information to figure it out already, but I will answer a few questions that may arise as you proceed. You may ask if the speed of the balls was such that the ball in the curve acted strangely in any way. Did it seem to lift off the inner grove? No. The initial ramp was actually somewhat lower than Steven's, and Peter may have done that on purpose, to make sure the ball in the curve stayed in it. Nonetheless, the speed through the circle was still quite fast, due to the fine milling I suppose. You may ask if the ball was in contact with the bottom of the track. No. I got no close-up, so I am not actually sure, but it looked to me like the balls touched the track only at two points, outside and inside. It appeared the balls were heavy enough to be held firmly in the track by their own weight, not wobbling side to side at all.

In fact, I will just give you a hint: the solution is relatively simple, and has nothing (or almost nothing) to do with details like that. Again, you can solve it just from the first basic facts I gave you. So think about it for a bit and meet me down below. But don't waste too much time on it, since if it doesn't come to you fairly quickly, it probably won't come at all.

The answer is that although the two tracks seem to be equal, they aren't. They are actually *very* different, which explains the roughly 21% error here. As one final hint, notice that compared to Steven's balls, the ball in the curve here is going faster. It gets around the track about 21% faster than Steven's circle ball (relative to the straight ball). Ask yourself why it would do that.

It does that because the curved track has far more freedom to move than the straight one in the same experiment. To say it another way, it constrains the ball far less. Or, it has less resistance. For me, the idea of constraint is the best way to understand it. Although the width at any one point is equal in the two tracks, they don't have the same areas. So the curved track *feels* wider and therefore freer than the straight track, in a way. If you don't see what I mean yet, draw the curve and go to a very small section of it. Now, compare that very small section to a very small section of the straight track. Take 1mm as your section. If both tracks are 1mm on the inside, the circle track will be far larger than 1mm on the outside, so that circle section has far more area. So that circle ball isn't being constrained like the straight ball, is it? It has a bit of forward freedom the straight ball doesn't have, doesn't it? It can move into that extra area, making it feel less friction from the outer runner.

You can also calculate this by calculating the total area of the curved track versus an equal length of straight track. The curved track has a much greater area.

You will say, "Well, don't compare them using the inside of the circle track. Use the center of the track for comparison". Doesn't matter. If you do that, the outside "half" of the circle track still has a far greater area than half the straight track. And the difference is not slight.

So using tracks like this was a mistake from the start. It would be *expected* to skew the results by large amounts, exactly in the direction of 3.14. It would allow the circle ball to move faster, dropping the results far below 4.

And it tells us why Steven's ball in the tube worked so well, avoiding this skewing of results. Because Steven's balls were not constrained laterally (being free to roll all the way around the tube if they desired), they did not exhibit this huge variance. Of course they were not unconstrained, but if you study the experiment closely, you see the difference is that they were constrained on *only one side at a time.* This is immediately clear with the circle ball, which is completely unconstrained from the inside. Due to the centrifugal effect, the ball will be pushed to the outside of the tube, where it will want to climb the wall. Gravity will prevent it from doing so, so we have a single point of lateral constraint. This means that, relative to the straight ball, the width of tubing is immaterial, and therefore the area of tubing. We don't have that area variable here. And the same applies to the straight ball, which is only constrained laterally on one side at a time. The straight ball is free to move to either side, but it can only move one way or the other, but not both. So at any one one time, it will be trying to climb one wall or the other. We would expect some wobbling side to side of that ball, and indeed that is the case. But as long as the wobbles are small (not enough to make the distance traveled greatly different), this only ensures the straight ball will be constrained on one side at a time, like the circle ball. This makes the constraints on the two balls far more nearly equal in the tubes than on the tracks.

You will say the tube still has a given width, so why doesn't everything I said about the track apply to it as well. I just told you: in the tube, the ball isn't constrained over a width. The ball isn't on a track, so the track has no width. The ball will climb the outside wall to some small extent, where the lateral forces offset the gravitational forces, so that, at any "instant" (dt), the forces will be focused at a point. Over any interval, those forces will stretch out into a line moving forward. Yes, that line will have some real width, but the width of that line will be very much smaller than the width of the track in the other experiment. As the effective width goes to zero, so does the area. So Steven's circle ball has far far less extra forward freedom than the ball on the circle track. And so it has more resistance or friction.

You may say, “I still don't see the difference. The circle ball on the track will also be feeling those centrifugal forces, and will therefore be pushed against the outside edge to the same extent.” Yes, it will feel those forces, but *not in the same way*, because that outside edge is predetermined by the track. The circle ball on the track *has to* feel those forces along that given outside edge. But the circle ball in the tube creates its own outside edge. The circle ball in the tube moves to the precise line of balance between the lateral forces and the gravitational forces. But the circle ball on the track can't do that. Therefore, there will be some width between the given edge and the desired edge, taking us right back to my initial analysis. Because there is a width, there is an area, and because there is an area, the ball in the circle doesn't match its cohort in the straight. This is how the track skews the problem, allowing the curved ball to move faster than the straight one.

You may say, “This extra forward freedom is just a myth. I don't see it. That 'extra area' in the curved track is immaterial, since the ball is only in one place at a time. It can't know what is ahead of it or around it, so you are just making stuff up. Its motion is determined by its original speed, gravity, and friction in the tube. Area has nothing to do with it.” I predict that will be the standard response. It just sounds like what we normally hear from the mainstream, doesn't it? But my answer is that the increased area in the curved track is a fact: how could it *not* affect this math? How could we even begin to assume that two balls traveling in such completely different tracks would be equal in that sense? And we do have to assume they are equal to find that pi=3.14 here. We have to completely ignore that variance in Peter's experiment in order to accept that pi=3.14. To say it another way: pi=3.14 *only if* the very different tracks cause *no* variance.

You would think the track would make the experiment “more precise”, but in fact it does the opposite.

What this new experiment tells me is that if this experiment has been done before, it may have been done like this on a track, skewing the results right back to 3.14, and adding to the historical confusion. The two balls need to be constrained in as nearly as possible the same way, and Steven's tubes do that far better than any track. But Peter's experiment is also very useful, since it tells us how this may have been misread previously. Just as Steven seems to have made a lucky hit on the right method, Peter seems to have made an equally lucky hit on the flawed historical method of looking at this.

I will be told that, given precisely the right ball speed and ratio of track width to ball width, we could recreate the tube drift with the track. Without further analysis, I would say that at a glance that appears to be true. In that case, the circle ball's desired line would match the given line, and there would be no width of influence. This could probably be tested with longer or shorter drops and larger or smaller balls, using the same track. I have suggested to Peter that his expensive track may be used for that.

This indicates the track could find a wide range of values depending on the set-up, and in fact the results here confirm that. Because his results were neither 4 nor pi [they were about 3.3], despite having very low tolerances, we have indication the results are dependent on the specific values. Peter thought it might be because his circle ball was “skidding”, but I saw no evidence of skidding the video. Given his mainstream assumptions, there was no reason for the result of 3.3. That's a 5% error, and his tolerances appeared to be way below 5%. Difference in friction over one short loop with chromed steel balls would not be expected to be 5%.

Another thing that confirms my analysis here is sub-positions of his circle ball. At ½ circle, the straight ball is at 1.6. With a steady ratio, the straight ball should be at 1.65 when the circle ball is at ½, but it isn't. The ratio should be steady, but instead it is changing noticeably over time. This didn't happen in Steven's experiment with the tubes, where we saw the circle ball hit the four quarters like clockwork. This by itself indicates this recent experiment isn't just showing us a relationship between a line and a curve, using pi. Pi is a constant, so the relationship should be constant. It appears to be telling us the circle ball on the track feels a lot more freedom to start with, but loses its edge over time, probably due to cumulative friction. As that ball slows, the centrifugal force falls and the ball presses less on the outside. It then feels more friction from the *inner* edge, giving it two points of strong friction like the straight ball. At that point the forces on the two balls starts to equalize, and the lead of the circle ball drops. We see almost none of that in the tubes, because, again, there is only one line of friction. As the circle ball in the tube slows, its line must shift down toward the middle a tiny bit, but even so it is still a single line. The line moves, but it stays a single line, since the ball is always seeking that line and is free to find it. Its companion ball in the straight is experiencing a similar thing, wobbling less and shifting toward the center, but maintaining a single line of influence. So the balls remain synched.

I will be told I am missing the obvious here: If the straight ball was at 1.6 at ½ circle, that is very close to ½ pi, indicating the initial relationship was nearly pi. As the two balls slowed, that relationship was lost a bit, that's all.

Yes, that would be the snap-judgment in favor of the mainstream, but I have shown you why it must be wrong. Due to the set-up, we would actually expect the reverse: we would expect that as the ball in the curve slowed, it would match pi more closely, not less. Why? Because in that case the two balls should be sitting in their tracks in more the same way. The straight ball isn't feeling any centrifugal effect, right? It is feeling two lines of friction from the two runners. Well, the faster the circle ball is going, the more it will hug the outer runner, due to the centrifugal effect. So the faster it is, the more *different* it should act; the slower it is, the more it should act like the straight ball. To get a match to pi, we would expect we would need to minimize the centrifugal effect, and therefore the speed of the circle ball. But the results here show precisely the opposite. As the circle ball slows and therefore settles more strongly into the middle of the groove, its ratio actually diverges from pi, moving to 3.3 and then 3.5. That is the opposite of expectation, indicating the initial match to pi in the first half was an accident. What is needed here to appear to prove pi is a much faster circle ball, and I have shown you how the flat wide fixed track provides that extra speed.

Here is my initial exchange with Peter, after informing that I thought area of track was a big factor. It may help clarify this.

`Peter: It seems to me that the ball must be constrained (a track, in this case) as the very idea of a circle is the constant and fixed radius, in this case a 5" radius making a 10" diameter circle. Otherwise, I don't see how the measurement would have a definite meaning (we're measuring circles, not near circles). My main concern is that the tubing allowed the ball to "wander" from a more perfect radius and that that was a limitation. You say: "It is gaining speed because it has more freedom to move." But the only acceleration given to the balls is what they are given by the matching ramps, which can be verified as the same speed by their reaching the starting point at the same time (same as was done in the tube experiment). The table is dead level in all directions. I do understand your point that the circle track is sweeping a larger area, but the ball stays centered in its 10" diameter path. It seems to me that whatever the characteristics are of the ball moving around the circumference of the circle are best explained by the fact that it is simply the nature of it being a circle and not a straight path, and` can't be otherwise.

Miles: "You say: The only acceleration given to the balls is what they are given by the matching ramps, which can be verified as the same speed by their reaching the starting point at the same time (same as was done in the tube experiment)." Not according to the mainstream. According the mainstream analysis, every body in circular motion is feeling a constant centripetal acceleration, remember? The ramp acceleration stops at the bottom of the ramp anyway, so it is of no concern to us here. I agree that it is equal, so we don't need to argue about it. You say the ball stays centered in its track, constrained by the width of the track, implying it is not free to move into any free space ahead. But if that were the case it couldn't move forward at all. It obviously has freedom forward, and I am showing you it has a DIFFERENT freedom forward than the straight ball. That has to affect its motion.

Also, no one can really believe Steven found exactly 4 because his circle ball was "wandering" a bit. That ball would have had to wander enough to create a 20% variance, and given the width of his tubing, that was impossible. Besides, we can watch the ball in the tube, and it is climbing the outside wall very little. Nor can the fact Steven's circle wasn't exactly circular explain it. If that had been an issue, we would have seen a big miss on the quarters, throwing everything off, but we didn't. The ball hit the quarters like clockwork, indicating that was a non-issue. I have read all the comments at Youtube and elsewhere, and have never seen anyone come close to explaining how Steven "just happened" to get exactly 4. They would have to explain it with wall climb or varying distances, as Peter just tried to; or they would have to explain it by a 20% variance in friction, which no one has gotten close to doing. How does curving a piece of plastic tubing *increase* its friction by 20%? I have just shown you the opposite is true, given a track: the curvature *decreases* the friction, by increasing the area. In fact, the ball in the tube is moving toward the outside specifically to inhabit that freer line, seeking a line of greatest area and *lowest* friction. We can confirm that from another fact: curving the tube compresses it on the inside and stretches it on the outside. The ball then flees to that outside area. So the ball in the tube is encountering a stretched-out tube, compared to the straight ball. That should also decrease the friction, not increase it.

But in Steven's experiment, those differences in friction are negligible, otherwise we wouldn't have found exactly 4. As I stated long ago, there is no chance that differences in friction just fortuitously provided the necessary 21% difference, gifting Steven with a perfect match at 4. The odds against that are enormously large.

Yes, I admitted above the ball in the curved tube encounters more friction than the ball on the curved track, but my point here is that the ball in the curved tube doesn't feel more friction *than the ball in the straight tube*. The ball in the circle tube doesn't seem slow in that comparison because of a friction differential of any kind, it seems slow because it is actually going further than we think. It isn't going pi(d), it is going 4(d). So relative to the pi expectation, it will look slow.

This means to me that although Steven's set-up may look a bit rough at first, his method isolates variables much better, allowing us to compare straight motion to curved motion without bringing width of track or area into it. His experiment actually equalizes friction much better than Peter's experiment, which is why Steven gets the right answer. Which is 4.

You may ask how I can remain so certain of that, even in the face of a negative experiment like that of Peter. Well, it isn't just due to the analysis above. Nor it is due to Steven's experiment. It is due to a large cache of data from the mainstream, compiled over centuries, since each of those facts in the cache also stands as an experiment in favor of pi=4. That includes the rocket anomalies, the use of the

Manhattan metric in quantum mechanics, and the equation failures in many fields all the way back to Newton. I have shown in dozens of papers that substituting 4 for pi gives us an automatic and easy correction to many mainstream equations, both at the quantum and macro levels. As just one important example, see the Stefan Boltzmann equation, which I was able to clarify, correct, and rewrite using pi=4 as just one correction of many. I even used it my fine tuning of my unified field equation and my quantum unification equation. I would not have been able to unify the electron and proton, explaining the number 1820, if I had been required to keep 3.14. So I don't just have these ball bearing experiments in hand, I have dozens of major equation corrections under my belt. I have experienced firsthand the way this correction acts like a magic bullet in many mathematical deadends, allowing me to climb over walls quickly and easily that the mainstream thought were unscalable.

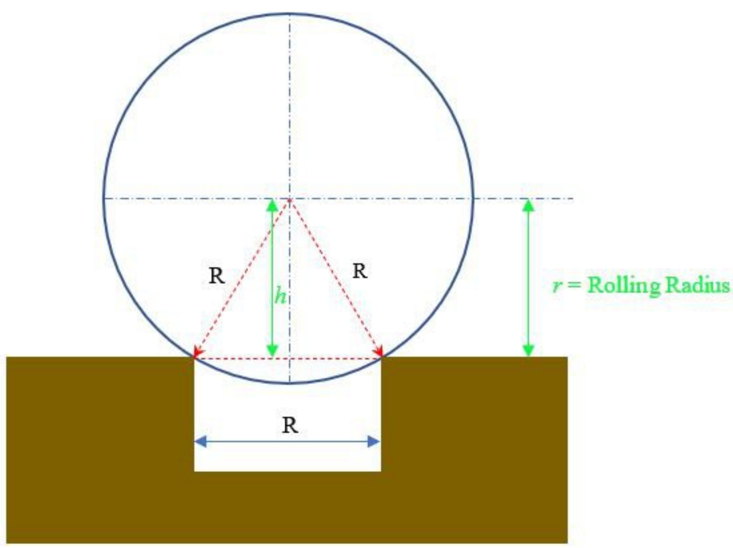
Thanks again to Peter, without whom none of this new analysis would have been possible.

Update February 14: Another reader who is professional engineer wrote in with some more analysis here, greatly extending my arguments above. He saw something I completely missed:

Comparison of Two "Pi = 4" Experiments

Peter's (slotted track) **vs Steven's** (plastic tube) **experiment**

When a spherical ball rolls on a flat[1] surface (or in a tube, like Steven's experiment), its **Rolling Radius** (RR) is the same as the radius of the ball, but on a slotted track like Peter's, the RR is less than the ball radius. In Peter's experiment, the RR of the ball on the slotted track (*r*) is the height of the ball centre above the top edge of the slotted track, which is always *less than* the ball radius (*R*).



With a 3/4" diameter ball in a 3/8" wide slot, its two contact points and the ball centre form an equilateral triangle in a vertical plane, with each side of the triangle being 3/8" long. So, ball radius is R = 0.375" and rolling radius is r is the vertical height (h) of that equilateral triangle, i.e.

1 I call Steven's track 'flat' to simplify discussion, as its main feature is to create a 'single-point' contact rather than a two-point contact.

$r = h = \sqrt{3})/2\ \pi\ R = 0.8660 \quad R = 0.8660 \quad 0.375 = 0.3248''$

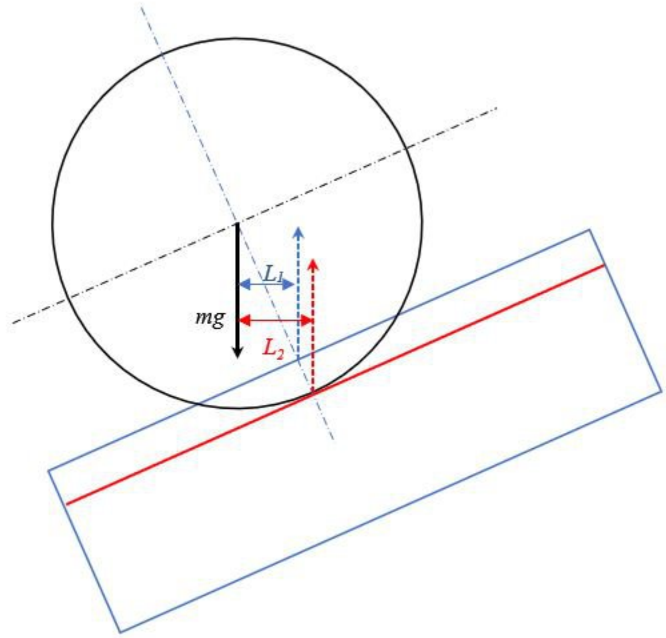
So, it is as if the balls do not have the same radius. They do not have the same *effective* radius.
This geometry has two major effects on Peter's experiment:

Two Effects of reduced Rolling Radius

The **first** effect of this small RR (***r* = 0.325"**) is on the speed at which the ball travels along the horizontal part of the track. Given the same ball size and ***rotational*** *speed* (rpm) on both tracks, the ball on the slotted track will travel **13.4% slower** than the same sized ball on a 'flat' track. So, if Peter and Steven were to set equal sized balls rolling at the same ***rotational*** *speed* on their different horizontal tracks, Peter's ball would only travel ***0.866 m*** along his track while Steven's travelled ***1 m***. To get a feeling for this effect of *reduced* RR, think of a large wheel (diam D) with a small diameter fixed axle (diam d) which rolls along such a slot *supported by the axle*. Even though the wheel may spin rapidly, the small diameter of the axle controls the *linear speed* of the wheel along the track slot. The **second** effect of reduced RR is on the acceleration of the ball on the sloping ramp, where the initial accelerating torque on the ball will be reduced because of the reduced RR (*r*), as shown in the following diagram, where the red lines refer to a *flat* sloping ramp (Steven) and the blue lines refer to the *slotted* sloping ramp (Peter).

Assuming the same ball size in each case, with ball weight (*mg*) acting vertically down, the horizontal *offset* of the weight vector from the respective contact points will generate an anticlockwise torque which will be $T_1 = mg \times L_1$ on the slotted (blue) ramp or $T_2 = mg \times L_2$ on the 'flat' (red) ramp.
Since $L_2 > L_1$, T_2 must be greater than T_1. So, all other dimensions being equal, this larger torque (T_2) will generate a larger angular acceleration on Steven's ball than the smaller torque (T_1) does on Peter's, and Peter's ball will rotate more slowly on the slotted ramp than Steven's on the unslotted 'flat' ramp with the same slope.

In addition to this slower rotation, the reduced RR in Peter's case also means that his ball will develop a still lower *translation velocity* along the track than Steven's ball will, due to compounding with the **first** effect (see above).



In essence, this means that, *ceteris paribus*, Peter's ball will *always* be *much slower* than Steven's ball in the straight line run, firstly because its *rotation* will be slower and secondly because the **first** effect reduces translation speed at any given speed of rotation.

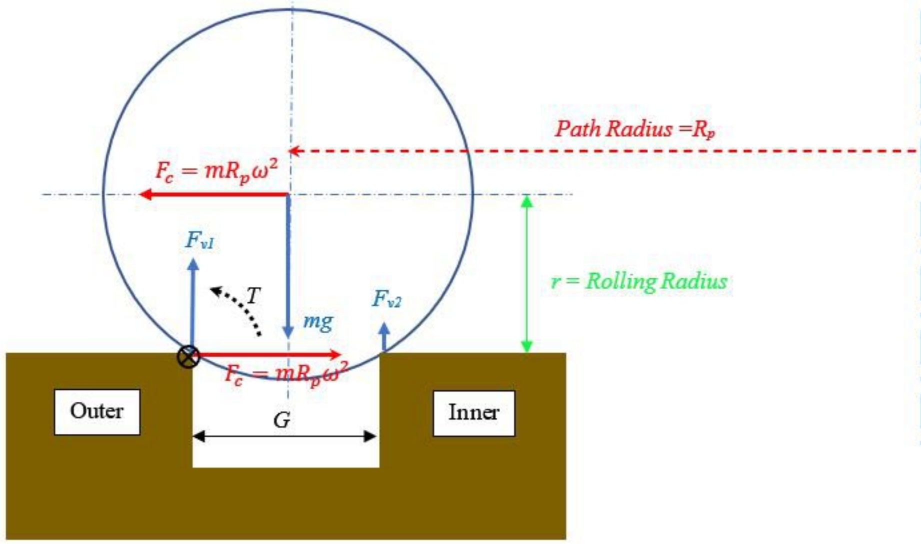
Travelling in the Circular Path Slotted Track (see following diagram)

In order to roll around a circular path (path radius = R_p) in a slotted track, the ball *must always skid* on at least one (or possibly both) of its contact points.

Call its orbital speed *angular velocity* (ω). Since orbital travel creates balanced outward (centrifugal) and inward (centripetal) forces ($Fc = mR_{p\omega}\omega^2$), the centripetal force must act horizontally inwards on the outer ball contact point (and cannot act at the inner contact point). This pair of horizontal centrifugal and centripetal forces (Fc) generates an anticlockwise torque on the ball ($T = F_c \times r$), which tries to flip the ball out of the slot by tending to rotate it about the outer contact point , as shown by the curved black arrow, below.

This torque necessarily reduces the vertical reaction force on the inner contact point (and increases the vertical reaction force on the outer contact point).

The following equations must therefore apply, simultaneously:
(for vertical stability), and,
(for rotational stability), but I'm not going to solve these equations here.



Obviously, the ball will flip out of the slot if *T* increases past a certain limit.

Discussion of Peter's Results

It is clear that, since $F_{v1} > F_{v2}$, it is certain that the ball in Peter's experiment will be more likely to *skid* on the less heavily loaded *inner contact point*. The ball would then tend to effectively 'roll' on a *single contact point*, creating a circular track path of radius equal to (R_p + G/2), while the orbital path of the ball centre point has radius of R_p. I therefore expect ball rotation to occur about the vertical axis of the ball itself and to be resisted somewhat by sliding friction against the inner contact point.

In this condition the behaviour of the ball in the circular slotted track will be somewhat akin to the behaviour of the ball in Steven's case, where it definitely has *single point contact* on the plastic tube. However, it is not worth the trouble of performing detailed calculations here because, given most speeds, we can assume the ball is rolling mainly on the outside point, in which case its rolling radius returns to R. **Which means the effective radius of the ball in the curve does not match the effective radius of the ball in the straight, in the same experiment.** That factor by itself gives the ball in the curve a 13.4% greater velocity than the ball in the straight. Since the extra width G/2 increases the radius of the circle by only 1.875%, the increase in velocity far outweighs the increase in distance, and the ball in the curve travels faster than the ball in the straight, just as Miles said. Because the ball in the curve gets around the circle faster, the ball in the straight has not gone as far, which is why its distance is far below the expected 4. So Miles is correct that Peter's experiment actually confirms pi=4 rather than contradicts it. Peter's actual result is about 3.3-3.4, so—using rough numbers—if we add the 11.5% difference we found here, we get about 3.8. That is much closer to 4 than to pi.

So am I saying Miles' analysis was wrong? No. I have confirmed that he was right in the main, and his "constraint" argument may turn out to be roughly correct as well, though incomplete. As I have shown, it must include the rolling radius factor, which is significant. In my mind, his constraint argument is just a variant explanation of the centrifugal force, which I used above, but for the purposes of this paper, it hardly matters.

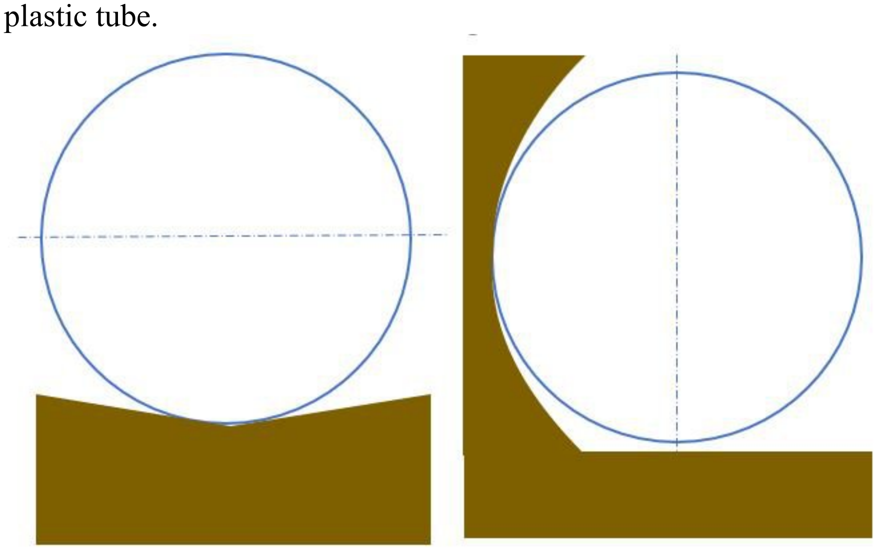
It would be easier to calculate the motions of the ball if the apparatus were redesigned, as suggested below.

Suggested Design Improvements

If Peter wants to improve his slotted rig, I would suggest he make the groove of the horizontal straight track a very shallow Vee-shape (see below, left), and use a circular (banked) profile for the circular track (see below, right) with a complex transition curve at all points between the horizontal straight and vertical circular track sections.
This track profile is designed to

- (i) give full control of the ball path,
- (ii) keep the ball RR as close as possible to the actual ball radius and
- (iii) create precisely known contact and force conditions in the circular path.

It will be interesting to see whether this modification confirms or improves on Steven's result (i.e.) with the simple plastic tube.



Miles: the second part of that last illustration would apply only to extremely high speeds. The actual banking required in this experiment would be far less than that, of course. The problem would be calculating exactly what banking IS required, since if the banking is wrong, all the work will be out the window. You can match the speed to the banking after the fact, within limits, but again you would need special cameras to determine that, making this solution very expensive and not likely to be done. Which is why Steven's experiment now looks so elegant, after the fact. His tubes solve this problem at near-zero budget. I am pretty sure he didn't run all these equations beforehand, making his choice based on them. As far as I know, he made his choice based on intuition. Or maybe my Muses were looking over his shoulder. So I take this opportunity to thank both Steven and my Muses once more.