Can thought experiments be resolved by experiment? The case of Aristotle's Wheel

Richard T. W. Arthur

McMaster University, Hamilton, Ontario, Canada

Abstract

In this paper I discuss "Aristotle's Wheel", a paradoxical scenario outlined in Problem 24 of the pseudo-Aristotelian treatise *Mechanical Problems*. Two concentric circles fixed together (say, a wheel and its hub) roll out equal lines on the surfaces with which they each make contact. But after several rotations, the two must travel different distances! I contend that this is a thought experiment—indeed one of the very earliest—and consider whether it can be resolved by experiment. Historically, two very different responses have been given to this question. Resolving this intriguing conundrum tells us much about the difficulty of identifying a given thought experiment in the first place.

I. *Introduction*

The question I am addressing in this paper is whether thought experiments can be resolved by experiment. But one might ask first, are thought experiments performable at all? The answer, of course, depends on how you define a thought experiment. According to one prevalent conception, "the key point of difference between ordinary experiments and thought experiments is that ordinary experiments are executed and thereby provide fresh information".[1] On this conception, the fact that a thought experiment cannot be performed is part of what makes it a thought experiment. If the experiment could be executed, then it would just be a design of an experiment, rather than a distinct category of thing.

But I can think of three counterexamples to this conclusion, two of them involving cases that are widely accepted as thought experiments, and the third an intriguing case that has not previously been discussed as an example of a thought experiment.

The first of these is John Bell's thought experiment, in which he constructs a case to demonstrate that local hidden variable approach to quantum theory is in conflict with the predictions of orthodox quantum theory. Eventually experiments were conducted, notably by Alain Aspect and co-workers, which confirmed what had never been in serious doubt.[2]

The second is Galileo Galilei's famous thought experiment concerning the dropping of two balls of different weights attached by a string, in refutation of Aristotle's theory that they would fall at different speeds. Here Koyré, and more recently Jim Brown, have claimed that the conclusion to be drawn follows with such rigour as not to require experimental testing, although of course the results do confirm Galileo's conclusion.

The third is the case I propose to treat here, the so-called *Rota Aristotelica* or "Aristotle's Wheel", which as far as I know has not been discussed in the literature of

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[1] This quotation is from Sorensen 1992, 241. He is merely summarizing common knowledge. The point of his discussion, however, is to draw parallels between thought experiments and ordinary ones.

[2] Of course, Aspect's actual experiments were not just trivial puttings into practice of Bell's thought experiment. Considerable ingenuity had to be put into constructing an apparatus that would test the abstract scenario Bell had described, and into devising further experimental arrangements to rule out extraneous explanations. Nevertheless, I contend, they still described what they were doing as experimentally testing Bell's inequalities, which Bell had derived by a thought experiment alone.

thought experiments. When I first selected this case I thought it was an interesting and straightforward example of a thought experiment that could in fact be performed. It had functioned, particularly in the hands of Galileo, just as a thought experiment should: it is an imagined scenario, with accompanying diagram, one that leads to paradox, so that it is apparently physically impossible; and it is designed to promote reflection on the principles involved. But it is easily performable, and the result is such as to undermine Galileo's use of it to argue for the composition of matter out of an actual infinity of atoms. What I discovered after further research, however, is that it is still an open question what conclusion should be drawn from the thought experiment. Thus in using this example to try to determine whether some thought experiments are resolvable by experiment, I was led inexorably into questions about what assumptions are constitutive of the experiment, and what idealizations are allowable. I still claim that this is an example of a thought experiment whose correct conclusion can be confirmed by actual experiment; but I conclude that in general the performability of a thought experiment depends on what assumptions are taken to constitute the experiment, and what idealizations are allowable.

In fact, I argue that similar considerations about the difficulty of identifying the constitutive assumptions and allowable idealizations of a thought experiment apply to the case of Galileo's thought experiment with the unequal weights, even though it is agreed that it *can* be performed. I take its conclusion to be that two unequal bodies of the same specific gravity but different weights falling in the same medium will reach the same terminal velocity, as is confirmed experimentally, and enshrined in Stokes' Law. Galileo's assumptions here are that the fall is taking place in a medium (less dense than the falling stones), and that in these circumstances a constant terminal velocity (the "natural velocity") will be reached by each of the bodies if they fall for long enough. His argument establishes that, despite their unequal masses, the weights will fall with the same natural velocity. Koyré and Brown interpret the thought experiment as concerning two bodies falling in a vacuum; Galileo's conclusion being that, despite their unequal masses, the weights will fall with the same acceleration, so that they will have the same final velocity after falling for the same time. On the first interpretation considerably more argument is required to establish what would happen in a vacuum, since the existence of a medium and a natural velocity (depending on the specific gravity of the material) are constitutive assumptions of the experiment. On Brown's and Koyré's interpretation, the existence of a vacuum is an idealizing assumption, a limiting case of a less and less resisting medium, so that the conclusion follows automatically. On the first interpretation, the case can be put to the experimental test by dropping two heavy balls of the same material in a very viscous fluid, and then dropping two balls of differing specific gravities in the same fluid. In the first case they will fall with the same speeds whether they are attached or not; in the second, they will fall with the same (intermediate) speed only if they are attached. On Brown and Koyré's interpretation, two bodies falling to Earth far above its atmosphere will fall together without separating, whether attached or not, thus providing empirical confirmation of Galileo's experiment; although, of course, pretty good confirmation can be obtained by dropping them from the Leaning Tower of Pisa, since the resistance of the air does not have a very significant drag effect on cannonballs falling through that height.

2. *The Rota Aristotelica*

Like many thought experiments, Aristotle's Wheel involves a paradox. The gist of this paradox is very easy to convey. Imagine two concentric wheels, one rigidly fixed inside another. More concretely, imagine a chariot wheel rotating in a rut in an ancient road in such a way that its hub is continuously in contact with the surface of the street, while the wheel itself is continuously in contact with the bottom of the rut. Suppose the hub has a radius r and the wheel has radius R. Then after n rotations of the wheel, the point A initially in contact with the road will have travelled a distance $2\pi Rn$ while the point C on the hub will have traced exactly $2\pi rn$. But $R >> r$. So $AB >> CD$!

What is the solution to this paradox? When I investigated by looking for contemporary resolutions on the Internet, I discovered two quite distinct, indeed incompatible, resolutions. According to the *Internet Encyclopedia of Science*,

> A one-to-one correspondence exists between points on the larger circle and those on the smaller circle. Therefore, the wheel should travel the same distance regardless of whether it is rolled from left to right on the top straight line or on the bottom one. This seems to imply that the two circumferences of the different sized circles are equal, which is impossible. How can this apparent contradiction be resolved? The key lies in the (false) assumption that a one-to-one correspondence of points means that two curves must have the same length. In fact, the cardinalities of points in a line segment of any length (or even an infinitely long line or an infinitely large n-dimensional Euclidean space!) are all the same.

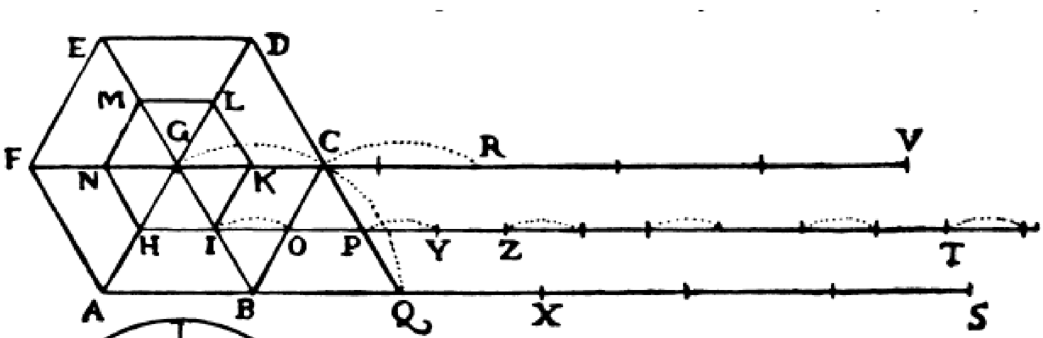
I confess that to me this seemed to miss the point of the paradox entirely. It is not being argued that $2\pi Rn = 2\pi rn$ because there is a 1-1 correspondence between the points on the rims of the two wheels. On the contrary, it is granted that $2\pi Rn >> 2\pi rn$, but this is paradoxical because the hub or nave of the wheel, being fixed to the wheel, must turn through precisely the same number of revolutions as it, so that AB must equal CD. This, it would seem, can only happen if either (case 1) with the outer rim the driver, the hub slips against the surface of the road (so that $CD > 2\pi rn$), or (case 2) with the hub the driver, the wheel slips against the bottom of the rut (so that $AB < 2\pi Rn$), or perhaps both. This is the standpoint taken by the second of the two resolutions I found on line:

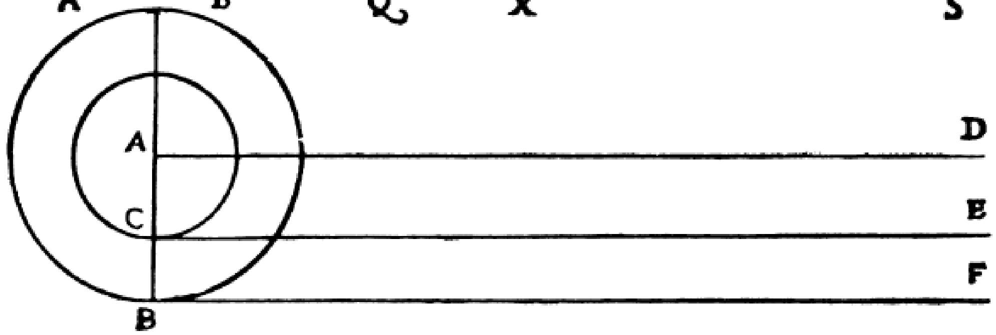
> As for the nave of the wheel, the case is otherwise. It is drawn in a right line by the same force as the wheel; but it only turns round because the wheel does so, and can only turn in the same time with it. Hence it follows, that its circular velocity is less than that of the wheel, in the ratio of the two circumferences; and

> therefore its circular motion is less than the rectilinear one. Since then it necessarily describes a right line equal to that of the wheel, it can only do it partly by sliding, and partly by revolving, the sliding part being more or less as the nave itself is smaller or larger. (*Archimedes Project: Rota Aristotelica)*

What is interesting about this is that these two responses to the paradox correspond quite well to the main lines of response taken historically. Galileo, for instance, offers a response that is similar to the "Cantorian" one offered by the *Internet Encyclopedia of Science*, in that it also appeals to considerations of 1-1 correspondence between an actual infinity of points on each of the hub and wheel, and to how this nevertheless allows for a difference in the lengths of paths travelled. On the other hand, his contemporary Mersenne, like many others, insists that the paradox is resolved by an appeal to slippage by whichever of the two circles is not driving the motion.

Let us begin with Galileo. In his discussion in *Two New Sciences*, Galileo began by considering two concentric hexagons. As the outer hexagon galumphs along the line ABQX…S one side at a time, one of its sides or one of its vertices is always in contact with the line. But as the outer wheel rotates through the first sixth of a revolution, the centre of the hub G travels through an arc to C, and with it the inner hub hexagon is also raised up in an arc. Thus whereas the outer hexagon pivots about B, on the line ABQX…S, the point I about which the inner one pivots traces an arc above the straight line HIOPYZT before its next side IK comes down to coincide with OP. There is therefore a gap IO in the contact of the perimeter of the inner polygon with the "road" HIOPYZ…T. This is, of course, repeated six times per revolution. Thus while the six sides of the outer polygon equal the line ABQX…S, the line traced by the inner polygon consists in six sides and five skipped arcs, so that it differs from it only by "the length of one chord of one of these arcs" (Galilei 1914, 22).





This talk of the smaller circle skipping parts of the line suggests that the same might be occurring when we extrapolate this analysis to infinity, regarding each circle as a polygon with infinitely many sides. Whereas the outer one's sides make continuous contact with the bottom of the rut, the inner one's contact is interrupted by an infinity of gaps, which add to the difference in the lengths of the paths. The vertices of the two circles are equinumerous, but those of the smaller circle do not make continuous contact with the road: it would slide. Galileo has Sagredo suggest something like this:

> SAGR. It seems to me that one may say that just as the center of the circle, by itself, carried along the line AD is constantly in contact with it, although it is only a single point, so the points on the circumference of the smaller circle, carried along by the motion of the larger circle, would slide over some small parts of the line CE. (Galilei 1914, 23)

But Galileo will have none of it. In the first place, his mouthpiece Salviati argues, there is no reason why one point rather than another should slide, so that, if there is any sliding, each of the infinite number of points of the smaller circle will slide over a finite segment of CE; but this would "make an infinitely long line, while as a matter of fact the line CE is finite" (24). Second, since the two circles are rigidly attached to one another, they change their point of contact equally often, so that no point of the smaller circle can be in contact with CE at more than one point. The solution, then, would appear to be to have the gaps in the smaller circle's passage over the road infinitely small, but still adding to a finite quantity.

In fact, this could be argued to hold generally. If the points are not parts, differences in lengths could simply be the result of different infinite aggregates of infinitely small gaps summing to different finite lengths. Thus it is that Galileo extrapolates this reasoning to solid bodies in general: "Now this which has been said concerning simple lines must be understood to hold also in the case of surfaces and solid bodies, it being assumed that they are made up of an infinite, not a finite, number of atoms." (25). Thus Aristotle's Wheel is used by Galileo as an argument for the composition of matter out of an infinity of point-atoms separated by infinitely small indivisible voids.

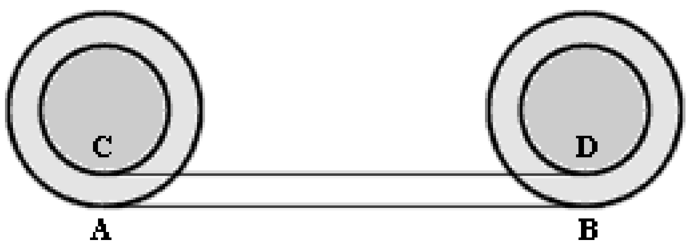
So in Galileo's case the paradox of Aristotle's Wheel functions as a thought experiment should. We have an apparently physically possible situation, described diagrammatically, leading to an anomalous result. Analysis reveals that the cause of the anomaly is a false conception of the continuous. If the continuum consists in indivisible points separated by indivisible gaps, then it is impossible for them to compose to a finite length so long as the indivisibles are finite. But if the gaps are infinitely small it is possible for an infinity of them to compose to a finite length, and also possible for there to be a difference in length of two lines containing the same, infinite number of points.

How was this solution received? Well, if we look to Galileo's chief advocate in France, Marin Mersenne, the translator and disseminator of his dialogues, the answer would have to be: not very well. To Galileo's solution Mersenne objects in his commentary on Galileo's *Two New Sciences* in 1639 that : "…if the smaller circle always jumps a point of its line without touching that point, it follows that the line is not continuous, and consequently that it is not a line."[3] Mersenne had actually treated the problem earlier in the preface to his translation of Galileo's *Mechanics* in 1634, where he proposed that the solution is that the smaller circle simply slides, as can be seen by actually performing the experiment: "And when the small circle is moved by the large

[3] "... si le moindre cercle saute tousiours un poinct de sa ligne sans le toucher, il s'ensuit qu'elle n'est pas continue, & partant qu'elle n'est pas ligne." Mersenne 1639, p. 31; quoted from Drabkin 1950, 175.

one, the same part of the small one touches a hundred parts of the large one, as experiment will make apparent to all those who perform it in sufficiently great volume."[4] Mersenne repeats this explanation in terms of sliding in his 1639 commentary (Drabkin 1950, 173). Fermat, apparently, sympathised with Mersenne's point of view, and also held that Galileo had misunderstood the problem.[5]

One thing that is apparent from this comparison of reactions to Aristotle's Wheel is that there is no consensus on precisely what the paradox is. Mersenne gives what I consider to be the correct solution. If there are *n* revolutions of the wheel with no slippage at the bottom of the rut, then a point on the rim of the hub will have travelled a distance of only $2\pi rn$, but the wheel will have travelled $2\pi Rn$ (which is greater than $2\pi rn$ in proportion to how much greater R is than r). This will only have been possible if the circumference of the hub has been carried slipping and sliding over the road to make up the difference. The proof of this is that if one actually does the experiment, one will observe this slipping and sliding.



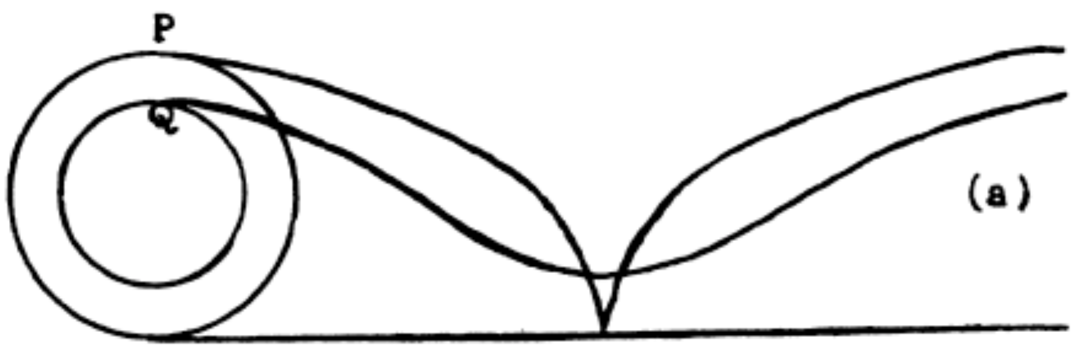
But on a second line of interpretation, that misses the point of the paradox, as well as the fruitful lines of inquiry that are engendered by interpreting it properly. Galileo represents this line of interpretation. So do Robert Boyle and others who discuss the *Rota Aristotelica* in the context of the composition of matter, and its rarefaction and condensation. According to its this interpretation, the two lines AB and CD are in fact

[4] "Et lors que le petit est meu par le grand, une mesme partie du petit, touche cent parties du grand, comme l'experience fera voir a tous ceux qui la feront en assez grand volume." Mersenne 1934, pp 18-19; quoted from Drabkin 1950, p. 174. Drabkin obseerves that this was not Mersenne's first attempt on the problem, and that in an earlier attempt he had mentioned how "some have sought an analogy between the action of the concentric circles and the processes of rarefaction and condensation", precisely as Galileo was later to do (Drabkin 1950, 173).

[5] Drabkin quotes a letter of August 10, 1638, from Fermat to Mersenne in which Fermat says that he had sent him long before his thoughts on problem 24 of the "*Méchaniques* d'Aristote" (Drabkin 1950, p. 188). But that manuscript is no longer extant, so we don't know what Fermat's objections were.

necessarily equal, and the point is to explain how this could be. It is not legitimate to try to resolve a thought experiment, such as this seems to be, by an appeal to merely empirical factors like slipping and sliding. For them the problem is this: given that the two lines are equal, how can this happen without slipping or sliding?

Interestingly, this is the line taken by Israel Drabkin, in his very scholarly and thorough treatment of the *Wheel*: "... our problem is how it is possible for the paths to be equal without compensatory slipping in the sense indicated." (Drabkin 1950, 166). Accordingly, he takes a significant advance in the treatment of the paradox to be Roberval's proof that the lines traced by a point on the rim of each circle *are* equal. If the outer wheel is the "driver", then its path will be a *cycloid*; and the path of the point on the rim of the hub will be the curve known as the *curtate cycloid*. Writes Drabkin,



> [F]rom a kinematical viewpoint, we may say that any point on the circumference of the smaller circle has a motion which is the resultant of (1) a simple rotation of the smaller circle (such as would, by itself, cause the point to describe a circle), and (2) a translational motion equal in direction and magnitude to the translational motion of the larger circle. The resultant path of the point is the prolate[6] cycloid [as Roberval first proved]. It is the magnitude of this translational component that makes it possible for the smaller circle, while making only as many revolutions as the larger, to trace a path equal to that traced by the larger. (Drabkin, 1950, pp. 163-64)

[6] As a referee has pointed put to me, "the point on the smaller figure should describe a curtate cycloid and not a prolate one. So Drabkin got the wrong name (but the right figure nonetheless). If it was the hub keeping its course, then we would have the point on the wheel describing a prolate cycloid."

Once this is established, the problem can be well defined. The condition is no sliding, which Drabkin, like Galileo, interprets to mean that no point on the rim can never be in contact with a finite part of the road:

> But this, unfortunately, is where the problem *begins*, not where it ends. For though the smaller circle traverses a distance equal to that traversed by the larger, it does not keep pace with the larger by sliding over the tangent, if by 'sliding' we mean that a point on the circumference is at any time in contact with a finite segment of the tangent. For the rotary motion of the smaller circle is continuous, as is that of the larger, and consequently the point of tangency is continuously changing. How it is possible for the paths to be equal without compensatory sliding in the sense indicated is the nub of the problem. (164-5)

Thus according to Drabkin, the “paradox involved belongs with those having to do with continuity and infinite divisibility.” He acknowledges the “Cantorian” solution, without committing himself to it. Those trying to resolve the paradox “on the basis of the Cantorian analysis.” he writes, “ will view the path of the smaller circle (equal to the circumference of the larger) as containing an infinite aggregate of points which may be put in one-to-one correspondence with (and is, in this sense, similar to) the aggregate represented by the points on the circumference of the smaller circle” (166). Although the path of a point on the circumference of the smaller circle traces a distance equal to the circumference of the larger circle, and this is greater than the circumference of the smaller circle, the two circles are aggregates of the same (infinite) number of points. The paradox therefore arises from confusing the number of points in the two unequal circumferences with their measures. But, Drabkin concedes, not everyone is happy with this resolution.

Certainly this was the case historically, as his analysis reveals. He examines the solutions given by Boyle, Tacquet and Jean-Jacques Dortous de Mairan (1718) in the century after *Two New Sciences* appeared, all of which reject Galileo’s actual infinity of extensionless points, and opt for some kind of infinitesimal sliding. Mairan’s is representative. “If at every infinitesimal moment each infinitesimal element of the arc of the smaller circle (in Case I) is in contact with a longer infinitesimal element of the base line, there is, in that sense, a 'sliding.'" (Drabkin 1950, 194). Drabkin comments:

> MAIRAN'S definition, which is generally accepted, in its essence declares that there is sliding whenever the path traced by a rolling circle in one revolution is not equal to the circumference of the circle. But in my opinion this definition does not, by itself, constitute an answer to the paradox. (195)

Thus we have a genuine disagreement about what the paradox consists in. On the interpretation I favour, the thought experiment engenders a paradox which can be resolved by thinking the problem through, and whose resolution can be confirmed by doing the actual experiment. On Drabkin's and Galileo's, the problem is conceptual. That is, the paradox is part of a thought experiment, and it *cannot* be resolved by actual experiment without violating one of the conditions of the thought experiment, namely, no sliding. Drabkin is disappointed that Mersenne, having once (like Galileo and Boyle) seen the solution to lie in considerations of rarefaction and condensation, should have reverted to the explanation in terms of sliding.[7] And he expresses surprise that "Mersenne strangely ascribes this explanation ['sliding'] to Aristotle" (174).

Let us then turn, somewhat belatedly, to the original formulation of the paradox. The author of the *Mechanical Problems*, it is now generally accepted, is not Aristotle, even though it has been traditionally ascribed to him. Most scholars argue that it must nevertheless have been someone of the Peripatetic school, because of the use of the Aristotelian terminology and the framework of mover and moved to describe motion.

Against this consensus, Thomas Winter (2007, iii-ix) has recently offered arguments that the author of *Mechanical Problems* was in fact Archytas of Tarentum (428-347 BCE). Winter argues for an early date based on the weapons and devices discussed,[8] and suggests that Archytas is the only one of twelve ancient writers on mechanics mentioned

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[7] Similarly, Drabkin writes: "The latest commentator on the passage shows a similar superficiality. He writes : 'It is not easy to be sure whether he <the author of the *Mechanica*> has seen the true solution of this problem, viz: in one case <Case II> the circle revolves on H9 while the larger circle both rolls and slips in ZI.'" (1950, p. 167, n. 8.

[8] Winter notes that the author "seeks the principle behind gear trains, windlasses, levers, and the slings with which the Greeks threw spears. But our sought author does not know about catapults." He does not explain why Archytas would not have known about catapults, which were invented by Dionysius the Elder of Syracuse in 399.

by Vitruvius that fits the bill. I neither want nor need, for present purposes, to become embroiled in scholarly disputes about authorship. But the content of the *Mechanical Problems* is consistent with what we know of Archytas' approach to mathematics: Diogenes Laertius reports that Archytas' attempt to duplicate the cube by constructing two mean proportionals, reproduced in Book VIII of Euclid's *Elements*, was the first geometrical construction to employ "mechanical motion", i.e. lines generated by moving figures.[9] If Archytas was in fact the author of the *Wheel*, that would make it one of the oldest thought experiments of which we have any record. In fact, it would be as old as another influential thought experiment attributed to the same Archytas, later made famous by Lucretius: the thought experiment that cast doubt on the finiteness of the extent of the cosmos by asking what would happen if someone standing near the boundary were to throw a javelin through it.[10]

At any rate, our thought experiment is stated as Problem 24 of the *Mechanical Problems*, as follows:

> **24**. A difficulty arises as to how it is that a greater circle when it rolls describes a line of the same length as a smaller circle, if the two are concentric. When they are rolled separately, then the lines they describe are in the same ratio as their respective sizes. Again, assuming that the two have the same centre, sometimes the line they describe is the same length as the smaller circle would describe by itself, and sometimes it is the length of the larger circle's path. Yet it is obvious the larger circle rolls out a longer line… (Aristotle 1936, 387, translation slightly altered)

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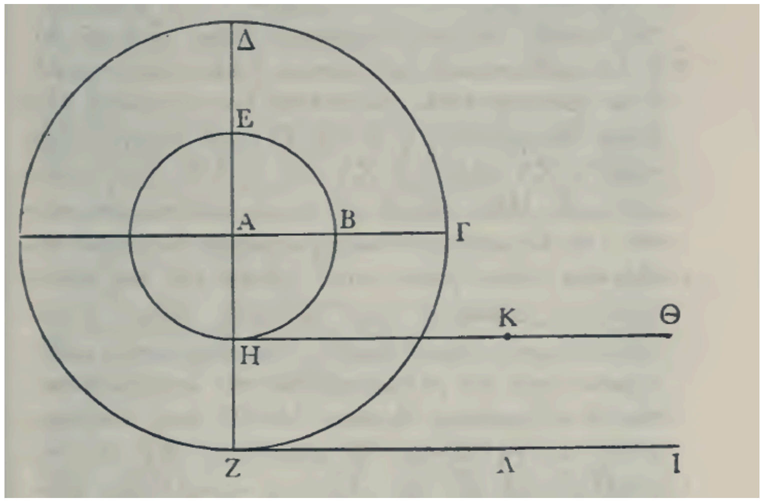
[9] Diogenes Laertius 1925: "**83.** He was the first to bring mechanics to a system by applying mathematical principles; he also first employed mechanical motion in a geometrical construction, namely, when he tried, by means of a section of a half-cylinder, to find two mean proportionals in order to duplicate the cube." (*Lives*, book VIII, §83). According to Diogenes Aristotle wrote "On the Philosophy of Archytas, three books" (*Lives*, book V, §25). Is it not possible that one of these three "books" is a transcription of a *Mechanical Problems* by Archytas?

[10] The other problems treated in the *Mechanical Problems* are all eminently practical in nature, and many exploit the same kind of combination of straight and circular motions for their resolution: (6) "Why is it that the higher the yard-arm, the faster the ship travels with the same sail and the same wind?", (8) "Why are round and circular things easier to move than things of other shapes?", (10) "Why is an empty balance easier to move than a weighted one?", (11) "Why are heavy weights more easily carried on rollers than on carts, though the latter's wheels are larger while the circumference of rollers is small?", (26) "Why is it more difficult to carry long timbers on the shoulders by the end than by the middle, provided that the weight is equal in the two cases?" (Diogenes Laertius 1925)



> As, then, nowhere does the greater circle stop and wait for the less in such a way as to remain stationary for a time at the same point (for in both cases both are moving continuously), and as the smaller does not skip any point, it is remarkable that in the one case the greater should travel over a path equal to the smaller, and in the other case the smaller equal to the larger. (Aristotle 1936, 391)

Here we see the condition of "no skipping" entrenched in the formulation of the problem, thus explaining why some authors have insisted that sliding could not be a solution. In fact, however, it seems that the author of the *Mechanical Problems* takes the view that since the problem, as stated, leads to a paradoxical conclusion, there is something wrong with the premises. Either the path traced out by the centres of each of the wheels is $2\pi Rn$, as it will be if the larger circle drives the motion; or it will be the smaller length, $2\pi rn$, as it will be when the smaller wheel is the driver. The speed of rotation will be the same in the two cases. But these are two different rotations, whose centres are only accidentally the same:

> When, then, the large circle moves the small one attached to it, the smaller one moves exactly as the larger one; when the smaller circle is the mover, the larger one moves according to the other's movement. But when separated, each of them has its own movement. If anyone objects that the two circles trace out unequal paths though they have the same centre, and move at the same speed, his argument is erroneous. It is true that both circles have the same centre, but this fact is only accidental, just as a thing might be both "musical" and "white". For

> the fact of each circle having the same centre does not affect in the same way in the two cases. When the small circle produces the movement the centre and origin of movement belongs to the small circle; but when the large circle produces the movement the centre belongs to it. Therefore what produces the movement is not the same in both cases, though in a sense it is. (395)

What leads to the paradox, then, on this interpretation, is the assumption that the case can be considered without identifying which of the circles is the driver. If it is the larger wheel, then since points on the smaller one are forced to make contact with a larger length, it will slide; if the smaller one is the driver, the larger one must be forced to trace out a shorter length than it rotates. Thus the two wheels, when fixed together, cannot trace the same length in *n* revolutions without one of them slipping against the surface, either the wheel in the rut or the hub on the road.

Let me now summarize my conclusions.

1. I contend that Aristotle's Wheel has a good claim to be regarded as a thought experiment, indeed one of the first. It is an imagined scenario, with accompanying diagram, that leads to a paradoxical conclusion; and it has generated much speculation about foundational questions, in this case the composition of matter and the continuum.

2. According to the so-called "Cantorian" interpretation, the paradox results from the fact that if the two unequal concentric circles trace out equal lines, this implies that their circumferences must be equal. But this involves confusing the equality of the number of points on each circumference, which can be put into 1-1 correspondence, with the equality of their measures.

   This, I claim, misses the point completely. No one claimed the wheel and hub were equal because the points on their rims could be put into 1-1 correspondence. The equality of the measures follows from the conditions of the paradox, namely that the wheel and hub, being rigidly attached together, are forced to travel the same distance. If there is no slippage and they trace equal lines, $2\pi rn = 2\pi Rn$, so that $r = R$.

3. This is what led some authors to see the paradox's resolution as entailing that either the circumference of the small circle must become expanded, or that of the large one contracted. Thus if the large one (the wheel) is the driver, the hub must expand while moving! This is, in effect, Galileo's solution.

   But this beggars belief: the hub clearly does not expand until it becomes the same radius as the wheel—if this happened, no chariot would ever get stuck in a rut again! Expansion and contraction are no solution.

4. A more charitable interpretation of Galileo's version of the thought experiment is that the hub skips over infinitely many infinitely small gaps, which sum to a finite length, namely $2\pi RN - 2\pi rn$. Mersenne and others regarded this solution as betraying the idea of a continuum. Moreover, one might add, if the gaps are, as Galileo claimed, "*non quante*", it is difficult to see how they could be greater or smaller. This led to Mairan's proposal that the non-driver wheel would undergo sliding along infinitesimal segments of the line, with the infinitesimals of that wheel in contact with infinitesimals of the line in the ratio of the radii of the two wheels.

   That solution, of course, breaks the condition that there should be no slippage; it relaxes it to Galileo's claim that there should be no slippage along any *finite* segment of the line.

5. What, then, of Roberval's proof that a point on the circumference of the hub would trace a curtate cycloid of the same length as the cycloid traced by a point on the circumference of the wheel? Roberval effected his proof by combining the tangential component of the point's rotational motion with its translational motion by the parallelogram of motions (itself stated for the very first time in Problem 23 of the *Mechanical Problems*!). But this assumes that, with the outer circle (wheel) the driver, the hub is forced to move with the translational velocity of the wheel. This can only happen if there is continuous slippage between the hub and the road, so this reduces to the same solution as Mairan's.

6. I therefore conclude that the answer to Drabkin's problem, of "how it is possible for the paths to be equal without compensatory slipping in the sense indicated"

> (Drabkin 1950, 166) is: it is only possible if one allows continuous infinitesimal slippage of the kind proposed by Mairan, which Drabkin rejects. If one regards it as a condition of the thought experiment that there should be no sliding at all, the paradox simply indicates that this cannot be done.
>
> Of course, continuous infinitesimal slippage is not physically realistic. It presupposes perfect frictional contact between the driver wheel and its surface, and perfectly frictionless contact between the trailer wheel and its surface. Whatever perfect symmetry there is in the ideal case, one can expect it to be broken in practice, where there will be no prefect evenness of friction between surfaces.

Thus I believe the correct interpretation is that implicit in the analysis of Archytas/ Aristotle, spelled out explicitly by Mersenne: there is indeed slippage, and that is the end of the matter. The ruts were deep in ancient roads, and sparks flew off the naves of the chariots! Real experiment resolves Problem 24 of *Mechanical Problems* completely—provided we agree on what the problem is!

It remains to consider what general morals can be drawn form this case. One might be that, before we can agree on the resolution of a thought experiment, we have first to agree on what the thought experiment is! Second, whether the thought experiment is performable as an actual experiment will depend on identifying exactly what it is, of course, but also on which idealizing assumptions are thought to be essential to the thought experiment, and which are thought to be accidental.

To further illustrate these points, let me very briefly consider in closing one of the two cases I mentioned at the beginning of this paper, Galileo's thought experiment concerning the dropping of two balls of different weights attached by a string, in refutation of Aristotle's theory that they would fall at different speeds. (The third, the Bell-Aspect case, I will reserve for another time.)

> Aristotle declares that bodies of different weight, in the same medium, travel with velocities which are proportional to their weights... and that a stone of 20 pounds moves 10 times as rapidly as one of 2; but I claim this is false, and that if they

> fall from a height of 50 or 100 cubits, they will reach the earth at the same moment. (*Two New Sciences*, 109-110; Galileo 1914, 65)

James R. Brown asserts that Galileo's thought experiment establishes this conclusion independently of experiment. As is well known, Salviati gets Simplicio to agree that a heavier body H will fall faster than a lighter one L, and that "the more rapid one will be partly retarded by the slower" (107; 63). But now if the two stones are tied together, H will be retarded by L, so H + L will fall more slowly than H. But H + L is heavier than H, so "the heavier body moves with less speed than the lighter", contrary to the supposition (107; 63). Says Brown, "We have a straightforward contradiction, the old theory is destroyed. Moreover a new theory is established; the question of which falls faster is obviously resolved by having all objects fall at the same speed." (Brown 1991, 77)

But as I have indicated elsewhere in my own analysis of this thought experiment (Arthur 1999), there are many subtleties here. First, from a modern point of view, two balls falling in a vacuum will undergo uniform acceleration, and if they fall through the same height, they will have the same terminal velocity; but this is an instantaneous velocity, a concept that is only approximated by Galileo's notion of degree of speed, and is not the same as the notion of velocity employed here, which just means how fast a given distance is covered. On the other hand, here the stones are falling in a medium; and in a medium they reach a terminal velocity, their "natural velocity", which is specific to the medium and the specific gravity of the body. This is how Galileo sets up the thought experiment:

> There can be no doubt but that one and the same body moving in a single medium has a fixed velocity which is determined by nature and which cannot be increased except by the addition of momentum [*impeto*] or diminished except by some resistance which retards it. (107; Galileo 1914, 63).

What Simplicio says here for Galileo is, in fact, known to be true: under the right conditions, bodies falling in a medium will eventually attain such a fixed velocity by Stokes' Law. So, under the assumption that the two bodies are falling in a medium, have the same specific gravities, and have reached their "natural velocity", adding one such body to another will make combined body with the same specific gravity as either of the two separately. This is why Galileo could have such confidence that "even without further experiment, it is possible to prove clearly, by a short and conclusive argument, that a heavier body does not move more rapidly than a lighter one, provided they are made of the same material, and in short such as those mentioned by Aristotle." (107; Galilei 1914, 62)

Thus my reading of what constitutes the thought experiment is very different from Brown's. The two stones' having the same specific gravity is a constitutive assumption, as is the fact that the bodies are falling in a medium. If two bodies with very different specific gravities are dropped in a jar containing very viscous fluid, one of them will fall faster than the other. If they are attached together, they will fall at some intermediate velocity. This shows that Brown's conclusion—that the thought experiment immediately establishes that "all bodies fall equally fast"—is itself too fast. Galileo's conclusion does not follow unless these conditions are fulfilled. If there is no medium, they will not reach a "natural velocity" at all, and we learn nothing about their behaviour in a vacuum directly from this thought experiment. (It takes Galileo many pages of further argumentation to establish what will happen to bodies falling in a vacuum.) But if these conditions of the thought experiment are met, then the thought experiment works perfectly. There is no need to do the experiment, but if it is done, it confirms the analysis.

This example also conforms to the morals I drew form the *Wheel*. It all depends on what the problem identified by the thought experiment is taken to be, what the constitutive assumptions are, and what idealizing assumptions are allowed. Having established these things, we see that in this case too, what was concluded from the thought experiment can be confirmed by an actual experiment. So a thought experiment cannot be defined as an imaginative or conceptual representation of a phenomenon that cannot be produced in practice: in at least some cases, it can, and in such cases we expect that actual experiments will either confirm resolutions of any paradox raised by the thought experiment, or throw more light on what the constitutive assumptions of the thought experiment are.

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