# A practical list of John Mandlbaurs's most common false claims and their rejection

## 1. The ball-on-a-string demonstration of COAM is a centuries-old historical example

This is, as most of John's claims, a fabrication. The notion that it is "historical" is completely made-up and so is the assertion that it is centuries-old. It is just a qualitative, casual classroom demonstration that serves no other purpose than providing a visual aid for novices who are learning an otherwise rather abstract concept and it is not even particularly mainstream. In fact, except for a minority of physics lectures and that one example from that specific edition of the introductory textbook by H&R, there is barely any trace of it in the literature. I don't know where John got this silly idea that this demonstration is some sort of "golden standard" but it simply isn't.

## 2. The only evidence for COAM is things that "spin faster"

Another fabrication. The law of COAM comes about as a particular case for $\boldsymbol{\tau} = 0$ of the more general law $\mathrm{d}\mathbf{L}/\mathrm{d}t = \boldsymbol{\tau}$, which is itself nothing else than a practical re-writing of $\mathbf{F} = m\mathbf{a}$ (or better said of $\mathrm{d}\mathbf{p}/\mathrm{d}t = \mathbf{F}$) for rotations. As such, COAM is just a small, consequential part of the broader theoretical framework known as classical mechanics. This relies on tons of experimental evidence collected over more than 300 years and initiated in particular by the study of celestial mechanics. The idea that this very successful theory relies on balls with strings, ice skaters, and other mundane, sloppy systems with lots of uncontrolled effects is idiotic and wrong.

## 3. 12000 rpm objectively falsifies COAM

First of all, a single individual cannot claim something objectively, especially when he has demonstrably piss-poor understanding of the subject and his uninformed opinion is contested by every single expert in the field. That is exactly the opposite of "objective". Secondly, nobody questions that 12000 rpm does not happen for a real-world ball on a string but the thing is that nobody expects it to happen and that prediction simply does not apply to the actual thing because it is come at with an ultrasimplified model that only holds to describe an idealised unrealistic situation aimed at teaching problem-solving skills to novices who are yet incapable of handling the complex maths required to tackle more realistic models of the considered system.

## 4. To defeat my proof you have to point at an equation and find an error in it / maths is proof

This is one of the many “rules” that John makes up as he goes and insists everybody should accept at face value while stubbornly refusing to conform himself to, whenever they are used against him. One can make a flawless mathematical argument from a physically unsound premise and the conclusion would still be wrong. John’s maths relies entirely on the incorrect premise that if COAM is correct it can be applied to the ball on a string expecting realistic results no matter what the actual conditions are. This assumption is physically incorrect and the argument that follows is wrong, irrespective of its mathematical correctness.

## 5. To defeat my proof you must show us 12000 rpm

First all, there is no “us”. John may pretend as much as he wants that he is not alone in this ridiculous crusade but he is nonetheless. Literally no other person in the world is asking anybody to show 12000 rpm. Physicists understand very well that a real ball on a string won’t achieve 12000 rpm unless spun in a vacuum while attached to a frictionless and infinitely stiff pivot but they also understand why is that and the reason is in no way a failure of COAM (or better said of $d\mathbf{L}/dt = \boldsymbol{\tau}$ because, as already stated, there are torques in a real ball on a string).

## 6. My proof is a reductio ad absurdum

It unquestionably is not. A reductio ad absurdum entails a premise and a following deduction that should lead to a logical contradiction in order to claim that this invalidates the premise. Just claiming that a “large” number is “absurd” is an argument from assertion and from personal incredulity that does not constitute a logical contradiction. This also shows why reductio ad absurdum and similar techniques are never used in the physical sciences.

## 7. My mathematical physics paper is undefeated because no error has been pointed out in it

The fact that John lacks the mathematical and scientific skills as well as the intellectual honesty required to understand and acknowledge the numerous mistakes that have been pointed out in his manuscript does not invalidate the criticism. Stamping his feet, restating the same wrong nonsense over and over, and gaslighting with strawman-versions of others’ arguments are not valid rebuttals. The main error is always the same: the manuscript starts from the wrong premise that an oversimplified scenario permitted within the safe sandbox of a sample problem for novices can be used to predict reality. That assumption is naively wrong and it invalidates the entirety of the argument.

## 8. I have addressed and defeated every argument against my papers

Actually, what John usually does is to ignore, evade, or strawman counterarguments. That is not addressing, forget about defeating. He likes to cite his list of prepared “rebuttals” (thus indicating also a certain degree of intellectual laziness) but those have been all replied to and invalidated point by point [https://docs.google.com/document/d/19w_XrLSHzj1O7yb33aypxa466uH-sog_SHOBtSy_i1I] . This is yet unanswered by John, i.e. his claim that he defeated anything is demonstrably false.

## 9. Ice-skaters, ballerinas, and professors on a turn-table disprove COAM and confirm COAE

This is another typical claim that John incessantly utters without ever providing evidence. All those examples, besides being obviously affected by an a priori unknown amount of losses, require a complex analysis of the moment of inertia of human bodies in various positions. Such a computation is tricky and potentially strongly affected by the chosen underlying assumptions. For instance Prof. Lewin’s approximation with a cylinder is extremely crude and can be easily off by a factor 3-4. This type of analysis is clearly beyond John’s very limited skillset, i.e. his claim is unsubstantiated wishful thinking because we know as a fact he could have never computed the angular momentum and rotational energy of such systems properly.

## 10. My equations are referenced

So what? I can reference a French law but that doesn’t mean I can invoke it to argue in a Japanese courtroom. Referencing should be in context and John’s equations are taken from a sample-problem in an introductory book that nowhere states it is presenting a valid model to describe a real ball on a string. In fact, the same book explicitly mentions whether it is describing an actual demonstration (like in the example at the following page) and it clearly doesn’t in John’s cherry-picked example. Moreover, there is no trace of that example in subsequent editions of the book nor in similar introductory texts, which incidentally also destroys John’s other pet argument that the “ball on a string” is some sort of historic example (see point 1). The idea that these equations can be used to describe a real-world system is stupidly wrong and “referencing” them doesn’t fix it in any way.

## 11. My proof that physics is wrong is wrong because physics is wrong

Another example of a gaslighting strawman. John’s “proof” is wrong because the incorrect picture of physics he made up in his uninformed mind is wrong. In particular, his silly idea that a sample problem consisting of an oversimplified idealisation with didactic purpose only is “existing physics” is another misunderstanding of his. Equally made up are similar claims

about physicists “shifting the goalposts” or similar nonsense: nobody shifted anything, it’s just that John never knew where the goalposts were all along to begin with (and still doesn’t).

## 12. The scientific method discards theories that make wrong predictions as Feynman says

This is another one of John’s misconceptions of complex ideas he only has a middle-school level grasp of. This naive take on the scientific method is in particularly triggered by a Feynman quote, extracted from a lecture [https://jamesclear.com/great-speeches/seeking-new-laws-by-richard-feynman] he gave for the general public and where John cherry-picks a sentence while stubbornly ignoring the bit that comes right after where Feynman very clearly explains that a careful check of both prediction and experiment must be performed before considering to toss a theory. John obviously failed on both these accounts. His prediction is clearly naive and faulty as it fails to account for lots of relevant factors. As to experimental evidence, he has basically none whatsoever. The best he can offer are fallacious arguments ad populum (“every rational person who has ever observed…”) or the amateurish LabRat demonstration which proves exactly nothing as explained in point 13. In none of these instances are the effects of the numerous dissipative mechanisms rigorously analysed, quantified, and factored in and they all fail henceforth to fulfil Feynman’s definition of a “carefully checked” experiment.

## 13. The LabRat independently confirms COAE perfectly and then yanks to obtain the desired result until he overshoots

First things first: the LabRat video is amateurishly conducted and cannot be taken as evidence of anything. The lack of an error analysis including estimates of the obvious systematic effects, that he partially identifies but fails to quantify, disqualifies it as “experiment” in any meaningful way. This is also the reason why “perfectly” means exactly nothing in this context: to determine whether a prediction matches an observation error-bars are crucial. The first run of LabRat’s demonstration returns a factor of 2 with an uncertainty of nobody knows how much. What if the error bar, including all possible sources of error, was actually 2.5? COAM would be very much in the picture. The truth is that we don’t know as this is not a carefully conducted experiment in the sense of Feynman’s quote. At any rate, the operator recognises that he is probably not considering some important factors (he correctly identifies friction and air-drag but fails to see the obvious wobble of the pivot as an additional source of error) and takes action to minimise them. Pulling in faster obviously shortens the time during which dissipative effects act so that their contribution is correspondingly reduced and this clearly leads to results that are increasingly close to the COAM’s prediction although this is also an inconclusive result without a proper error analysis. There is thus nothing wrong with pulling faster: “yanking” is a concept that John made-up and it means exactly nothing. It is also worth mentioning that John is adamant that a radial force cannot change the ball velocity so his complaints about yanking are incoherent nonsense. Similarly nonsensical are his attempts at explaining the effect away with “chaos” (a buzzword that explains exactly nothing, he could have said “magic” for all it’s worth): the

dependence of the result upon the pulling-in speed is a feature of the system that must be considered and that cannot be explained without taking into account the aforementioned dissipative effects. This is a fact irrespective of how hard John ignores it. Also the complaint that he “overshoots” is meaningless without an error analysis: a value of 4.1±0.2 does not “overshoot” a prediction of 4.0 by any meaningful sense for instance. Finally, it is also worth mentioning that all of the LabRat’s tests are carried out with a radius reduction by 50% whereas John’s arbitrary and strict parameters for the demonstration entail a reduction by 90%. This further disqualifies, by his own rules, this amateurish demonstration as a proof of his claims. As explained above, it is actually not a proof of anything, really.

## 14. Theoretical predictions are idealisation, friction is something one minimises in the experiment

Another fabrication. While it is certainly a good practice to minimise friction in any experiment aimed at testing a conservation law it is by no means illegal to factor in whatever is left after all corrective measures are taken. This stance of John’s is particularly irrational considering that he refuses to accept any suggested modification to the experiment that would minimise friction. An obvious example of this is his unmotivated rejection of a faster pull-in that he stubbornly dismisses by labelling it with the nonsense made-up concept of “yanking”. He also digs in his heels that the demonstration must entail a radius reduction by 90% while he is perfectly happy to take the LabRat as “golden standard” despite the radius reduction being only 50%. His motivated reasoning goes as far as demanding arbitrary made-up constraints for the experimental parameters like a pulled-out-of-thin-air limit of a 85° angle between velocity and radius.

What the actual procedure shall entail is designing the experiment to minimise the dissipative effects **and**, by all means, carefully quantify and factor in whatever is left. This actually goes as far as tossing experimental designs where said minimisation happens to be insufficient. The ball on a sting clearly seems to be such an example unless similar measures as in the LabRat’s demonstration are taken. It is also worth restating that friction is not the only dissipative effect to be considered and most likely not even the dominant one.