Galileo's and Descartes' evidence problem

By: Lo Min Choong Julian | 3 May 2024

Written for the partial completion of NTU's HY4130 Special Topics in Philosophy of Science, History and Philosophy of Science: From Ptolemy to Newton

---

In this essay, I will show that Galileo and Descartes provided poor quality evidence for their respective "laws", i.e. free fall, projectile motion and inertia, based on what was accessible in 1651. The following serves as a benchmark of how a "law" should be justified—Kepler's elliptical orbit "law" was supported by high-quality evidence, namely it being in agreement with observational data to within observational accuracy. In the process, I shall discuss why Galileo and Descartes experienced "evidence problems" in their lines of inquiry, and how it limited the quality of evidence that they could have provided.

To show why the status of Kepler's elliptical orbit "law" is relevant, I will give a brief overview of Kepler's construction. In *Astronomia Nova* (1609), Kepler constructed his theory of the orbits by focusing on the discrepancies between provisional hypotheses and observational data. He ultimately showed that the orbits are elliptical, but his construction is exceedingly complex to follow, even with Wilson's reconstruction. Kepler began with Brahe's Earth-Sun theory, which allowed him to construct his vicarious theory of Mars, which gave him information about the orbit of the Earth (Wilson, 1986, p. 6; Kepler, 1992). He found that it was not perfectly circular, and that his Earth-Sun orbit had a bisected eccentricity. From there, he derived his area rule, i.e. a planet sweeps out equal areas over regular intervals (Wilson, 1986, p. 16). He also debated the need for an equant, which he substituted for an arc length velocity (of a planet) which varies inversely with the distance of the Sun. He eventually arrived at the possibility of oval orbits; in that regard, he considered alternatives, i.e. elliptical orbits (Wilson, 1986, p. 9). His complex reasoning alone was insufficient in making his construction a competing theory; Agreement with observational data, as of 1651, elevated his construction's status. It is true that Kepler's construction rests on whether his vicarious theory yields true heliocentric longitudes, whether his area rule is true, and whether the octant error is exactly equal and opposite, which is an idealisation. Thus, I have shown that Kepler's construction is complex, and rests on certain conditions being true. It is worth noting that Kepler has never once referred to his results as laws, which suggests that it is not a proof of physical phenomena, but rather something else (e.g. educated conclusions, tested hypotheses, &c; out of scope). The point is that the status of Kepler's elliptical orbit "law" was supported by observations of the relevant celestial bodies made post-construction, which agreed with his conclusions to the point where Kepler's theory competed with existing theories. I draw parallels in regards to Galileo's and Descartes' constructions, as all three theories are complex, the status of which was influenced by agreements or the lack thereof with observational data.

Galileo had poor quality evidence in support of his "law" of free fall, given the flaws with his experimental set-up, and because of his "evidence problem". In Day Three of *Two New Sciences* (1638), Galileo constructed his theory of local motion (Galileo, 1991). In Proposition 2, he outlined his "law" of free fall, stating that in uniform acceleration, how far a body falls is proportional to the square of the time interval (Galileo, 1991). Following this, his first corollary outlined his 1, 3, 5 arithmetic progression, where a body will fall 1 unit of distance within the first interval, 3 units in the second interval, 5 units in the third interval, and so on. Galileo noted the distinction between a theory of motion in a vacuum and a theory of motion in a resistive medium. This is significant as it made precise, to a limited extent, the role of a medium regarding motion. He noted, in some capacity, his "evidence problem". Coined by George Smith, an "evidence problem" occurs when one enquires about a phenomenon one has no access to. In this case, Galileo had no access to a vacuum, and could only conduct experiments within a resistive medium. Galileo believed that a theory of motion in a vacuum should be the primary theory, and that a theory within a resistive medium was secondary; he believed that a resistive medium is an additional layer of complexity which could be accounted for in a more advanced theory. The question is to what extent did Galileo's evidence problem limit the quality of evidence he could have produced? After all, his experiments were conducted within a resistive medium. How relevant, if at all, were his experimental results? Galileo stated his theory held only in the absence of air resistance. He believed his experiments adequately supported his theory.

This was not the case. Galileo had poor evidence for two main (and interlinked) reasons: his flawed experimental set-up, and his conflation of certain phenomena. Galileo justified his "law" of free fall via a series of tests, where spheres rolled down an inclined plane (Galileo, 1991). The critical flaw here is that Galileo's experiment was absent of any free fall motion. If you had two setups: one where a sphere dropped from a height, and another of the same vertical height, where it instead rolled down an inclined plane, the instantaneous velocity of the two spheres right before they hit the ground is significantly different. Thus, one can argue that Galileo did not have proof for his "law" of free fall. He only had circumstantial evidence which was incidentally true. It was Ricolli, in his 1651 publication, who experimented properly, and demonstrated Galileo's 1, 3, 5 progression. Thus, one could argue that Galileo provided poor-quality evidence in support of his theory of free fall, given the lack of investigation into the phenomena in question. One could also question whether Galileo had access to timekeeping instruments which were precise and accurate enough for him to produce meaningful and significant results, but this is a secondary concern. Apart from experimental setups, Galileo also conflated free fall with rotational-translational motion. He believed his results were relevant to his theory because both setups describe a body in (translational) motion. This is not the case. A sphere rolling down a plane is a different kind of motion compared to free fall. The former bears little relevance to the latter. Furthermore, Galileo incorrectly extended the range of his results beyond its relevance. His inclined planes were only 12 braccia long (1 braccio = 22.99 inches; Drake). Galileo incorrectly assumed that acceleration was uniform, thus believing that because the 1, 3, 5 progression demonstrated uniform motion for a limited period, the progression would continue to hold for extended periods. Thus, Galileo produced poor quality evidence for his "law" of free fall because his experimental set-up was flawed, he conflated key concepts, and he extended the relevance of his results beyond what was appropriate.

Galileo also produced poor quality evidence for his "law" of parabolic motion as he only had one piece of evidence for his geometric theory. In Day Three of *Two New Sciences*, specifically Proposition 7 Theorem 4, he laid out his geometric "proof". He derived the optimal angle (i.e. the angle which attains the maximum range of a projectile) of 45 degrees by establishing a relationship between an angle and range, after assuming the effects of uniform acceleration. This was in agreement with what was known at the time (i.e. 45 degrees would attain a maximum range). He hypothesised uniform acceleration only acts vertically (i.e. downwards), which allowed him to work out a "proof". Furthermore, his construction only involves a semi-parabola. Geometrically, he only considers the motion of a projectile from its maximum vertical height to ground level. This is significant because he did not begin from the initial position of a cannonball. Instead, his construction began in the air, at a cannonball's maximum vertical height. In his corollary, he assumed symmetry, and extended his results to the other half of the parabola by mirroring what he had already derived. Thus, one could argue his geometric proof is incomplete, or was a false construction, as Galileo began not at ground level, but at a height. Additionally, one could argue Galileo merely conjectured symmetry to be true, instead of deriving it. It is worth noting that Galileo also encountered the same evidence problem within this construction, given the lack of access to a vacuum. Thus, Galileo only had evidence relevant to a specific range of motion, i.e. downward from the maximum height in the vertical direction. Thus, the evidence he had was insufficient to cover his entire theory.

Descartes had poor quality evidence for his "law" of inertia as it only bore limited relevance to his claims. In *Principles of Philosophy* (1644), Descartes outlined his theory of motion, which contained his "law" of inertia. He had several formulations of his law of inertia. I focus on the main, positive formulation: "Any body, if moving at all, will continue to move at a uniform speed in a straight line unless it is made to deviate from that motion by an external force" (Descartes, 1986). His "law" of inertia, and his conceptualisation of forces, were significant because they challenged conventional thought. However, this significance does little to "improve" the fact that Descartes only provided poor quality evidence. In Part III, Article 60, Descartes stated that celestial motion and local motion are of the same kind, i.e. the same physical principles govern both mechanical domains (Descartes, 1986). He supported this via his vortex theory, which stated an unseen medium physically carries the planets around, thus giving rise to their motions. Descartes justified his vortex theory by appealing to God and to everyday experience of objects in motion. This is relevant as he offered two separate justifications of inertia, one under his vortex theory, and one involving a slingshot. Thus, the conceptual leap of celestial and local forces being of the same kind, and the "law" of inertia rests on the justification of his vortex theory, which was unconvincing, given the lack of mathematical/empirical evidence. Considering inertia only, one should consider the second justification. In Article 59, he described a weight which is attached to a sling. The weight is slung around a fixed centre point. Descartes appealed to experience that when the object is released, it will retain its motion, and move off in a straight line from its release position. Thus, through various methods, Descartes constructed a comprehensive explanation of inertia and planetary motion. The problem is his slingshot "experiment" only yielded a straight-line motion over an extremely limited period. The object would move in a straight line, for a brief period, but after which it would fall to the ground. Thus, Descartes failed to adequately demonstrate inertia as he described it, as his appeal only held for a limited period, given his lack of access to an ideal environment where one could observe straight-line motion without a resultant force acting on a body. Thus, one could say Descartes only demonstrated an instance of inertia, not the fact that inertia held for extended periods.

Thus, while Galileo and Descartes had theories which aimed to comprehensively explain certain phenomena, the evidence they provided only limitedly justified some of their claims. Compared to Kepler's elliptical orbit "law", which was in agreement with observational data, Galileo's and Descartes' theories did not have the same kind of agreement/justification that Kepler's rule had. While Galileo could have produced higher quality evidence, as Ricolli demonstrated, it is uncertain how Descartes' slingshot "experiment" could have been improved to show that inertia held for extended periods.

References

Descartes, R. (1974-1986). *Principia Philosophiae*, vol. X in Oeuvres de Descartes (Adams, C., Tannery, P., Trans.). Elzevier. 2nd ed. (Original work published 1644)

Galileo, G. (1991). *Dialogues Concerning Two New Sciences* (Crew, H., de Salvio, A., Trans.). Prometheus Books. ISBN-10: 1616401893.

Kepler, J. (1992). *New astronomy* (W. H. Donahue, Trans.). Cambridge University Press. https://doi.org/10.1086/289846 (Original work published 1609)

Wilson, C. (1968, March). *Kepler's Derivation of the Elliptical Path*. The University of Chicago Press 59(1), 4-25. https://www.jstor.org/stable/227848.