INVERSE SQUARE LAW

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A: PRIORITY

Here's a scoop:

> [I say *Cylinder*, not a piece of a *cone*, because, as I may elsewhere shew in the Explication of Gravity, that *triplicate* proportion of the shels of a Sphere, to their respective diameters, I suppose to be removed by the decrease of the power of Gravity] (*Micrographia*; p. 227. Square parenthesis in the original)

Robert Hooke, the curator of experiments for the Royal Society of London, is providing here a justification for a calculation. He produced a set of experiments with which he hopes to substantiate his claim that the different layers of the atmosphere have different density. The experiments are simple and ingenious, and it is a shame we have no time to discuss them. They are based on Torricelli's example and consists, basically, in measuring the expansion and compression of air which is being pressured or stretched by the weight of mercury in the tubes figured. Hooke collects the results in tables, and uses the data to calculate the height of the atmosphere.

Now, to produce the calculation, Hooke submits, "**we will suppose a *cylinder* indefinitely extended upwards**." The atmosphere, however, envelops the earth as a sphere, so, strictly speaking, the tube supports a truncated cone of air rather than a cylinder. The argument Hooke outlines in the square brackets explains the move.

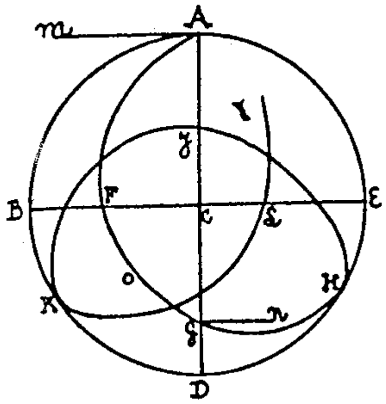
The volume of a cone is in "***triplicate* [cubic] proportion**" to its height. The volume of a cylinder varies only in simple ratio to its height. The difference between the cubic ratio and the simple ratio is, however, "**removed by the decrease of the power of Gravity**." Namely: if gravity decreases with distance, it will compensate for the difference between the real and the assumed volume of air, and the atmospheric pressure can be calculated, for convenience sake, as if it was a cylinder of air that the tube supports, rather than a cone. Of course, it is not enough that gravity will vary inversely as the distance—it must be inversely proportional to the square of distance. Hooke assumes, off hand, the Inverse Square Ratio between gravity and distance

To understand why this is a scoop we need to advance somewhat in time. About 15 years after he published these experiments in his 1665 *Micrographia*, Hooke inherited the position of the secretary of the Society from his nemesis Henry Oldenburg. One of the first things he did in his new post was to write, on Nov. 24, 1679, a cordial letter to Newton in Cambridge, suggesting that all their previous strives were to be blamed on the deceased, and inviting Newton to correspond with him, Hooke, and through him with the Royal Society. The heart of the letter was the following proposal. "**I shall take it as a great favour**," wrote Hooke to Newton,

> if you will let me know your thoughts of that [hypothesis of mine] of compounding the celestial motions of the planets of a direct motion by the tangent & an attractive motion towards a central body" (*Correspondence*, II; p. 297).

This is the essence of what historians came to title *Hooke's Programme*. It was an extremely revolutionary proposal: to account for the revolutions of the planets as rectilinear motion continuously curved by an attraction to the center about which they revolve.

How is it relevant to the issue of the Inverse Square Law? Well, Newton was not immediately convinced. When Hooke finally succeeded to draw his attention to the notion that a rectilinear motion coupled with a rectilinear attraction from the center will create a closed orbit, he voiced the following doubt: let's assume, he wrote, that the operation of a central force may result in the attracted body moving around the center of attraction. Why, however, should we also accept that the outcome will be a closed curve? Why not of the following shape:



Hooke had, of course, only a partial answer. Had he known how to calculate the orbits, he would not have turned to Newton in the first place. But he did have some ideas. The question, as he saw it, was one of balance between the rectilinear motion along the tangent and the attraction towards the center. Such a balance can be achieve, he suggested to Newton, if we assume (i) an inverse proportion between the planets' velocity and their distance, which is a false version of Kepler's second law; (ii) a square ratio between force and velocity, which is a force law which Hooke has been using throughout his career—an early version of the *vis viva*, if you wish; and (iii) "**that the Attraction**," in his own words, "**always is in subduplicate proportion from the Center Reciprocal**." In other words—our friend the Inverse Square Law.

Newton did not respond, and after another attempt by Hooke the correspondence came to an end. Four years later, in the summer of 1684, Edmund Halley paid a visit to Newton in Cambridge, especially to inquire about his views concerning what was by then a trendy speculation in London caf's: assuming that the sun attracts the planets by a force which diminishes with the square of their distance, so the suggestion went, one could calculate the trajectories of the planets and even come up with elliptical orbits, as Kepler suggested some seventy years earlier. To Halley's surprise and delight, Newton replied that he has already done just that a few years ago. Not being able to produce the calculations on the spot, Newton promised Halley to send them to him. A few months later Halley received an 11 pages tract in wonderful handwriting—*De Motu Corporum in Gyrum*, which though not exactly an answer to his question, offered much more in exchange. In less than two more years Newton turned the *De Motu* into the *Principia*, which was published in 1687.

When Hooke learned about the *De Motu* and the book being written under the auspices of the Royal Society he was incensed. He thought he deserved at least an acknowledgment that Newton never intended to grant him. Halley related Hooke's claims and rage to Newton, who replied that it is true, indeed, that the correspondence with Hooke enticed him to think about the subject, but that is where his debt ends. As for the ISL between gravity and distance, Newton insisted,

> I can affirm yt I gathered it from Keplers Theorem about 20 years ago. (*Correspondence*, II; pp. 444-5: Newton to Halley, 14 July 1686)

That was the end of the beautiful and short-lived friendship between Hooke and Newton and a beginning of a priority dispute lasting three centuries. Almost all common acquaintances of the two and all historians of the period have had a view to offer on the issue. David Gregory, for example, testified that in 1694 Newton showed him a manuscript written before 1669, containing all the principles of Newton's philosophy, and in 1957 A. R. Hall found a manuscript fitting Gregory's description, containing, indeed, a proof of the ISL based on Kepler's third law. He dated it to the *annus mirabilis*—1666 and the matter seemed to be closed.

That is, then, the scoop. The *Micrographia* was written in the years 1663-4 and published in 1665. Hence Hooke's use of the ISL precedes Newton's earliest one by at least one year.

You may be thinking to yourselves that a historiography seminar is not the proper stage to announce such a finding, and you'll definitely have a point. It is, in fact, the very point I would like to make here, only in more general terms. In spite of the appeal that historians find in priority disputes, and in spite of the great temptation to adjudicate them in hindsight and the pleasant feeling of 'hard facts' it provides, I find that this occupation brings more damage than reward to the historiography of science. In the rest of the time allocated to me I'll try to convince you that the important debates concerning the ISL were not the ones over the credit due for its discovery, but rather the ones by which this law was established—the disputes and exchanges constituting, if you like, the dialectics by which the ISL came to exist.

B: The History of ISL

The incidental manner in which Hooke employs the ISL hints at the first argument against treating this mathematical apparatus as an unknown continent to be named after the first traveler to stumble upon its shores. When he was writing the *Micrographia*, Hooke definitely did not perceive the notion that gravity decreases by the square of distance as meriting a priority struggle, otherwise he would not have alluded to it but rather proudly pronounced it. In fact, the structure of his argument suggests Hooke's resources. Medieval optics, at least since the 9th century Baghdadi scholar al-Kindi, took it for granted that light rays are scattered in a cone with the light source as apex. This means that a fixed of light 'quantity' is falling on an ever increasing base of the cone, hence the amount of light falling on each point on this base is in an inverse proportion to its area. Since the area of the base of a given cone varies with the square of its height, this quantity (or intensity) of light decreases in proportion to the square of the distance from the light source.

I am not sure as to who was the first to explicate this argument, but Kepler, already in his juvenile letters to his master Maestlin, mentions it as self-evident. Light is the obvious relation between the sun and the planets, and Kepler indeed treated the notion that this relation is governed by an ISL as a commonplace. Kepler, you probably remember, explained the motion of the planets as resulting from the sun's rotation around its axis, and assumed a force connecting the planets to the sun like a spoke, dragging them along. In the *Astronomia Nova* he develops this notion of "virtus motrix" in close and explicit analogy to light, and used the ISL to highlight the *limits* of the analogy:

> the intension and remission of the motion of the planets is in a simple [direct] proportion to their distance. But the virtue emanating from the sun seems to increase and decrease in double or triple proportion to their distance ... It follows that the intension and remission of the motion of the planets will not be [caused] by the attenuation of the virtue emanating from the sun.

Kepler's main popularizator in Western Europe—Ismael Boulliau—reversed the argument. He was fascinated enough with the analogy between the solar 'motive virtue' and light to discard Kepler's reservations, and embraced an IS relation while criticizing Kepler for his hesitation. Both Newton and Hooke were familiar with Boulliau's version of the medieval quasi-quantitative argument: in a manuscript from 1685 Hooke deduces the inverse square law from the notion **"that gravity is a continuall impulse expanded from the center of the earth indefinitely by a conicall Expansion,"** (in his *Lectures of Light*, in :*Posthumous Works* p. 114 there is adirect reference to "Bulialdus"), and in the angry and petty letter Newton sent to Halley in 1686 in response to Hooke's allegations, he insinuates that "**this general proposition in Bullialdus**" was Hooke's only recourse to the ISL.

The problem with the priority dispute, however, is not that neither Hooke nor Newton deserve the copyrights over the ISL. It is, rather, that in asking 'who was the first to discover the ISL' historians assume the law, in it final configuration and function, instead of inquiring how did it receive this configuration and that function. The fact that different versions of the ISL were available to both our protagonists does not diminish their role in the story, quite the contrary; it clarifies that the relation is a mathematical tool, devoid of any independent metaphysical status, which has been endowed with meaning and significance only in its various applications by various practitioners. And this significance changes completely with Newton's adoption of *Hooke's Programme*.

C: *On Circular Motion*

To understand the change, let us look first at that manuscript of Newton from 1666—*On Circular Motion* (*OCM*). In this manuscript Newton proves the ISL from Kepler's third law, the harmonic law. With the help of the following simple diagram:

and a few bold mathematical moves, he proves that for a body revolving in a uniform motion, the '**conatus from the center**' (already labeled by Huygens '**vis centrifuga**') is proportional to the radius of the circle divided by the square of its period of revolution. I shall not have time to attend to the diagram and proofs in any details, so I would like to concentrate on Newton's uses of the proportion $\mathbf{f} \propto \mathbf{R} / \mathbf{T}^2$. The first of these is in calculating the centrifugal force (and the corresponding gravity) on the earth's equator and at the moon's orbit (the famous "moon test"). After accomplishing this Newton applies his findings to regular and conic pendulums. But before doing so he adds the following as a rather marginal remark at the end of his corollary:

> Finally since in the primary planets the cubes of their distances from the sun are as the squares of the numbers of revolutions in a given time: the endeavours of receding from the sun will be reciprocally as the squares of the distances from the sun. (*Herivel*; p. 197)

This is not a difficult derivation:

if $\mathbf{f} \propto \mathbf{R} / \mathbf{T}^2$ and $\mathbf{T}^2 \propto \mathbf{R}^3$, then $\mathbf{f} \propto \mathbf{R} / \mathbf{R}^3$ or $\mathbf{f} \propto 1 / \mathbf{R}^2$,

namely: the "**conatus a sole recedendi**" is inversely proportional to the square of the distance from the sun.

Unless enchanted by the very pronouncement of an inverse square ratio between distance and force, one cannot fail to notice the provisional, conditioned status of ISL in *OCM*; it is true to the degree that Kepler's third law is correct. Newton's conclusion is a remark about the consequences of Kepler's novel astronomical endeavors. It is of some significance if one is willing to adopt this strenuous combination of Copernican convictions, Tychonian observations, approximation techniques, and theoretical speculations. But Newton's trust in any of these, especially at this stage of his career, was very reserved. Thus, this corollary is not presented as a major—certainly not *the* major—achievement of the paper. It is neither the first nor the last theorem, it is limited to the primary planets, the derivation, simple as it is, is only hinted at, and the effort at precision is minimal.

In particular, Newton makes no effort to accommodated this corollary with the two other Kepler laws. He does not explain how could one move from uniform circular motion to non-uniform elliptical motion. Nor does Newton attempt to clarify what he means by the "**distantia**" of the planets, if their orbits are indeed, as Hooke will later paraphrase Kepler, "**not circular nor concentricall**." More important yet: this presentation of ISL is its final appearance in *OCM*. Newton does not attempt to put it to any further use, and it serves no obvious theoretical purpose.

D: *De Motu*

This status will change completely in Newton's writing following the correspondence with Hooke.

In *De Motu*, that paper sent to Halley in 1684, Newton proves the ISL twice, in two different ways. The first is rather similar to the proof in *OCM*, and I will not dwell on it.

Immediately following this proof, however, Newton adds five corollaries, all simple derivations from $\mathbf{f} \propto \mathbf{AD}^2 / \mathbf{R}$, where **AD** is an infinitesimal arc. Since the motion is uniform, **AD** is proportional to the body's velocity. And combining $\mathbf{AD} \propto \mathbf{V}$ with $\mathbf{f} \propto \mathbf{AD}^2 / \mathbf{R}$, it follows that:

1. $\mathbf{f} \propto \mathbf{V}^2 / \mathbf{R}$.

Since the velocity is inversely proportional to the period, namely; $\mathbf{V} \propto 1 / T$, this is equivalent to:

2. $\mathbf{f} \propto \mathbf{R} / \mathbf{T}^2$.

This is basis enough to find a force law—a ratio between force and distance—for *any* given ratio between the radius of an orbit and the period of revolution, and Newton indeed offers three different force laws:

3. if $\mathbf{T}^2 \propto \mathbf{R}$, then **f** is distance-independent,
4. if $\mathbf{T}^2 \propto \mathbf{R}^2$, then $\mathbf{f} \propto 1 / \mathbf{R}$, and
5. if $\mathbf{T}^2 \propto \mathbf{R}^3$, then $\mathbf{f} \propto 1 / \mathbf{R}^2$.

Of course, as the Scholium has it, "**the case of the fifth corollary holds for the celestial bodies … astronomers are now agreed**." But the new and exciting skill Newton demonstrates is not that of mathematical interpretation of a particular empirical generalizations "**astronomers are *now* [jam] agreed,**" but rather his ability to construct a force law for *any* set of proportions between times and distances; what astronomers agree upon is here no more than a reference point.

This baroque presentation of a variety of force laws sheds light on the more rigorous introduction of ISL presented later in *De Motu*. Again—summarily: with the aid of Kepler's area law, which he proves most generally—for any law of force and any center of force—Newton establishes a completely general geometrical expression of centripetal force—for any center of force and any curve about it (the first proof holds, of course, only for circular orbit whose center of force is in the geometric center). He proudly presents the exceptional power of these tools in a corollary:

> if any particular figure is given and a point in it to which the centripetal force is directed, it can be found what law of centripetal force makes the body revolve in the perimeter of that figure. (*Herivel*; p. 280)

Employing these two powerful tools—the area law and the geometrical expression for force—Newton embarks upon a series of '***Problemata***', following the conditions set in the corollary. Much more than an attempt at a geometrical account of what "**astronomers are now agreed,**" this is a tour-de-force of geometrical control over the relations between force and orbital motion. As if to highlight this point, Newton commences with a clearly fictive *Problem*: to find a force law for a circular orbit with a center *on* the perimeter, which is found to be inversely proportional to the fifth power of the distance. This serves as pretext to a scholium about paths which are not, strictly speaking, orbits, such as a spiral, for which Newton claims (without a proof) that the force would be inversely as the third power of the distance. The second *Problem* deals with elliptical orbit with the force tending towards the center, which turns out to be inversely as the distance.

'Give me an orbit and a center and I shall produce a force law'—this is the approach in which Newton tutor his readers. Only in the 3rd *Problem*, after the unparalleled scope and power of this new approach is fully established does ISL make a new appearance, and with it Kepler's picture of the heavens:

> A body orbits in an ellipse. It is required to find the law of centripetal force tending towards the focus of the ellipse.

The force is indeed found to be inversely proportional to the square of the distance, and from here on Newton will continuously enrich the relations between ISL and Kepler Laws.

First, he explicates this relation in a scholium, presenting the first and second laws, for the first time, as an integrated whole, which follows as such from the solution of *Problem 3*:

> Therefore the major planets orbit in ellipses having a focus at the center of the Sun; and by radii drawn to the Sun describe areas proportional to the times, just as Kepler supposed.

And in the following *Theorema IV* this particular force law, endowed with its new significance, is used to derive Kepler's *third* law:

> Supposing that the centripetal force is inversely proportional to the square of the distance from the center, the squares of the periodic times are as the cubes of the transverse axes.

The direction of this last theorem is opposite to that of *Theorema II*; the ISL is now assumed, and used to prove the harmonic proportion between distances and periods. The conditions, as in the *Problem 3*, are planetary-like; uniform circular motion has irrevocably given way to non uniform motion on an ellipse with a center of force in one of the foci. In the next exercise, *Problem 4*, Newton finally approaches the question posed to him by Halley's, viz.: if the force varies inversely as the square of the distance, what will be the trajectory? Assuming ISL and an elliptical orbit, he demonstrates how the particular parameters of the ellipse can be found. The journey around Kepler laws has been completed. Still, this is not Newton's final word: "**Matters are thus when the figure is an ellipse**" he carefully qualifies, "**For it can happen that the body moves in a parabola or hyperbola**." If the initial velocity of the body stands in a different proportion to the latus rectum, "**the figure will be an a parabola having its focus at the point S ... But if the body is projected in yet greater velocity, it will move in a hyperbola.**" Kepler's ellipse, Newton is demonstrating, is one particular conic section which an orbiting body might assume as its trajectory, contingent upon the proportions between its initial velocity and distance from the source of force.

E: The New Use of ISL

This is a new type of astronomy, in which the dynamic parameters of force, motion, distance and the proportions among them determine the particular—real, possible or imaginary—orbits and periods. And in this newly-formulated astronomy the inverse square ratio between force and distance is assigned a role it never had before. A marginal afterthought in the early tract *OCM*, ISL is the primary theoretical tool at Newton's disposal in *De Motu*. Employing it, he is able to link Kepler laws to each other theoretically, and embed them into the well charted mathematical realm of conic sections. Newton, we saw, may assume Kepler's so-called third law, prove the ISL, and then use it to deduce the first law. He can, conversely, begin with assuming Kepler's second law, deduce the ISL and use it to prove the third law. And he can, finally, produce any other conic section by changing the parameters of the law.

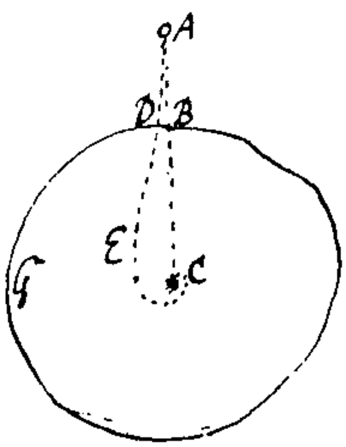
Two points, just to highlight Newton's achievement. First, Kepler laws, as a set of empirical generalizations, were desperately interconnected. Framing them into a coherent theoretical whole, using a theoretically-motivated law was, therefore, a tremendous success, especially since the parameters of the law—the particular ratio between distance and attraction—were also entrenched in Kepler's own speculations. Secondly, Newton always claimed that Kepler merely "**knew y^e^ Orb to be not circular but oval & guest it to be Elliptical.**" Using the ISL Newton could give the ellipse a theoretical interpretation; it is a conic section, as would be described by any body which is, as he puts it, "**projected from a given point with a given velocity along a given straight line.**"

It is not the 'discovery' of ISL, then, but its new use, its new meaning and import, that marks the difference between Newton's early and late work on planetary motion. Still, making a force law—any force law—the central explanatory means in an astronomical theory was a far more radical and consequential move on Newton's part than setting the particular constants of that law. This is the point Newton forcefully establish with *De Motu*'s five corollaries; ISL is only one possible ratio between force and distance, producing particular outcomes when plugged into the newly constructed formulas connecting centripetal forces, tangential motions and planetary trajectories.

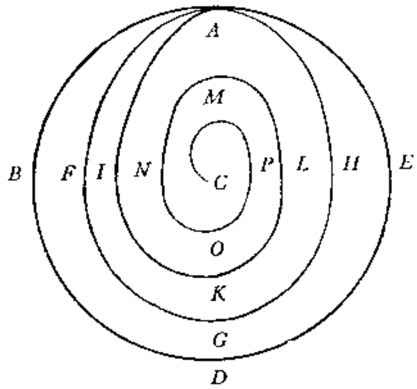
F: THE CORRESPONDENCE

So, despite the scoop, I do not find the interesting question concerning the ISL to be the chronological one of its pronouncement but rather the pragmatic one of its use and the dialectic one of its establishment. I shall conclude, as much as time will allow, with locating the answer to the latter in short and intensive exchange between Hooke and Newton in the winter of 1679/80.

As you may recall, at the heart of Hooke's first letter stood his *Programme*—the suggestion that planetary motion should be accounted for as rectilinear motion continuously curved by external force—and the invitation to Newton to express his opinion. Newton, as you may also recall, was not immediately seduced, but not wishing to rudely turn down Hooke's polite approach, he offered him, in exchange to Hooke's ideas concerning the annual motions, a little clever experiment to prove the diurnal one:



Let us throw a stone from a high tower A. The unenlightened would think that the stone should fall 'behind' the tower—to its west.. In fact, since the head of the tower is further removed from the center of rotation—the center of the earth—than its feet, the stone at the top moves east faster than at the bottom, and it will therefore fall towards the east. The effect will of course be minute, but if we were to draw a line on the ground under the point of release, and in consecutive experiments the stone will consistently fall to the east of the line, it could be considered as a conclusive evidence to the motion of the earth around its axis.



Up to here the idea is impeccable, and Hooke indeed adopted the experiment enthusiastically and promised to perform it (as he did). But Newton added another interesting remark: if the stone were allowed to continue and fall through the earth, it would have described a spiral coming to rest in the center of the earth. This remark gave Hooke an opportunity to set Newton straight, an opportunity he was not going to miss: if we imagine the earth sliced, he was only too happy to explain, so that the stone could fall through it while still experiencing the attraction from the center, it would not fall on that center, but describe the "Elleptueid" **AFGHA** around it. Only in the case of additional resistance, such as the air's, will this planetary-like ellipse collapse into the spiral **AIKL** etc. terminating in the center **C**.

This short negotiation exemplifies the sharpest distinction between *Hooke's Programme*, as it was formulated, investigated and published by Hooke since the mid 1660s, on the one hand, and, on the other, the basic, one might be even tempted to say 'instinctive' understanding of the relations between force and motion as they which Newton's thought experiment expresses and his work (just like Huygens' work) embodies until the correspondence with Hooke. Force, according to this conception, either attracts or repels. Hence, a body under the operation of force will either be drawn further and further away from the source of force if (like centrifugal force) the force is repulsive, or if the force is (like gravity) attractive, it will accelerate and approach its source until it will finally fall on it. To adopt *Hooke's Programme*, one need to accommodate the idea that an attracted body may orbit around the attracting one; that force is not only the cause of acceleration, but also of uniform cyclic motion.

It should be stressed that the difficulty in Hooke's idea is not a mathematical one. The mathematical tools that Newton employs in *De Motu* are not more sophisticated than the ones he and Huygens were using already in the 1660s, and the only novelty is the proof of Kepler's second law. But the use of these tools until the correspondence always assumed a that the force is the effect of the circular motion—the idea that it might cause it was never investigated. The astronomical-physical project suggested by Hooke is completely new, viz.: to treat planetary motion as continuously curved and to give a mathematically-adequate account of planetary orbits as a product of centripetal force and tangential motion, an account in which the particular parameters of this force and this motion—the force law, the distance between the orbiting body and the center of force, the initial inertial velocity—are the factors determining the shape of the orbit. Moreover; these parameters determine if such an orbit will ever be formed, if the motion will repeat itself, if the planet will travel around the sun. This marvelous idea, that the planetary orbits are a contingent matter, dependent on a precarious balance between parameters of force and motion, is what is captured in the new use of ISL, is the change that allowed the *Principia*, and is the product of a particular exchange—a miniature dialectic process, if you like—the correspondence of 1679/80.