Letter to Edmund Halley on the doctrine of projectiles and motions of the heavens

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Sir

In order to let you know the case between M^{r} Hook & me I gave you an account of what past between us in our Letters so far as I could remember. For tis long since they were writ & I do not know that I have seen them since. I am almost confident by circumstances that Sir Chr. Wren knew the duplicate proportion when I gave him a visit, & then M^{r} Hook (by his book Cometa written afterward) will prove the last of us three that knew it. I intended in this Letter to let you understand the case fully but it being a frivolous business, I shal content my self to give you the heads of it in short: viz^{t} that I never extended the duplicate proportion lower then to the superficies of the earth & before a certain demonstration I found the last year have suspected it did not reach accurately enough down so low: & therefore in the doctrine of projectiles never used it nor considered the motions of the heavens: & consequently M^{r} Hook could not from my Letters which were about Projectiles & the regions descending hence to the center conclude me ignorant of the Theory of the Heavens. That what he told me of the duplicate proportion was erroneous, namely that it reacht down from hence to the center of the earth. That it is not candid to require me now to confess my self in print then ignorant of the duplicate proportion in the heavens for no other reason but because he had told it me in the case of projectiles & so upon mistaken grounds accused me of that ignorance. That in my answer to his first letter I refused his correspondence, told him I had laid Philosophy aside, sent him only the experiment of Projectiles (rather shortly hinted then carefully described) in complement to sweeten my Answer, expected to heare no further from him, could scarce perswade my self to answer his second letter, did not answer his third, was upon other things, thought no further of philosophical matters then his letters put me upon it, & therefore may be allowed not to have had my thoughts of that kind about me so well at that time. That by the same reason he concludes me then ignorant of the duplicate proportion he may as well conclude me ignorant of the rest of that Theory I had read before in his books. That in one of my papers writ (I cannot say in what year but I am sure some time before I had any correspondence with M^{r} Oldenburg & that's) above fifteen years ago, the proportion of the forces of the Planets from the Sun reciprocally duplicate to their distances from him is exprest & the proportion of our gravity to the Moon's conatus recedendi a centro Terræ is calculated thô not accurately enough. That when Hugenius put out his Horologium Oscillatorium a copy being presented to me; in my letter of thanks to him I gave those rules in the end thereof a particular commendation for their usefulness in Philosophy, & added out of my aforesaid paper an instance of their usefulness in comparing the forces of the Moon from the earth & earth from the Sun in determining a Probleme about the Moons phase & putting a limit to the Sun's parallax. Which shews that I had then my eye upon comparing the forces of the Planets arising from their circular motion & understoo**{**d**}** it: so that a while after when M^{r} Hook propounded the Probleme solemnly in the end of his Attempt to prove the motion of the earth, if I had not known the duplicate proportion before I could not but have found it now. Between 10 & 11

years ago there was an Hypothesis of mine registred in your books, wherein I hinted a cause of gravity towards the earth Sun & Planets with the dependance of the celestial motions thereon: in which the proportion of the decrease of gravity from the superficies of the Planet (thô for brevities sake not there exprest) can be no other then reciprocally duplicate of the distance from the center. And I hope I shall not be urge{d} to declare in print that I understood not the obvious mathematical conditions of my own Hypothesis. But grant I received it afterwards from M^{r} Hook, yet have I as great a right to it as to the Ellipsis. For as Kepler knew the Orb to be not circular but oval & guest it to be Elliptical, so M^{r} Hook without knowing what I have found out since his letters to me, can know no more but that the proportion was duplicate quam proximè at great distances from the center, & only guest it to be so accurately & guest amiss <55v> in extending that proportion down to the very center, whereas Kepler guest right at the Ellipsis. And so M^{r} Hook found less of the Proportion then Kepler of the Ellipsis. There is so strong an objection against the accurateness of this proportion, that without my Demonstrations, to which M^{r} Hook is yet a stranger, it cannot be beleived by a judicious Philosopher to be any where accurate. And so in stating this business I do pretend to have done as much for the proportion as for the Ellipsis & to have as much right to the one from M^{r} Hook & all men as to the other from Kepler. And therefore on this account also he must at least moderate his pretenses.

The Proof you sent me I like very well. I designed the whole to consist of three books, the second was finished last summer being short & only wants transcribing & drawing the cuts fairly. Some new Propositions I have since thought on which I can as well let alone. The third wants the Theory of Comets. In Autumn last I spent two months in calculations to no purpose for want of a good method, which made me afterwards return to the first Book & enlarge it with divers Propositions some relating to Comets others to other things found out last Winter. The third I now designe to suppress. Philosophy is such an impertinently litigious Lady that a man had as good be engaged in Law suits as have to do with her{.} I found it so formerly & now I no sooner come near her again but she gives me warning. The two first books without the third will not so well beare the title of Philosophiæ naturalis Principia Mathematica & therefore I had altered it to this De motu corporum libri duo: but upon second thoughts I retain the former title. Twill help the sale of the book which I ought not to diminish now tis yours. The Articles are with the largest to be called by that name. If you please you may change the word to sections, thô it be not material. In the first page I have struck out the words uti posthac docebitur as referring to the third book. Which is all at present from

Your affectionate friend &

humble Servant

Is: Newton.

Cambridge
June 20. 1686.

Since my writing this letter I am told by one who had it from another lately present at one of your meetings, how that M^{r} Hook should there make a great stir pretending I had all from him & desiring they would see that he had justice done him. This carriage towards me is very strange & undeserved, so that I cannot forbeare in stating that point of justice to tell you further, that he has published Borell's Hypothesis in his own name & the asserting of this to himself & completing it as his own, seems to me the ground of all the stir he makes. Borel did something in it & wrote modestly, he has done nothing & yet written in such a way as if he knew & had sufficiently hinted all but what remained to be determined by the drudgery of calculations & observations, excusing himself from that labour by reason of his other business: whereas he should rather have excused himself by reason of his inability. For tis plain by his words he knew not how to go about it. Now is not this very fine? Mathematicians that find out, settle & do all the business must content themselves

with being nothing but dry calculators & drudges & another **{**that**}** does nothing but pretend & grasp at all things must carry away all the invention <55ar> as well of those that were to follow him as of those that went before. Much after the same manner were his letters writ to me, telling me that gravity in descent from hence to the center of the earth was reciprocally in a duplicate ratio of the altitude, that the figure described by projectiles in this region would be an Ellipsis & that all the motions of the heavens were thus to be accounted for: & this he did in such a way as if he had found out all & knew it most certainly. And upon this information I must now acknowledge in print I had all from him & so did nothing my self but drudge in calculating demonstrating & writing upon the inventions of this great man. And yet after all, the first of those three things he told me is fals & very unphilosophical, the second is as fals & the third was more then he knew or could affirm me ignorant of by any thing that past between us in our letters. Nor do I understand by what right he claims it as his own. For as Borell wrote long before him that by a tendency of the Planets towards the sun like that of gravity or magnetism the Planets would move in Ellipses, so Bullialdus wrote that all force respecting the Sun as its center & depending on matter must be reciprocally in a duplicate ratio of the distance from the center, & used that very argument for it by which you, Sir, in the last Transactions have proved this ratio in gravity. Now if M^{r} Hook from this general Proposition in Bullialdus might learn the proportion in gravity, why must this proportion here go for his invention? My letter to Hugenius which I mentioned above was directed to M^{r} Oldenburg who used to keep the Originals. His papers came into M^{r} Hooks possession. M^{r} Hook knowing my hand might have the curiosity to look into that letter & thence take the notion of comparing the forces of the Planets arising from their circular motion & so what he wrote to me afterwards about the rate of gravity, might be nothing but the fruit of my own Garden. And its more then I can affirm that the duplicate proportion was not exprest in that letter. However he knew it not (as I gather from his books) till five years after any Mathematician could have told it him. For when Hugenius had told how to find the force in all cases of circular motion, he had told them how to do it in this as well as all others. And so the honour of doing it in this is due to Hugenius. For another five years after to claim it as his own invention, is as if some Mechanick who had learnt the Art of surveying from a Master should afterwards claim the surveying of this or that piece of ground for his own invention & keep a heavy quarter to be in print for it. But what if this surveyor be a bungler & give in an erroneous survey? M^{r} Hook has erred in the invention he pretends to & his error is the cause of all the stirr he makes. For his extending the duplicate proportion down to the center (which I do not) made him correct me & tell me the rest of his Theory as a new thing to me & now stand upon it that I had all from that his letter: notwithstanding that he had told it to all the world before & I had seen it in his printed books all but the proportion. And why should I record a man for an Invention who founds his claim upon an error therein & on that score gives me trouble? He imagins he obliged me by telling me his Theory, but I thought my self disobliged by being upon his own mistake corrected magisterially & taught a Theory which every body knew & I had a truer notion of then himself. Should a man who thinks himself knowing, & loves to shew it in correcting & instructing others, come to you when you are busy, & notwithstanding your excuse, press discourses upon you & through his own mistakes correct you & multiply discourses & then make this use of it, to boast that he taught you all he spake & oblige you to acknowledge it & cry out injury & injustice if you do not, I beleive you would think him a man of a strange unsociable temper. M^{r} Hooks letters in several respects abounded too much with that humour which Hevelius & others complain of & therefore he may do well in time to consider whether after this new **{**pro**}**vocation I be much more bound (in doing him that justice he claims) to make an hon**{**ourable**}** <55av> mention of him in print, especially since this is the third time that he has given me trouble in this kind.

For your further satisfaction in this business, I beg the favour you would consult your books for a paper of mine entitled, <u>An Hypothesis explaining the properties of light</u>. It was dated Decemb. 7th 1675 & registred in your Book about Ian or Feb following. Not far from the beginning there is a Paragraph ending with these words. <u>And as the Earth so perhaps may the Sun imbibe this spirit copiously to conserve his shining & keep the Planets from receding further from him & they that will may also suppose that this spirit affords or carries thither the solary fewel & materiall principle if light: And that the vast ethereal spaces between us & the stars are for a sufficient repository for this food if the Sun & Planets. But this if the constitution if ethereal natures by the by</u>. In these & the foregoing words you have the common cause of gravity towards the earth Sun & all the Planets, & that by this cause the Planets are kept in their Orbs about the Sun. And this is all the

Philosophy Mr Hook pretends I had from his letters some years after, the duplicate proportion only excepted. The preceding words contein the cause of the phænomena of gravity as we find it on the surface of the earth without any regard to the various distances from the center: For at first I designed to write of nothing more. Afterwards, as my manuscript shews, I interlined the words above cited relating to the heavens, & in so short & transitory an interlined hint of things, the expression of the proportion may well be excused. But if you consider the nature of the Hypothesis you'l find that gravity decreases upward & can be no other from the superficies of the Planet then reciprocally duplicate of the distance from the center, but downwards that proportion does not hold. This was but an Hypothesis & so to be looked upon only as one of my guesses which I did not rely on: but it sufficiently explains to you why in considering the descent of a body down to the center I used not the duplicate proportion. In the small ascent & descent of projectiles above the earth the variation of gravity is so inconsiderable that Mathematicians neglect it. Hence the vulgar Hypothesis with them is uniform gravity. And why might not I as a Mathematician use it frequently without thinking on the philosophy of the heavens or beleiving it to be philosophically true?

For Mr Edmund Halley.

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Mr Newton of
June $\overline{20}^{o}$ 1686

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