No 2Sir John Frederick's replyMy dear Sir — I am not at all surprised that you shouldhave been somewhat startled by that passage in my addressin which I speak of M. Comte's argument in favor of thenebular hypothesis — still less that you should stand forwardin his defence until he can reply for himself. I should (of course)have sent him a copy of my speech had I known howto address him. Perhaps you know, & will in that case obligeme by forwarding to him one of two copies of the Athenaeumin which it appeared & accepting the other yourself. — they arecorrected for some very absurd errors of the press. One affectsthe subject in question.

As to that subject, I do not think you will find my criticism of M. Comte's position quite so easily disposed of as you appear to regard it. It was and is my deliberate opinion (formed it is True from the perusal of no other work of M. Comte's than his Phil. Pos.) that his whole reasoning as there stated is really vitiated by the fallacy, which (whether clearly or not) I have endeavored to expose, & I must be met by much stronger arguments (excuse the expression) than those you adduce, to drive me from that persuasion.

If, in some other work which I have not read, M. Comte have gone fairly into the formidable problem of the cooling and shrinking process of the Nebulous hypothesis —— if he have shown, <u>without making any arbitrary assumptions</u>, or superadding any additional hypothesis, that, as a necessary consequence from the shrinking of dimension & rearrangement of parts resulting

from the abstraction of heart, the period of rotation of the sun's surface on its axis at every instant during the shrinkage or at least at those instants when the planets were detached, must have been proportional to the $\frac{3}{2}$ power of its equatorial diameter at that or those instants — if he have done so, I at once admit, he has <u>proved</u> the nebulous hypothesis, & must rank with Newton as a discoverer and above Laplace and Lagrange as an analyst. But that is quite another case; & for that case (improbable, nay impossible as I conceive it to be in the present state of physical knowledge) I have expressly provided by the expression in my note appended to the passage in my address. —— "if his fundamental principle be really what he states" (viz. a mere combination of $\frac{V^2}{R}$ with Newton's $\frac{M}{R^2}$). But it is to me inconceivable that if he <u>had</u> gone into or even made any tolerable attempt at the dynamical problem in question leading him to each conclusion, he should have no understated, or rather so completely misstated his case, as on that supposition he must have done in the Cours de Ph. Pos.

Reterning to your own work ---- I hope you will excuse me if I remark (& the remark is in no way in compatible with the general high opinion I have formed & expressed of it in a philosophical point of view) that I regard as the least felicitous portions of it, those in which points of physical science & mathematics are touched upon. I should have no objection if you received it, to specify some particular instances which have occurred to me inter legendum to which this remark applies provided always that I were distinctly understood as only pointing them out for your own reconsideration, & not as holding myself obliged to defend, or even to explain my objections against them should I be so unfortunate as to state them obscurely — a thing for which I really have not time at my disposal. It was at one time my intention to have reviewed your book in the same sort of spirit that I did Whewell's (i.e. pointing out what I regarded as its defects with the same freedom as it merits) but want of time prevented me. Now I cannot but fancy that it must be useful to an author of a philosophical work to know what parts a <u>possible</u> reviewer <u>would</u> have raised objections to.

I remain Dear Sir yours very truly J. F. W. Herschel 10 July 1845.