35.

EXPLANATION OF THE COINCIDENCE OF A THEOREM GIVEN BY Mr SYLVESTER IN THE DECEMBER NUMBER OF THIS JOURNAL, WITH ONE STATED BY PROFESSOR DONKIN IN THE JUNE NUMBER OF THE SAME.

I WISH to state, without loss of time, that in the theorem given by me * for the composition of two successive rotations about different axes, I have been anticipated by Prof. Donkin in the June Number of your *Journal*.

To my shame I must confess, that, although an occasional contributor to, I am not invariably a constant reader of your valuable miscellany, otherwise I should not have introduced the theorem in question without due acknowledgment of Professor Donkin's claims to whatever merit may attach to the priority of publication. The fact is, that I made out the theorem for myself nine years ago, and had some communication on the subject with Professor De Morgan, who was then writing the seventeenth chapter of his *Differential Calculus*. A recent conversation with this gentleman has brought back to my mind a vivid recollection of the course of that communication. I brought under Professor De Morgan's notice the analytical memoir of Sr Gabrio Pola on the subject in the Memoirs of the Italian Society of Modena, and satisfied myself of the existence of the single axis of displacement by compounding the two rotations in the manner given in my paper, which, for the case of two axes fixed in space, is the same as Professor Donkin's, and for two axes fixed in the rotating body is materially, although not formally the same.

It then occurred to me that a more simple demonstration ought to be deducible from the possibility of always *finding* the point on a sphere, by revolution about which, as a pole, one equal arc could actually be shown to be transportable into the place of another. But in proceeding to work out this idea I fell into a remarkable blunder, in which I have since been followed by more than one able friend to whom I have proposed the question. The

[* p. 158 above.]

blunder was of this kind:—Two arcs have to be drawn, bisecting at right angles the arcs joining the extremities of two equal arcs; the point of inter-section of the two bisecting arcs *must* in all cases fall outside the quadrilateral formed by the equal and joining arcs. I supposed it to fall inside. There appears to be a fatal tendency to do so in all who take the subject in hand. In consequence of this error, the cause of which I did not at the moment perceive, I was driven to deny and admit in one breath the same proposition. Mr De Morgan sent me the correct proof after this method (the same as that given by him at page 489 of his *Calculus*), I am inclined to think after I had myself detected my error; but of this I cannot feel certain.

This is the method alluded to by me in the words "it is right to bear in mind, &c.," at the time of writing which all recollection of the same thing having been published by Mr De Morgan had vanished from my memory.

The proof of the triangle of rotations is so simple, that, as Professor Donkin states (in a letter which he has done me the favour of addressing me on the subject) was the case with himself, I thought it incredible that it should not have appeared in some elementary work, and I was therefore at no pains to publish it as my own; nor should I have written at all on the subject, had it not been for the surprise occasioned to my mind by falling in with Professor Stokes's article in the *Cambridge and Dublin Mathematical Journal,* to demonstrate the existence of an instantaneous axis, which proceeds in apparent unconsciousness of the so simply demonstrable law, that any number of rotations of any kind (and therefore those that take place in an instant of time) are representable by a single rotation about a single axis. I shall feel obliged by the early insertion of this explanation, more in justice to myself than to Professor Donkin, whose high and worthily earned reputation, not to speak of the disinterested love of truth for its own sake, apart from personal considerations, which animates the labours of the genuine votary of science, must make him indifferent to whatever credit might be supposed to result from the first authorship or publication of the very simple (however important) theorem in question.