1.

ON THE ROTATION OF A RIGID BODY.

[Three letters to *Nature*, Vol. I. (1870), pp. 482, 532, 582.]

The Motion of a Free rotating Body.

I SHALL feel obliged if, through the medium of your widely-circulated journal, you will allow me to point out an extraordinary mistake into which M. Radau has fallen, in a memoir inserted in the *Annales Scientifiques de l'École Normale Supérieure*, tom. VI. 1869, in which he criticises certain of my conclusions about the representation of the motion of a free rotating body contained in a paper published by me in the *Philosophical Transactions* for 1866 *. In his preamble, M. Radau says, speaking of the theory of rotation in connection with the names of Poinsot, Rueb, Jacobi, and Richelot:— "Tout récemment M. Sylvester a essayé d'appliquer au même sujet des considérations nouvelles qui l'ont conduite à des résultats intéressants, à côté d'autres dont l'exactitude peut être contestée."

Later on in his memoir M. Radau points out, and accompanies with very biting (albeit toothless) criticism, the nature of his objection, which is, in short, that I suppose Poinsot's ellipsoid, under the influence of an original impulse, to roll without slipping by virtue of its friction against the plane with which it is in contact. My answer is, that of course I do. And why not? when I suppose the plane "indefinitely rough" (see p. 761 of *Philosophical Transactions*, 1866 †), and have actually determined the friction and pressure at each point of the motion, so that by solving a maximum and minimum problem of one variable, the extreme value of the ratio of one of these forces to the other, or if we please to say so, the limiting angle of friction, or, in other words, the necessary degree of roughness of the plane, may be analytically determined for every given case. M. Radau falls into the school-boy blunder of making the *ratio between the friction and pressure constant throughout the motion*, confounding the actual friction with its limiting maximum value! It is, indeed, surprising that such a perversion

[* Vol. II. of this Reprint, p. 577.] [† *ibid.* p. 582.]

of the facts of the case should have found insertion in a serious journal, such as that published by the École Normale Supérieure, and I might fairly have expected from M. Radau the courtesy habitual with his adopted countrymen, of applying to me for information on anything in my paper which might have appeared to him obscure or erroneous, before rushing into print with such a *mare's nest*.

But out of evil cometh good. M. Radau says:—"Mais M. Sylvester va plus loin; il pense que le problème pourrait se résoudre par l'observation directe du mouvement d'un ellipsoide matériel tournant sur un plan fixe en même temps qu'il tournerait autour de son centre également fixe. On ne se figure pas facilement par quel artifice on fixerait le centre d'un ellipsoide matériel."

In a future number of your esteemed journal (as time at present fails me) I propose to show how, by the simplest contrivance in the world, a downright material top of ellipsoidal form may be actually made to roll, with its centre fixed, on a fixed plane and so exhibit to the eye the surprising spectacle of a motion precisely identical *in time*, as well as in its successive displacements of *position*, with that of a body, turning round a fixed centre, but otherwise absolutely unconstrained.

This mode of representation, which flashed upon my mind almost instantaneously when my eye first lighted upon M. Radau's objections, is the compensating good to the evil of being made the victim (to the temporary disturbance of my beloved tranquillity) of so hasty and futile a criticism as has been allowed insertion in the "Scientific Annals" of so great an institution as the École Normale of Paris.

The *bureau de rédaction* must surely have been nodding when they allowed such observations, so easily refuted by turning to the original memoir, to pass unchallenged. It was only within the last few days that I received M. Radau's paper.

Rotation of a Rigid Body.

My previous communication about the rotating ellipsoid to this journal, has attracted the attention of M. Radau. "One touch of *Nature* makes the whole world kin." In a note addressed to me full of true dignity, this gentleman has made much more than sufficient reparation for his previous trifling act of inadvertence, and states that to his great regret he had misunderstood my meaning, in the passage of my memoir in question, and that "sa critique n'est pas fondée." I, on my part, deeply lament the unnecessary tone of acerbity in which my reference to this criticism was couched, and wish I could recall every ungracious expression which it contains. "When I spoke that, I was ill-tempered too."

I will pass over this, to me, painful topic, to say two or three words on the mode in which the rotating ellipsoid may be supposed to roll or *wobble* on a rough plane, with its centre fixed. My solution may remind the reader of Columbus' mode of supporting an egg on its point—or, rather, of a fairer mode which Columbus might have employed, and which would not have necessitated the breaking of the shell, namely, by resting the blade of a knife or rough plate on the upper end of his egg.

So, to make an ellipsoidal or spheroidal top roll, with its centre fixed—say, upon a rough horizontal plane—imagine a second horizontal plane in contact with the upper portion of its surface; then the line joining the two points of contact will pass through the centre of the top. We may conceive a slight perforation in either or each plane at its initial point of contact with the top, and a screw wire introduced through this, and inserted into a female screw in the body to be set rolling (a mode of spinning which Sir C. Wheatstone recommends as the most elegant in any case, and in this case evidently the most eligible). On withdrawing the wire with a jerk, the top may be set in motion about its centre, in such a direction as to remain in contact with the two planes, and if these be sufficiently rough the motion will eventually be reduced to one of pure rolling between them, the axis (that is, the line joining the two points of contact), continually shifting, but the centre remaining absolutely stationary: for, vertical motion this point cannot have, so long as the top continues to touch both planes, and any slight horizontal motion (if it should chance to take on such at the outset) would be checked and ultimately destroyed by the friction, which would also keep the two points of contact stationary (like the single point of contact of a wheel rolling on a rail), in each successive atom of time. Thus the motion upon the lower plane would in the end be precisely the same as if the upper plane were withdrawn, and the centre of the top kept fixed by some mechanical adjustment. If the spin were not sufficiently vigorous, after a time the rolling top might quit the upper plane, and of course sooner or later by the diminution of the *vis-viva* due to adhesion, resistance of the air, imperfection or deformation of the surfaces, and other disturbing causes, this would take place, but abstracting from these circumstances the principal axes of the spheroidal or ellipsoidal top would move precisely in place and time like the "axes of spontaneous rotation" of any free body of which the top was the "Kinematic Exponent."

I do not pretend to offer an opinion what materials for the planes and rolling body (ground glass and ebony or roughened ebonite have been suggested to me) it would be best to employ, or whether the "wobbling top" could easily be made to exhibit its evolutions. It is enough for a non-effective, unpractical man (as unfortunately I must confess to being) to have shown that there is no intrinsic impossibility in the execution of the conception.

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James Joseph Sylvester
Excerpt
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With regard to the friction and pressure: if W be the weight of the body, F and P the friction and pressure in the case of a single plane (the values of which are set out in my memoir, pp. 764—766, *Philosophical Transactions*, 1866*), it may easily be proved that eventually the friction at each point of contact will be $\frac{F}{2}$, the pressure upwards at the lower point $\frac{P+W}{2}$, and downwards at the upper one $\frac{P-W}{2}$, so that if P should become equal to W the top would quit the upper plane and the experiment come to an end. At p. 766 of my memoir the factor $\sqrt{M\Lambda}$ has accidently dropped out of the expression for P which I mention here, in case any one should feel inclined to consult the memoir in consequence of this note. Mr Ferrers has taken up my investigations, and given more compendious expressions than mine for F and P; with the aid of these it would probably be not difficult to determine the maximum value of $\frac{F}{P}$ so as to assign the necessary degree of roughness of the confining planes, and also to ascertain under what circumstances $P-W$ would become zero, but I do not feel sufficient interest in the question, nor have I the courage to undertake these calculations with the complicated forms of P and F contained in my memoir. Mr Ferrers' results are contained in a memoir ordered to be printed in the *Philosophical Transactions*, and will shortly appear.

In my memoir will be found an exact kinematical method of reckoning the time of rotation by Poinsot's ellipsoid when the lower surface is made to roll on one fixed plane at the same time that its upper surface is sharpened off in a particular way (therein described) so as to roll upon a parallel plane which turns round a fixed axis; this upper plane is compelled to turn by the friction, and acts the part of a moveable dial in marking the time of the free body imaginarily associated with the ellipsoid. I have also shown there that the motion of any free body about a fixed centre may be regarded as compounded of a uniform motion of rotation and the motion of a disc, or, if one pleases, a pair of mutually bisecting cross-wires left to turn freely about their centre. But I fear that *Nature*, used to a more succulent diet, has had as much as it can bear upon so dry a topic, and, although having more to say, deem it wiser to bring these remarks to an end.

An after-dinner experiment.

Suppose in the experiment of an ellipsoid or spheroid, referred to in my last letter, rolling between two parallel horizontal planes, we were to scratch on the rolling body the two equal similar and opposite closed curves (the *polhods* so-called), traced upon it by the successive axes of instantaneous rotation; and suppose, further, that we were to cut away the two extreme

[* Vol. II., above, pp. 585, 587.]

segments marked off by those tracings, retaining only the barrel or middle portion, and were then to make this barrel roll under the action of friction upon its bounding curved edges between the two fixed planes as before, or more generally, imagine a body of any form whatever bounded by and rolling under the action of friction upon these two edges between two parallel fixed planes; it is easy to see that, provided the centre of gravity and direction of the principal axis be not displaced, the law of the motion will depend only on the relative values of the principal moments of inertia of the body so rolling, in comparison with the relative values of the axes of the ellipsoid or spheroid to which the *polhods* or rolling edges appertain; and consequently, that, when a certain condition is satisfied between these two sets of ratios, the motion will be similar in all respects to that of a free body about its centre of gravity.

That condition (as shown in my memoir in the *Philosophical Transactions**) is, that the nine-membered determinant formed by the principal moments of inertia of the rolling body, the inverse squares and the inverse fourth powers of the axes of the ellipsoid or spheroid shall be equal to zero—a condition manifestly satisfied in the case of the spheroid, provided that two out of the three principal moments of inertia of the rolling solid are equal to one another.

My friend Mr Froude, the well-known hydraulic engineer, with his wonted sagacity, lately drew my attention to the familiar experiment of making a wine-glass spin round and round on a table or table-cloth upon its base in a circle without slipping, believing that this phenomenon must have some connection with the motion referred to in my preceding letter to *Nature*: an intuitive anticipation perfectly well-founded on fact; for we need only to prevent the initial tendency of the centre of gravity to rise by pressing with a second fixed plane (say a rough plate or book-cover) on the top of the wine-glass, and we shall have an excellent representation of the free motion about their centre of gravity of that class of solids which have, so to say, a natural momental axis, that is (in the language of the schools) two of their principal moments of inertia equal. For greater brevity let me call solids of this class uniaxal solids. I suppose that the centre of gravity of the glass is midway between the top and bottom, and that the periphery of the base and of the rims are circles of equal radius. These circles will then correspond to *polhods* of a spheroid, conditioned by the angular magnitude and dip of the spinning glass; to determine from which two elements the ratio of the axes of the originally supposed but now superseded representative spheroid is a simple problem in conic sections; this being ascertained, the proportional values of the moments of inertia of the represented solid may be immediately inferred. The wine-glass

[* Vol. II., above, p. 583.]

itself belonging to the class of uniaxal bodies, the condition that ought to connect its moments of inertia with the axis of the representative spheroid (in order that the motion may proceed *pari passu* with that of a free body) is necessarily satisfied.

The conclusion which I draw from what precedes is briefly this—that a wine-glass equally wide at top and bottom, and with its centre of gravity midway down, spinning round upon its base and rim in an inclined position between two rough but level fixed horizontal surfaces, yields, so long as its *vis-viva* remains sensibly unaffected by disturbing causes, a perfect representation, both in space and time, of the motion of a free uniaxal solid, as for example, a prolate or oblate spheroid, or a square or equilateral prism or pyramid about its centre of gravity, and conversely that every possible free motion about its centre of gravity of every such solid admits of being so represented.

To revert for an instant to the general question of the representative rolling ellipsoid, I think it must be admitted that the addition of the time element to the theory and the substitution of a second fixed plane in lieu of a fixed centre, considerably enhance the value and give an unexpected roundness and completeness to Poinsot's image of the free motion of rotation of a rigid body, of which so much and not altogether undeservedly has been made. From an idea or shadow Poinsot's representation has now become a corporeal fact and reality, as if, so to say, Ixion's cloud, in a moment of fruition, had substantified into a living Juno. I heard the late Professor Donkin, of revered and ever-to-be-cherished memory, state that when as a referee of the Royal Society he first took in hand my paper on rotation, he did so with a conviction that all had already been said that could be said on the subject, and that it was a closed question; but that when he laid down the memoir he saw reason to change his opinion. I owe my thanks to M. Radau and the editors of the Annals of the École Normale Supérieure for having been at the pains to disentomb the little-known conclusions therein contained from their honourable place of sepulture in the *Philosophical Transactions.*

2.

ON RECENT DISCOVERIES IN MECHANICAL CONVERSION OF MOTION.

[*Proceedings of the Royal Institution of Great Britain*, VII. (1873—75), pp. 179—198. Also *La Revue Scientifique*, 1874—75, pp. 490—498, and *Van Nostrand's Engineering Magazine* (New York) XII. (1875), pp. 313—321.]

THE speaker stated that the subject he proposed to bring under the notice of the meeting related mainly to the discovery of a perfect parallel motion,—that is to say, of a mode of producing motion in a straight line by a system of pure link-work without the aid of grooves or wheel-work, or any other means of constraint than that due to fixed centres, and joints for attaching or connecting rigid bars. This important discovery was made by M. Peaucellier, an officer of Engineers in the French army*—and first published by him, in the form of a question, in the *Annales de Mathématique* in the year 1864, and subsequently formed the subject of two communications to the *Société Philomathique* of Paris by Captain Manheim, but seems not to have received the attention it deserved from that learned body, and may be said to have passed into oblivion; so much so, that when rediscovered by a young student of the University of St Petersburg, of the name of Lipkin, several years subsequently, the discovery was attributed to Lipkin instead of to Peaucellier even in works published in the French language, and so recently as 1873 by M. Colignan, in his *Traité de Cinématique*. The eminent Professor Tchebicheff had long occupied himself with the question, but with less than his usual success in overcoming difficulties insuperable to the rest of the world. Lipkin was a student in his class, and may thus have had his attention turned to the question; at all events, Professor Tchebicheff's warm interest in the subject was displayed by his bringing Lipkin's name before the Russian Government, and securing for him a substantial reward for his

* Now Colonel Peaucellier, and in command of the fortress of Toul; at the time of his discovery lieutenant and officier d'ordonnance on the staff of the "illustrious Marshal Niel."

supposed original discovery. Before Peaucellier's time all so-called parallel motions were imperfect, and gave merely approximate rectilinear motion*; in substance they will be without exception found to be merely modifications of Watt's original construction, and to depend on the motion of a point in, or rigidly connected with, a bar joining the extremities of two other bars rotating round fixed centres, which may be described briefly as three-bar motion. Peaucellier's exact parallel motion depends on a link-work of seven

* The late lamented Professor Rankine, in his treatise on Millwork, and elsewhere, mentions a so-called "exact parallel motion," the invention of which he dubiously assigns to Mr Scott Russell. In its *exact* form this is no parallel motion at all, for it works by means of a slide, and in its modified form it ceases to be *exact*, the motion produced being no longer truly rectilinear.

Mr Kaulbach, a mechanical draughtsman, resident in London, has shown the speaker a sketch of a very ingenious *quasi*-parallel motion, which he took the first steps to patent a year or two ago, but has not thought it worth his while to proceed with further. Its principle depends upon finding a curve made to rotate about a fixed point, and enjoying the property that the tangent to each point of it, as that point passes a given vertical line, shall take up a horizontal position. A piston-rod is guided in the direction of such vertical line, and the beam, which always presses on a friction wheel attached to the rod, is so shaped in its outward contour as to satisfy the above condition; the consequence is that the reaction on the piston-rod can only take effect vertically, that is, in the direction of its motion, and no lateral pressure is produced.

Peaucellier's invention effects the perfect conversion of circular into linear motion. An easy practical deduction from this is the conversion of spherical into plane motion, by aid of universal joints and other familiar modes of effecting free motion in space, of a shaft about a fixed point or round another shaft. The announcement of these facts has occasioned many persons unacquainted with the technical language of mechanism to suppose that the discovery of Peaucellier is connected with the quadrature of the circle or cubature of the sphere, and led to the idea that the speaker was in possession of some secret for flattening spheres and turning circles into right lines. Such a misconception was one (as indeed the wide extent of its prevalence demonstrates) quite likely to occur even to intelligent persons untrained in mathematical science. Technical names are a frequent occasion of traps to the uninitiated. A lady present at one of Mr Norman Lockyer's course of lectures on Spectral Analysis, near the close of it was overheard inquiring with some anxiety as to "when the spectres might be expected to make their appearance." Names are of course all-important to the progress of thought, and the invention of a really good name, of which the want, not previously perceived, is recognized, when supplied, as having ought to be felt, is entitled to rank on a level in importance with the discovery of a new scientific theory. Imagine *plane*, *straight*, *circle*, and you are potentially a geometer. Think the meaning of the one word *Syzygy*, and the logic of algebra has become part of your being. But, on the other hand, there are cases where over-naming does harm. The speaker has no doubt that if reading music on the piano with the fingers were taught without the intervention of learning the names of the notes, twice the velocity of execution (and quick reading is here the *sine-quâ-non* for the existence of every other kind of excellence) might be acquired in half the time required under the present system. The names of the notes of course would have to be learned at a later stage as a medium for discourse; but they should not be used as a vehicle for obtaining command of digitation, as such use amounts to throwing upon the brain the labour of going through two steps when one would suffice, and the passage of a direct nervous current from the eye to the touch in the act of reading, even at an advanced stage, becomes by force of habit interrupted and diverted into a broken channel. The new method for learning to read on the pianoforte here suggested may be distinguished as the abnominal or undenominational or tactile method. The writer is prepared to show in detail how it can be carried out in practice.

bars moving like Watt's, and the other imperfect parallel motions of the same class, round two fixed centres*.

To understand the principle of Peaucellier's link-work, it is convenient to consider previously certain properties of a linkage† (to coin a new and useful

* The perfect parallel motion of Peaucellier looks so simple and moves so easily that people who see it at work almost universally express astonishment that it waited so long to be discovered. The idea of the facility of the result by a natural mental illusion gets transferred to the process of conception, as if a healthy babe were to be accepted as proof of an easy act of parturition. No impression can be more erroneous. The speaker, on the contrary, the more he reflects upon the problem that was to be solved, and the nature of the solution (essentially a process of transformation operating on polar co-ordinates), wonders the more that it was ever found out, and can see no reason why it should have been discovered for a hundred years to come. Viewed *à priori* there was nothing to lead up to it. It bears not the remotest analogy (except in the fact of a double centring) to Watt's parallel motion or any of its progeny. In the three-bar motion the two fixed points are so to say one as good as the other, there is no distinction to be drawn between them; whereas the two fixed centres (hereafter designated as the fulcrum and pivot) in Peaucellier's seven-bar arrangement are absolutely dissimilar in position and function. Peaucellier's apparatus naturally resolves itself into a cell and a spare link; no such decomposition presents itself in the three-bar motion. Again, looking at the matter *à posteriori*, it occurs to many well-grounded mathematicians to suppose that, as the most general motion of a link-work of seven or any number of bars for each possible mode of conjunction and centring must be capable of being expressed by a general algebraical equation, the particular combination for rectilinear motion, when such motion is possible, ought to be contained therein and inferrible therefrom by studying under what conditions the characteristic of the general equation can degenerate into a power of a linear function or, as might perhaps happen (and would be sufficient if it did), into such power multiplied by a function incapable of changing its sign. But the answer to this is that *practically* there could be little or no hope of ever obtaining the general equation. In one-bar motion the general curve (that is, a circle) is of the 2nd order; in three-bar motion, as is well known, of the 6th order; very likely, therefore, in five-bar motion it would be of the 24th order at least; and in seven-bar motion, of the 120th order at least. The equation or system of equations of the 120th order, supposed to be applicable to seven-bar motion, one could hardly dream of obtaining, or of being able to manipulate if obtained. Written out at full length in a handwriting of moderate size, the area of a very large room might be insufficient to contain the whole of its terms, which would consist of 7381 groups, and might be tens or hundreds of thousands in number. No; it must either have been fallen upon in a chance or experimental way, and subsequently verified theoretically, or else hit off in some sudden glow of insight akin to but of a much intenser degree of illumination than that under which Professor Stokes was able to see that the hydrodynamical theorem of Lagrange before him, proved imperfectly by its author and others, and correctly but with great difficulty by Cauchy, was an immediate inference from the pretty nearly self-obvious fact of the complete time-derivatives of the three quantities to be proved *if ever then always zero*, being by virtue of the well-known general hydrodynamical equations, syzygetic functions of these quantities themselves. Dr Tchebicheff has informed the writer that he has succeeded in proving the non-existence of a five-bar link-work capable of producing a perfect parallel motion; he is probably therefore in possession of the actual numerical order of the general equation or system of equations applicable to this case. It is not proved, and may not be true, that Peaucellier's is the only seven-bar link-work that will solve the problem of a perfect parallel motion. Who shall say whether there may not exist some other combination of seven bars in which the same or an analogous zig-zag symmetry to that which exists in the three-bar arrangement may reappear! This is a point which should not be allowed to remain subject to doubt.

† A link-work consists of an odd number of bars, a linkage of an even number. A linkage may be converted into a link-work *additively* by fixing one point of it as a fulcrum and attaching

word of general application), consisting of an arrangement of six links, obtained in the following manner:—first conceive a rhomb or diamond formed by four equal links joined to one another; and now suppose a pair of equal links to be joined on to two opposite angles of such figure and to each other. All six links are supposed to lie (and to be constrained by the nature of their attachments to remain) in the same plane. The point of junction of the last-named pair of links (which it will be found convenient to call the fulcrum), according as they are greater or smaller than the sides of the diamond, will lie outside or inside the diamond. The linkage consisting of the six links may be termed a positive *cell* in the one case and a negative *cell* in the other*. It is easily seen, as a geometrical necessity, that the fulcrum,

a second point disconnected from the first by a new link to another fulcrum, or *ablatively* by fixing two ends of a link, which may then be removed. When one point only of a linkage is fixed, any other point may be made to describe an arbitrary curve, but then the path of every other point becomes prescribed. In order for a combination of links to fulfil this so to say fatalistic condition, and to entitle it to the name of a linkage in the speaker's sense, which when greater precision is required may be distinguished as a *perfect* linkage, equivalent to the French *système de tiges à liaison complète*, a numerical relation must be satisfied between the number of links and the number of joints, namely, three times the number of links must be four greater than twice the number of joints. In applying this rule it must be understood that, if three links are jointed together, the junction counts for two joints; if four are jointed together, for three joints; and so on. A compass or a pair of scissors is the simplest kind of linkage; a set of lazy-tongs is another; a Peaucellier cell, subsequently described in the text, a third. If no three joints lie on the same link, the above numerical relation between joints and links may be stated in another form, namely, twice the number of joints is four greater than the number of links. But in applying the rule in this form all joints count alike as units, and for a simple compass the ends must be reckoned as joints.

* Mr Penrose, the eminent architect and surveyor to St Paul's Cathedral, the scientific expositor and elucidator in succession to Mr Pennethorne of the surprising law of curvilinearity in the temples of the Greeks, has put up a house-pump worked by a negative Peaucellier cell, to the great wonderment of the plumber employed, who could hardly believe his senses when he saw the sling attached to the piston-rod moving in a true vertical line, instead of wobbling as usual from side to side. There seems to be no reason why the perfect parallel motion should not be employed with equal advantage in the construction of ordinary water-closets. The author has been admitted to see the geometrical pump at work in Mr Penrose's kitchen at Wimbledon. A sister pump of the ordinary construction stands beside it. The former, although quite as compact as its neighbour, throws up a considerably larger head of water with the same sweep of the handle. Its elegance, and the frictionless ease with which it can be worked (beauty as usual the stamp and seal of perfection) have made it the pet of the household. Some circular steps outside St Paul's Cathedral very lately requiring repair, Mr Penrose employed a circulo-circularly-adjusted Peaucellier cell to cut out templets in zinc for the purpose. The radius of the steps is about 40 feet, but to the great comfort and delectation of his clerk of the works, they were able to operate with a radius of not more than 6 or 7 feet in length. General Sir H. James, R.E., lately gave a lecture on the subject at Southampton, and informs the writer that this has been the means of inducing a gentleman of fortune residing there, well known in the yachting world, to fit up a marine engine with a Peaucellier parallel motion to use on board a steam yacht.

A very good idea of the form and operation of a negative cell may be gained by putting together the fore-fingers and ring-fingers of the two hands, and placing one middle finger a little over the other so as to keep all six fingers in the same plane. The first Peaucellier cell constructed in this country was a positive one, made by the speaker's friend, the eminent musician