The Kepler Problem from Newton to Johann Bernoulli

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Invited Paper

Summary

NEWTON solved what was called afterwards for a short time "the direct KEPLER problem" ("le problème direct"): given a curve (*e.g.* an ellipse) and the center of attraction (*e.g.* the focus), what is the law of this attraction if KEPLER's second law holds?

The "problème inverse" (today: the "problème direct") was attacked systematically only later, first by JACOB HERMANN, then solved completely by JOHANN BERNOULLI in 1710 and following BERNOULLI by PIERRE VARIGNON. How did BERNOULLI solve the problem? What method did he use for this purpose and which of his accomplishments do we still follow today?

In the second part various questions connected to the first part are dealt with from the point of view, Conflict and Cooperation, suggested by J. VAN MAANEN to the participants of the Groningen conference.

To Clifford A. Truesdell,
"Notre Maître à tous!"

Galileo's principle

In the Terza Giornata of the *Discorsi*[1] GALILEO discusses first the motion at a constant speed, the "motus aequabilis", as he calls it, starting much in the classical, Euclidean way from four axioms. He presents and discusses systematically the kinematics, as we would say today.

But for treating the fall of a body he must proceed differently. He knows that it is a "naturally accelerated motion", a "motus naturaliter acceleratus". What does this mean? It means that the increase of the velocity is proportional to the time of fall

$$\Delta v \simeq \Delta t.$$

[1] GALILEO GALILEI, Discorsi e Dimostrazioni Matematiche intorno a due nuove scienze, Elsevier Leyden, 1638.

This definition is proposed by Salviati, GALILEO's spokesman in the dialogues, but only after a lengthy discussion, which brings them close to some of the very questions solved half a century later by infinitesimal analysis. The three participants in the dialogue accept eventually this definition, repeated by Sagredo in Latin:

> "We call the motion equally or uniformly accelerated, which, when one starts from rest, adds to its speed equal increments in equal times".

How GALILEO arrived at this "definition" — he is really selecting the acceleration that describes the fall of a body — whether empirically or, as the text seems to indicate, by reflection — an alternative that seems more natural to Simplicio is rejected by him as it leads to contradictions — is not important for our present purpose. What is important here, is that GALILEO lets Salviati reply immediately: "Once this definition is agreed, the author demands only **one principle** which he supposes to be true", namely: "I accept that the degrees of speed which a body acquires if it descends various inclined planes are the same only if the heights of these planes are identical".

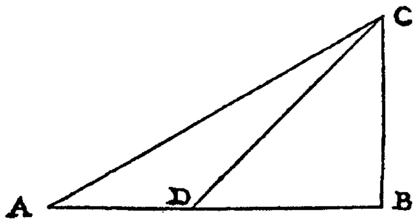

Fig. 1. Discorsi p. 166.

In other words, on whichever plane CA, CD, or CB, *etc.* the body descends, when it arrives on the plane AB, its speed will in all cases be the same. Of course, this holds for any horizontal plane parallel to AB.

The important, indeed fundamental point to note here, is that GALILEO realizes that he needs here a new, as we would say, a dynamical principle beyond EUCLID's geometrical ones, beyond his own which are kinematical, and also different from ARCHIMEDES' dynamical principle of the lever. All propositions that will be derived later on in the *Discorsi* will be derived from this principle alone.

On the other hand GALILEO is not quite satisfied with the principle: it seems that he wished to derive the principle itself from another, deeper and more basic one. In any case, as he says after a brief discussion, during which he indicates an elegant experimental confirmation with the pendulum, "For the time being we accept this as a postulate; its absolute truth will be then established for us when we see that other conclusions, erected upon this hypothesis, will answer to it and agree point by point with experience".

What does this principle say from today's point of view? It certainly does not speak of a force, as any scientist, trained today, would expect. Rather we may call it a "rudimentary energy principle", foreshadowing the notion of potential energy. Indeed, we know that for the kind of force which GALILEO tacitly presupposes, the kinetic energy of a body depends only on its height and is therefore the same, wherever the body lies on a horizontal plane. The same holds for the speed, since it is proportional to the square root of the kinetic energy.

Later scientists have laid other, stronger, more powerful foundations for mechanics, but we can already see here the ingredients necessary for such an undertaking: new notions, a fundamental hypothesis, formulated in a way adequate for deriving other results and, of course, confirmation of these results directly, and indirectly by experiments, that is by observations and measurements. To what extent GALILEO arrived at his theories by observation and experiment, I shall not discuss here.

Huygens

We can observe the progress which science made in the time between GALILEO and HUYGENS by comparing the *Discorsi* (1638) with HUYGENS's *Horologium Oscillatorium*[2] (1673). There in Part II the results are derived from a system of three hypotheses, in which we can easily recognise the precursors of NEWTON's three Axioms. GALILEO's hypothesis is now proved as proposition VI, just as GALILEO had expected such a possibility. It is likely, but we cannot be sure that JOHANN BERNOULLI had learned the theorem here rather than in the *Discorsi*.

Having noted this progress made by HUYGENS beyond GALILEO, I find it historically interesting to observe, that, when HUYGENS attacks in Part IV the more difficult problem of the physical pendulum, *i.e.* the oscillation of an extended, rigid body, his system of hypotheses used in part two is inadequate, and he too must now have recourse to a rudimentary energy principle!

Newton

NEWTON has derived KEPLER's famous three laws from his system of three "Axioms or Laws of Motion": how did he do this, and what exactly did he achieve?

In the second section of the first book of the *Principia*[3] NEWTON, from his first two axioms and the additional assumptions that a force attracts a body towards a center, proves his Theorema I: the motion of this body obeys KEPLER's second law of planetary motion. Namely, the area covered by the line

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[2] CHRISTIAAN HUYGENS, Horologium Oscillatorium, Muguet Paris, 1673.

[3] ISAAC NEWTON, Philosophiae Naturalis Principia Mathematica, London 1687; *cf.* especially Liber de Motu I Sects. II, III, VII.

connecting the body to the center is proportional to the time of the motion; it is equal during equal times.

Immediately after Theorema I NEWTON states and proves its inverse in his Theorema II: if a body moves in a plane on any curve such that a line from the body to a certain point covers areas that are proportional to the time of the motion, then this body is attracted to this point by a central force. Furthermore, since we know from our daily experience, that there are forces between all bodies, we may conclude from KEPLER's second law of planetary motion that all planets are attracted to the sun by a central force.

It is *this*, *i.e.* the inverse theorem, which NEWTON uses for deriving the results stated in the remainder of Sect. II and in Sect. III. Besides a few general theorems these are the following.

1. Proposition VII: if a body moves on a circle and if the center of force lies on the same circle, then this force of attraction is inversely proportional to the *fifth* power of the distance from the center to the body.
2. Proposition IX: if a body moves on a logarithmic spiral and the center of force is at the center of the spiral, then the force of attraction is inversely proportional to the *third* power of the distance from the center to the body.
3. Proposition X: if a body moves on an ellipse and the center of the force is at the center of the ellipse, then the force of attraction is *directly* proportional to the distance from the center to the body.

Finally the most important

Proposition XI, section III: if a body moves on an ellipse and the center of force is at one of the foci, then the force is inversely proportional to the *square* of the distance from the center to the body.

Applying this latter theorem (Proposition XI) to the elliptical orbit of a planet, NEWTON infers later, in the third book of the *Principia*, the form of his law of universal gravity. Because of "the dignity of this Problem" he will not simply treat the cases of the hyperbola and the parabola by a reduction to the same case but prove each case separately in Proposition XII and Proposition XIII respectively.

The reader trained by modern textbooks may ask: is that really all NEWTON does? Does he not derive the whole mechanics of central forces? There is, of course, an enormous number of results in the *Principia* after Sects. II and III, especially in Sects. VII and VIII. If we mean by this question the exploration of various given laws of a central force $F = F(r)$, it is indeed essentially all, except for the general theorems which I have mentioned and the various corollaries. But even so, this is a great achievement which should not be underrated. In fact it can hardly be overrated. Today we know that only three problems involving central forces are elementary: precisely those which NEWTON dealt with in Propositions IX to XIII. Given the question he asked, he could hardly solve any other, except that he also found the answer to the problem stated in Proposition VII! But the greatest accomplishment in all this is how he set up and ordered his whole chain of reasoning, from the Axioms and the additional special assumptions to the theorems and their proofs.

Before asking why he proceeded from the orbit to the law of force and not the other way around as we do today, a word may be said about the order in which the four cases are stated and solved. Does this order merely reflect the order in which NEWTON solved them? This is conceivable, but it is equally possible or even probable that NEWTON presented them artfully in increasing order of importance and in increasing order of the insight which he had gained. However that may be, the other question, why did NEWTON proceed starting from the orbit is the more important one, and there are essentially two reasons for that. The first is simply that history, as it presented itself to him, had proceeded this way. KEPLER's second law, proposed in 1609, was in NEWTON's days well established and had become common knowledge, while the idea of a central force of attraction was a new one, which few before NEWTON had even considered. Thus, starting from a system of "Laws of Motion", *i.e.* his axioms and from the hypothesis of a central force, NEWTON could now prove the form of this law:

$$F \simeq 1/r^2.$$

To the second reason I shall return later.

We must now ask, did NEWTON also solve the inverse problem ("le problème inverse"), *i.e.*, did he find in each of the four cases all solutions? Concerning the first two cases, Proposition VII and IX, the answer is obviously in the negative since there are clearly other solutions as well; for instance in both cases there can be a circular orbit around the center. For Proposition X, on the other hand, the answer is clearly affirmative; NEWTON states it explicitely in the first of the two subsequent corollaries, and in the second one, he proves that the period of the motion is the same for all orbits.

How is it with the all-important Proposition XI? JACOB HERMANN, JOHANN BERNOULLI and PIERRE VARIGNON thought that in this case NEWTON had not answered the question. When I lectured for the first time publicly on the *Principia*, I expressed the opinion that he had not, but then I received a letter from a former teacher writing that in fact NEWTON had done so. I followed his opinion in the printed version of the lecture, only to receive a sharp objection from an American historian, who insisted emphatically that NEWTON had not given such a proof. The reader can find both opinions defended in the *Studia Leibnitiana* of the Leibniz-Bernoulli Symposium in Basel 1987. The affirmative one is defended by the late E. J. AITON,[4] the opposite one by the late P. COSTABEL.[5] In both articles the reader can find much valuable information.

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[4] ERIC J. AITON, The Contributions of I. Newton, Joh. Bernoulli and J. Hermann to the Inverse Problem of Central Forces both in Studia Leibnitiana Sonderheft 17, F. Steiner Stuttgart, 1989.

[5] PIERRE COSTABEL, Courbure et Dynamique. Jean I Bernoulli correcteur de Huygens et de Newton.

I feel inclined today to believe that NEWTON gave a proof, even if it is indirect and rather well hidden. But I do not think that the proof itself is a matter of great historical importance. The importance of the achievement of HERMANN and BERNOULLI lies somewhere else as will be seen presently.

Jacob Hermann

JACOB HERMANN, the pupil of JACOB BERNOULLI and author of the Phoronomia, was the first to attack the inverse problem systematically, and he published his solution in 1710.[6]

Although an important accomplishment, his paper is less transparently written than that of JOHANN BERNOULLI[7] for which, however, it provided the stimulus. And with respect to one essential point at least only BERNOULLI's solution is complete. For this reason, and since this Symposium is dedicated to JOHANN BERNOULLI, I shall concentrate here entirely on BERNOULLI's work.

It may well be that in HERMANN's paper, NEWTON's equation was written for the first time in differential form.

Johann Bernoulli

In 1710 BERNOULLI, you will recall, had not only the mechanics of DESCARTES and of HUYGENS at his command, he had also penetrated deeply into the *Principia*, — perhaps more deeply than anyone on the Continent at that time. Thus, we may ask, how did he attack this problem, and where do we still follow him today, as for instance in our teaching? For answering these questions it seems best to look at the procedure most often used today. Doing so we must, however, recall that NEWTON's equation

$$m\delta_{ik}x^k = F_i \tag{1}$$

was written in this form, — *i.e.* in a Cartesian, space-fixed frame, in which we write it today, — only 40 years later.

Starting from eq. (1) most authors derive KEPLER's first law in essentially four steps.[8]

1. One multiplies eq. (1) by $\dot{x}$ reducing the force equation to the power equation

$$\frac{dT}{dt} = -\frac{dV}{dt} \tag{2}$$

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[6] JACOB HERMANN, Extrait d'une lettre de M. Hermann à M. Bernoulli datée de Padoüe le 12 Juillet 1710. Joh. B. Op. 85.

[7] JOHANN BERNOULLI, Extrait de la Réponse de M. Bernoulli à M. Herman, datée de Basle le 7. Octobre 1710. Both in Mémoires de l'Ac. Royale des Sciences, Boudot Paris 1710 (1712).

[8] ARNOLD SOMMERFELD, Mechanik, Geest und Portig Leipzig, 1947, *Cf.* p. 38. ff.

and by integrating it, into the energy equation

$$T + V = E \tag{3}$$

or, using polar coordinates and observing that the coordinate φ is cyclic

$$\frac{m}{2}(\dot{r}^2 + r^2\dot{\vartheta}^2) + V(r) = E \tag{4}$$

2. Using now the areal theorem, *i.e.* conservation of angular momentum

$$mr^2 d\vartheta/dt = L \tag{5}$$

one replaces the second kinetic term with r by the kinetic potential L/r.

3. Using once more the areal theorem in the form

$$\frac{d}{dt} = \frac{L}{mr^2}\frac{d}{d\vartheta},$$

one replaces the time as the independent variable by the angle. Thus one obtains the equation of the orbit

$$\frac{L}{2m}\left(\frac{r'^2}{r^4} + \frac{1}{r^2} + V(r)\right) = E \tag{6}$$

where $'$ denotes the differentiation with respect to ϑ.

4. Thus far every step was valid for any central force $F = F(r)$. For determining the form of the orbit, and indeed of all orbits, one must now assume one special law of force, or one special potential, namely $1/r$. In this case

$$r = 1/u, \quad r' = -u'/u$$

transforms the integral after quadratic completion into the standard form of an elementary integral. Its solutions are **all** conic sections: an ellipse, parabola or a hyperbola.

How does BERNOULLI proceed? He does not know NEWTON's axiom in modern form and so, instead, for the first step he uses NEWTON's Theorem XIII, Section VIII, as a Lemma, but simplifies its proof greatly by returning to GALILEO and HUYGENS. Considering two orbits passing through the same point and comparing the fall of the body along each of them, he applies GALILEO's principle to the infinitesimal triangle formed by the differentials on each orbit and the infinitesimal arc.

The principle says now that the speed at the end of both infinitesimal orbits is the same, and continuing, or, as we would say, integrating, he finds that the speed of the body is on each orbit the same on all circles around the center of attraction. This holds especially for the vertical fall. For this case it is easy to see, that if $\phi(r)$ is the force, the quantity $\int \phi(r)\,dr$ now appears in the equation. The vertical fall had been investigated by NEWTON in Section VII of the first book of the *Principia*, and BERNOULLI quotes him.

He then takes the other three steps in essentially the same way as we do today. The small differences concern merely the writing. Thus, for instance,

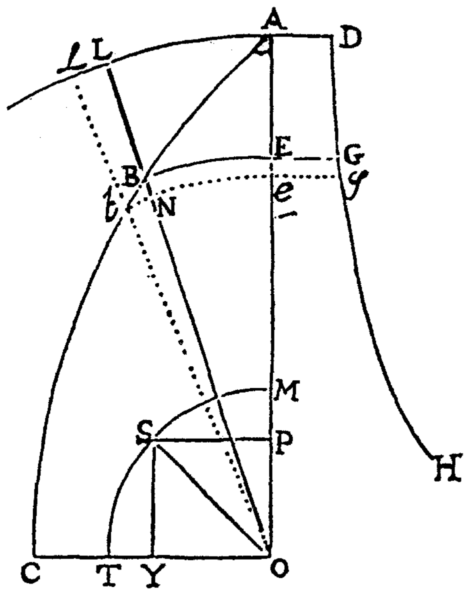

Fig. 2. Mém. Paris, p. 524.

throughout he uses Cartesian coordinates; instead of $r\,d\vartheta$ he writes $x\,dy - y\,dx$, *etc.*; he does not have a special symbol for the angle and for angular velocity, both introduced only later by EULER, but indicates an angle by the length of the arc of an arbitrarily chosen circle, divided by its radius, and he chooses the constants of integration somewhat differently from our modern procedure.

The formula obtained after the third step is

$$dz = aac\,dx\colon\ (abx^4 - x\textstyle\int \phi\,dx - aaccxx)^{1/2}$$

while we would write for the same

$$d\vartheta = L\,dr\colon\ (2Er^4 - 2V(r)r^4 - Lr^4)^{1/2}.$$

The main point to note here is that this expression is valid for *all* central forces. For **certain forms** of the law of force the equation can be integrated with the help of a known function be it an elementary, an elliptic or a transcendental one; for others the integral can at least be qualitatively discussed.

BERNOULLI does not pursue these possibilities; it cannot even be said that he had clearly grasped the potential, but the great progress due to HERMANN and BERNOULLI is now clear. NEWTON like HUYGENS earlier and JACOB BERNOULLI later had always started from the geometry of the problem, a curve, a surface or a streamline, *etc.* In contrast, here the dynamic concepts, the energy, the force, the angular momentum provide the starting point. And this starting point leads to much more powerful and general methods. In short, **Mechanics had now come into its own.** Geometry was still needed, but the required knowledge of it diminished with time, while Mechanics became more and more an independent discipline. For this reason the papers by HERMANN, BERNOULLI and VARIGNON mark a turning point.

BERNOULLI had reduced the problem of computing the orbit of a body which moves in the field of a central force to an integration. An equally big step was taken only later by the following generation, by his son DANIEL, by CLAIRAUT, EULER and D'ALEMBERT, namely the formulation of many-body problems. This, as we know today, is literally impossible if we try to start from the orbits. But starting from the forces it costs nothing to write down at least the dynamical equations, and often enough we can easily find some integrals such as the energy integral and apply approximation methods.

We can sum up the achievement of HERMANN and BERNOULLI by saying that they had opened up the door, if only a little bit, to analytical celestial mechanics.

Euler

I may be permitted to continue from JOHANN BERNOULLI's achievement to the work of the greatest of his disciples, EULER. If BERNOULLI was the first who penetrated deeply into the *Principia*, EULER was the first for whom Newtonian Mechanics was the motor which incited and drove him to his great discoveries.

Earlier, in the *Mechanica*[9] he had derived again, in his Propositions 80 and 81, after HERMANN, BERNOULLI, and VARIGNON, KEPLER's ellipse from NEWTON's law of force. However, we are surprised to see that his presentation is, for once, less transparent than that of his teacher.

The great turning point, the second one in this account, came in 1750. In that year he wrote his paper E 177, *Découverte d'un nouveau principe de Mécanique.*[10] Here we find, for the first time, NEWTON's second law formulated in the form we know and use it today, namely in a space fixed Cartesian coordinate system. The importance of this step was explained and underlined by TRUESDELL and recently by G. MALTESE.[11] At that time EULER believed, erroneously though, he had found the common basis for the entire mechanics. The

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[9] LEONHARD EULER, E 15 Mechanica Tomus I, St. Petersburg 1736, Op. Omnia Ser. II Vol. 1.

[10] LEONHARD EULER, E 177 Découverte d'un nouveau principe de mécanique, 1750 (1752) Op. Omnia Ser. II Vol. 5.

E 301 De motu corporis ad duo centra ... attracti, 1764 (1764) Op. Omnia Ser. II Vol 6.

E 328 De motu corporis ad duo centra ... attracti, 1765 (1767) Op. Omnia Ser II Vol. 6.

E 337 Problème. Un corps étant attiré en raison .... 1760 (1767) Op. Omnia Ser. II Vol. 6. I am grateful to D. O. MATHÚNA who drew my attention to these papers of EULER.

[11] GIULIO MALTESE, La Storia de "F = ma", Olschki Firenze, 1992.

immediate fruit was the definite foundation of Hydrodynamics through the equations which bear his name. Of Hydrodynamics we shall hear more in the second part of this talk. The great role played by celestial mechanics in EULER's thinking and in his work is well known, but the series of three papers which he devoted to the problem of a point moving according to NEWTON's law around two, fixed, centers of attraction may be mentioned here especially.[10] BERNOULLI is not mentioned by name, yet the papers testify to his influence on the former disciple even more than a decade after his death. It is noteworthy that these three papers have exerted a scientific influence on mechanics even today.

Conflict and Cooperation

I should like to add to this presentation of a historic development a few remarks concerning the official subject of this symposium. This gives me an opportunity to correct mistakes which I made myself some time ago by accepting what established authorities on JOHANN BERNOULLI had stated. One mistake had caused no more than the missing of an important point, but the other one can only be called a slander, and it is a serious one indeed. It also gives me an opportunity to say a few words about BERNOULLI's *Hydraulica* as Dr. VAN MAANEN had wished me to do.

JOHANN BERNOULLI's attitude towards JACOB HERMANN was not one of conflict, though, certainly one of competition; his attitude towards his friend PIERRE VARIGNON was one of collaboration. BERNOULLI was steeped in many conflicts indeed, and always very much emotionally so. Thus it might be useful to consider first two scientific topics where he reacted emotionally to scientific ideas and to ask whence these strong emotions?

We just saw one important achievement in BERNOULLI's receptivity to, and his contribution to the development of, Newtonian Mechanics. But there were ideas in NEWTON's *Mechanics* with which he was in deep conflict and quite emotionally so. The most important ones were NEWTON's "action at a distance" and the vacuum. Concerning both ideas we can see his motives in his *mémoire* for winning a prize in the year 1734. The Académie Royale des Sciences had asked the question: "Quelle est la cause physique de l'inclinaison des Plans des Orbites par rapport au plan de l'Equateur de la revolution du Soleil autour de son axe, et d'où vient que les inclinaisons de ces Orbites sont différentes entre elles?"

First we can see in BERNOULLI's mémoire[12] clearly that he was no "Cartesian" with respect to gravitation. He says, § VI,: "Le systéme des Tourbillons . . ., ne laisse pas d'être exposé aussi à de grandes objections: on sçait que la gravitation des Planetes vers le Soleil, attribuée à l'effet de la force centrifuge de la matiére du Tourbillon, ne devroit pas se faire directement au

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[12] JOHANN BERNOULLI, Essai d'une nouvelle Physique celeste, Recueil des pièces. . . Tome III, G. Martin et al. Paris, 1735. Joh. B. Op. CXLVI.

centre du Soleil, mais perpendiculairement vers l'axe du Tourbillon, de même que les corps graves sur la terre devroient avoir une tendance perpendiculaire à l'axe, & ne point tendre au centre de la terre".

So much for my first mistake. I have explained elsewhere why BERNOULLI still uses DESCARTES's vortex, and why, doing so, he went in the right direction. He turned the question into the more appropriate one: what determines the **present** shape of the solar system? Thus, his paper, together with the paper of his son, with whom he shared the prize, belongs to the prehistory of astrophysics.

But he fights NEWTON's action at a distance: why? After having insisted that NEWTON's attitude is moderate compared to the one of his followers, he writes, § IV,: "Or le choc se fait par pression; c'est donc une action dont il résulte un effet. Qui veut concevoir une action sans effet, il veut concevoir une chimére. ... ainsi je ne vois pas comment deux corps éloignés & en repos peuvent s'attirer mutuellement, *c.à.d.*, se mettre en mouvement d'eux mêmes. ... Vouloir recourir à la volonté immédiate de Dieu, ... seroit bannir les causes secondes de la Nature; il vaudroit autant dire que ... tout ce qui arrive dans l'univers, s'execute immediatement ... par la volonté divine".

Thus BERNOULLI, the "savant du siècle des lumières" speaks. DESCARTES and HUYGENS, through introducing mechanisms for which they had discovered laws, had freed science from the "qualités obscures", and one should avoid anything that might induce us to slip down again into obscurantism. He could not know that NEWTON was of the same opinion, and both, provided one frees oneself, as indeed EINSTEIN did, from too narrow an understanding of the notion of mechanism were proved right. It is a different story with the vacuum. First I must say that the vacuum was at that time often an emotional subject, as several contributions to a symposion in Wolfenbüttel, two years ago, showed, and it would be worthwhile to find out why.

Earlier here in Groningen BERNOULLI had directed a philosophical thesis by his student SCATO TRIP.[13] The main point of the thesis is that "body" and "space" are different only *modo* but not *in re* and that therefore the notion of a vacuum is a mistaken one. Leaving this slightly scholastic argumentation aside, we may say that BERNOULLI had apparently not understood that NEWTON's vacuum was more than just an empty Euclidean space. In the mémoire he says, § XIII,: "Qui dit corps, ne dit autre chose que ce qui est étendu, mobile & impénétrable; voilà tout ce que l'dée du corps doit renfermer; ... ." But according to NEWTON space-time carries the *vis insita* or *vis inertiae*, as he called it. Later EULER understood this point clearly. He added to BERNOULLI's three properties a fourth one, inertia, and he showed that this force singles out from all transformations those which we today call "Galileo transformations". That is why BERNOULLI, for performing what I called the first step, had to fall back on the energy principle of GALILEO and HUYGENS.

[13] SCATO TRIP, Disputatio Phil. de Vacuo, C. Barlinck-hof Groningen, 1705.

You will now see how this failure to understand NEWTON on this point indeed also impeded him in another question, in spite of the undoubted progress which he had achieved.

Johann and Daniel Bernoulli

Of the many conflicts and quarrels in which JOHANN BERNOULLI was party I will restrict myself to the most ugly one: the rivalry with his own son.

DANIEL BERNOULLI had written in St. Petersburg a first version of the *Hydrodynamica*, which he then revised in Basel. It should have appeared in 1734, but because of the usual difficulties it did so only in 1738.[14] At about the same time, the father published his *Hydraulica* indicating 1732 as the year of writing.[15] At that time the work certainly did not exist in the form in which it was printed, and therefore JOHANN has often been accused of fraud. His general boasting seemed to make the accusation plausible. Some went even so far as to accuse him of having plagiarized his son, and naively I myself repeated this accusation, an error for which I still feel sorry. Leaving aside for a moment the predating of the *Hydraulica*, I will deal first with the accusation of having plagiarized.

In fact, EULER's statement, printed in BERNOULLI's *Opera*, should have warned the historians. EULER says " ... your theory of water in motion following the true and genuine method, which you, most excellent Sir, did apply as the first and only one for treating problems of this kind adequately ... ." What does EULER mean by "the true and genuine method"? To answer the question, we must look at both books.

In the *Hydrodynamica* 12 of the 13 chapters are based on the energy principle ("balance of energy"); the last one is based on the momentum principle ("balance of momentum"). But we need only open the introduction of the *Hydraulica* and we see that JOHANN uses NEWTON's equation!

DANIEL's book is the more carefully written by far and also the more understandable of the two; I doubt that there are many more carefully written books in the whole history of physics. But JOHANN's, while containing, at least one serious mistake, was at the time the more advanced: **it introduced NEWTON's concept of force into Hydrodynamics**. This is what EULER meant by "the true and genuine method", which he was later to explore himself with such success. To quote TRUESDELL:" ... he had calculated the force acting on an infinitesimal element, ... ".[16] True, as G. MIKHAILOV observed, JOHANN, here too, has not

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[14] DANIEL BERNOULLI, Hydrodynamica, J. R. Dulsecker Strasbourg, 1738.

[15] JOHANN BERNOULLI, Hydraulica CP IX, 1737 (1744); X, 1738 (1747) — Joh. B. Op. CLXXXVI.

[16] CLIFFORD AMBROSE TRUESDELL, Editor's introduction to Euleri, Op. Omnia II 12, Part IV. p. XXIII ff.

fully grasped the notion of force, but TRUESDELL is right when he says, that the roots of EULER's hydrodynamics lie in JOHANN's rather than in DANIEL's work. Thus to speak of plagiarizing is absurd. I. SZABÓ has defended JOHANN in even stronger terms than TRUESDELL, and he showed that this accusation is, in fact, only of quite recent origin.[17] Ironically it originated in Basel from one whose very hero, and the favourite among all members of the family to boot, was JOHANN BERNOULLI.

The predating is a different story. To compete with a son shows always questionable wisdom and taste, and BERNOULLI chose a heavy-handed and miserable way to do so. But, however we judge his action, it is unlikely that he invented the date 1732 out of the blue; at any rate the burden to disprove him rests with us.

In fact, we know that during the period prior to 1734, when he wrote his great mémoire, he again studied hydrodynamical theories for explaining why the planets do not move in the same plane. His frequent references to his book "*La Manoeuvre des Vaisseaux*" testifies to this. I am glad that Mme. J. PEIFFER showed how important the ideas developed in the book were for him, and that the prize offered by the Académie incited him to work on them again. Thus I suggest that the date 1732, although unacceptable according to our norms, refers to results obtained by these studies and stored, probably, in a drawer.

I wish to conclude by stating an impression, the subjective character of which I am quite aware. We are presently preparing the plans for the edition of the various BERNOULLI correspondences. Together with Dr. NAGEL, who knows the letters better, I have now looked through all those of JOHANN I.

With respect to his character and personality, two impressions stand out. One is, I need not insist, his love for disputes and fights. But one must note that almost all of his fights were directed against peers or even persons of superior standing. After all, in his fight for LEIBNIZ against NEWTON and the English he was the one of the three, who was in the least exalted position, and therefore needed the greatest courage: the quatrain of VOLTAIRE, who was a great admirer of NEWTON, testifies to this.

The other strong impression I receive is his great dedication to his students. He spent much time on them and showed them freely and generously indeed what they needed to learn. He always recognized unhesitatingly and without reservation EULER's superiority. Indeed, that he should be the teacher of one of the greatest scientists and teachers ever, was the most fitting tribute which history possibly could pay to him.

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[17] ISZTVÁN SZABÓ, Geschichte der mechanischen Prinzipien, 2, erweiterte Auflage, Birkhäuser Basel, 1979 p. 171 ff.

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