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# FOUR LECTURES ON MATHEMATICS

DELIEVERED AT COLUMBIA UNIVERSITY
IN 1911

BY
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On the seventeenth day of December, nineteen hundred and four, Edward Dean Adams, of New York, established in Columbia University "The Ernest Kimpton Adams Fund for Physical Research" as a memorial to his son, Ernest Kempton Adams, who received the degrees of Electrical Engineering in 1897 and Master of Arts in 1898, and who devoted his life to scientific research. The income of this fund is, by the terms of the deed of gift, to be devoted to the maintenance of a research fellowship and to the publication and distribution of the results of scientific research on the part of the fellow. A generous interpretation of the terms of the deed on the part of Mr. Adams and of the Trustees of the University has made it possible to issue these lectures as a publication of the Ernest Kempton Adams Fund.

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# Preface

The "Saturday Morning Lectures" delievered by Professor Hadamard at Columbia University in the fall of 1911, on subjects that extend into both mathematics and physics, were taken down by Dr. A. N. Goldsmith of the College of the City of New York, and after revision by the author in 1914 are now published for the benefit of a wider audience. The author has requested that his thanks be expressed in this place to Dr. Goldsmith for writing out and revising the lectures, and to Professor Kasner of Columbia for reading the proofs.

# Contents

# Lecture I
# The Determination of Solutions of Linear Partial Differential Equations by Boundary Conditions

In this lecture we shall limit ourselves to the onsideration of linear partial differential equations of the second order.

It is natural that general solutions of these equations were first sought, but such solutions have proven to be capable of successful employment only in the case of ordinary differential equations. In the case of partial differential equations employed in connecction with physical problems, their use must be given up in most circumstances, for two reasons: first, it is in general impossible to get the general solution or general integral; and second, it is in general of no use even when it is obtained.

Our problem is to get a function which satisfies not only the differential equation but also other conditions as well; and for this the knowledge of the general integral may be and is very often quite insufficient. For instance, in spite of the fact that we have the general solution of Laplace's equation, this does not enable us to solve, without further and rather complicated calculations, ordinary problems depending on that equation such as that of electric distribution.

Each partial differential equation gives rise, therefore, not to one general problem, consisting in the investigation of all solutions altogether, but to a number of definite problems, each of them consisting in the reserach of one peculiar solution, defined, not by the differential equation alone, but by the system of that equation and some accessory data.

The question before us now is how these data may be chosen in order that the problem shall be "correctly set." But what do we mean by "correctly set"? Here we have to proceed by analogy.

In ordinary algebra, this term would be applied to problems in which the number of the conditions is equal to that of the unknowns. To those our present problems must be analogous. *In general*, correctly set problems in ordinary algebra are characterized by the fact of having soultions, and in a finite number. (We can even characterize them as having a unique solution if the problem is linear, which case corresponds to that of our present study.) Nevertheless, a difficulty arise on account of exceptional cases.

Let us consider a system of linear algebraic equations:

$$a_1x_1 + \ldots\ldots + a_nx_n = b_1 \ldots \tag{1.1}$$

the number $n$ of these equations being precisely equal to the number of unknowns. If the determinant formed by the coefficients of these equations is not zero, the problem has only one solution. If the determinant is zero, the problem is in general impossible. At a first glance, this makes our aforesaid criterion ineffective, for there seems to be no difference between that case and that which the number of equations is greater than that of the unknowns, where impossibility also generally exists. (Geometrically speaking, two straight lines in a plane do not meet if they are parallel, and in that they resemble two straight lines given arbitrarily in three-dimensional space.) The difference between the two cases appears if we chose the $b$'s (second members of the equation 1.1) properly; that is, in such manner that the system becomes again possible. If the number of equations were greater than $n$, the solution would (in general) again be unique; but, if those two numbers are equal, the problem when ceasing to be impossible, proves to be *indeterminate*.

Things occur in the same way for every problem in algebra. For instance, the three equations

$$\begin{aligned} f(x, y, z) &= a \\ g(x, y, z) &= b \\ f + g &= c \end{aligned}$$

between the three unknowns $x$, $y$, $z$, constitute an impossible system if $c$ is not equal to

$a+b$, but if $c$ equals $a+b$, that system is in general indeterminate.

Moreover, this fact has been both extended and made precise by a most beautiful theorem due to Schoenflies.

Let

$$f(x,y,z) = X,\ \ g(x,y,z) = Y,\ \ h(x,y,z) = Z \tag{1.2}$$

be the equations of a space-transformation, the functions $f,\ g,\ h$ being continuous. Let us suppose that within a given sphere ($x^2+y^2+z^2=1,$ for instance), two points $(x,y,z)$ cannot give the same point $(X,Y,Z)$: in other words, that $f(x,y,z) = f(x',y',z'),\ \ g(x,y,z) = g(x',y',z') =\ \ h(x,y,z) = h(x',y',z')$ cannot be verified simultaneously within that sphere unless $x=x',\ \ y=y',\ \ z=z'$. Let $S$ denote the surface corresponding to the surface $s$ of the sphere; that is, the surface described by the point $(X,Y,Z)$ when $(x,y,z)$ described $s$. If in equation (3.2) we consider now $X,\ Y,\ Z$ as given and $x,\ y,\ z$ as unknown, our hypothesis obviously means that those equations cannot admit of more than one solution within $s$. Now *Schoenflies' theorem* says that *those equations will admit of a solution* for any $(X,Y,Z)$ that may be chosen within $S$. Of course the theorem holds for spaces of any number of dimensions. It is obvious that this theorem illustrates most clearly the aforessaid relation between the fact of the solution being *unique* and the fact that that solution necessarily exists.[1]

As said above, the theorem is in the first place remarkable for its great generality, as it implies concerning the functions $f,\ g,\ h$ no other hypothesis but that of continuity. But its significance is in reality much more extensive and covers also the functional field. I consider that its generalizations to that field cannot fail to appear in great number as a consequence of future discoveries. This remarkable importance will be my excuse for digressing, although the theorem in question is only indirectly related to our main subject. The general fact which it emphasizes and which we stated in the beginning, finds several applications in the questions reviewed in this lecture. It may be taken as a criterion whether a given linear problem is to be considered as analogous to the algebraic problems in which the number of equations is equal to the number of unknown. This well be the case always when the problem is possible and determinate and sometimes even when it is impossible, if it cannot cease (by further particularization of the data) to be impossible otherwise than by becoming

[1]We must note nevertheless, that in it the unique solution is opposed not only to solutions in infinite number (as above), but also to any more than one. For instance, the fact that $x^2 = X$ may have no solution in $x$, is, from the point of view of Schoenflies' theorem, in relation with the fact that for other values of $X$, it may have two solutions.

indeterminate.

Let us return to partial differential equations. Cauchy was the first to determine one solytion of a differential equation from initial conditions. For an ordinary equation such as $f(x, y, dy/dx, d^2y/dx^2) = 0$, we are given the values of $y$ and $dy/dx$ for a particular value of $x$. Cauchy extended that result to partial differential equations.

Let $F(u, x, y, z, \partial u/\partial x, \partial u/\partial y, \partial u/\partial z, \partial^2 u/\partial x^2, \dots) = 0$ be a given equation of the second order and let it be granted that we can solve it with respect to $\partial^2 u/\partial x^2$. Thus we obtain $(\partial^2 u/\partial x^2) + F_1 = 0$ where $F_1$ is a function of all the above quantities, except $\partial^2 u/\partial x^2$. Then Cauchy's problem arises by giving the values

$$u = \varphi(y, z), \ \frac{\partial u}{\partial x} = \psi(y, z) \tag{1.3}$$

of $u$ and $\partial u/\partial x$ for $x = 0$. (These data must be replaced by analogous data, if instead of the plane $x = 0$, we introduce another surface.) Indeed, under the above hypothesis concerning the possibility of solving the equation with respect to $\partial^2 u/\partial x^2$, and on the supposition that the functions $F_1$, $\phi$ and $\psi$ are holomorphic, Cauchy, and after him, Sophie Kowalevske, showed that in this case there is indeed one and only one solution. This solution can be expanded by Taylor's series in the form $u = u_0 + xu_1 + x^2 u_2 + \dots$ where $u_0, \ u_1, \dots$ can be calculated.

The above theorems are true for must equations arising in connection with physical problems, for example

$$\nabla^2 u = \frac{\partial^2 u}{\partial t^2}. \tag{E}$$

*But in general these theorems may be false.* This we shall realize if we consider Dirichlet's problem: to determine the solution of Laplace's equation

$$\nabla^2 u = \frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} = 0 \tag{e}$$

for points within a given volume when given its values at every point of the boundary surface $S$ of that volume.

It is a known fact that this problem is a correctly set one: it has one, and only one, solution. Therefore, this cannot be the case with Cauchy's problem, in which *both* $u$ and one of its derivatives are given at every point of $S$. If the first of these data is by itself (in conjuenction with the differential equation) sufficient to determine the unknown function, we have no right to introduce any *other* supplementary condition. How is it therefore that, by the demonstration of Sophie Kowalevska, the same problem with both data proves to be possible?

Two discrepancies appear between the sense of the question in one case and in the other: $(a)$ In the theorem of Sophie Kowalevska, $u$ has only to exist in the immediate neighborhood of the initial surface $S$. In Dirichlet's problem, it has to exist and to be well determined in the whole volume limited by $S$. We therefor require more in the latter case than in the former, and it might be thought that this is sufficient to resolve the apparent contradiction met with above.

In fact, however, this is not the case and we must also take account of the second discrepancy. $(b)$ The data, in the case of the Cauchy-Kowalevske demonstration, are, as we said, supposed to be analytic: the functions $\varphi,\ \psi$ (second members of (3.3)) considered as functions of $y,\ z$, are taken as given by convergent Taylor's expansions in the neighborhood of every point of the plane $x = 0$ in the region where the question is to be solved. Nothing of the kind is supposed in the study of Dirichlet's problem. Not even the existence of the first derivatives of $u$, corresponding to displacements on $S$, is postulated, and in some researches, certain discontinuities of these values are admitted. Both these circumstances play their role in the explanation of the difference between the two results discussed above.

That $(a)$ is one reason for that difference is evident, for of course, if a function is required to be harmonic (i.e. to admit everywhere derivatives and to verify Laplace's equation) within a sphere, its values and those of its normal derivative, may not together be chosen arbitrarily on the surface even if analytic.

To show that $(a)$ is not sufficient for the required explanation, let us take the geometric terms of the problem in the same way as Cauchy. We therefore suppose that, $u$ being defined by Laplace's equation, the accessory data given to determine it are the values of $u$ and $\partial u/\partial x$ on the plane $x = 0$, or, more exactly, on a certain portion $\Omega$ of that plane; $u$ will also not be required, now, to exist in the whole space; its domain of existence may be limited, for instance, to a certain distance, however small, from our plane $x = 0$ (in the environs of $\Omega$) provided that distance be finite and not infinitesimal.

Now under these conditions, in general such a function $u$ does *not* exist, if the data are not analytic and are chosen arbitrarily. One sees then a fact which never appeared as long as ordinary differential equations were alone concerned, namely, that the results are utterly different according as the analytic character of the data is postulated or not.

Of these two opposite results which is to be considered as giving us a more correct and adequate idea of the nature of things? I do not say as the true one, for of course each one is so under proper specifications.

Som mathematicians still incline to prefer the old point of view of Cauchy, one of their reasons being that, as known since Weierstrass, any function, analytic or not, can be replaced with any given approximation by an analytic one, (more precisely by a polynomial). Therefore the fact that a function belongs to one or the other of those two categories seems to them to be immaterial. I cannot agree with this point of view. That the thing is *not* immaterial, seems to me to follow directly from what we have just stated. And it cannot fail to be put in evidence if we think not only of the mere existence of the solution, but of its properties and the means of calculating it. If Cauchy's problem, for equation (e), ceases to be possible, as a rule, when the functions disgnated by $\varphi, \psi$ are not analytic, then every expression for the solution must depend essentially on that analyticity and especially upon the radii of convergence of the developments of $\varphi, \psi$. In other words, let us imagine that the functions $\varphi, \psi$ be replaced by other functions $\varphi_1, \psi_1$, the differencces $\varphi_1 - \varphi, \psi_1 - \psi$ being very small for every system of real values of $y, x$ within $\Omega$ (and perhaps also the differences of some derivatives being small). However slight the alteration may be it rigorously follows from the aforesaid theorem of Weierstrass, that the radii of convergence of the developments in power series (if existing at all) may and will be, in general, completely changed; so the calculations leading to the solution will necessarily be changed also.

If that solution itself should undergo but a slight change, this would at once show us that these methods of calculation ought to be of quite an artificial nature, masking completely the qualitative properties of the required result.[2] But in fact, it is clear that matters are not as just assumed above. The alteration $u_1 - u$ produced on the values of $u$ by our slight modification of $\varphi, \psi$ will be genneraly important and often complete, as is evident[3] by the

[2]The solution by development in Taylor's series is, in general, for problems of that kind, the only one which can be given. I know but one exception, which is Schwarz's method for minimal surfaces, when a curve on the surface and the corresponding succession of tangent planes are given. This method rests on the favorable and exceptional circumstance that complex variables can be employed for the study of real points of such a surface.

[3]If $u_1 - u$ should be uniformly very small at the same time as $\varphi_1 - \varphi, \psi_1 - \psi$, it follows from the well-

fact that $u$ will cease completely to exist when $\varphi, \psi$ become non-analytical. This proves, first of all, that the application of Weierstrass' theorem in that case is illegitimate, since it gives an approximation for the data but nothing of the kind for the unknkown.

Then we see also that such a problem and calculation, the results of which are utterly changed by an infinitesimal error in starting, can have no meaning in their applications.

This leads to my second and chief reason for considering only the results which correspond to non-analytic data, namely, the remarkable accoerdance between them and the results to which physical applications bring us.

This accordance is the more interesting from the fact of its results being unexpected. Our former point of view–i.e. that of the Cauchy-Kowalevska theorem–evidently constitutes a complete analogy to the case of ordinary differential equations. But from our latter point of view–which is also the point of view in problems set by physical applications–every analogy seems to be upset. The results often seem almost incoherent; they will give opposite conclusions in apparently similar questions.

A first instance of this was given above. We know that Cauchy's problem is now impossible for Laplace's equation

$$\nabla^2 u = \frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} = 0; \tag{e}$$

but, on the contrary, in the equation of spherical waves

$$\nabla^2 u = \frac{\partial^2 u}{\partial t^2}, \tag{E}$$

or of the cylindrical waves

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} - \frac{\partial^2 u}{\partial z^2} = 0, \tag{E'}$$

we may assign arbitrarily the values (whether analytical or not) of $u$ and $\delta u/\delta t$ for $t = 0$, and Cauchy's problem set in that way has a solution (which is unique). In this latter case it is like a problem in algebra in which the number of equations is equal to the number of

known convergence theorem of Cauchy that, letting the analytic functions $\varphi_1, \psi_1$, converge towards certain (non-analytic) limiting functions $\varphi, \psi$, the corresponding solution $u_1$ ought to converge uniformly towards a certain limit $u$, which would be a solution of the problem with the data $\varphi, \psi$.

unknowns; in the former, like a problem in which the number of equations is superior[4] to the number of unknowns.

It never could have been imagined a *priori* that such a difference could depend on the mere changing of sign of a coefficient in the equation. But it is entirely conformable to the physical meaning of the equations. Equation (E'), for instance, governs the small motions of a homogeneous and isotropic medium, like a homogeneous gas; and the corresponding Cauchy's problem, enunciated above, represents the definition of the motion by giving the state of positions and speeds at the origin of times. On the contrary, equation (e), which also governs many physical phenomena, never leads to problems of that kind but exclusively to problems of the Dirichlet type. The analytical criterion by which those two kinds of partial differential equations are to be distinguished, is known: it is given by what are called the *characteristics of an equation.* The characteristics of an equation correspond analytically with what the physicist calls the *waves* compatible with this equation, and are calculated in the following way. Let a wave be represented by the equation $P(x, y, z, t) = 0$. In the given equation, for instance if $\nabla^2 u - 1/a^2 \cdot \partial^2 u/\partial t^2 = 0$ and $\nabla^2 u$ be replaced by $(\partial P/\partial x)^2 + (\partial P/\partial y)^2 + (\partial P/\partial z)^2$ and $-(1/a^2)(\partial^2 u/\partial t^2)$ by $-(1/a^2)(\partial P/\partial t)^2$ the condition thus obtained is

$$\left(\frac{\partial P}{\partial x}\right)^2 + \left(\frac{\partial P}{\partial y}\right)^2 + \left(\frac{\partial P}{\partial z}\right)^2 - \frac{1}{a^2}\left(\frac{\partial P}{\partial t}\right)^2 = 0$$

(which is a partial differential equation of the first order). It must be verified by the function $P$. When this holds, $P(x, y, z, t) = 0$ is said to be a characteristic of the given equation.

For equation (E), such characteristics exist (that is, are real); this case is called the *hyperbolic one.*

Laplace's equation, $\nabla^2 u = 0$, on making the above substitution, leads to the equation

$$\left(\frac{\partial P}{\partial x}\right)^2 + \left(\frac{\partial P}{\partial y}\right)^2 + \left(\frac{\partial P}{\partial z}\right)^2 = 0$$

which has no real solution. Therefore, in this case there are no waves and we havet he

[4]We could be tempted to apply in that case the remark made in the beginning (1) concerning such impossible problems, which, notwithstanding that circumstance, must be considered as resembling "correctly set" ones. This, however, is not really applicable; for we have seen that the category alluded to is recognized by the fact that the problem may, under more special circumstances, become indeterminate. Now, this can never be the case in the present question: it follows from a theorem of Holmgren ("Archiv für Mathematik") that the solution of Cauchy's problem, if existent, is in every possible case unique.

so-called elliptic case.[5] Cauchy's problem can be set for a hyperbolic equation, but not for an elliptic one. Does this mean that for a hyperbolic equation Cauchy's problem will always arise? No, the matter is not quite so simple. For instance, in equation (E) or (E'), we could not choose arbitrarily $u$ and $\partial u/\partial y$ for $x = 0$; this would lead us again to an impossible problem (in the non-analytic case, of course).

The physical explanation of this lies in the fact that there are, besides the partial differential equation, two kinds of conditions determining the course of a phenomenon, viz., the initial and boundary conditions. The former are of the type of Cauchy and they alone intervene in Cauchy's problem quoted above for the equation of sound.

But the boundary conditions are always of the type of Dirichlet. They are the only ones which can occur in an elliptic equation, but even in a hyperbolic one they generally present themselves together with initial ones. This gives place to so-called *mixed problems* where the two kinds of data (belonging respectively to the Cauchy and to the Dirichlet type) intervene simultaneously for the determination of the unknown.

In equation (E), $t = 0$ represents the origin of time and can give place to initial conditions, having the form of Cauchy. But no such conditions can correspond to $x = 0$, which represents a geometri boundary.

More or less complicated cases can arise for various dispoisitions of the configurations, giving place to other paradoxical and apparently controductory results, which can however all be explained in the same way. Moreover, there are other types of linear partial differential equations,[6] which do not govern any physical phenomena. The determination of solutions has been studied[7] in the analytic case but no sort of determination of that kind for non-analytic data has been discovered hitherto.

We see that from this non-analytic point of view the accordance between mathematical results and the suggestions of physics holds perfectly. This accordance must not surprise us, for, as we saw above, it corresponds to the fact that a problem which is possible only

[5] An intermediate case exists $\nabla^2 u - k(\partial u/\partial t) = 0$. This is semi-definite and is termed the parabolic one (example: the equation of heat).

[6] The so-called *non-normal* hyperbolic equations, such as

$$\frac{\partial^2 u}{\partial x_1^2} + \dots \frac{\partial^2 u}{\partial x_m^2} - \frac{\partial^2 u}{\partial y_1^2} \dots \frac{\partial^2 u}{\partial y_n^2} = 0 \quad (m > 1, n > 1).$$

[7] By Hamel (Inaugural Dissertation, Göttingen) and Coulon (thesis, Paris).

with analytic data can have no physical meaning. But it reamins worth all out attention. No other example better illustrates Poincaré's views[8] on the help which physics brings to analysis as expressed by him in such statements as the following, "It is physics which gives us many important problems, which we would not have though of without it," and "It is by the aid of physics that we can foresee the solutions."

[8] Lectures delivered at the first Internation Mathematical Congress, Zurich, 1897; reproduced in "La Valeur de la Sciences."

# Lecture II
# Contemporary Reseaches in Differential Equations, Integral Equations, and Integro-Differential Equations

## 2.1 Partial Differential Equations and Integral Equations

I reminded you at the end of the last lecture what indispensable help the physicist renders to the mathematician in furnishing him with problems. But that help is not always free from inconveniences, and the task of the mathematician is often a thankless one. Two cases generally occur: it may happen that the physical problem is easily soluble by a mere "rule of three" method, but if not, it is so extremely difficult that the mathematician despairs of solving it at all; and he will strive after that solution for two centuries and, when he obtains it, our interest in the particular physical problem may have been lost. Such seems to be the case with some problems concerning partial differential equations. Just after the discovery of infinitesimal calculus, physicists began by needing only very simple methods of integration, the problems in general reducing to elementary differential equations. But when high partial differential equations were introduced, the corresponding problems almost immmediately proved to be far above the level of those which contemporary mathematics could treat.

Indeed, those problems (such as Dirichlet's) exercised the sagacity of geometricians and were the object of a great deal of important and well-known work throught the whole of the nineteenth century. The very variety of ingenious methods applied showed that the question did not cease to preserve its rather mysterious character. Only in the last years of the century were we able to treat it with some clearness and understand its true nature. This clearness seemed to come too late, for at that time, physics began its present evolution in which it seems to disregard partial differential equations and to come back to ordinary differential equations, but of course in problems profoundly different from the simple cases which were familiar to Bernoulli or Euler.

Happily, for it would have been a humiliating thing to work so uselessly, this disregard was only in appearance, and the ancient problems have not lost their importance by the fact that other ones have been superposed on and not substituted for them. In fact, the solution now obtained for Dirichlet's problem has proved use ful in several recent researches of physics.

Let us therefore inquire by what device this new view of Dirichlet's problem and similar problems was obtained. Its perculiar and most remarkable feature consists in the fact that the partial differential equation is put aside and replaced by a new sort of equation, namely, the integral equation. This new method makes the matter as clear as it was formerly obscure.

In many circumstances in modern analysis, contrary to the usual point of view, the opeartion of integration proves a much simpler one than the operation of derivation. An example of this is given by integral equations where the unknown function is written under such signs of integration and not of differenetiation. The type of equation which is thus obtained is much easier to treat than the partial differential equation.

The type of integral equations corresponding to the plane Dirichlet problem is

$$\phi(x) - \lambda \int_A^B \phi(y) K(x, y) dy = f(x) \tag{2.1}$$

where $\phi$ is the unknown function of $x$ in the interval$(A, B)$, $f$ and $K$ are knwon functions, and $\lambda$ is a known parameter. The equations of the elliptic type in many-dimensional space give similar integral equations, containing however multiple integrals and several independent variables. Before the introduction of eqwuations of the above type, each step in the study of elliptic partial differential equations seemed to bring with new difficulties; not only did the various methods imagined for Dirichlet's problem not cast more than a partial light on the equation, but the principles of most ofthem were peculiar to that special problem: they seemed to disappear if Laplace's equation was replaced by any other equation of the same type, or even (except for Neumann's method, which, as we shall soon see, is directly related to integral equations) if for the same Laplace's equation Dirichlet's problem was replaced by any analogous one such as presented by hydordynamics or theory of heat. Each of them, besides, was rather a proof of existence than a method of calculation.

Then they seemed again quite insufficient for another series of questions which mathematical physics had to solve, viz., the sutdy of harmonics. The existence of those harmonics (such as the different kinds of resonance of a room filled with air) was physically evident, but for the mathematician it offers an immense difficulty. Schwarz, Picard and Poincaré gave a first solution which was rather complicated as each harmonic requires for its definition a new infinite process of calculation after the preceding one has been determined. Nevertheless it has demonstrated rigorously the chief properties of the quantities in question (namely, certain special values of the parameter in equation (2.1)), i.e. that they exist and form a discrete infinity, only a finite number of them lying within and finite interval.

But at the same time a discovery even more important, in a certain sense ,was made by Poincaré, namely the near relation between that question of harmonics and the method which had been indicated by Neumann for Dirichlet's problem. This discovery of Poincaré paved the way for Fredholm's work. The latter treats every one of the aforesaid questions, and any which can be assimilated to them, by one and the same method, which consists in the reduction to an equation such as (2.1). This gives all the required results at once and for all the possible types of such problems.

In all this, the mathematician seems to play again the unfortunate rôle we alluded to in the beginning; for those results are nothing but the mathematical demonstration of facts each of ewhich was familiar to every physicist long before the beginning of all those researches. But of course their interest is not in fact limited in demonstration; they can and do serve as starting points for the discovery of new facts. They are useful as giving the proper method of calculation. Previously, in the calculation of the resonance of a room filled with air, the shape of the resonator had to be quite simple, which requirement is nota necessary one for the case where integral equations are employed. We need only make the elementary calculation of the function $K$ and apply to the function so calculated the general method of resolution of integral equations.

There are two chief methods for the solution of the equations. It is not always wasy to get numerical results.

Liouville and Neumann (in solving Dirichlet's problem) really worked out a method of solving integral equations. A second method is due to Fredholm. The first method leads to series which may converge slowly but they are easy to calculate. The method of Fredholm gives a quotient of two series (entire functions of $\lambda$) the terms of which have to be calculated independently, while in the first method each is obtained from the one immmediately, while in the first method each is obtained from the one immediately preceding it. While we must add that Erhard Schmidt has shown how the first method can be made to supply a more rapidly convergent series, Fredholm's method is of greater value to physics because of the theoretical point of view. It gives easily (what was impossible before its appearance) not only the existence of harmonics, but their properties. For instance, older methods could not have succeeded, at least not without great difficulties and a large amount of calculation, in obtaining the order of magnitude of the successive upper harmonics (i.e. the corresponding great values of $\lambda$). They would probably have been quite unable to predict the order o magnitude, as is done in the recent works of Hermann Wayl, so as to show its relation the volume of the room to which they correspond. But it has even proved of great importance for physics to know mathematically, and not only empirically, that the harmonics corresponding to equations of the form (2.1) are a discrete infinity. For in the case of the spectral frequencies we get series which tend to accumulate towards definite positions. Since Fredholm's theory we can assert that such series are not compatible with the form of integral equation given at the beginning of this lecture.

Fredholm himself investigated new forms (as also did Walther Ritz). The introduction of the integral equation has made even the above problem accessible. The older method would not have been able to decide whether the distribution in question was possible or not. The hypothesis proposed by Fredholm leads to an integral equation such as

$$\phi(x) - \frac{1}{k-\lambda^2}\int_a^b \phi(y)K(x,y)dy = f(x) \tag{2.2}$$

Here the frequencies will accumulate in the neighborhood of $\lambda = \sqrt{K}$.

I must immediately add that, as Ritz showed, Fredholm's type is not sufficient to give a correct explanation of the phenomena. But this does not change the essential fact that by the aid of the new method we are immediately able to decide what the asymptotic distribution of harmonics can or cannot be, so that comparison with observation becomes possible; and this we own entirely to Fredholm's method.

## 2.2 Coming Back to Ordinary Differential Equations

As we said in the beginning, the subject of partial differential equations which was the main and almost the only occupation of mathematical physics, ceases nowadays to be so. As a consequence of the general admission of the discrete structure of matter, physical problems tend now to lead to ordinary differential equations. These differential equations are to be studied under the most difficult circumstances because we must follow the form of the solutions for very long periods oftime, that is, of the independent variable $t$. One can say that such a study did not exist before Poincaré, and even his researches on the subject, I mean especially his four chief memoirs in the "Journal de Mathemtiques," 1887 (*On the shape of Curves Defined by Differential Equations*), lead us, like Socrates, to begin to feel that we know nothing.

We cannot, in this place, lay stress on the extraordinary complications and paradoxes which he discovered. We shall mention only one of them, because it helps to correct an error frequently committed in hydrodynamical and electrical problems, concerning the lines of force and the lines of flow. These lines are all defined by ordinary differnetial equations. The general form is $dx/X = dy/Y = dz/Z$. In a very general category of cases the vector $XYZ$ has the property that

$$\operatorname{div}(XYZ) = \left(\frac{\partial X}{\partial x} + \frac{\partial Y}{\partial y} + \frac{\partial Z}{\partial z}\right) = 0$$

Now, whenever such conditions existed, physicists used to say that the tubes of force–or tubes of flow, or tubes of vortices–were closed (if they did not go to infinity or come to the boundaries of the domain of existence of the vector $X,\ Y,\ Z$).

They were, I think led to say so by the examples given by some simple peculiar cases in which the differential equations could be integrated, for one could not suspect before Poincaré's work that such cases are exceptional, generally giving a quite inadequate and

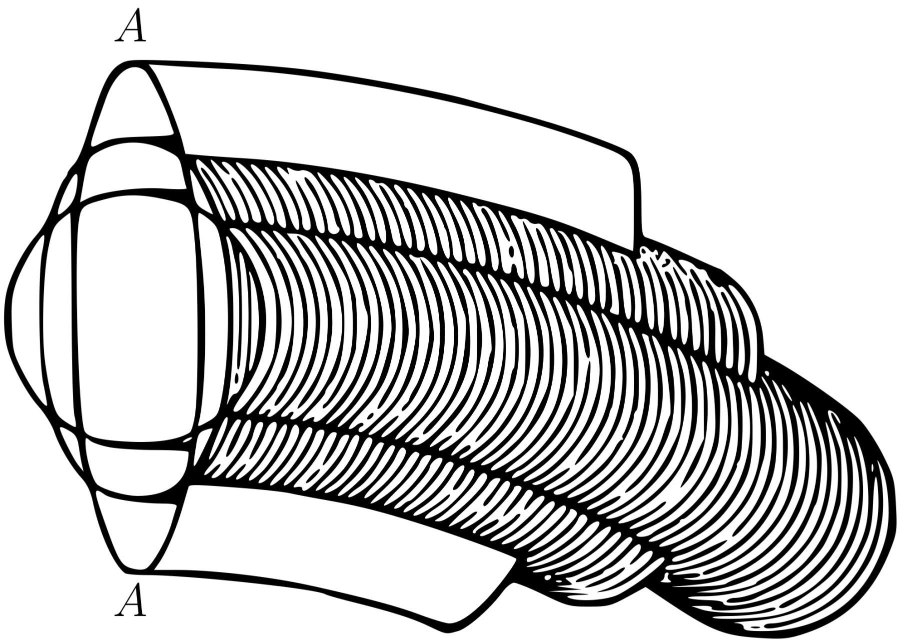



Figure 2.1:

deformed view of things. In fact, the assertion in question is an utteryl false one.[1] If you allow me such a crude comparison, it is not true that the tube of force must get back home and put its key in the lock. Rather does it put its key above and below and on either side, and never succeeds in getting it in exactly. It will, it is true, nearly get back an infinite number of times. The only consequence which can be correctly drawn from the equation $\mathrm{div}(XYZ) = 0$ is that the area of the cross section of the tube cannot have changed. But its shape may, and generally will, have done so. If it were, let us say, circular in starting , it will have become elliptic when coming back and its ellipticity will increase at each return. Finally it will become a long flat strip and only a part of it will come back to the neighborhood of its original position. In Fig. 1, the successive appearances of the same tube of force are shown. The tube of force may have been originally circular, but on its first recurrence of return, it may have become elliptic in cross section and thus it has only partly returned to its original position. Still more is this that case in the second recurrence of the tube of force, which may be assumed by this time to have become very flat in cross section.

[1] A demonstration is frequently given to justify it, the error of which consists in an incomplete enumeration of possible cases.

As Mr. Birkhoff kindly pointed out to me, it is interesting to remark that in most cases, the deformed and falttened tube will even pass *simultaneously* indefinitely near to any point of the considered medium.

A rather curious fact must nevertheless be stated. Although the principle that the tube is closed is completely false, the conclusions drawn from it by physicists are most often true. Why is this so? Perhaps the explanation lies in the fact that under the same hypothesis, $\text{div}(X, Y, Z) = 0$, a line defined by our differential equations generally returns indefinitely near and an infinite number of times to its starting point. (This is called "Stabilité a la Poisson.") Poincaré has shown that though not every line in question necessarily does this, the fact occurs for an infinitely greater number of cases than those in which it does not occur.

## 2.3 Application to Molecular Physics

We see by this single example how complicated and unexpected the shapes of curves defined by differential equations may be, and how far we are from understaning them when considered for great values of the independent variable.

But could we be satisfied with our work if we succeeded in doing so? This even is doubtful. I cannot help thinking of a bequest left to the French Academy of Science for a prize to the first person who should be able to communiate with a planet other than Mars! The case of molecular physics reminds me of that rather difficult requirement. The discussion of the molar effects (i.e. the effects on quantities of matter accessible to observation) of molecular movements is a mathematical problem, which, logically speaking, would presuppose a rather advanced knowledge of curves defined by differential equations, and take this as a starting point, in order to discuss the questions of probability connected with such curves.

That probabilty plays its role in the movements of almost any dynamical system, follows from the statements we just quoted. If the initial positions and the initial speeds of the moving points are exactly given, so will be the final positions and speeds after any (however long) given period of time. But if this period is long, and if we make a very small error in the initial conditions, the small error will have a muchmagnified effect and even cause a total change in the results at the end of the long period of time, and this is precisely Poincaré's conception of hazard. It is like a roulette game at Monte Carlo where we do not know all the conditions of launching the ball which induces the hazard. And so we know nothing more about the conditions than the gamblers. In other words, molecules are finally mixed just as cards after much shuffling. It is this fundamental hazard which plays the main part in Gibbs's method. A sort of mixing function ought to be introduced. Let us start on one of the lines of force. If we know exactly the point of departure $A$ we should know accurately the point of arrival. If $A$ is but approximately known, that point of arrival may occupy all sorts of positions; and indeed, in many differential problems, it may coincide (approximately) with any point $B$ within the domain where the differential system is considered (though this is exactly so for dynamical problems on account of the energy integral or other uniform integrals which the equations may admit.)

Therefore, the starting point being approximately $A$, there will be a certain probability that the point of arrival will be in a certain neighborhood of another given point $B$; and that probability will be a certain function of the positions of the two points $A,\ B$.

Now, logically speaking, in order to solve the question set for us by kinetic theories, we ought to take such a "mixing function," assuming it to be known, as a base for further and perhaps complicated reasoning. In fact, the main present theories in statistical mechanics rest on certain assumptions concerning that function, which are very playsible. But, rigorously speaking, we are not able to consider them as theorems.

Happily, things are greatly simplified by the fact that in such mixings the aforesaid function, characteristic of the law of mixing, only intervenes by some of its properties and may be changed to a large extent without changing the final result. This is what Poincaré showed for the ordinary shuffling of cards in his "Calcul des Probabiltiés" (second edition). In one shuffling the peculiar habits of the player certainly intervene and so do they more or less after only a few shufflings. But after many shufflings the results become totally independent of those habits. Poincaré also shows (though with some exceptions which do not however seem to play a great practical role), that such is likewise the case in the kind of mixing introdued by molecular theories.

Some known facts in the history of these theories give a striking instance of this. Such is the work of Blotzmann and Gibbs in the treatment of the kinetic theory of gases and statistical mechanics. They both obtained the result that if we consider the probability of the average number of molecults in 6-dimensional space and call it $P$, and integrate $\log P$ over the whole mass, the conclusion drawn will be that the integral obtained is constantly increasing. Crtiics, and among them myu colleague and friend Brillouin, say: "We have not to congratulate ourselves onf the result, because the two speak of quite different things and yet they agree. Gibbes does not mention the collision of molecules, while Boltzmann's analysis is founded on the collisions of molecules. The primitive order of the molecules is disturbed by such collisions and a mixing is produced. Gibbs gets a similar mixing by the mere consideration of differential equations existing over long periods of time." In both cases, if we consider systems which are "molecularly organized," after a certain time the molecules will be so much less organized and more mixed up.

We are surprised to find this coincidence of the results of Gibbs and of Boltzmann in such circumstances. We shall, however, cease to consider it as fortuitous and perceive its true signification by precisely what we just remarked on the shuffling of cards, which makes us understand that such final results may and do depend on properties which are, in general, common to utterly various laws of mixing.

But the difficulties met with in partial or ordinary differential equations are not the only ones which we had to consider at the present time. The mathematicians have contrived to intoduce a new sort of equation, more difficult than the previous ones, the integro-differential equation.

## 2.4 Integro-Differential Equations

We are now forced to consider this new form. Here the unknown function simultaneously appears in integrals and in differentials. We have at least two completely different cases of such equations to consider. Their difference corrseponds to the two sorts of variables which intervene in all physical problems, the space variables $x, y, z$, and the time variable $t$. (There may be more than three variables in the first group.)

Type 1: Differentiation with respect to $x, y, z$; integration relative to $t$. Type 2: Differentiation with respect to $t$; integration relative to $x, y, z$. And even though this type dates only from 1907, we have already found cases of both kinds.

Volterra was led to consider the first one in connection with "The Mechanics of Heredity." This is the case where the properties of the system depend on all the previous facts of its existence (such as magnetic hysteresis, straings of glass, and permanent deformations in general).

Volterra considers elastic hysteresis. Let $T$ by any component of strains; $E$ the component of deformation. (There are six $T$'s and six $E$'s.) Then formerly we considered $T_{hk} = \sum a_{hk}E + hk$. There are 6 equations of this type. There are 21, 36, 6 or 2 $a$'s depending on the theories. If we consider heredity, we must introduce new terms, Suppose that at the time 0 there were no strains; then $T_{hk} = \sum aE_{hk} + \int_0^t (\sum aE)_t d\tau$ where $\tau$ is the variable time. This is an equation in which we have derivatives with respect to $x, y, z$, and an integral with respect to the time; and the same character subsists if, from those values of the $T$'s, we deduce the equations of movement. Water waves furnish us with an instance of the opposite type. One knows that waves on the surface of water are the most common examples of an undulatory phenomenon and that, for this reason, they are most frequently used to give to the beginner a first idea of what such phenomena are.

But it is a general, though astonishing fact, that the most simple of daily phenomena are the most difficult to understand. While the theory of aërial or even elastic waves is rather simple, at least as long as viscosity is left aside,[2] and now classically reduced to analytical principles (related to notion of characteristics as we saw in the preceding lecture), the properties of surface waves in liquids are much more hidden. The few results classically known on that subject are even of a contrdictory nature. One of them is the differential equation given by Lagrange in the case of small (and constant) depth, which has served as a model for the dynamical theory of tides, the equation obtained as governing the phenomenon being in both cases a partial differential equation of the *second* order. But, for the same phenomenon on a liquid of indefinite depth, Cauchy gets a partial equation of the *fourth* order. The truth is that the problem does not lead to a differential equation at all, but to an integro-differential equation. For an orginally plane surface with small displacements, where $z$ is the vertical displacement at $(x, y)$, then

$$\frac{d^2z}{dt^2} = \iint Z_Q \phi(P, Q) dS_Q.$$

Thus, for any determinate point $P$ of the surface defined by its coordinates $(x, y)$ the vertical acceleration depends on the values of $z$ in every other point $Q(x', y')$. Here $S_Q$ is $dx'dy'$ and

[2] In a viscous gas, waves cannot exist, strictly speaking. They are replaced by quasi=waves which were first considered by Duhem, and more profoundly studied in an important memoir presented by Roy to the French Academy of Science.

$\phi$ is a known function of $(x, y, x', y')$. The above equation is of the second form of integro-differential equations.

Volterra succeeded in the case of isotropic bodies in reducing the problem to the solution of a partial differential equation and an ordinary integral equation. But things are not so simple for crystalline media.[3]

The two types of integro-differential equations, which we just enumerated, are completely different in their treatment. Volterra's type resembles the partial differential equations (of the elliptic or sometimes parabolic genus in the examples hitherto given). The equation must be completed by accessory conditions which are nothing else than boundary conditions (cf. Lecture I). The methods given by Volterra run exactly parallel to those which are applied for Dirichlet's problem (such as the formation of Green's functions).

In the second type described above, the accessory considitions are initial ones; and are to be treated in the manner, not of partial, but of ordinary differential equations–such methods as Picard's successive approximations being of great use in that case.

[3]Since these lectures were delievered, Professor Volterra has given a comprehensive view of his methods and solutions in a course of lectures at the University of Paris. See the issue of those lectures by J. Peres (Paris, Gautheir Villars).

# Lecture III
# Analysis Situs in Connection with Correpondences and Differential Equations

## 3.1

We are going to speak of the role of analysis situs in our modern mathematics. This theory is also called the geometry of situation. It is the study of connections between different parts of geometrical configurations which are not altered by any continuous deformation. We suppose that we can let a system undergo any deformation whatever, however arbitrary it may be, only that it preserves continuity. For instace, a sphere and a cube are considered as one and the same thing from te point of view of the seometry of situation, because one can be transformed into the other without separating parts, or uniting parts which formerly were separated. The circle and the rectangle are identical from the same point of view. But the lateral surface of a cylinder and the surface of a rectangle are not identical, because, from the transformation of one into the other, we must make a cut along a generatrix. Also one is limited by two lines (the base circles) while the other is limited by one. The total surface of a cylinder is entirely closed; it is identical withe the surface of a sphere. There is no difficulty in the transformation.

If we consider the "anchor ring," the case is different. This is a closed surface but it has a hole which is not found in the surface of the sphere, and the surface of the sphere cannot

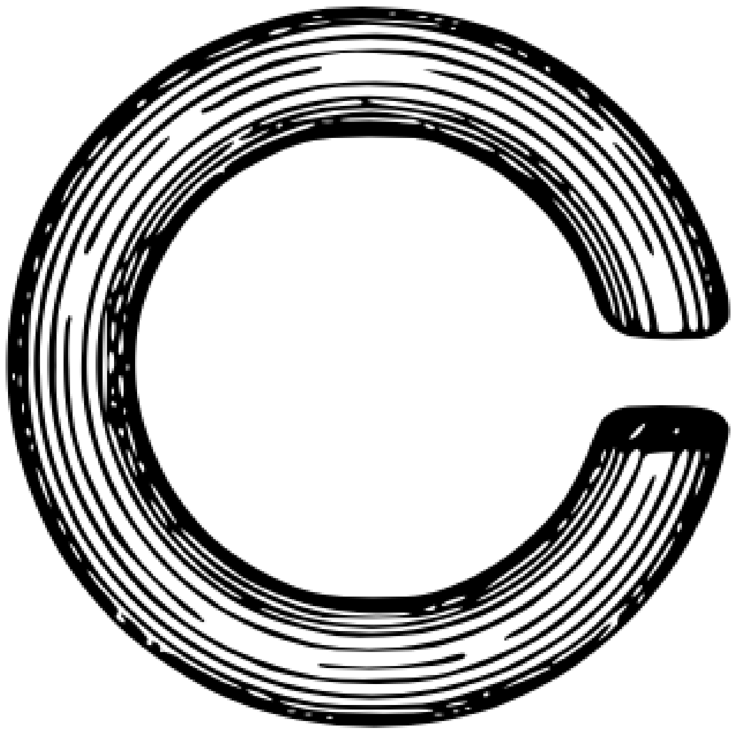


Figure 3.1:

be transformed continuously in it. It would have to be transformed by several cuts, the first of them (3.1) giving a broken ring, which for us is identical with the lateral surface of a cylinder. This may be cut into a rectangle and then transformed into a sphere. But the transformation of an anchor ring into a sphere cannot be done without cutting and piecing. The principles of analysis situs, for surfaces in ordinary space, are well known and I do not intend to go over them at this moment. We shall take them for granted. According to them, a surface of two dimensions is defined from our present point of view by the number of boundaries and another number, namely the *genus*. The genus is zero for the sphere and one for the anchor ring. For a pot with two "ears" (3.2) we have the genus two.

Analysis situs started with trifling problems, such as that treated by Euler of the bridges of Königsberg over the Pregel river. There are seven bridges; the problem is to go over all of them without passing twice over any one (3.3). The great Euler did not disdain to occupy himself with this and many other apparently childish problems. But what interests us in this one especially is that it involves the geometry of the situation, in the sense in which we have used the term. For even if the islands in the rives had other shapes and the bridges

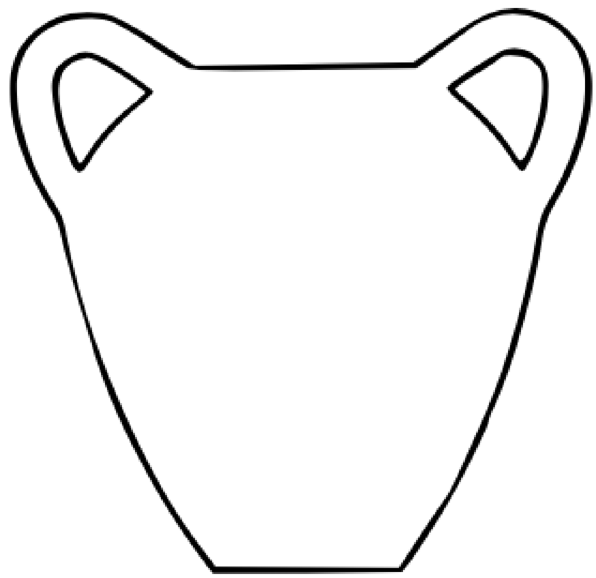


Figure 3.2: A pot with two ears.

had the queerest forms, the reasoning would be exactly the same, provided the numbers of islands and bridges should not change, and each bridge should join the same islands in both cases.

We have here an example of an important theory which develops from a childish exercise. Some would think that it was a disadvantage to mathematics that we should occupy ourselves with such problems. The fact is, as we see, that they may, though exceptionally, lead to valuable results.

That this notion of analysis situs was really an important one, appears first from the researches of Riemann. You know that Riemann was the fellow founder with Cauchy of the modern theory of analytic functions. These two schools applied their theories to the study of algebraic functions. Cauchy's methods, the the hands of their author and of Puiseux, were capable of casting light on some important parts of the problem, but did not however completely elucidate it, and (in particular) Riemann alone could discover the fundamental notion of the *genus* of an algebraic curve.

What were the elements of Riemann's success and superiority over Cauchy? A remark must first be made which perhaps, strictly speaking, would not be within our subject, but which is nevertheless, as we shall see, most closely and necessarily connected with it.

Let us consider the real domain. Suppose that we have to study the algebraic function $y$ defined by $x^2+y^2=1$ (or any quadratic equation defining $y$ as a function of $x$ corresponding to an ellipse). This function is real only for values of $x$ which are comprised between $-1$ and $+1$ (in the second case, for values between $x_0 and x_1$). Riemann considered the function in the segment comprised between these values. He remarked that this is an incomplete view of the equations, for $y$ is not well defined, because it has two different values. But if we change our straight line into two slightly different straight lines, then we may admit that the superior segment corresponds to the + value of $y$, and the inferior one to the – value, the two segments being supposed to join each other at their common ends. To each point of the drawing, after that modification, one and only one system of values of $x$ and $y$ verifying the given equation will correspond. Besides, in that case, we obtain a figure which from the point of view of analysis situs, is identical with the ellipse represented by the given equation itself.

But Riemann applied that same method in the complex domain, and was led to the celebrated kind of representing surfaces which bear his name.

This principle is a very general one. It must be applied, in any case, before using the geomtery of situation. We must inquire whether the domain used is adequate to represent the states of variation to be studied. I shall give an instance which I think is due to Sophus Lie. It is concerned with the singular solution of differential equations of the first order. Given the general equation

$$f(x, y, y') = 0 \tag{3.1}$$

the question, as well known, is whether some solution exists which is not represented in the general integral. In that case such a solution must verify not only the original equation, but also

$$\frac{\partial f}{\partial y'} = 0 \tag{3.2}$$

Darboux showed that this was not sufficient, and that, in general, the system of equations (3.1) and (3.2) does not represent an actual solution, but that the curve which it defines is the locus of the cusps of the solutions of equation (3.1) (3.4). We now shall see that this result, the analytical proof of which requires some complicated calculations, appears of itself by the above geometric considerations.

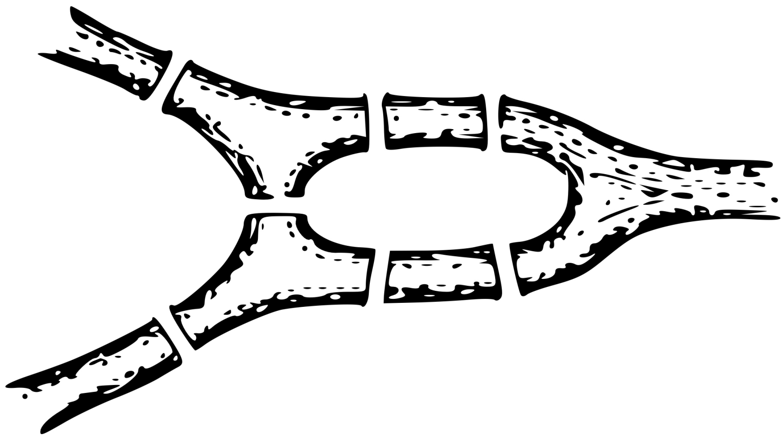
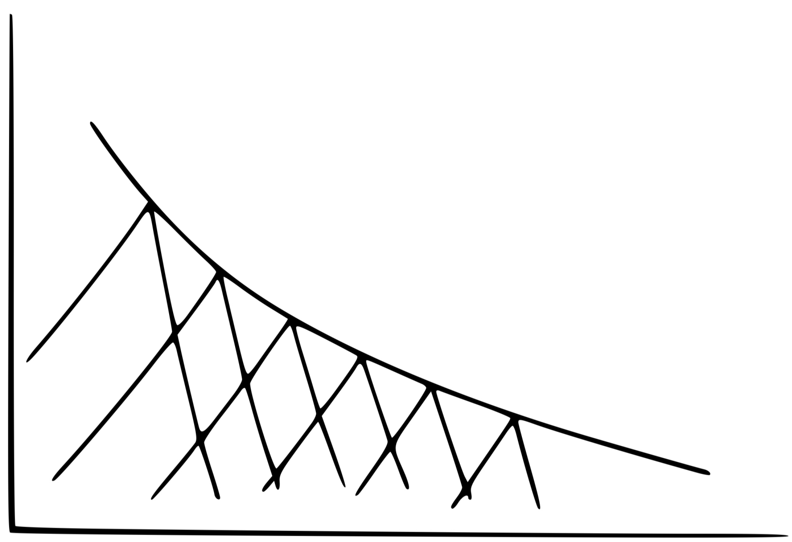


Figure 3.3:



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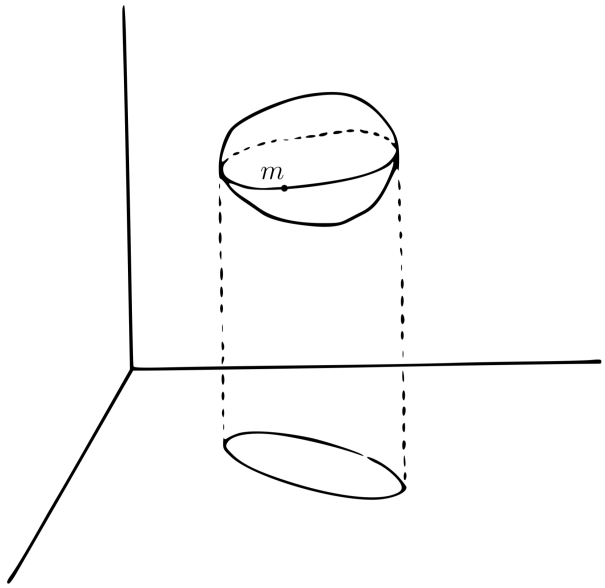


Equation (3.1) defines $y'$ as a function of $x$ and $y$, but this funciton has several determinations or branches. This state of things is not satisfactory from out point of view above. In order to avoid this, let us consider the surface $f(x, y, z) = 0$ is space. For each point of that surface, we have

$$dy/dx = z \tag{3.3}$$

So that the problem becomes to trace on the surface, those curves which have $dy/dx$ equal to $z$. Geometrically speaking, such curves must, in each point, be tangent to a certain direction, viz., the intersection of the tangent plane to the surface with a certain vertical plane (represented by (3.3)). The system (3.1) and (3.2) represents the "horizontal boundary" of the surface. At each point $m$ on it, the tangent plane is vertical (3.1). What happens there? We see that in $m$, the two planes which define the tangent to our curve are vertical (the plane corresponding to (3.3) being to in any case). Therefore, this tangent itself is also vertical. This gives immediately the desired result; for it is well known that by projecting a psace curve on a plane perpendicular to one of its tangents, we obtain a projection curve which has a cusp. The only exception would be when our two planes would coincide and this indeed gives the supplementary condition for the existence of a singular solution.