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From "Mixed" to "Applied" Mathematics: Tracing an important dimension of mathematics and its history

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Abstract. The workshop investigated historical variations of the ways in which historically boundaries were drawn between 'pure' mathematics on the one hand and 'mixed' or 'applied' mathematics on the other from about 1500 until today. It brought together historians and philosophers of mathematics as well as several mathematicians working on applications. Emphasis was laid upon the clarification of the relation between the historical use and the historiographical usefulness and philosophical soundness of the various categories.

Mathematics Subject Classification (2010): 01A40 to 01A99.

Introduction by the Organisers

For a long time, research in the history of mathematics has mostly focused on developments in pure mathematics. In recent years, however, some significant research has changed this situation. The conference brought together historians of mathematics actively involved in this reorientation, in order to take stock of what has been achieved, and to identify historical problems yet to be solved. In having done so, the organizers hope to have advanced the general understanding of the relations of mathematics with neighbouring scientific fields, and with technological, economic and social practices involving mathematical methods, in the period since ca. 1500.

A fundamental historical insight provided a starting point for planning this meeting: There is no, and there has never been, a once and for all fixed notion of 'applied' mathematics. Rather, we have to deal with a field of interactions of the production of mathematical knowledge with a large and variable number of scientific, technological and social areas beyond the core disciplines of 'pure' mathematics. For want of a better term, and without taking the term literally, we call this field the 'applied field' of mathematics.

At different times in history, conflicting views on the value and possibilities of using mathematical methods in other contexts have generated controversial discussions e.g. about the unity of mathematics, about the role of mathematics as a tool, or as an aim in itself. Motives for engaging in such debates came both from the side of the practitioners of the mathematical sciences themselves and from the side of real or potential 'users' of mathematical methods. Moreover, the very notion of the 'application' of ready-made mathematical methods and knowledge to extra-mathematical domains is problematic; in fact in many cases mathematical methods emerged from interactions with such domains, thereby changing and challenging the existing ideas about mathematics.

In the early modern period, the mathematical sciences in Europe were formed with reference to an inner distinction between *mathematicae purae* and *mathematicae mixtae.* However, the field denoted as 'mixed mathematics' was complex, varying in inner structure, and not coinciding with what later came to be called 'applied' mathematics (a denotation whose meaning was hardly less variable). The situation was further complicated by the fact that mathematical methods became part of the very notion of 'natural philosophy', or science per se, during the 17th century. Several sciences – and first and foremost the new science of 'mechanics' in all its branches from celestial mechanics to hydrodynamics – were inextricably linked with the production of mathematical knowledge. During the 18th century, the notion of 'application' of one mathematical science to another was gaining interest (e.g. in the approach of the French Encyclopedists, in particular of Jean d'Alembert). Moreover, in several fields of the mathematical sciences (such as hydrodynamics and hydraulics), a lack of mutual integration of 'theory' and 'practice' was lamented by several authors at the time. Nevertheless, as is clearly shown in the work of Euler and many of his contemporaries, there was still no delineated field of 'applied mathematics'. A more explicit distinction between 'pure' and 'applied' mathematics gradually came to the fore during the 19th century, while an institutional separation of 'applied' and 'pure' mathematical research in journals, university positions and, eventually, institutes was a matter of the 20th century. During the early 20th century, moreover, the idea of 'mathematical modelling' was shaped, and it rapidly gained importance in a wide variety of areas from economics to engineering and medicine. The revolutionary development of new calculation devices contributed to a new balance between analytical, graphical and numerical methods and led finally even to a new notion of the solvability of mathematical problems. These developments, initially taking place primarily within applied contexts, also became important for certain areas of pure mathematics. In recent decades, however, voices are gaining strength that argue for a renewed integration of pure and applied mathematics, both on the institutional and cognitive level (such voices are already well-known from the beginning of the 20th century when

Richard Courant was one of their strongest speakers). Striking a balance between the development of the applied field and the unity of mathematics remains a challenge.

In view of the above, the scope of topics dealt with during the conference needed to span the period between the 16th century and the present, in order to help us better understand how the ‘applied field’ developed and was structured. Throughout, the emphasis was not to establish a separate historiography of applied mathematics per se, but rather to understand how the whole of mathematics was internally structured both with a view to ‘applications’ and to its relation with extra-mathematical domains.

By giving short introductory talks the three organizers tried to raise some general issues and questions for the ensuing 20 presentations and the discussion. These talks went into the notion of applied mathematics originating around 1800, into historical changes in the classification of sub-branches of mathematics according to the pure/mixed or pure/applied divide (with particular reference to influential works such as the French *Encyclopédie* and comparing them with modern changes in the relationship between science and technology), and, thirdly, the very important features of modeling within modern applied mathematics, the latter with emphasis on models in economics and biology.

While biology as an area of application was followed up in the conference in a talk on Karl Pearson, last minute cancellations resulted in a reduced discussion of applications in economics. However, the latter topic came up in connection with a presentation of social statistics in Late Imperial China. This contribution was also exceptional with respect to its non-European focus. At the same time the two last named presentations touched upon statistics as an important field and tool of applied mathematics, as did other talks on applied mathematics in industrial surroundings, which in particular discussed applied work by Iris Runge and B.L. van der Waerden. Engineering mathematics was recognized in the workshop as an important historical bridge and stimulus for renewing the relationship between mathematics and the applied field since the 18th century. Related to this topic were talks on mathematizing water powered machines and on ballistics in the 18th and 19th centuries and another presentation on the important hybrid discipline of fluid dynamics around 1900. The important theme of visualization of mathematical concepts and its changes from material models of the pre-computer age (used for instance by Felix Klein) to modern computer imaging was touched upon in several talks. Mathematical physics, which had often been a topic of historical discussions before, was less completely represented in the workshop although it came up particularly in connection with talks discussing the development of French, German and English applied mathematics during the 19th century. In this context regional and national differences between the various mathematical cultures, which partly still exist in modern globalized research, came clearly to the fore.

The workshop aimed from the outset at emphasizing the role of the applied field in the period of early modern mathematics. This was done in presentations on hitherto little known areas such as 'mathematical gardening' or landscape design, and it was related to mathematical practices in early modern treatises on mathematical perspective and the development of mathematical instruments during the same period. The aspect of auxiliary instruments and means for computation and construction in applied mathematics and the development of the material infrastructure were discussed in various contributions in connection with the treatment of observational data, with mathematical tables, handbooks and textbooks of mathematics. This discussion reached as far as touching the topic of computer design at the hands of John von Neumann in its relation to theoretical work by Alan Turing. However, similar to mathematical physics, the development of computing technology – a topic frequently treated in other contexts and occasions – was not a focus of the workshop.

Throughout the workshop problems of conceptual development were discussed, e.g. the relation between (astronomical) observation and mathematical theory, the discussion of 'inner-mathematical' applications in the case, for instance, of Hermite, the conflict between the factual and indubitable use of notions such as 'mixed' and 'applied' mathematics and the philosophical and logical insufficiencies of the latter.

Thus both in philosophical and conceptual respects and as to the possible fields and directions of application (economy, non-European cultures, mathematical physics, mathematical instruments and computers) there is room for enlarging the future discussion of the history of mathematics in the applied field.

Workshop: From "Mixed" to "Applied" Mathematics: Tracing an important dimension of mathematics and its history

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The establishment of the notion and of the word ‘applied mathematics’ around 1800

Reinhard Siegmund-Schultze

The meaning of names such as “applied mathematics” or even “mathematician” changes through history and is relative even within any period, as we know. But a change of names gives the historian first clues to look more closely.

In the “Prolegomena” to Johann Friedrich Weidler’s (1691–1755) Latin *Institutiones Mathematicae* of Wittenberg 1718 there is (to my knowledge) the first appearance of the word “applied mathematics” in any language. At page 5 Weidler speaks about “Mathesis applicata, quam nonnulli mixta appellant.” The *Institutiones* became a textbook for Russian mathematics students at the new university in Moscow, founded by M.V. Lomonosov in 1755. Abraham Gotthelf Kästner’s *Anfangsgründe der angewandten Mathematik* (Göttingen 1759) is the first book to mention “applied mathematics” on the title page. In the German countries in the 18$^{\text{th}}$ century, the word “applied mathematics” gradually replaced the older “mixed mathematics”, supported by some philosophical reflexions about “pure” and “applied” mathematics in the work of Immanuel Kant. The two short-lived journals edited by the influential combinatorialist Karl Friedrich Hindenburg, the *Leipziger Magazin für reine und angewandte Mathematik* (1786–1789) and the *Archiv für reine und angewandte Mathematik* (1795–1799) were the first to have “applied mathematics” in the title. In France “mixed mathematics” (“mathématiques mixtes”) was still in use in the famous *Encyclopédie* of Diderot and d’Alembert (1750), and even in the second edition of J.-E. Montucla’s *Histoire des mathématiques* (1798–1802). In 1792 the (former) Marquis de Condorcet, who was also involved in the metrical reform, was apparently the first Frenchman to use the name “mathématiques appliquées,” though only in the appendix of his report to the National Assembly. For the future so-called “Instituts” (which did not materialize) he proposed hiring:

> “Un professeur de mathématiques pures. Un professeur de mathématiques appliquées, qui comprendra dans ses leçons les éléments de mécanique, d’optique, d’astronomie, et les applications élémentaires les plus utiles du calcul et de la géométrie à physique, aux sciences morales et politiques.” [Titre IV, p. 65, thanks to Christian Gilain for this information]

An effect of the Industrial Revolution around 1800 was more systematized engineering education first in France and in the event in other continental countries such as Germany, Austria, Switzerland, and Italy as well. Mathematics as a science and as a teaching subject received an institutional boost both at Technical Universities (École Polytechnique) and later at traditional Universities. Aspects of national competition and pride played a role when mathematicians and politicians underscored the (at least potential) use of mathematics. In Norway, which had just introduced a constitution and was emancipating from Danish rule, Christopher

Hansteen's position as "lecturer for applied mathematics" (Lector i den Anvendte Mathematik) at the newly founded University in Christiania was expressly justified in May 1814 by "the broad scope of applied mathematics and its importance for Norway". In 1815 Hansteen was promoted to "Professor Matheseos applicatae".

Applied mathematics figures in the titles of Gergonne's *Annales des mathématiques pures et appliquées* from 1810 and of Crelle's *Journal für die reine und angewandte Mathematik* from 1826, however already the prefaces and later the published articles make it clear that pure mathematics was the real aim in both cases. In spite of sweeping accusations (for instance from German mathematicians) about a prevailing utilitarian attitude in French mathematics (for instance in a famous but partly misinterpreted quote by C.G.J. Jacobi of 1830, which was discussed in the present talk) the general tendency was the one as described by Jesper Lützen in his biography of Liouville:

> "Generally, Liouville's production is characterized by a slow movement from applied to pure mathematics; here, Liouville followed a general tendency of the time." [1, x]

A tentative conclusion of the talk is the following:
Around 1800, in the age of the Industrial Revolution and of continued nation building, state funding and political and ideological support (revolution in France, Neo-Humanism in Germany) led to a new level of recognition of **"mathematics" as a discipline** (teaching, journals). The older bifurcation pure/mixed mathematics, was replaced in Germany and somewhat later in France (but not yet in England!) by pure/applied, the difference basically being that before only mixed mathematics (extended by 'rational mechanics' in the 18^{th} century) had broader interests behind it and was (if not systematically) funded, while now the whole of mathematics was beginning to be supported and recognized. "Applied Mathematics" was increasingly considered to be part of a "discipline mathematics", while "mixed mathematics" had been something between mathematics and other sciences. So in this sense and slightly **paradoxically**, in spite of the general importance of the **Industrial** Revolution as a historical background, it is **pure mathematics** which increasingly gets systematic public support for the first time and thus **receives the relatively stronger boost than applied mathematics**. Carl G.J. Jacobi (1804–1851) can be considered a representative of these changes towards "pure mathematics." However, exactly in the case of Jacobi, who gradually turned towards applied mathematics at the end of his life, the dangers of a one-sided picture become obvious, which describes 19^{th} century mathematics as a succession of "utilitarian" French followed by "pure" German mathematics. The institutionalization of "applied mathematics" as a separate sub-discipline of its own, with funding for journals and institutes (for instance C. Runge, R. von Mises) was reserved for a later period of "reemergence of applications" and the so-called second Industrial-technical revolution under Felix Klein and others around 1900.

References

[1] Lützen, J. (1990): *Joseph Liouville (1809–1882): Master of Pure and Applied Mathematics*; New York: Springer-Verlag.
[2] Mehrtens, H., H. Bos and I. Schneider (eds. 1981): *Social History of Nineteenth Century Mathematics*; Boston etc.: Birkhäuser.

On “application” and categorizations of mathematics

Moritz Epple

I.

Let us begin with a look at the most recent Mathematical Subject Classification (MSC 2010).

This comprehensive classification of topics in mathematical literature appears to follow a broad general strategy of dividing the field of mathematics into major branches, some of which are broadly refered to as ‘pure mathematics’ whereas others are understood to be ‘applied’ mathematics. Introductory texts in the web in several languages distinguish the following top-level areas: General/foundations (00 ff), discrete mathematics/algebra (05 ff), analysis (26 ff), geometry and topology (51 ff), and then: applied mathematics / other (60 ff). However, this seemingly obvious partition is often undercut within MSC, see e.g. 37Nxx “Applications” in the section on Dynamical Systems, or 47Nxx “Miscellaneous applications of operator theory” – etc. Still, the same divide between pure and applied mathematics seems to be operative on a smaller scale as on the top level – we glimpse an understanding of a polar overall divide between pure and applied mathematics with self-similar structure.

How did such a classificatory scheme develop? Is MSC 2010, and in particular, the way in which it operates with the distinction pure/applied, the result of a ‘natural history’ of the inner organization of the branches of the tree of mathematics?

To sharpen this question, let us look at an earlier classification of these branches, given in the “Explication détaillée du système des connoissances humaines” in the *Discours Préliminaire* of the first volume of the French *Encyclopédie* (Paris, 1751). Here we find another ‘canonical’ division, based on the following explanations:

> Une autre propriété plus générale des corps, & que supposent toutes les autres, savoir, la *quantité* a formé l’objet des Mathématiques. On appelle *quantité* ou *grandeur* tout ce qui peut être augmenté & diminué.
>
> La *quantité*, objet des *Mathématiques*, pouvoit être considérée, ou seule & indépendamment des individus réels, & des individus abstraits dont on en tenoit la connoissance, ou dans ces individus réels & abstraits; ou dans leurs effets recherchés d’après des causes réelles ou supposées; & cette seconde vûe de la réflexion a distribué les *Mathématiques* en *Mathématiques pures, Mathématiques mixtes, Physico-mathématiques.*

This division is basically a binary one, with ‘physico-mathematics’ still a kind of anomalous singleton at the time. Now look at the branches of mixed mathematics.

> Les *Mathématiques mixtes* ont autant de divisions & de sous - divisions, qu'il y a d'êtres réels dans lesquels la *quantité* peut être considérée. La *quantité* considérée dans les corps en tant que mobiles, ou tendans à se mouvoir, est l'objet de la *Méchanique*. La *Méchanique* a deux branches, la *Statique* & la *Dynamique*. La *Statique* a pour objet la *quantité* considérée dans les corps en équilibre, & tendans seulement à se mouvoir. La *Dynamique* a pour objet la *quantité* considérée dans les corps actuellement mus. La *Statique* & la *Dynamique* ont chacune deux parties. La *Statique* se distribue en *Statique proprement dite*, qui a pour objet la *quantité* considérée dans les corps solides en équilibre, & tendans seulement à se mouvoir; & en *Hydrostatique*, qui a pour objet la *quantité* considérée dans les corps fluides en équilibre, & tendans seulement à se mouvoir. La *Dynamique* se distribue en *Dynamique proprement dite*, qui a pour objet la *quantité* considérée dans les corps solides actuellement mus; & en *Hydrodynamique*, qui a pour objet la *quantité* considérée dans les corps fluides actuellement mûs. Mais si l'on considere la *quantité* dans les *eaux* actuellement mûes, l'*Hydrodynamique* prend alors le nom d'*Hydraulique*. On pourroit rapporter la *Navigation* à l'Hydrodynamique, & la *Ballistique* ou le jet des Bombes, à la Méchanique.
>
> La *quantité* considérée dans les mouvemens des Corps Célestes donne l'*Astronomie géométrique*; d'où la *Cosmographie* ou *Description de l'Univers*, qui se divise en *Uranographie* ou *Description du Ciel*; en *Hydrographie* ou *Description des Eaux*; & en *Géographie*; d'où encore la *Chronologie*, & la *Gnomonique* ou l'*Art de construire des Cadrans*.
>
> La *quantité* considérée dans la lumiere, donne l'*Optique*. Et la *quantité* considérée dans le mouvement de la lumiere, les différentes branches d'*Optique*. Lumiere mûe en ligne directe, *Optique proprement dite*; lumiere réfléchie dans un seul & même milieu, *Catoptrique*; lumiere rompue en passant d'un milieu dans un autre, *Dioptrique*. C'est à l'*Optique* qu'il faut rapporter la *Perspective*.
>
> La *quantité* considérée dans le son, dans sa véhémence, son mouvement, ses dégrés, ses réflexions, sa vîtesse, *&c.* donne l'*Acoustique*.
>
> La *quantité* considérée dans l'air, sa pesanteur, son mouvement, sa condensation, raréfaction, *&c.* donne la *Pneumatique*.
>
> La *quantité* considérée dans la possibilité des événemens, donne l'*Art de conjecturer*, d'où naît l'*Analyse des Jeux de hasard*.
>
> L'objet des Sciences Mathématiques étant purement intellectuel, il ne faut pas s'étonner de l'exactitude de ses divisions.

These are two rather different systems of classifications of what the 18$^{\text{th}}$ century called the mathematical sciences, or of what in the more recent picture is seen as subfields of one science of mathematics. The distinction *pure/mixed* was conceived and functioned in a different way than the distinction *pure/applied*.

Given that we find different ways of partitioning the mathematical sciences, there is perhaps no timeless *essence* of such a partitioning. What is the role of the opposition 'pure vs. applied' in one period, and the role of the opposition 'pure vs. mixed' in the other? What was (and is) the conceptual framework making either of these oppositions seem plausible? How did the former come to replace the latter? When and why did this happen?

II.

Moreover, the central notions used in making such classifications may have rather different meanings in different times and places. This can be illustrated by a brief look at a reflection on the very notion of *application* in the period in which the distinction between pure and mixed mathematics structured the large field of mathematical sciences. Indeed, in the French *Encyclopédie* the notion of 'application' was the object of a rather different epistemological consideration than the reflections usually at the basis of the modern distinction. All texts referred to in the following were authored by Jean d'Alembert. In this reflection, there are two competing tendencies at work.

In the article "Application", printed in the first volume of the *Encyclopédie*, in 1751, the notion is first introduced as a relation between any two sciences:

> Application d'une science à une autre, en général, se dit de l'usage qu'on fait des principes & des vérités qui appartiennent à l'une pour perfectionner & augmenter l'autre.
>
> En général, il n'est point de science ou d'art qui ne tiennent en partie à quelqu'autre. Le Discours préliminaire qui est à la tête de cet Ouvrage, & les grands articles de ce Dictionnaire, en fournissent par - tout la preuve. (Art. 'Application'.)

One can see from the emphasis of the last sentences that d'Alembert thinks highly of application as a tool in advancing knowledge. However, the *Encyclopédie* emphasizes symmetry: Not only can a more abstract branch of the mathematical sciences such as algebra (or analysis) be applied to a more concrete branch such as geometry, geometry can also be applied to algebra, or arithmetics. Not only can a pure branch such as analysis and geometry be applied to the mixed branch of mechanics, but mechanics can also be applied to geometry.

In addition, there is a second, related but more local notion of application at work within the sciences, d'Alembert calls it "the application of one thing to another":

> Application *d'une chose à une autre*, en général se dit, *en matiere de Science* ou d'*Art*, pour désigner l'usage dont la premiere est, pour connoître ou perfectionner la seconde. Ainsi l'*application* de la cycloïde aux pendules, signifie l'usage qu'on a fait de la cycloïde pour perfectionner les pendules, *Voyez* Pendule, Cycloïde, *&c.* & ainsi d'une infinité d'autres exemples. (Art. 'Application'.)

As before, there is potential symmetry in this relation, and there is no intrinsic structure of what can be applied to what, it is just a matter of useful practice for advancing knowledge.

In contrast to the above, we find a different line of thought which is less symmetrical and linked with the distinction between pure and mixed mathematics: While in the genealogy of human knowledge (outlined e.g. in the *Discours préliminaire* of the *Encyclopédie*) abstraction bringing the fields of knowlegde into an ordered sequence ranging from medicine and agriculture via experimental physics to algebra (=analysis), 'application' makes more abstract sciences useful to more concrete sciences. In that respect, abstraction and application function as complements to each other, and whereas in the field of mathematical sciences, abstraction moves up from mixed to pure mathematics, application is the guarantee that the purest areas (geometry, arithmetics, algebra=analysis, in that order) will be *useful* for the more concrete fields, and thus in the last consequence for human life. The point here is: Abstraction and application are linking mathematics to a whole world of *sciences* and *arts* beyond mathematics. D'Alembert is trying to make an argument why abstract mathematics belongs *into* this world, and is *helpful* for it.

However, this view was not uncontested among the encyclopedists, see the following criticism by Diderot, taken from his *Pensées sur l'interpretation de la nature*, published 3 years after vol. 1 of the *Encyclopédie*, which reads just as another addition to the entry *Application*:

> Une des vérités qui aient été annoncées de nos jours avec le plus de courage et de force, qu'un bon physicien ne perdra point de vue, et qui aura certainement les suites les plus avantageuses, c'est que la région des mathématiciens est un monde intellectuel, où ce que l'on prend pour des vérités rigoureuses perd absolument cet avantage quand on l'apporte sur notre terre. On en a conclu que c'était à la philosophie expérimentale à rectifier les calculs de la géométrie, et cette conséquence a été avouée, même par les géomètres. Mais à quoi bon corriger le calcul géométrique par l'expérience? N'est-il pas plus court de s'en tenir au résultat de celle-ci? d'où l'on voit que les mathématiques, transcendantes surtout, ne conduisent à rien de précis sans l'expérience; que c'est une espèce de métaphysique générale où les corps sont dépouillés de leurs qualités individuelles; et qu'il resterait au moins à faire un grand ouvrage qu'on pourrait appeler l'Application de l'expérience à la géométrie, ou Traité de l'aberration des mesures.

Note that this criticism again plays with the idea of symmetry in the notion of application! Indeed d'Alembert as well has a number of reflections on mistaken "applications" of mathematics to other sciences. For instance, there is the use of the "geometric method" in metaphysics, an application he views as more or less absurd:

> Plusieurs ouvrages métaphysiques, qui ne contiennent souvent rien moins que des vérités certaines, ont été exécutés à la maniere des Géometres; & on y voit à toutes les pages les grands mots d'*axiome*, de *théorème*, de *corollaire*, &c. – Les auteurs de ces ouvrages se sont apparemment imaginés que de tels mots faisoient par quelque vertu secrete l'essence d'une démonstration, & qu'en écrivant à la fin d'une proposition, *ce qu'il falloit démontrer*, ils rendroient démontré ce qui ne l'étoit pas. (Art. Application)

Secondly, there is the problem of applying geometry to areas of experimental physics where no substantial mathematization is available (at least for the time being). This "abuse" is related to the making of unjustified hypotheses:

> Il faut avoüer cependant que les différens sujets de Physique ne sont pas également susceptibles de l'*application* de la Géométrie. Plusieurs expériences, telles que celles de l'aimant, de l'électricité, & une infinité d'autres, ne donnent aucune prise au calcul; en ce cas il faut s'abstenir de l'y appliquer. Les Géometres tombent quelquefois dans ce défaut, en substituant des hypotheses aux expériences, & calculant en conséquence: mais ces calculs ne doivent avoir

> de force qu’autant que les hypotheses sur lesquelles ils sont appuyés, sont conformes à la nature; & il faut pour cela que les observations les confirment, ce qui par malheur n’arrive pas toûjours. D’ailleurs quand les hypotheses seroient vraies, elles ne sont pas toûjours suffisantes. S’il y a dans un effet un grand nombre de circonstances dûes à plusieurs causes qui agissent à la fois, & qu’on se contente de considérer quelques - unes de ces causes, parce qu’étant plus simples, leur effet peut être calculé plus aisément; on pourra bien par cette méthode avoir l’effet partiel de ces causes: mais cet effet sera fort différent de l’effet total, qui résulte de la réunion de toutes les causes. (Art. Application, a similar passage appears in the *Discours Préliminaire.*)

Here, d’Alembert is close to admitting that the criticism by Diderot, quoted above, has some substance, at least in certain areas of experimental science. There are even are certain areas of physical study in which the abuse of mathematics is ending up close to charlatanry:

> Il faut avoüer pourtant que les Géometres abusent quelquefois de cette application de l’Algebre à la Physique. Au défaut d’expériences propres à servir de base à leur calcul, ils se permettent des hypothèses les plus commodes, à la vérité, qu’il leur est possible, mais souvent très - éloignées de ce qui est réellement dans la Nature. On a voulu réduire en calcul jusqu’à l’art de guérir; & le corps humain, cette machine si compliquée, a été traité par nos Medecins algébristes comme le seroit la machine la plus simple ou la plus facile à décomposer. C’est une chose singuliere de voir ces Auteurs résoudre d’un trait de plume des problèmes d’Hydraulique & de Statique capables d’arrêter toute leur vie les plus grands Géometres. (*Discours Préliminaire.*)

III.

Let us return to the topic of our meeting. Comparing the modern idea of applications of mathematics, and of the distinction between pure and applied mathematics, with earlier notions such as the ones sketched above (which, by the way, are rather specific to the encyclopedist project and not shared throughout the early modern period, or throughout Europe even in the 18^{th} century) raises a general issue which we hope to pursue during this week. There might even be a future in which distinctions between pure and applied (or pure and mixed, for that matter) will loose their relevance for structuring the field of mathematical knowledge. Again this could happen in rather different ways. One, for instance, might be to just drop the idea of a *pure* field of mathematical science. All we would be left with, then, would be applications... In this connection, compare the EU’s new policy description HORIZON 2020 – a new horizon for placing mathematics in a world of ‘useful’ science? The striking observation here is that many of the classical disciplines of science, including mathematics, are not explicitly mentioned in the policy descriptions. Have we ended up in a world of applications?

Indeed, in our discussions of pure/mixed and pure/applied we should be aware of potentially changing hierarchies and valuations. A few years ago, Paul Forman has published a long article elaborating what has come to be called the *second Forman thesis* [1]. In this article, Forman claims (and deplores) that the traditional ranking of science *above* technology (and of pure mathematics *above* applied mathematics) that dominated scientific attitudes for a long time in the modern era has reversed a few decades ago. Now, technology (understood in a very broad way, including social technologies) is leading the hierarchies of knowledge, and science has become subordinated to their advancement. While Forman's material for making this claim are mostly writings on technology (and, in particular, histories of technology) he might indeed have a point. HORIZON 2020 almost looks as if it was a confirmation of his thesis. Thus, if we are discussing the changing ways in which mathematics was related to the 'real world', to physical nature and to society, we should not forget that we are at the same time discussing a small, but not irrelevant part of the question of which place has been, and should be given to science in the world in which we live.

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[1] Paul Forman: "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology." *History and Technology* 23 (2007), pp. 1–152

The idea of mathematical models and modelling in 20th century

Tinne Hoff Kjeldsen

Mathematical modelling gained importance in a wide variety of areas in the 20th century. It has been perceived of as having induced a new (or a change in) scientific practice in these areas. I will give two examples of research from the first half of the 20th century that reflect the emergence of the modelling approach, one in economics and one in biology.

In economics, the emergence of mathematical modelling has been linked to the modern axiomatic approach in mathematics. Research that developed in ways that reflect the modelling approach is found in John von Neumann's work on game theory and economic behaviour. In their joint book, he and Morgenstern explained their approach to modelling [7, p. 33]:

> At this stage the reader will observe a great similarity with the everyday concept of games. We think that this similarity is very essential; indeed, that it is more than that. For economic and social problems the games fulfill – or should fulfill – the same function which various geometrico-mathematical models have successfully performed in the physical sciences. Such models are theoretical constructs with a precise, exhaustive and not too complicated definition; and they must be similar to reality in those respects which are essential in the investigation at hand. To recapitulate in detail: The definition must be precise and exhaustive in order to make a mathematical treatment possible. The construct must not be unduly complicated, so that the mathematical treatment can be brought beyond the mere formalism to the point where it yields complete numerical results. Similarity to reality is needed to make the operation significant. And this similarity must usually be restricted to a few traits deemed "essential" pro tempore – since otherwise the above requirements would conflict with each other.

Besides illustrating the modelling approach, von Neumann’s work on game theory and economics also illustrates – as stated in the extended abstract for this workshop – that “the very notion of the ‘application’ or ‘use’ of ready-made mathematical methods and knowledge to extra-mathematical domains is problematic; in fact in many cases mathematical methods emerged from interactions with such domains, thereby changing and challenging the existing ideas about mathematics”. The areas in the Mathematical Subject Classification with index 90 (operations research, mathematical programming) and 91 (game theory, economics, social and behavioral sciences) developed as a result of mathematizing games of strategy, economic behaviour and logistic planning. Linear programming, e.g., grew out of the work with the development of programming models in the U.S. Air Force. In 1948 a research program was set up to explore its connection with game theory, and the underlying mathematical structure, creating new research areas in mathematics (index 90 and 91) and influencing new developments in ‘pure’ mathematics [3].

An early example of research that reflects the modelling approach in biology can be found in Nicolas Rashevsky’s paper from 1934 “Physico-Mathematical Aspects of Cellular Multiplication and Development” [4], which he presented to biologists at a Cold Spring Harbor symposium. He used the term ‘physico-mathematical’ which was used to indicate investigations of the cause of a phenomenon. Rashevsky was investigating the cause of cell division. He singled out one feature that is present in all cells (metabolism) and examined whether this process could be the cause of cell division. His strategy was to calculate the forces acting on a unit volume that are produced in an idealized (homogenous and spherical) cell by a gradient of concentration of a substance. He was able to deduce, that when the radius of such a spherical cell reaches a critical seize, division of the cell will cause a decrease of free energy. Nature favors configurations with smallest possible value of free energy, so Rashevsky was, as he wrote: “tempted to infer that therefore division of a cell will occur spontaneously as soon as, ... the cell will exceed the critical seize”. The biologists were very critical and hostile towards Rashevsky’s approach [2]. They criticized what we would call Rashevsky’s modeling process. They wanted to know the ‘model’s’ relation to nature: “What is the nearest example in nature to this theoretical case?”, they asked, arguing that: “ ... it (the ‘model’) doesn’t help as a general solution because a spherical cell isn’t the commonest form of cell” [4]. This discussion between Rashevsky and the biologists illustrates an important epistemological issue, the question of how scientists and mathematicians come to an agreement about what counts as knowledge in such uses of mathematics.

The migration of mathematics into biology was slower than in economics, but its significance has been growing during the past decades. The Society for Mathematical Biology was founded in 1973 and The European Society for Mathematical and Theoretical Biology was founded in 1991. Mathematical modelling has introduced a new scientific practice in biology. In 1989 the mathematician Pedro Miramontes from the biomathematics group at the National Autonomous University of Mexico used the term *in silico paradigm* for a new kind of experiments in the life sciences that are carried out entirely within a computer.

The increasing use of mathematical modelling in scientific practices raises the question of what are the function and status of mathematical modelling in the production of scientific knowledge. How is "knowledge" negotiated between disciplines? How do mathematicians and scientists build mathematical models, how do they learn from mathematical models, how do they argue with them, how do they integrate knowledge across the disciplinary boundaries of mathematics and the extra-mathematical domain, how do they validate models, how does mathematical modelling interact with the production of mathematical knowledge, how does it change and challenge existing ideas about mathematics? To deal with such questions, Amy Dahan concludes from her study in the history of meteorology:

> In order to understand how scientific practices and knowledge results relate to each other, the notion of model needs to be historicized through a study of its workings and functions in different historical configuration of scientific research. It also needs to be subjected to sociological analysis: modeling activities should be reinserted into their institutional, technical, and political environments, without separating the cognitive from the social elements combining within each model. We need to pay attention to the actors – the researchers, engineers, and users of models – and to the actions themselves. [1, pp. 126-127]

As it was pointed out in the extended abstract of this workshop, by focusing on such historical studies, we can gain insights which can be helpful in contemporary discussions about the social role of mathematics and the function (action) of mathematical modelling in other fields. Such issues are also discussed in *Critical Mathematics Education*. As pointed out by Ole Skovsmose [6], we have new sites for knowledge production. We find a variety of companies, institutions and organizations involved in knowledge production – e.g. pharmaceuticals, biotechnology, etc. – sites that can be overloaded with different business interests. The social role of mathematics and the function and status of mathematical modeling in other areas, challenges the assumption of neutrality of mathematics and science, inherent in traditionally science and mathematics education. An important concern in critical mathematics education is to reflect upon and criticise mathematics in its variety of forms of actions [5], to show how power and mathematics come together.

In discussing the changing ways in which mathematics was related to the 'real world' in the context of mathematical modelling and its role and function in scientific practice, technology and society – it seems to be the case that as mathematics becomes more important its role seems to become less visible – a point that is reflected in the recent EU policy for research and innovation, Horizon 2020. Mathematics and mathematical modelling will be a key component of many of the areas of expertise in the call, though without being mentioned explicitly.

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“Of a Gardiner, and how he is to be qualified”: Landscape design and the early modern mathematical sciences

Volker Remmert

The *scientiae* or *disciplinae mathematicae* were generally subdivided into *mathematicae purae*, dealing with quantity, continuous and discrete as in geometry and arithmetic, and *mathematicae mixtae* or *mediae*, which dealt not only with quantity but also with quality – for example astronomy, gnomonics, optics, music, the science of waters and architecture. The Jesuit Gaspar Schott enumerated almost thirty fields among the *mathematicae mixtae* in his *Cursus mathematicus* of 1661. Schott’s division is in the tradition of Clavius who in his very influential Euclid commentary of 1574 discusses the division of the mathematical sciences (*disciplinarum mathematicarum divisio*) and explicitly uses mixed mathematics (*mathematicae mixtae*) as a category [3].

What is it that you do in a garden?

The garden entertains and delights by means of the mathematical sciences, mostly mixed: music and acoustic effects (echo), fountains and sundials, geometric forms, automata and many more things that could be found in early modern gardens would be part of a thorough or encyclopedic course of the mathematical sciences [1].

In the preface of the Mario Bettini’s (S.J.) *Apiaria universae philosophiae mathematicae* (3 vols., Bologna 1642) a forceful metaphor comes up: the *hortus mathematicus*, the garden of the mathematical sciences. This metaphor is more than a typical blossom of early modern rhetoric and iconography aiming at the legitimization of the mathematical sciences. Rather the *hortus mathematicus* is perceived of as a field where the theory and practice of gardening and the mathematical sciences interact and all the things you need to create gardens and be entertained in them are covered by the mathematical sciences: practical geometry, architecture, perspective, optics, music, etc. The garden was a location to be entertained in, but simultaneously a place of the mathematical sciences, which were used as well for the creation of the garden as the recreation in the garden.

The mathematical sciences and gardening/landscape design

In the 17^{th} and early 18^{th} century authors writing on gardening often stress the importance of the mathematical sciences for gardening. The French Royal gardener Jacques Boyceau, for instance, in his *Traité du Jardinage* (Paris 1638) demanded that young gardeners be thoroughly instructed in geometry, architecture, arithmetic and perspective. By training young gardeners in the arts and in the relevant mathematical sciences Boyceau wished to emancipate gardening from the crafts and raise it to the status of an art. His programme was very successful and had an important impact on young gardeners as, for instance, André Le Nôtre, the creator of the gardens of Versailles.

Between 1600 and the mid-18^{th} century practitioners of the mathematical sciences and of gardening and landscape design shared the conviction that they could to a certain extent control and dominate nature. And, indeed, the methods and knowledge of the mathematical sciences opened up new ways to do so. Such new options deeply affected the realm of landscape design and gardening in various ways and thus directly reached into the political sphere by offering new possibilities and forms of representation – and not only in the gardens of Herrenhausen or Sanssouci where Leibniz and Euler were drawn into the international fountain competition. The art of gardening and landscape design was, perhaps, the most prominent representative of the early modern urge to dominate nature. From this perspective an alliance with the mathematical sciences seems natural.

To design a garden, theoretical decisions have to be made concerning the design and the organization of space, the planting, the features and the artistic equipment of the garden. Thus knowledge of geometry is indispensable. Also knowledge of architecture is a must in order to design a garden (fountains, pavilions, orangeries etc.) as well as familiarity with perspective and acoustics (artificial echos). All these, geometry, architecture, perspective, acoustics, belong to the mathematical sciences.

In practice or rather in the field, before planting and equipping the gardens, challenges have to be met which also require knowledge of the mathematical sciences: the grounds have to be surveyed and to be modified according to the design. This task was often entrusted to military engineers who had the necessary expertise in changing landscapes and moving vast amounts of soil [4]. The theoretical knowledge needed as well as the familiarity with mathematical instruments are typical of fortification, geodesy and practical geometry – again parts of the mathematical sciences.

On a more theoretical level one of the most determined propagators of the use of the mathematical sciences in gardening was the English virtuoso John Evelyn (1620–1706). Beginning in the 1650s he became a key figure for the introduction of Continental writings on gardening to England [2]. In his *Elysium Britannicum, or*

The Royal Gardens [5] Evelyn pursued the somewhat ambitious goal to turn the art of gardening into a scientific enterprise. For this purpose he drew on the whole range of the mathematical sciences (geometry, optics, astronomy and astrology, perspective, architecture, acoustics, the science of waters etc.). He refers to the volumes of contemporary practitioners of the mathematical sciences as Bernard Lamy, Salomon de Caus, Marin Mersenne and William Oughtred, but above all to the heavy Latin folios of Jesuits authors such as Athanasius Kircher, Gaspar Schott or Mario Bettini. The Jesuit publications were essential for the teaching of the mathematical sciences in their whole range and naturally also covered the practical branches, which were equally relevant for landscape design as for fortification and military engineering. This corresponds well to the important role that Jesuit colleges had in the teaching of mixed mathematics.

During the 17th century in order to learn the necessary techniques landscape designers and gardeners often relied on the wide-spread introductory texts written for young military officers, such as Sébastien Leclerc’s *Pratique de la geometrie* (Paris 1669, 1682, 1691) or Alain Manesson-Mallet’s *Géométrie pratique* (4 vols., Paris 1702). However, in the 18th century authors writing on gardening would include the elements of geometry which they deemed necessary in their works (Dézallier d’Argenville in his *Théorie et Pratique du Jardinage* of 1709, Switzer in his *Ichnographia Rustica* of 1718 and Langley in his *New Principles of Gardening* of 1728).

Concluding remarks

A detailed analysis of the relationship between landscape design and the mathematical sciences in the early modern period has not yet been undertaken. It is obvious that there is a vast field of research to be done on the intersection of history of mathematics, science and technology and the history of landscape design and gardening. To embrace this field would mean to tackle some of the following questions:

(1) Who were the individuals or groups concerned with landscape design and gardening and the relevant forms of knowledge from the mathematical sciences?
(2) Who codified the relevant forms of knowledge? What are the reasons and what are the forms of communication involved?
(3) What role do the mathematical sciences, new instruments and new technological developments play in the art and practice of landscape design? And how does the interaction feed back into the mathematical sciences?

All these questions and topics are closely connected to the more general trends of scientization and mathematization that have shaped European societies since the early modern period [6].

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An attempt to characterize what was termed Messung and considered mathematical practice in 16th century mathematical treatises

Jeanne Peiffer

The purpose of this talk was to question some 16th century practices situated at the nexus of geometry, optics, perspective and instrument making, in order to give an idea of the use of geometry in arts and crafts. The nexus seems to be best described for the German area by the polysemic "Messung", introduced by Albrecht Dürer in his famous *Underweysung der messung* ([Dürer 1525, Strauss 1977, Peiffer 1995]). This term points to a constructive geometry, without demonstrations and measurements. Starting with the example of a skew curve, a conical helix seated on a cylindrical one [Dürer 1525, Book I, fig. 15], which has been interpreted by 20th century historians of mathematics ([Amodeo 1908, Taton 1954]) as an early occurrence of descriptive geometry, I have shown that Dürer makes a distinction between an abstract curve represented by two projections on two mutually perpendicular planes on one hand, and a mathematical description of a spiralling staircase in a conical tower on the other. In this latter case, and in others belonging to the architectural context of Book III of his *Underweysung*, like the construction of a round twisted column [Dürer 1525, Book III, fig. 9-10], Dürer alters the representation of the curve in order to conform to the experience of steps being smaller and steeper when climbing up in the tower. Dürer offers a mathematical construction that allows to obtain the desired effect. It is based on the 4th postulate of Euclidean optics (magnitudes seen under equal angles are perceived as equal), well known from the use Vitruvius has made of it for the purpose of optical corrections. When the curve is associated with architecture, it is perceived in a physical space, which is also visual. Dürer is however silent on the link to vision which is made only later in book III (fig. [28] concerning the proportioning of letters in inscriptions high up on buildings). One possible ex planation for Dürer's silence is that he learned the technique in a workshop environment and applied it tacitly [Peiffer 2004]. This hypothesis is confirmed by Daniele Barbaro, who in his *La pratica della perspettiva* (1568), commented on Dürer's "artificio" or "instrument" and established a double link between painters' and architects' knowledge on one hand and Euclid's optics on the other. Some of Dürer's Southern German followers identified "kunst des messens" with perspective, which relates in this context to optics. Augustin Hirschvogel for instance speaks in his *Geometria* (1543) of "dise edle und Nützliche kunst des messens (Perspectiva in Latein genant)" [Hirschvogel 1543, dedication].

To sum up, Dürer's procedure can be described as an intermingling of geometrical constructive methods, optics, workshop techniques and (simple) mathematical instruments. This is reflected in the following quote from Johann Stoeffler's *Von künstlicher Abmessung*: "Der nutz unnd dienstbarkeit diser kunst [Geometria]/würt durch erfarung und brauch bekant...Fürter bringt sie herfür vil künst/die handtwirckung unnd Perspective damit sie zu menschlichem brauch dienlich" [Stoeffler 1536, p. $\mathrm{A_{ij}}$].

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How relevant is the category of 'mixed mathematics' to the sixteenth century?

Jim Bennett

As someone working in the history of practical mathematics and mathematical instruments, I have often come across the assumption that the term 'mixed mathematics' is relevant to my research. I have been advised by editors and referees to include references to this category in publications, so that readers will appreciate the character of the mathematics about which I am writing. I have resisted doing so, because I do not come across the term in the sources I use, which has made me reluctant to reinforce an assumption that might be surviving merely by being passed on from one historian to another. I did not see that the people I was writing about used this term, or, more importantly, subscribed to the assumptions that lay behind it.

The category of 'mixed mathematics' was of course used in periods in the past, particularly in the 18th and 19th centuries, and my comments refer only to the early period covered by the workshop, the 16th century in particular. In fact discussions in the workshop itself revealed that my concerns must be restricted further: to the practical mathematical tradition and perhaps especially, though not exclusively, to mathematical practice in England. Among Jesuit mathematicians, for example, we learned that the category is commonly used. Nonetheless, even if my strictures have a more limited application, it is important to be careful not to import assumptions unthinkingly into areas of practice where they are not used.

An earlier study ([1]) found no instances of the term 'mixed mathematics' before 1600 and traced its origins in English to Francis Bacon, *Of the Proficiency and Advance of Learning*, published in 1605. Those results were effectively challenged in the workshop, but they might still be helpful in formulating a qualified pattern for the term's occurrence. There is in fact a conspicuous early instance of the use of 'mixed' in the English mathematical literature not mentioned by Brown, and it is dealt with here more to set it aside as something different and distinctive than as a case that qualifies his account in a substantial way.

John Dee's well-known preface to the first English edition of Euclid's *Elements*, published in 1570, was accompanied by a 'groundplat' or diagram, setting out the relationships between the mathematical sciences. For both the 'principal' mathematical, sciences, arithmetic and geometry, Dee distinguishes between 'simple' and 'mixt', but he is referring to mixtures between the sciences themselves. So 'mixt arithmetic' is a mathematical science where geometry is used within arithmetic, a science 'which with aide of Geometrie principall, demonstrateth some Arithmeticall Conclusion, or Purpose.' Similarly mixed geometry accepts the aid of arithmetic; for Dee, Euclid's *Elements* is itself an example of mixed geometry. This has nothing to do with what historians tend to call 'mixed mathematics' in the period and the many 'Artes Mathematicall Deriuatiue', as Dee calls them, appear in a separate branch of his 'groundplat'. It is worth noting that Dee considers the term 'mixt' a sufficiently unoccupied descriptor in the landscape of the mathematical sciences that he is free to adopt and use it in the way he does.

There have been suggestions in the secondary literature that Bacon's use of 'mixed mathematics' drew on Aristotle's notion of 'subordinate' sciences and on the development in medieval Aristotelian thought of the notion of 'scientiae mediae' – sciences such as optics, measurement and perhaps astronomy, that sit between mathematics and physics. A problem with that is that in the medieval organisation of knowledge and hierarchy of disciplines, mathematics is considered inferior to physics, so for a science to be 'scientia media' involves an aspiration to a *higher* status than mathematics by partaking of some of the qualities of physics, with its accounts of the nature and causes of things. Most historians who invoke the mixed mathematics category imply that pure mathematics is being compromised or qualified by an engagement with the mechanical world. For Bacon the ‘mixing’ involved in mixed mathematics is with natural philosophy, but this is not aspirational on the part of mathematics, since he ranks mathematics alongside physics and metaphysics. For him mathematics can be a tool for certain branches of natural philosophy, improving its performance in invention, demonstration and use.

This is not at all in line with what we find in practical mathematics, where engagement with natural philosophy is rare and generally considered not relevant to mathematical practice. It is almost always the case in the 16th century that the practical mathematical arts and sciences, where mathematics is put to use and becomes a sphere of action with a disciplinary range of individual sciences, has nothing to do with natural philosophy.

Bacon’s meaning of ‘mixed mathematics’ can be found in 17th-century English writers, such as John Wilkins, Robert Boyle and Ralph Cudworth, for whom Bacon was a very useful figurehead with polemical value to experimental philosophers, particularly within the Royal Society, but who sat outside mathematical practice. The first examples of ‘insiders’ using the term come towards the end of the 17th century – William Molyneux and William Whiston – but by then Newton’s *Principia* was an example for the kind of mathematics they would have seen as ‘mixed’.

Reverting to the 16th century, we should be cautious about using ‘mixed mathematics’ in that context because it could well be misleading. If it assumes that some ‘mixing’ is happening, what are the ingredients being mixed? In practical mathematics, techniques of geometry and arithmetic are not being invoked in accounts of the natural world, so any mixing is not with natural philosophy. Instead they are made relevant to the artificial world, where man and not God is the inventor and the other ingredient in the mix, along with pure mathematics, would be the practical arts.

The vigorous development of practical mathematics in the 16th century certainly involved the use of geometrical techniques to achieve material ends in a growing range of mathematical arts – from astronomy, navigation and surveying to cartography, warfare, horology, architecture, drawing and painting in perspective. Mathematical instruments were ubiquitous in these developments. The mathematicians who were carrying them forward did not think of themselves as applying an independent or self-sufficient domain of pure mathematics, finding content pre-existing in this distinct domain that could be used for their more mundane problems and *mixing* it with practical techniques. It would be more in keeping with their attitudes to say that there was a set of techniques or resources that characterised mathematical practice (and these included instruments) that could be modified, developed, extended, revised, augmented and used, and that constituted the disciplinary core of their practice. They thought of new areas of use simply as further instances of their work as mathematicians. They were aware that there was what was called ‘speculative’ mathematics, where developments were not worked out through challenges thrown up in the practical arts, but did not see their procedure as mixing this with mechanical or material practice.

The more complex instruments of the period and the books published to explain their 'construction and use', in the common organisation adopted in such works, provide instructive examples of how the practitioners approached their discipline. The example taken in the talk was the Regiomontanus sundial; there is insufficient space to deal with it here, but see [2].

The dial illustrates the contemporary use of the 'theoric', where a systemic technique (most often a geometrical construction or projection) could deliver a much wider range of data than were used in its creation. A map drawn to scale, for example, was such a device. A theoric was largely devoid of natural philosophical content, just as a map could take a variety of forms, according to the projection being used, the test of success being its usefulness for a particular purpose, such as Mercator sailing, rather than its fidelity to the nature of things. Theorics are not 'mixed' through an engagement with natural philosophy, in the sense used by Bacon, but neither are they 'mixed' because a kind of 'pure' mathematics is an ingredient in their construction. In 16th-century accounts of the Regiomontanus dial, as with other mathematical instruments, the reader is told in considerable detail, step by step, the procedure for the construction, but is not offered the geometrical proof a modern mathematician might expect to establish the instrument's legitimacy. It is typical of this genre of mathematical practice that legitimacy comes through the skilful use of a set of geometrical techniques, accepted within mathematical practice. This is a genre of operative mathematics, using protocols of practice rather than regimes of proof, and to apply the descriptor 'mixed' to it endangers our appreciation of this essential characteristic.

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Tobias Mayer's use of Observations

Steven Wepster

The lunar tables of Tobias Mayer of 1753 and 1762 were of epoch-making accuracy and made the determination of longitude at sea feasible. I show that they incorporate elements of an outdated lunar motion model going back to Jeremiah Horrocks of 1640. I also show how Mayer adjusted the coefficients on which the tables depend. For this, he used an iterative technique with a high ad-hoc nature, which I reconstructed from certain undocumented papers, very similar to modern spreadsheets, in his archives. Dynamical lunar theory is a much less important factor in the success of the tables than has been supposed until recently. Mayer also proposed to study world temperature distribution by a similar procedure of model fitting as he had used for the lunar tables. His extensive use of overdetermined systems for parameter fitting was not universally accepted but it clearly showed what an ‘engineering approach’ could accomplish when a more theoretical approach failed.

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Different ways of mathematizing water powered machines in the 18th and early 19th century

Gerhard Rammer

At the beginning of the 18th century, Antoine Parent developed the first theory of water wheels by mathematizing the dynamics of the operating wheel. He established his theory via application of the newly invented differential calculus and could then use it to calculate the optimal operation conditions for the wheel, i.e., its velocity and the load it could lift. While the theory did not provide any information about design details, and despite its results seeming dubious, it was still highly praised as an important achievement because of its mathematical method.

During the entire 18th and the first half of the 19th century, many attempts were made to find an appropriate mathematization of water wheels. The theory for undershot wheels had remained a cumbersome endeavor for a long time while the theory for overshot wheels had soon been established and their overall dimensions could be calculated. But there was still need of a theory that also gave hints on the design details. In 1809, Franz Josef von Gerstner published his idea on how rules could be given for every design parameter for overshot wheels, based strictly on mathematical methods. Although his copious calculations led to complex systems of equations that could not be solved and had thus to be simplified, the mathematical method itself was highlighted in order to raise the value of the results, a rhetoric that was common in the engineering literature of the time. On that note it is also interesting to point out that the engineering approach of this time was often purely theoretical and did not rely on empirical data.

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The Decline of 'Mathesis mixta' in Rational Mechanics and its Philosophical Implications, 1788–1869

Helmut Pulte

In the 17th and 18th century, rational mechanics was widely understood as part of 'mathesis mixta', i. e. as an integral part of mathematics that has to do with numbers, magnitudes, figures, formulas etc. that refer to concrete physical objects. Under the premise of different paradigms of mechanism, the primary aim of rational mechanics was to uncover the primary laws of motion of material particles. Motion itself being regarded as a genuine mathematical concept, mechanics was, first of all, understood as a mathematical science. 'Mathematical' in this sense does not mean 'mathematics applied to science' but rather 'science, having essentially to do with mathematical entities'. Being a deductively organized part of mathematics, it seemingly participated from the evidence and certainty that was ascribed to mathematical knowledge in general.

Due to internal and (even more) external reasons, this understanding of rational mechanics changed dramatically in the course of the 19th century: Rational or – to use the modern term – theoretical mechanics developed into a discipline that was primarily understood as a part of physics to which certain mathematical methods and concepts were 'applied'. One important consequence of this process was that 'first principles' or 'axioms' of mechanics lost their status as *both* mathematical *and* empirical truths. This dissolution paved the way for a modern understanding of mechanics as a fallible empirical science. In turn, the development in question was a historical precondition of the later 'revolutions' in theoretical physics.

The talk outlined this 'meta-theoretical' change in rational mechanics – which can be described as a decline of 'Euclideanism' in I. Lakatos' sense – and discussed its main causes. The starting point was Lagrange's *Méchanique Analitique* (1788) and its terminal point was C. Neumann's *Principien der Galilei-Newton'schen Theorie* (1869), which was – due to its criticism of the basic principles of mechanics in conjunction with its criticism of Newtonian absolute space – arguably the 'beginning of the end' of classical mechanics.

A careful historical analysis of this general change reveals some remarkable cornerstones which deserve to be mentioned in detail: *First,* already Lagrange's *Méchanique Analitique*, which aimed at an integration of the results of different research programs of the 18th century, had to pay a high prize for its unification efforts: Its principles became formal axioms of science rather than laws of nature. The rise of these principles was accompanied by a 'semantic unloading' of their basic mathematical concepts (like moment, action or potential). The basic problem Lagrange had to face was that this formalisation led to a conflict with the traditional meaning of 'axioms' as true and self-evident first propositions, which are *neither* provable *nor* in need of a proof. While he first stuck to this understanding, he later had to admit that his first principle (namely that of virtual velocities) lacks one decisive characteristic of an axiom in the traditional meaning,

i. e. that it was 'not sufficiently *evident* to be established as a primordial principle'. He and other mathematicians (like Fourier, Laplace, Poisson, Poinsot and Ampère) attempted to prove this 'axiom' – a process indicating a degeneration of traditional mechanical Euclideanism to 'Rubber Euclideanism'.

Second, within the German reception of Lagrange's mechanics and French mathematical physics in general, a new understanding of mathematics as a 'pure' science became essential. Strongly influenced by philosophy and the broader cultural movement of Neohumanism, doing mathematics was primarily understood as a mere mental activity based on laws of thought which are independently from any experience or empirical intuitions. This conception first brought about a genuine problem of the applicability of mathematics to nature and articulated this problem sharply. C. G. J. Jacobi was the most important representative in this respect. In his last Berlin lectures on *Analytische Mechanik* from 1847/48, he rejected Lagrange's approach for its inability to describe the behaviour of real physical bodies and he sharply criticised his attempts to mathematically demonstrate so-called 'axioms' of mechanics: They cannot be based on unquestionable rules of thinking and mathematical deduction. At this point Jacobi – as an exponent of pure mathematics – totally dismissed Euclideanism as an ideal of empirical *science* in general: The formal similarity between the deductive system of analytical mechanics and systems of pure mathematics (like number theory) *must not* lead to the false belief that both theories meet the same epistemological standards. Jacobi was the first representative of the analytical tradition who saw and drew this consequence. According to his view, mathematics offers a rich supply of possible first principles, and neither empirical evidence nor mathematical or other reasoning can determine any of them as true. Empirical confirmation is necessary, but can never provide certainty. First principles of mechanics, whether analytical or Newtonian, are not certain, but are only probably true. Certainty of such principles, a feature of mechanical Euclideanism, cannot be achieved. Moreover, the search for proper mechanical principles always leaves space for a choice between different alternatives. Jacobi, well educated in classical philology and very conscious of linguistic subtleties, consequently called first principles of mechanics 'conventions', exactly 50 years before H. Poincaré did. According to his understanding of rational mechanics, mathematics is applied to empirical reality, and not 'mixed up' with it. It is here, within mathematics, where the conventional character of the principles has to be located, because free mathematical invention offers more possibilities than nature can realize.

Third, mathematicians like B. Riemann and C. Neumann picked up Jacobi's criticism of traditional Euclideanism in rational mechanics and developed it further. Neumann's Leipzig inaugural lecture *Ueber die Principien der Galilei-Newton'schen Theorie* from 1869 is very significant in this respect: Like K. R. Popper about six decades later, he described even the basic principles of mechanics (as the law of inertia, for example) as 'neither true nor probable'. They, too, are 'arbitrary' and 'moveable' mathematical hypothesis; they can always be overthrown by further developments. Modern fallibilism lurks around here. In this context, the methodological meaning of his famous 'body Alpha' becomes relevant: Neumann decomposed Newton's law (or hypothesis) of inertia as an indubitable, dogmatic principle to three different propositions (existence of Alpha, rectilinearity, uniformity), which together form the empirical content of this law. His explication of the different empirical attributes can be understood as a perfect example for the process of explicating conditions and establishing conventions which are typical for the rise of hypothetical thinking in general.

There are some lessons which might be learned from this 'short story' linking 1788 to 1869: The modern understanding of mechanics as a genuinely physical science should not blind us to the fact that in the 18^{th} and first half of the 19^{th} century it was, as part of 'mathesis mixta', credited with the evidence and certainty of mathematics, being *de facto* regarded as epistemologically equivalent to Euclidean geometry by nearly all scientists and most philosophers of science. Moreover, the 'top down-perspective' of the working mathematical physicist implied that the dissolution of mechanical Euclideanism and the rise of hypothetical thinking began (and *had* to begin) here, at the top: In a way, there could happen no 'bottom up'-dissolution by empirical falsifiers before. Last, in the course of the 19^{th} century, a 'shrinking-process' of mathematical evidence and certainty took place, and physical geometry as well as mathematical physics were affected by this process. The concept of *pure mathematics*, isolating arithmetic, algebra and analysis as the remaining mathematical 'paradise' of evidence and certainty from the larger area of the mathematical sciences, played a crucial role in this process. Mechanics, however, was repudiated from this paradise once and for all.

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Ballistics during 18th and 19th centuries: What kind of mathematics?

Dominique Tournès

Two recent papers ([1], [7]) have studied the scientific and social context of ballistics during and around the First World War, and have put in evidence the collaborations and tensions that have been existing between two major milieus, the one of artillerymen, that is engineers and officers in the military schools and on the battlefield, and the other one of mathematicians that were called to solve difficult theoretical problems. My aim is to give a similar survey for the previous period, that is to say during the second half of the 18th century and the 19th century.

The main problem of exterior ballistics – I won't speak of interior ballistics, which is nearer to physics and chemistry than mathematics – is to determine the trajectory of a projectile launched from a cannon with a given angle and a given velocity. The principal difficulty encountered here is that the differential equations of motion involve the air resistance $F(v)$, which is an unknown function of the velocity v. In fact, the problem is more complex because we must take into account other factors like the variations of the atmospheric pressure and temperature, the rotation of the Earth, the wind, the geometric form of the projectile and its rotation around its axis, etc. However these effects could be often neglected in the period considered here, because the velocities of projectiles remained small.

For a long time, artillerymen have made the assumption that the trajectory is parabolic, but this was not in agreement with the experiments. Newton was the first to research this topic taking into account the air resistance. In his *Principia* of 1687, he solved the problem with the hypothesis of a resistance proportional to the velocity, and he got quite rough approximations when the resistance is proportional to the square of the velocity. After Newton, Jean Bernoulli discovered the general solution in the case of a resistance proportional to any power of the velocity, but his solution, published in the *Acta Eruditorum* of 1719, was not convenient for numerical computation. After Bernoulli, many attempts have been done to treat mathematically the ballistic equation. We may organize these attempts throughout two main strategies, one analytical and one numerical.

The analytical strategy consists in integrating the differential equation in finite terms or, alternatively, by quadratures. Reduction to an integrable equation can be achieved in two ways: 1) choose an air resistance law so that the equation can be solved in finite form, leaving it to the artillerymen to decide after if this law can satisfy their needs; 2) if a law of air resistance is imposed through experience, change the other coefficients of the equation to make it integrable, with of course the risk that modifying the equation could modify also the solution in a significant way.

In 1744, D'Alembert restarts the problem of integrability of the equation. Acting here as a geometer, concerned only with progress of pure analysis, he finds four new cases of integrability. His work went relatively unnoticed at first: Legendre in 1782, and Jacobi in 1842 have found again certain of the same cases of integrability, but without quoting D'Alembert.

During the 19th century, we can observe a parallelism between the increasing velocities of bullets and cannonballs, and the appearance of new instruments to measure these velocities [2]. Ballisticians have therefore felt the necessity of proposing new air resistance laws for certain intervals of velocity [3]. Thus, certain previous theoretical developments, initially without applications, led to tables that were actually used by the artillerymen. The fact that some functions determined by artillerymen from experimental measurements fell within the scope of integrable forms has reinforced the idea that it might be useful to continue the search for such forms.

It is within this context that Francesco Siacci resumes the theoretical search for integrable forms of the law of resistance. In two papers published in 1901, he discovers ten families of air resistance laws corresponding to new integrable equations. The question of integrability by quadratures of the ballistic equation is finally resolved in 1920 by Jules Drach [5], a brilliant mathematician who has contributed much in Galois theory of differential equations. Drach exhausts therefore the problem from a theoretical point of view, but his very complicated results are greeted without enthusiasm by the ballisticians, who do not see at all how to transform them into practical applications.

Another way was explored by theoreticians who accepted Newton's law of the square of the velocity, and tried to act on other terms of the ballistic equation to make it integrable. In 1769, Borda proposes to assume that the medium density is variable and to choose, for this density, a function that does not stray too far from a constant and makes the equation integrable. Legendre deepens Borda's ideas in his essay on the ballistic question [8], with which he won in 1782 the prize of the Berlin Academy. After Legendre, many other people, for example Siacci at the end of the 19th century [9], have developed similar ideas to obtain very simple, general, and practical methods of integration.

The second strategy for integrating the ballistic differential equation, that is to say the numerical approach, contains three main procedures: 1) calculate the integral by successive small arcs; 2) develop the integral into an infinite series and keep the first terms; 3) construct graphically the integral curve.

Euler is truly at the starting point of the calculation of firing tables in the case of the square of the velocity [6]. In 1755, he resumes Bernoulli's solution and puts it in a form that will be convenient for numerical computation. The integration is then done by successive arcs: each small arc of the curve is replaced by a small straight line, whose inclination is the mean of the inclinations at the extremities of the arc. A little later, Grävenitz achieves the calculations of the program conceived by Euler and publishes firing tables in Rostock in 1764. In 1834, Otto improves Euler's method and calculates new range tables that will experience a great success, and will be in use until the early 20th century.

Another approach is that of series expansions. In the second half of the 18th century and early 19th, we are in the era of calculation of derivations and algebraical analysis. The expression of solutions by infinite series whose law of formation of terms is known, is considered to be an acceptable way to solve a problem exactly, despite the philosophical question of the infinite and the fact that the series obtained, sometimes divergent or slowly convergent, do not always allow an effective numerical computation. Lambert, in 1765, is one of the first to express as series the various quantities involved in the ballistic problem. On his side, Français applies the calculation of derivations for obtaining a number of explicit new formulas. However, he himself admits that these series are too complicated for applications.

Let us mention finally graphical approaches providing artillerymen with an easy and economic tool. Lambert in 1767, and Obenheim in 1818 have the similar idea of replacing some previous ballistic tables by a set of curves carefully drawn by points. In 1848, Didion [4], following some of Poncelet's ideas, constructs some ballistic curves that are not a simple graphic representation of numerical tables, but are obtained directly from the differential equation by a true graphical calculation.

Artillery was so the first domain of engineering science in which graphical tables, called "abaques" in French, were commonly used.

In conclusion, throughout the 18th and 19th centuries, there has been an interesting interaction between analytic theory of differential equations, numerical and graphical integration, and empirical research through experiments and measurements. Mathematicians, ballisticians and artillerymen, although part of different worlds, collaborated and inspired each other regularly. All that led however to a relative failure, both experimentally to find a good law of air resistance, and mathematically to find a simple solution of the ballistic differential equation.

Mathematical research on the ballistic equation has nevertheless played the role of a laboratory where the modern numerical analysis was able to develop. Mathematicians have indeed been able to test on this recalcitrant equation all possible approaches to calculate the solution of a differential equation. There is no doubt that these tests, joined with the similar ones conceived for the differential equations of celestial mechanics, have helped to organize the domain into a separate discipline at the beginning of the 20th century.

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Applications in the 19$^{\text{th}}$ century

Jesper Lützen

The 19$^{\text{th}}$ century saw a shift in the relation between pure and applied mathematics. In the beginning of the century the French polytechnic tradition valued applied mathematics highly and the majority of mathematical researches were closely connected with applications. Towards the end of the century German neo-humanistic ideas emphasized that mathematical research was done for the honor of the human mind. However, this tendency should not hide the fact that even at the end of the century mathematical activities continued to a great degree to be directly or indirectly inspired by applications.

While the notion of mixed mathematics was still in use at the beginning of the century, the notion of applied mathematics soon took over. However, it is not an easy task to get a precise idea of the 19$^{\text{th}}$ century meaning of this notion. In most research papers by working mathematicians it is taken as a self explanatory notion, and in encyclopedias and similar works about mathematics there is no shared agreement about the meaning of the word and the way applied mathematics differ from pure mathematics.

In his second edition of his *Histoire des Mathématiques* Montucla (1799) stuck to the traditional division between a pure mathematics dealing with abstract quantity and a mixed mathematics dealing with concrete quantities of nature. A similar distinction can be found in some early 19$^{\text{th}}$ century distinctions between pure and applied mathematics. Another way that applied mathematics was distinguished from pure mathematics was through its purpose: Applied mathematics was done in order to understand nature. Finally also the certainty of the research was used as distinguishing factor. While pure mathematics was often seen as certain knowledge, applied mathematics did not share this certainty. However, already Montucla pointed out that once the hypotheses of nature that were to play the role of basic principles in a branch of mixed (or applied) mathematics were taken as axioms the rest of the mathematical deductions acquired the same "moral certainty" as pure mathematics. This opinion was repeated by Fourier and others and led Whitehead (1940) to declare that in principle there is no such thing as applied mathematics.

When reading definitions of applied mathematics in 19$^{\text{th}}$ century encyclopedia and the indices in journals of "pure and applied mathematics", one is struck by the scientific view of applied mathematics. By this I mean that according to these works, the purpose of applied mathematics is to get to grips with the workings of physical nature, and in particular to uncover hypotheses about the causes of natural phenomena. Here, the 19$^{\text{th}}$ century saw great breakthroughs. Traditional fields like mechanics, including celestial mechanics, astronomy, optics, acoustics and probability theory (that was usually, but not always, classified among the applied mathematics) were carried to much higher levels of mathematical and physical perfection, and new fields, in particular heat theory (first heat conduction and then thermodynamics) and electromagnetism were dealt with using new mathematical techniques and theories created for this purpose.

As some of the other talks of this meeting indicate engineering applications in areas such as surveying, architecture, navigation, fortification, ballistics, chronology, geodesy, hydraulics, crystallography, mechanical engineering, and in rare cases political economy were wide spread in the 19$^{\text{th}}$ century but the encyclopedists seem to have valued such applications less than applications to scientific enquiry.

The widespread 19$^{\text{th}}$ century view that the purpose of applied mathematics was primarily to reveal the physical causes of natural phenomena shows that applied mathematics was in fact more a successor of the so called physico-mathematical sciences as it was a successor of mixed mathematics, in its original form. Indeed, according to Schuster (2012) mixed mathematics was originally considered to be a science subordinate to the science of natural philosophy. According to Aristotle, natural philosophy dealt with matter and causes but these subjects were outside the realm of mixed mathematics. Such a view of mixed mathematics was still put forward by Montucla in 1799. According to him a mathematician doing research in optics does not study the nature of light but only the geometric consequences of the simple laws of reflection and refraction.

Such an inhibiting view of what mathematics could do for the study of nature was denied by Descartes who coined the concept of physico-mathematical sciences for mathematical investigations of physical causes. This distinction between mixed mathematics and physico- mathematics can still be found in d'Alembert's paper on mathematics in the Encyclopédie but towards the end of the 18th century the notion of physico-mathematical sciences had according to Montucla become a widely used synonym for mixed mathematics. Still, one can find traces of the original meaning of the phrase well into the 19th century.

In fact Liouville used this notion in two of his early memoirs. First in a 132 page long unpublished memoir presented to the Académie des Sciences in 1830 entitled *Recherches sur la théorie physico-mathematique de la chaleur*. One of the main objects of the memoir was to study the way the law according to which heat is transmitted between two molecules. This part of the memoir was later extended to a separate (also unpublished) memoir entitled *Mémoire sur les questions primordiales de la théorie de la chaleur*. Such studies of the primordial causes of heat conduction certainly falls under the old concept of physico-mathematics so it seems that Liouville still knew the original meaning of the concept (see Lützen 1990 for a more detailed discussion of Liouville's contributions to applied mathematics).

This conjecture is corroborated by Liouville's second use of the word in his paper *Sur quelques questions de géométrie et de mécanique, et sur un nouveau genre de calcul pour résoudre ces questions* (1832). In this paper where he presented his new theory of differentiation of arbitrary (complex) order he emphasized: "The solution of most physico-mathematical problems basically depends on a question similar to those we have dealt with, namely, the determination of an arbitrary function placed under the integral sign,... Thus, the properties of differentials of arbitrary order are linked with the most tricky and most useful mathematical theories".

One of the problems that he solved with his new calculus was to derive the interaction between two infinitesimal conducting elements from experimental "facts" obtained by Ampère concerning the interaction between two finite conductors. He showed how this problem led to an integral equation for the function determining the dependence of the interaction between two conducting elements as a function of their distance, and demonstrated how this equation could be solved using his new calculus. This was probably the problem that had originally led him to the creation of his theory of differentiation of arbitrary order (usually called fractional calculus today). He had followed Ampère's lectures at the Collège de France on electrodynamics and had learned of the idea that electrodynamic phenomena could be explained as a result of elementary interactions between infinitesimal conducting elements; he seems to have invented his new calculus as a method to find such elementary interactions from empirical facts about interactions between finite systems. For this reason he considered it an important contribution to Laplace's program in physics, and it was certainly a contribution to the original program of physico-mathematics.

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Contrasting styles: Thomas Young (1773–1829) natural philosopher, and William Wallace (1768–1843) mathematician

Alex D. D. Craik

Thomas Young and William Wallace were near contemporaries, but could hardly have been more different in outlook. Yet both played a part in preparing for the revival of British mathematics that finally took place from about 1840, mainly due to graduates of the reinvigorated Cambridge University. As introduction, prevailing views of natural philosophy and mathematics are summarised, citing Charles Hutton's *A Mathematical and Philosophical Dictionary* (1795), and W.W. Rouse Ball (1888). The latter wrote: "The mathematicians of the nineteenth century... may roughly be divided into those who have specially studied pure mathematics (in which I should include theoretical dynamics and astronomy) and those who have specially studied physics..." That Rouse Ball includes "theoretical dynamics and astronomy" as branches of *pure* mathematics, seems surprising today. But his rationale was that these subjects had well-established governing equations. In contrast, subjects that involved speculation about underlying principles and equations should be regarded as "physics" or "natural philosophy." We shall see how Thomas Young and William Wallace respectively typify "natural philosophy" and "pure mathematics" in Rouse Ball's sense.

Thomas Young (1773–1829) is one of Britain's great polymaths: see Robinson (2006). He is best remembered for: The wave theory of light (interference and diffraction); Elasticity and bending of beams ("Young's modulus"); Surface tension ("Young-Laplace formula"); Accommodation of the eye and colour vision; Decipherment of the scripts of ancient Egypt. Other writings on mechanical oscillators and fluid dynamics are discussed in Craik (2010). His *A Course of Lectures on Natural Philosophy and the Mechanical Arts* (1807), given at London's Royal

Institution, emphasises practical applications and contains *no mathematics whatsoever.* According to George Peacock (1855): "If... the lectures were delivered nearly in the form in which they were printed, they must have been generally unintelligible..." However, they contain valuable physical insights, particularly on elasticity, optics and the tides.

His *Elementary Illustrations of the Celestial Mechanics of Laplace. Part the first, comprehending the first book* (1821) reworks Laplace's material to render it "perfectly intelligible to any person who is conversant with the English mathematicians of the old school only." He introduces many diagrams, and reorganises it in "Theorems", "Corollaries", "Lemmas" and "Scholia" that look back to an earlier age. In "Of the Motion of Fluids", he gives a long quotation from Poisson's *Traité de Mécanique*, that he thinks is clearer than Laplace. But, again, Young makes many insightful remarks, based on sound physical intuition rather than mathematical analysis.

Papers of 1800 and 1808 on hydraulics explore similarities between sound and light; give perhaps the first description of transition to turbulence; examine resistance of flow through tubes; and attempt a theory of the hydraulic jump. A notable 1823 encyclopaedia article on *Tides* describes observed tides around the world, and explores the analogy with the motion of a periodically-forced pendulum. On the latter, Peacock wrote: "The methods adopted here make a bold and... tolerably successful inroad upon the solution of a problem of great difficulty by means which are apparently hardly sufficient for the purpose".

The so-called *Laplace equation*, or *Young-Laplace equation* of surface tension expresses the difference Δp in pressure across a fluid interface as $\Delta p = \gamma[(1/R_1) + (1/R_2)]$ where γ is the coefficient of surface tension and R_1, R_2 are two mutually perpendicular radii of curvature of the surface. Young stated this result *entirely in words* in 1805, without proof; and Laplace gave a mathematical derivation in 1806 without mentioning Young. Young responded with unjustified and polemical criticisms of Laplace, to which Laplace later replied.

Despite his mathematical limitations and prejudices, Thomas Young made many worthy contributions to physics. His many speculations and analogies show sound physical insight, but he is a frustrating author to read. Even his friend Hudson Gurney (1831) wrote that: "... from a dislike of the affectation of algebraic formality, which he had observed in some foreign authors, he was led into something like an affectation of simplicity, which was equally inconvenient to the scientific reader."

Less well-known than Young, **William Wallace** (1768–1843) was more of a mathematician and less of a natural philosopher. Born in Dysart, Scotland, he received little formal education. But his talent was recognised by Edinburgh professors John Robison and John Playfair; and in 1803 he became a mathematical master at the Royal Military College, Marlow (later Sandhurst), alongside James Ivory and Thomas Leybourn. There, Wallace assisted with the *Mathematical Repository*, a 'popular' magazine that tried to promote the continental calculus as opposed to Newtonian fluxions. Among Wallace's contributions is a translation of a long article by Legendre. Both Ivory and Wallace also wrote for *Encyclopaedia Britannica*, prepared textbooks, and published mathematical papers. Wallace's most significant publication was the inappropriately-titled article "Fluxions" (1815) for the *Edinburgh Encyclopaedia*: this was the first treatise of differential and integral calculus to be published in the English language. Had it been issued as a separate volume, its influence would surely have been greater.

In 1819, Wallace became professor of mathematics at Edinburgh University. There, he published texts on geometry and trigonometry; revived the astronomical observatory; and invented two instruments for use in cartography and surveying. These are the *eidograph* (a copying instrument for making images of any desired size) and the *chorograph* (an instrument for establishing position on a chart from known angular measurements). The former had considerable success; but the latter did not, as it failed to replace the *station pointer* then in use. The perhaps sole surviving chorograph is described, and also some correspondence of Wallace that belongs to descendents of Wallace: the chorograph has recently been donated to National Museums Scotland in Edinburgh.

In 1832, Wallace contributed to the political debate on the Reform Bill: how to determine the relative economic importance of the various boroughs of England and Wales for taxation and electoral purposes. This is an early mathematical contribution to socio-economic theory, in which Wallace derived a simple functional relation expressing the importance of each borough in terms of its number of houses h, and the total assessed taxes t for the previous year. His model supported that proposed in the draft bill, but was inconsistent with a rival proposal.

Thomas Young was an amazingly versatile scholar, who made fundamental contributions to physics by means of inventive and fruitful analogies and insights; but his mathematical skills were limited and antiquated. On the other hand, William Wallace was one of the best British mathematicians of his day, conversant with "continental analysis" as well as traditional geometry. His interests in practical applications of mathematics drew him to astronomy and surveying, and also to a socio-political application; but, unlike Young, not to speculative "natural philosophy".

[Note: In the discussion of this paper, Andrea Bréard commented that several of Wallace's articles in *Encyclopaedia Britannica* were translated into Chinese in the 1870s, so aiding the transmission of "Western" mathematics to that country.]

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Charles Hermite between pure and applied mathematics

Catherine Goldstein

In 1885, the French mathematician Charles Hermite gathered into a book, *On Some Applications of Elliptic Functions*, [5], a long series of notes from the *Comptes rendus hebdomadaires des séances de l'Académie des sciences* that he had presented before the Academy between 1877 and 1882. In this work, Hermite studied the Lamé equation, [4]

$$\frac{d^2y}{dx^2} - [n(n+1)k^2sn^2(x,k) + h]y = 0.$$

where $sn(.,k)$ designates as usual the Jacobi elliptic function.

Gabriel Lamé had introduced this second-order linear differential equation in his study of the distribution of heat on an ellipsoid,[7, 8], and solved it only for particular values of the constant. Hermite solved it in general. He introduced new complex functions (the so-called elliptic functions of the second order) which retrospectively foreshadow Poincaré's Fuchsian functions. And alongside this equation, he also solved in a uniform way several problems arising in mechanics, such as the problem of the rotation of a body subject to no external forces around a fixed point or that of the conical pendulum.

The question I addressed in my talk is a simple one: can we, as historians, decide if this particular work belongs to pure or to applied mathematics, and how, that is, by means of which criteria?

The current (re)classification of the *Jahrbuch*'s reviews following the MSC 2000 provides a dubious answer: Hermite's book appears both in section 33E, analysis, and in the applied subsection 70–99, "Mechanics of particles and systems." However, the classifications of the time all agree in putting it in pure mathematics. For the *Jahrbuch ueber die Fortschritte der Mathematik*, it belonged in the section: "Siebenter Abschnitt. Functionentheorie. Capitel 2. Besondere Functionen. B. Elliptische Functionen", while the *Répertoire bibliographique des sciences mathématiques* (which explicitly distinguished pure from applied topics) invented a special subsection for it: "H5d. Équation de Lamé".

The question thus appears to be settled. However, if one takes into account not just Hermite's book alone, but the entire set of references used in it, in order to delineate how Hermite positioned his own work inside the mathematical disciplines, no clear picture emerges. In the first note presented to the Academy, in 1877, for instance, before he even discussed the applications, Hermite refered to Poisson's *Traité de mécanique*, and Poinsot's *Théorie nouvelle de la rotation des corps* (an article published in the journal of Liouville), classified in the R-chapter (mechanics) of the *Répertoire bibliographique*, as well as to Lamé's original memoirs, classified in T4; both R and T4 belonging to applied mathematics. But Hermite also used Weierstrass's *Théorie des fonctions abéliennes* and Heine's *Einige Eigenschaften der Laméschen Functionen*, classified as pure mathematics. New questions thus arise, on the transformation from applied to pure mathematics and reciprocally, and on nationally divergent representations of such a division. Moreover, the same variety will appear in later papers quoting and using Hermite's book, from Gylden's astronomical works to Klein's articles on mechanics (both classified as applied) as well as in Poincaré's analytical papers (classified as pure).

Moreover, contemporary discourses on Hermite's work also display some ambiguity. Émile Picard described the book in those terms: "These studies on Lamé's equation opened the way to many analytical results. But what interested Hermite the most, was that he could apply them to Mechanics and Astronomy. The title he gave to his memoir is significant here," [9, p. xxxiii]. On the other hand, Hermite himself dismissed his own qualifications as an applied mathematician : "I am not aware of recent research on mathematical physics and in particular on electricity, as I am, as you know, a contemplative analyst," he wrote for instance in 1880 to Mittag-Leffler,[6, p. 29]. Darboux tried to adjust this double image by depicting the applications themselves as the main agents in the process: "These discoveries [on Lamé's equation] allowed Hermite to give new and original solutions of several very interesting mechanical problems: the rotation of a solid body around a fixed point, the movement of the conical pendulum, the determination of the figure of equilibrium of a spring. Applications [...] came to Hermite almost as if they were soliciting him, through the good offices of his beloved elliptic functions."[1]

Is then applied mathematics to be described by its content, or by the professional situation of its creator? Was pure vs applied mathematics in the nineteenth century the exact equivalent of the pure vs mixed distinction of earlier works, or, on the contrary, were large parts of mixed mathematics included in pure mathematics, as Hermite's mechanical work may suggest? Was the distinction linked to the nature of the results or the nature of the mathematician? Or was the very idea of a division into pure and applied at stake?

Hermite's case is particularly interesting because despite his auto-description as a contemplative analyst, one is often tempted to see him as a natural scientist doing mathematics. Indeed, he believed that "even the most abstract analysis is for the most part an observational science, [he] completely assimilate[d] the complex of concepts, known and to be known in this domain of analysis, to those of the natural sciences,"(quoted in [3, p. 154]). This conviction is expressed at every level of his mathematical practice, [2, 3]. Solving Lamé's equation, in particular, does not mean for him giving a general formula as the solution, but rather finding explicit forms of it which would allow computations of key elements in each mechanical application. For him, computations and observations should provide mathematical "facts," the task of the mathematician being to detect the key characters allowing him to classify those facts, exactly as a natural scientist would classify organisms. Applications are made possible by a unity of all aspects of the world, including the purest topics in mathematics. In such a coherent view, "Analysts seem to [Hermite] to be natural scientists who, with the eyes of the mind, (in a world as real as the natural one) look on beings external to themselves, which they have not created, and whose existence is as necessary as that of animals and vegetables. And [...] the world of the Analysts has the most intimate relations with the physical world because the first contains the elements of the laws that govern the second. The study of the subjective world thus allow[ed him] a glimpse, a view of the real world, and [he] believe[d] that this idea, which is so common, of the continuity in the laws of nature directly proceed[ed] from the early analytical notions about functions that ruled until Riemann,"[6, p. 40]. Such a position is not isolated (see for instance Lamé's views in [10]). It suggests that while the distinction between pure and applied mathematics was certainly activated at various levels by nineteenth-century mathematicians, it was neither stable during the period, nor uniformly understood. It did not pinpoint distinctive types of practice, nor did it hamper a view of mathematics in which applications to astronomy or to number theory were all of a kind. Even if such labels may be treated as intriguing (if complex) actor categories, they thus do not appear to be efficient historiographical categories.

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Observatory Mathematics in the Nineteenth Century: Mathematization and Observation

David Aubin
(joint work with Charlotte Bigg)

This talk is a preliminary investigation of the relationship between the changing sense of *mathematization* and of *observation* between the end of the 18th century and the beginning of the 19th century. To start, let us note that there is a crucial linguistic difference between those terms. While *observation* remained relatively close to its Latin root and had for a long time been in constant use although increasingly in the field of science in the 18th century, *mathematization* was extremely rare and its meaning had not stablized (on *observation* and its cognates, see Marie-France Piguet, "De la connaissance des astres à celle des hommes : *Observer*, *Observation*, *Observateur*, *Observatoire* dans les dictionnaires du 18ème siècle," Séminaire "Cosmos," Centre Alexandre–Koyré, Paris, 7 February, 2013. On the history of scientific observation in general, see [1]). *Mathématiser* indeed was an entry in Louis–Sébastien Mercier's dictionary of neologisms, which was applied to the young men to whom a lot of mathematics was taught rather than to the phenomena one wished to study with the help of mathematical techniques [2, p. 115–116].

My claim here is that we can distinguish two different forms of mathematization in the nineteenth century and that they can be seen as broadly corresponding with two different understandings of observation. Let us begin by briefly discussing mathematization. The turn of the 19th century was, in this context, identified by T.S. Kuhn as the "Second Scientific Revolution" because of the strong impulse from was then called the “Laplacian program” to mathematize the Baconian sciences [3]. Although this view was strongly constested, the history of electromagnetism nonetheless offers an iconic contrast than can be used to demarcate the two types of mathematization I want to draw attention to [4]. The respective ways in which Carl Friedrich Gauss and Adrien–Marie Ampère mobilized contemporary mathematics in order to produce a new science of electricty and magnetism indeed stood in marked constrast. In short, while Ampère used small hand-held aparatuses in his makeshift laboratory to exhibit effects that could be represented through differential equations, Gauss drew on the full resources of his observatory. Studying the magnetometer with a theodolite and an astronomical clock, insisting moreover on inserting these measures in a global network of geomagnetic observations (which was called the “magnetic crusade”), Gauss produced a science of electricity where quantitative precision and comparability of data were cardinal values [5, 6]. Following Sophie Roux, I suggest that the best labels for these two kinds of mathematization perhaps are, respectively, “formalization” and “quantification” of phenomena [7] (see also [8]).

Turning to the story of observation, the situation is more complex. Focusing on francophone literature, I selected three main bodies of work to try and delineate the semantic context of *observation* in this period: (1) discussions on the art of observation around 1770; (2) the emergence of the idiosyncratic expression “*sciences d'observation*” in the first half of the nineteenth century; and (3) discussion of observation in published courses of physics, especially at the École polytechnique.

1. In the *Encyclopédie*, the article on “observation” published by Pierre–Jacques Malouin is surprising on two main counts. To begin, Malouin summarized observation as being “the attention of the mind [*esprit*] turned toward the objects offered by Nature.” In this understanding, observation had more to do with reason than with the senses. Then, he clearly placed observation above experimentation as a way to investigate nature, underscoring that while in the former process, one examined nature as it is, in the latter process, one examined nature as it was artificially manipulated. What I call the “*Art d'observer*” tradition was instantiated by two other texts written for the competition anounced by the Academy of Sciences of Haarlem in 1770 on the topic of “What is required in the art of observing & to what extent does this art contribute to the perfection of our understanding [*entendement*]” (for more details, see [9]).

For these authors, if, to some extent observation required inherent talent, nonetheless it could be learnt. For Senebier, “the science that I propose, instead of being the art of thinking would be the art of perceiving, it would be a logic for the senses, it would teach their use and their operation, it would teach the means of grasping what sets them in motion and to profit from the sensations they excite, as from the ideas they give birth to.” Similarly for Carrard, “in order to enlighten, to awaken their attention, & from now on to train good observers could one not show what is required to succeed in the art of observing & then to determine what fruits may be gained from this Art, as practised to perfect human Understanding?” A significant characteristic of the discourses on observation in these decades, especially compared to later writings, was their emphasis on method, on training the body, mind and sensory organs, of learning the proper rules for observing. The topics covered by our authors reflect these concerns. Though Carrard and others brought up the issues of comparing observations, of communicating results, of the importance of long series of observations as only Societies can produce, the focus was mostly put on the individual observer. Here logic and "*entendement*" are related to the researcher's personal qualities. Indeed, one of the first to discuss the role of observation in his scientific work, Georges Buffon indeed offered it as an alternative when more mathematical approaches [10, p. 62].

2. Though the term "science d'observation" is absent from the Encyclopédie, it slowly appeared at the end of the 18th century as a meaningful concept for thinking the sciences and their relations with mathematics. Condorcet casually used the expression in his "Rapport et projet de décret sur l'organisation générale de l'instruction publique" (1792). Fifteen years later, while he was warning against the dangers of only considering principles (or theories) in medicine at the expense of observation, the physician Georges Cabanis wrote that this was also true of all "sciences d'observation." Interestingly, as a model of an observation science that had lately progressed rapidly, Cabanis mentioned physics. Contrary to his predecessors, he referred not to the Newtonian tradition but to the new Laplacian physics. In a contemporary physics textbook, Jean-Nicolas Hachette, professor at the École polytechnique, concurred with Cabanis to say that something new was happening in physics, but not that physics belonged to the observation sciences. Hachette praised Haüy's work in crystallography as an example of a science formerly merely concerned with "more or less methodical classification" whose fundamental principles could now be solidly established on the foundation of "our most certain knowledge, geometry." What is interesting here is that Hachette used the term "science d'observation" disparagingly for the older type of crystallography. While Cabanis thought that the "sciences d'observation" heralded the future, Hachette anchored them in the Enlightenment naturalistic practices of the "art d'observer" tradition.

Nevertheless, from the late 1810s to well into the 1840s, most branches of knowledge, including physics, claimed to be part of the rising "sciences d'observation." Medicine, of course, was a prime example, just as physiology, psychology, and meteorology, but also political economy, moral philosophy, or even metaphysics. "Statistics is a science of observation," the philosopher of probability theory Augustin Cournot wrote in 1843. "There are no sciences but the sciences of observation," the spiritualist philosopher Charles de Rémusat (1797–1875) added. Around 1830, in francophone literature, the term "*sciences d'observation*" suddenly flourished as a convenient label for a set of scientific domains all seemingly based on "observation." Although the exact definition of the "sciences d'observation" was subject to strong variations, the label became central for many people's understanding of the classification of knowledge. As we shall see, physics would come to play a central role in the redefinition of the boundaries between observation and experimentation. But at first, as we can gather from Hachette or Cabanis, the meaningful contrast was not between observation and experiment but rather between observation and *raisonnement* (i.e. reasoning or the way in which one should use one's reason). The best source to examine this are the *Annales des sciences d'observation*, published by Jean-Baptiste Saigey and François-Vincent Rapsail in the early 1830s.

3. In francophone literature, observation science came to be opposed not to the experimental science anymore, but to reasoning science [*sciences du raisonnement*]. Positivist thought expressed a belief in the fact that the new physics of heat (with Fourier), of light waves (Fresnel), of electricity (with Coulomb and Poisson), of magnetism and electromagnetism (with Ampère) were sciences of observation whose analogues in pure reason had been found, through mathematization. Subsequently physics professors at the École polytechnique started to underscore the distinction between observation and experimentation. Contrary to astronomy, Gabriel Lamé for example argued, physics was not yet completed, and so, this discipline offered examples of all the stages through which a branch of science needed to go. Lamé contrasted the art of experiment with observation:

> The art of experimenting, which consists as far as possible in isolating each pair of force and effect, is a powerful help to the physicist. Observation, which consists in the study of phenomena as they naturally present themselves with all their complication, more rarely leads to the knowledge of the laws that govern these phenomena [11, p. 1:i].

The hierarchy drawn by Malouin had thus been reversed. Experimentation was more powerful than observation because it allowed the physicist to single out the effects he wanted to study. In the four successive editions of his course, spanning nearly thirty years, Lamé's successor at the École polytechnique, Jules Jamin, forcefully repeated the message that by allowing experiment to play a more important role than observation, physics was progressing faster than most observation science [12, 1:3] (first ed. 1859). This would lead to the passive understanding of observation most famously expounded by Claude Bernard [13].

To conclude, I would like to underscore that the changing sense of observation and experimentation, as well as the changing values invested in those terms, went hand in hand with changing conditions for carrying out observations and expertimentation. The model of the observatory sciences which emphasized the quantification of observation was slowly replaced by that of the laboratory sciences which favored the mathematical "formalization" (in the sense of the above) of experimental results [15, 14, 16]. I suggest that what we need to understand is not only the transition from mixed mathematics to applied mathematics, but rather to the dyad applied mathematics–theoretical physics. My claim is that the changing sense and changing conditions of observation have a lot to do with this process.

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Mathematical Models as Artefacts for Research: Felix Klein and the Case of Kummer Surfaces

David E. Rowe

Already as a student in Bonn, Felix Klein was exposed to model-making through his teacher, Julius Plücker, who used models to visualize the properties of special surfaces that arise in line geometry. Afterward, Klein attended Kummer's seminar in Berlin, where he began to explore the connections between general Kummer surfaces and so-called complex surfaces, first studied by Plücker. In the late 1870s, Klein and Alexander Brill supervised the work of several students at the Munich Technische Hochschule who designed a large collection of models there. There followed a new era in model production, based in Munich but marketed through the Darmstadt firm owned by Brill's brother. By 1900 the plaster models of L. Brill could be found at leading universities around the world. My talk described early interest in models of algebraic surfaces in the era before mass production of plaster models began. It centered especially on Kummer surfaces, starting with the special cases studied by Plücker but also including Kummer's own famous models, which began as research artefacts in Berlin, before passing to Munich and then proliferating to points beyond (for further details, see [15]).

During this early period, model-making went hand in hand with cutting edge research. In Bonn, Klein assisted Plücker in designing around thirty different models that displayed select features of a certain class of quartic surfaces linked to quadratic line complexes. Klein emphasized the connection between these mathematical models and Plücker's earlier research in physics, especially his famous experiments on electrical discharges in rarefied gases [12]. Plücker carried these out with the assistance of Heinrich Geissler, famous for his invention of the glass tubes that bear his name. In both of these research fields, Plücker was drawn to describe complex, never-before-seen spatial phenomena [3].

Although little appreciated in Germany, Plücker had an excellent reputation as an experimental physicist in England, where his work was championed by Michael Faraday, a physicist who thought in pictures, not formulae. Klein later recalled how Plücker once told him that Faraday had given him the initial impetus to build models illustrating different types of the so-called complex surfaces he unveiled as the centrepiece of his new line geometry [10, p. 7]. Faraday was by no means the only one in England to take an interest in these exotic spatial artefacts. Thomas Archer Hirst, who had studied under Jakob Steiner in Berlin, was another enthusiast for Plücker's models. In 1866, Plücker delivered a well-received lecture at a meeting in Nottingham in which he employed a subcollection of his models [2]. Hirst was intent on acquiring copies, and so Plücker afterward donated a set of these made in boxwood to the London Mathematical Society (the correspondence between Plücker and T. A. Hirst can be found at http://www.lms.ac.uk/content/plucker-collection). These attractive wooden models can still be seen today on display at the Science Museum in London.

Immediately after Plücker's death in 1868, Klein was entrusted to complete his classic monograph, *Neue Geometrie des Raumes gegründet auf die Betrachtung der geraden Linie als Raumelement.* By probing deeper into Plücker's theory of quadratic line complexes, he began to realize its close links with Kummer's work in geometrical optics from the 1860s. So what better place to delve more deeply into these matters than in Kummer's own seminar? Kummer was an old-fashioned, serious-minded Prussian who thought of Gauss as the embodiment of the highest ideals in mathematics [1, pp. 81–82]. He had no advanced students at this time, however, so Klein naturally felt starved for intellectual nourishment. Luckily, he soon realized there was one other attendee in the seminar with a background comparable to his own, Sophus Lie. He and Klein made for an odd pair, but they nevertheless hit it off immediately. Soon Klein had all the mathematical stimulation he needed. Lie was already deeply immersed in research on curves associated with a special type of quadratic line complex formed by the lines that meet the faces of a tetrahedron in a fixed cross ratio. The properties of such tetrahedral complexes and their curve systems would continue to occupy Lie and Klein for well over a year [14]. In Paris they met Gaston Darboux, who quickly directed their attention to the field of sphere geometry, a French specialty. Soon Klein and Lie were eagerly pursuing links between Plücker's line complexes and their analogues, 3-parameter families of spheres in 3-space. By early July, Lie had made one of his most spectacular discoveries: his line-to-sphere transformation. After the outbreak of the Franco-Prussian War they were separated, but Klein continued to think about the role of Kummer surfaces in the theory of quadratic line complexes. With the help of a friend, they designed a model of a Kummer surface and used this to study the singularities of its asymptotic curves. The latter, as Lie had discovered, were algebraic of degree 16, a result he deduced directly from his line-to-sphere mapping.

It was only five years earlier that Kummer had uncovered these remarkable quartics in connection with his studies of ray systems in geometrical optics. These Kummer surfaces arise as the focal surfaces of algebraic ray systems of the second order and class. Their discovery eventually opened the way to numerous subsequent investigations [7]. This era, in fact, witnessed several such discoveries in algebraic geometry. In rapid fire fashion, mathematicians now began to study a huge variety of algebraic surfaces beyond the realm of quadrics, which had already been studied and classified by Euler. An early breakthrough came around 1850 when Cayley and Salmon discovered that non-singular cubic surfaces contain 27 lines that form complicated spatial configurations [4].

Geometry at this time was still strongly tied to the study of figures in 3-space. Leading mathematicians continued to think of geometrical objects as idealizations drawn from the world in which we live. Klein and his teachers were thus intent on gaining a visual image of these new-fangled mathematical objects, an interest that prompted several contemporary geometers to build various types of models in order to study their properties in greater detail. Bezout's theorem made it possible to exploit the invariant degree of algebraic curves and surfaces by moving to the complex domain, thereby posing a new challenge: how to interpret the imaginary elements that lie outside the realm of *real* 3-space, the arena of interest for the geometer. Special incidence configurations – like Hesse's inflection point configuration for cubic curves, or Schläfli's double-six in connection with the 27 lines of a cubic surface – also helped identify key features.

Not long after Plücker's death, Klein designed four additional models to show the main types of real singularities that can arise with these Plücker surfaces [10, pp. 7–10]. Klein's models, like those designed by Plücker, were produced and sold by the firm of Johann Eigel Sohn, located in Cologne. Originally made in zinc, they were far heavier than the more familiar plaster models built afterward. For his *Habilitation* lecture in Göttingen, held in January 1871, Klein presented one of these models in order to convey some newly discovered properties of special curves associated with Plücker's complex surfaces. Later the next year, Klein and Clebsch presented two new models of cubic surfaces to the Göttingen Scientific Society. It was on this occasion that Clebsch unveiled his famous diagonal surface with its special configuration formed by the 27 lines on a non-singular cubic [6, Kommentarband, pp. 7–14]. Klein's model, built by his doctoral student Adolf Weiler, illustrated a cubic surface with four real singular points, the maximum possible. Already at this time Klein claimed that by starting from this particular surface one could derive the entire 19-dimensional continuum of cubics by means of deformations. This theory later inspired Carl Rodenberg to produce a series of 26 plaster models that featured the striking surfaces modelled by Clebsch and Klein. Published by L. Brill in 1881, the Rodenberg series represents a higher water mark in the art of model design and production [17].

Perhaps even more famous were the special quartics studied by Kummer in the mid 1860s [11, pp. 418–432]. These objects exhibit a special configuration of 16 singular points and 16 singular planes in space that soon led to a plethora of investigations connected with the properties of these so-called Kummer surfaces. Each plane passes through six of the sixteen singular points, which are constrained to lie on a conic, forming a (16,6) configuration. Such surfaces are self-dual, hence of the fourth order and class. This means that the lines in space intersect the surface in four (real or imaginary) points, whereas four tangent planes will pass through any given line. Kummer noted that these sixteen singularities are the maximum possible for a quartic, as confirmed by the formula for the class of a surface with d ordinary nodes: $k = n(n-1)^2 - 2d = 4(3)^2 - 2(16) = 4$.

Klein took note of Kummer's work in a fundamental paper [9, pp. 53–80] written earlier that same summer of 1869, just before he departed for Berlin. There he focused attention on the singularity surfaces of quadratic line complexes, which are formed by the locus of bi-planar points; those at which the cone of lines passing through them degenerates into two planar pencils of lines. After introducing so-called Kleinian coordinates, he went on to show that for a general quadratic line complex–in fact, for a whole 1-parameter family of such complexes–the associated singularity surface will be a quartic with precisely the same configuration of singular points and planes found by Kummer in 1864 [7, chapters 4, 5].

In the meantime, Kummer found an elegant and surprisingly simple description for a 2-parameter system of quartic surfaces in which the real points lie within a bounded portion of space. This system included as a special case the so-called Roman surface of Jacob Steiner, which Kummer had independently re-discovered in the mid 1860s [19, p. 180]. To give a vivid picture of the organic connections underlying the various types of singularities that can arise, he designed seven plaster models, presented to the Prussian Academy at a meeting held on 20 June 1872 [11, pp. 575–586]. All seven exhibit tetrahedral symmetry, as reflected in the form of their quartic equation:

$$\varphi^2 = \lambda pqrs = 0$$

where $\varphi = 0$ is the equation for a family of concentric spheres with parameter μ:

$$\varphi = x^2 + y^2 + z^2 - \mu\, k^2.$$

Over the next twelve years, these seven plaster models could only be seen by visiting the Berlin mathematics seminar. Surely a number of mathematicians knew of their existence from Kummer's publication, but very few had any idea of what they actually looked like. Not until the 1880s did these models leave their local confines in Berlin, after which they quickly became familiar objects to mathematicians around the world.

The production of mathematical models took on a far more public face in the late 1870s. This began in 1875 with Klein's appointment as professor of mathematics at the Technische Hochschule in Munich, where he succeeded the elderly Otto Hesse. There Klein was joined by Alexander Brill, who had studied under Clebsch in Giessen in the 1860s and afterward formed a close working relationship with Max Noether. Brill's background and training, however, were highly unusual, though ideally suited for model making [5].

At the TH Munich, Klein was eager to take up research on Kummer/Plücker surfaces again. He brought with him the zinc models he had designed back in 1871, but these he found unsatisfactory in certain respects. So he was pleased to enlist the support of a student, Karl Rohn, who built three new models under Klein's watchful eye. All three were prepared in plaster of Paris and marketed in the second series of models offered by L. Brill. In the catalogues they are identified as Kummer surfaces, but Rohn emphasized that these models illustrate the passage from a general Kummer surface to the special cases that arise in Plücker's theory of quadratic line complexes [13]. So the latter two models represent, first, a general Plücker complex surface with eight real singular points and, second, a degenerate Plücker surface for which only four such points remain. The former case appears in the middle of the third row of the advertisement above. A more aesthetically pleasing image is reproduced below [7, chapter 6].

Although the motivation behind many of these models was primarily didactical, some cases involved new results of potential interest to researchers. In such cases, fairly elaborate explanations were sometimes necessary. These could be found in brochures written by the respective designers, many of whom provided detailed mathematical descriptions without which even an educated observer would surely remain baffled. This literature accompanied the models upon purchase, but was otherwise unavailable to a wider public. By 1899, when Ludwig Brill sold the publication rights for his collection of models to the firm of Martin Schilling in Halle, the demand for them had not yet reached its peak; nor had the number of different models available for purchase flattened out. Indeed, a dozen years later M. Schilling, now located in Leipzig, put out a catalogue listing nearly 400 models available for delivery [17]. Some years earlier, after repeated requests for the technical brochures that came with the models, Schilling also published the entire collection for the first 23 series of models [16]. Many of these are an invaluable resource for understanding the mathematical meaning of the objects on display. By this time, the era of mathematical model-making had long since entered an era of mass production. While the use of models and other visual aids took on a growing importance in mathematics education, these objects rarely played a role any longer as artefacts for research. Indeed, their demise as tools for research was already under way when the Brill firm began to market them.

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“Social statistics have no merit for theoretical study” – Conflicting and complementary views on statistics in late Imperial China

Andrea Bréard

In late Imperial China, the sciences were part of *shixue* 實學 (‘concrete/solid/real learning’), as crucial tools for statecraft, but mathematics were not a discipline until the late 19th century, and neither ‘mechanics’ nor ‘physics’ or ‘optics’ were an independent branch of learning. Thinking and knowledge concerning mechanics or physics existed in scattered philosophical texts, technological texts, astronomical texts and mathematical texts. Looking at the bibliography in the official history of the last Chinese dynasty, the Qing (1644–1911), one can find two categories related to mathematical writings: ‘astronomy & mathematics’ and ’magical computations’ (*shushu* 術數) usually linked to impostors and to heterodoxy. The notions of pure and applied mathematics were not even actors’ categories in China, at least not before the 1930s. As can be seen in divers textual sources in the 1930s, it is then that reflections upon a dichotomy between pure (or theoretical) and applied mathematics emerge, that relate to a methodological distinction in terms of precision or logical rigor. Other authors instrumentalize the pure-applied distinction as an explanation for China’s backwardness in the sciences in the early 20th century.

In order to make my contribution fit into the framework of this workshop, I had thus taken the liberty to shift a little bit the terms, and decided not to attempt a historicization of the relationship between applied and pure mathematics for the Chinese case at the turn of the 20th century. By adopting a more China-centered approach, which is in general more fruitful when dealing with non-European science and traditions, I rather discussed a distinction between what in Chinese terms are the ‘fundamental principles’ (*ti* 體) and the ‘applications’ (*yong* 用).

During the late Qing, the slogan ‘Chinese studies as essence/for fundamental principles – Western studies for practical application’ (*Zhongti xiyong* 中體西用) was instrumentalized in scientific and political discourse and served more and more as a framework for rationalizing foreign knowledge. ‘Chinese studies for essence/fundamental principles’ primarily meant traditional Chinese political and economic systems and their corresponding ideologies but the expression also referred to Chinese culture and its Confucian underpinnings, while ‘Western studies for practical use’ was limited primarily to Western science and technology, in particular mechanics, physics, chemistry, mathematics and military technology. Western institutions (particularly political institutions) and thought were seen as heresy and dichotomous to ‘Chinese studies’ and were subjected to rejection. When introducing statistical theories from abroad and creating a nationwide statistical institutional network in the early 20th century, the Chinese government urgently needed precise numbers as a new tool in political decision.

In my paper, I analyzed how reformers legitimized this ‘statistical revolution’ (see [1]) by making statistics fit into the ‘essence’ – ‘application’ paradigm and by pointing out the importance of the ‘concreteness’ of numbers. In late imperial and early Republican China, the notion of ‘statistics’ was a multi-layered one and different kinds of statistical theories and practices co-existed: administrative statistics which had a very long tradition in China itself, social statistics which were imported/naturalized from Germany via Japan and ‘purely’ mathematical statistics which came from the anglo-saxon tradition and were at first brought to China by returning students from abroad. When statistical theories (for application to social questions) were imported in China at the turn of the 20th century, China already had a long (and institutionalized) administrative tradition of collecting numbers and reporting them to the throne. Statistics, although not defined as a discipline, then were sets of descriptive tables produced for the state. Reformers defended the import of foreign statistical theories as something that was merely grafted onto old administrative practices. Official and unofficial discourse during the reforms at the end of the Qing dynasty abounds in justifications of the new (the ‘application’ from abroad) by referencing the old (the Chinese ‘essence’). By underlining the existence of a statistical tradition in the Chinese imperial administration since high Antiquity, the memorialists subtly convinced the Emperor and other high officials to conduct reforms based on traditional patterns (‘reform within tradition’). Adopting anything foreign/Western thus was reduced to a simple reintroduction of Chinese methods that had partly gotten lost over the centuries.

More specifically, in my paper I used the writings of two important statistical figures, Shen Linyi 沈林一, the first director of the Central Statistical Bureau, and Meng Sen 孟森, translator of the most influential statistical manual [3], who were representative of these two complementary and by times conflicting aspects of statistics. Meng had studied law and politics in Japan where he learned about statistics and translated Yokoyama Masao’s *General Discussion of Statistics* [5]. His translation was the first statistical manual in China, and it certainly circulated widely not only in government circles but also in institutions where statistics were taught. Both, the original and the translation were reprinted and edited many times until the 1930s. But the manual was not a mere translation. It was interspersed with Meng Sen’s personal commentaries, which were not uncritical towards China’s conservative forces. The chapter on population statistics, presented by the author as the most important type of statistics, takes up the major part of the book. Among mathematical methods one finds only three short sections on summation, proportions and mean values, and although Meng Sen obvisouly had some notions of probability theory applied to vital statistics, he only refers to these in his extensive comments upon the calculation of mortality tables (see for example the reference to [2] in [3] p. 270-273). Shen Linyi was less interested in statistics from a mathematical point of view. His publications do not refer to any theoretical works he might have encountered through contacts with his contemporaries or in the library of the Constitutional Commission, of which the Statistical Bureau was part of and which owned two copies of Meng Sen’s Chinese language statistical manual. Shen Linyi was more familiar with statistical work, with the practices that related to the collection and recording of fiscal data or to the standardization of measures and weights (see [4]).

In summary, using statistics as a case study, I showed the ways in which the ‘essence’ – ‘application’ distinction was entangled with a dichotomy between Chinese and Western (or more generally foreign) science in the accompanying political and scientific discourses. I also showed how reformers argued that statistical practices, in a dire need to be reformed at the end of the Qing, could fit well into the neo-Confucian concept of ‘concrete studies’. The numbers produced should correspond to precise facts, social or economic actualities so important for determining quotas for local assemblies or to show the success of educational reforms for example.

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Karl Pearson and Darwinian Evolution: The Development of Applied Statistics

M. Eileen Magnello

By the end of the 1890s, the content and practice of statistics was transformed into a highly specialised mathematical discipline. These intellectual and later institutional changes largely occurred due to a statistical translation of Charles Darwin's redefinition of the biological species, as something that could be viewed in terms of populations rather than simply classifying species as types. The joint-efforts of two Cambridge-trained Victorians, the mathematician Karl Pearson (1857–1936) and the Darwinian zoologist Weldon (1860–1906) led to their mathematical reconceptualisation of Darwinian biological variation and "statistical" populations of species in the 1890s, which provided the framework within which a major paradigmatic shift occurred in statistical techniques and theory. Their work thus brought about an epistemic rupture from the work of the vital statistics of the state where averages were the unit of measurement, to that of mathematical statistics, where the principal unit of measurement became individual variation.

Weldon's work on the shore crab (*Carcinus maenas*) in Naples and Plymouth from 1892 to 1895 not only brought them into the forefront of ideas of speciation (or the multiplication of species) *and* provided the impetus to Pearson's earliest statistical innovations, but it also led to Pearson shifting his professional interests from having had an established career as an elastician to becoming an applied mathematical statistician. The ground-breaking statistical work that Pearson developed with Weldon in 1892 and later with Francis Galton (1820–1911) in 1894 enabled him to lay the foundations of modern mathematical statistics.

After graduating as the Third Wrangler in Mathematics at Cambridge, Pearson began to plan his eighteen-month academic sojourn to Germany. Having made arrangements with Kuno Fischer, Pearson left for Heidelberg in April 1879 to improve his conversational German and to study physics and metaphysics under Fischer and Gustav Kirchhoff. After reading the works of Berkeley, Fichte, Locke, Kant and Spinoza, Pearson found that his 'faith in reason ha[d] been so shattered by the merely negative results to him which he found in these great philosophers that he despaired his little reason leading to anything'. He subsequently abandoned philosophy because 'it made him miserable and would have led to him to inevitably short-cut his career'. Later that summer Pearson was at such 'low ebb of despair', in his search for the truth, that he was tempted to become a Roman Catholic. His time in Germany became a period of self-discovery: the romanticist and the idealist discovered positivism.

Pearson thus adopted and coalesced two different philosophical traditions to fulfil two different needs, for idealism was concerned with nature and personal feelings, whereas positivism dealt with science and professional goals. His passionate Germanic interests, which underscored his desire to find the truth, were pursued whilst he was writing papers and books on elasticity, engineering, mechanics, philosophy and optics. Having abandoned the study of philosophy and physics in Heidelberg, he went to Berlin and spent his time making measurements of physical quantities in Heinrich Quincke's laboratory. A few months later he began to attend lectures on Roman International Law and Philosophy by Bruns and Theodor Mommsen (1817–1903). During this time, Pearson also continued to read books on medieval and sixteenth century German literature.

Despite these trials and tribulations, Pearson's time in Germany was one of enormous enjoyment. A Cambridge friend, Ralph Thickness, visited Pearson in the Black Forest and had a wonderful time with him. Ralph found that Carl's enjoyment of life in Germany was so immense that he even cracked jokes in a Swabian dialect whilst drinking his beer. He was very much at home there and, according to Ralph, 'Carl got quite enthusiastic at times and was far more emotional and enthusiastic in Germany than he was in London'. Pearson even established himself as a welcome visitor in the little village of Saig, in the Black Forest, where years afterward he was pleased to meet those whom he had known as children. He returned to Germany quite regularly, especially as he 'longed for freedom from the social constraints of a rigid class system in England' and bemoaned that 'the English masked their feelings and that [their] society had thus become artificial'.

Having returned to London in 1880, he was called to the Bar, but hated the law and decided instead, to 'devote his time to the religious producing of German literature before 1300'. By the 1880s, London was full of idealistic young men who, like Pearson, were dissatisfied with conventional politics and religion, and were searching for new ways of understanding and changing their society. Pearson was particularly interested in pursuing further study of German folklore and literature, the history of the Reformation and German humanists. From 1880 to 1884 he lectured in various academic institutes and working men's clubs in London. He lectured on Martin Luther's influence on the material and intellectual welfare of Germany, and on German social life and thought, from the earliest times up to the sixteenth century. He also gave lectures on Karl Marx and Ferdinand Lassalle at revolutionary clubs in Soho. In his pursuit of German history, Pearson consulted his friend and the Cambridge University librarian, Henry Bradshaw (1831–1886), who taught him the meaning of thoroughness and patience in research. With Bradshaw's help, Pearson finished in 1887 *Die Fronica: Ein Beitrag zur Geschichte des Christusbildes im Mittelalter* (a collection of the Veronica images of Christ). He became such an accomplished philological scholar of medieval German folklore, literature and its language, that in May 1884, he was short-listed for the newly created post in German at Cambridge. However, in spite of his accomplishments with German literature, Pearson 'longed to be working with symbols and not words'. He would thus have to find work as a mathematician.

A month later he received the Chair of 'Mechanism and Applied Mathematics' at University College London He lectured single-handedly for 11 hours weekly, and taught mathematical physics, sound, electricity, light, magnetism, wave-motion and hydrodynamics to engineering students. Still dissatisfied a month later, he eventually applied for the Professorship of Geometry at Gresham College in 1890. Having abandoned the pursuit of religious, philosophical and literary truth, Pearson began his search for the numerical truth at Gresham. As a Cambridge Wrangler, Pearson learned to use applied mathematics as a pedagogical tool for determining the truth (one that provided the means of producing reliable knowledge). Given the dominance of Euclidean geometry in Victorian Britain, Pearson regarded 'geometry as a mode of ascertaining numerical truth', and a 'fundamental process of statistical enquiry'. It was at Gresham College where Pearson first began to teach statistics and where he began to think of formulating a mathematically based statistical methodology, which would be dramatically different from the vital statistics espoused by the mid-Victorian statisticians.

At Gresham College, Pearson divided statistics into two parts: *Pure Statistics* was that branch of mathematics which dealt with the computation, representation and the handling of statistics, whilst *Applied Statistics* was the application of pure statistics to special classes of facts, which could be used in the biological and social sciences, anthropology and physics. The catalyst that would bring about this transformation of statistics came in November 1892 when W.F.R. Weldon was looking for a working hypothesis that fit Darwin's theories. When Charles Darwin suggested that evolution proceeded by the accumulation of minute differences between individuals, he introduced the idea of continuous variation into biological discourse. This idea of continuous variation forced nineteenth century naturalists to reconsider the traditional definition of the biological species. Up until the middle of the nineteenth century, species were defined in terms of types by many biologis ts, including museum taxonomists.

This transition in measuring biological variation rather than simply calculating averages enabled Pearson and Weldon to create the tools that led to a new statistical paradigm when they translated Darwin's ideas about what kinds of natural processes occur in the world into statistical concepts. Consequently, they had to create a new way of thinking about statistics. To help Weldon interpret his statistical data, Pearson had to create new statistical methods to measure this Darwinian variation, develop a system that did not rely on the normal distribution to determine how a new species emerged, and, moreover, Pearson had to standardise his system of frequency distributions that could accommodate large sample sizes (of more than one thousand) to find empirical evidence of natural selection by detecting disturbances in the distribution. After Pearson formulated a standardised system of frequency distribution, it then became possible to make comparisons and generalisations abou t data sets that had previously been impossible to make.

By November 1893, Pearson had worked out the foundations to his statistical methodology, based on the method of moments, which he first learnt from Benoît Clapeyron's 1890 paper Theorem of the Three Moments. The term 'moment' originates in mechanics, and is a measure of force about a point of rotation, such as a fulcrum. In statistics, moments are averages. Using the first moment, Pearson taught his students how to calculate the arithmetic mean by determining the point about which the lever balances on the fulcrum. The second moment is used to find what Pearson called the 'squared standard deviation' (or the 'variance'). The third moment, which indicates if the lever balanced on the fulcrum, is used to find a measure of the skewness of a distribution: an unbalanced lever is analogous to an unbalanced normal distribution or a skewed distribution. The fourth moment measures how flat or peaked is the curve of the distribution; for which Pearson coined the word 'kurtosis' (which means "arched" or "bulging"). It has three components: (a) 'leptokurtic' referred to what Pearson called the peakedness of a distribution; (b) 'platykurtic' was so-called because its flatter shape resembled a platypus; and (c) 'mesokurtic' referred to the normal curve. These four parameters describe the essential characteristics of any empirical distribution: the system is parsimonious and elegant. These statistical tools are essential for interpreting any set of statistical data, whatever shape the distribution takes. Pearson created a new mathematically-based statistical methodology, partly by amalgamating the probability work from the French in the 18^{th} century, moving outside the realms of vital statistics, using his maths from Cambridge and, most importantly, by devising and then applying these methods to Darwinian ideas of variation, population, speciation and natural selection and expanding this to correlation, which not only led to the foundations to the modern theory of applied statistics, but it also helped to create the modern world view. Moreover, his statistical methodology not only transformed our vision of nature, but it also gave scientists a set quantitative tools to conduct research, accompanied with a universal scientific language that standardised scientific writing in the twentieth century.

Fluid Mechanics: A Challenge for Mathematics ca. 1900

Michael Eckert

Felix Klein's role in the history of mathematics is well known, both with respect to mathematics proper and the applications of mathematics ([6], [9], [10] and [11]). He seems therefore an appropriate candidate to explore the issue of our workshop in more detail. I focus on fluid mechanics ca. 1900 as a test case for mathematical applications. It became a particular challenge for contemporary mathematicians because of the proverbial gulf between hydraulics as an engineering practice and hydrodynamics as a theoretical science based on mathematics and physics. Klein dedicated considerable effort to hydrodynamics at three occasions in the beginnings of the 20th century. First in a lecture in the winter semester 1899/1900, then in two seminars in the winter semester in 1903/04 and in 1907/08.

Klein's perception of applications developed gradually. When he was appointed as professor of mathematics at Erlangen University in 1872, he emphasized in particular the service of mathematics for other sciences and "the formal educational value that its study provides." (quoted in [8]). A few years later, in his inaugural lecture at Leipzig University, he added "modern technology" as an additional field that requires mathematical underpinning. This certainly reflects that he had spent in the meantime a period of five years at the Munich Polytechnic School [2, Kap. 4.3]. However, he did not yet challenge at that time the primacy of polytechnic schools for the training of engineers: "We have to limit ourselves to a discourse of the principles and leave aside the many details that are indispensible for the engineer" [3]. Only when he was called in 1886 to Göttingen did Klein assume the ambitious view to "develop the universities in such a way that they can fully account for the modern disciplines; in particular, the polytechnic schools should be attached to the universities." (quoted from a letter to Friedrich Althoff, 27 May 1888, cited in [6, p. 85]). Backed by Althoff from the Prussian Ministry, Klein's perception of "applications" became highly political and developed into a set of strategies on the national and international level. It involved new curricula for high schools and universities, approaches to the engineering community, editorial efforts such as the *Enzyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen* and the establishment of new applied institutes. With this perspective, Klein started to address in his lectures and seminars specialties that he regarded as suitable targets for exposing the need of mathematical underpinning.

Hydrodynamics became a particular target as a result of Klein's increasing focus on technical mechanics. He had included technical mechanics as part of applied mathematics, for example, in 1898 in a new regulation for Prussian high-school teacher examinations. "Technical mechanics is no isolated specialty," he explained again in 1900 in a talk before an audience of high school teachers. "We have occasionally included technical mechanics in our Göttingen university lectures. I myself use to take it into account as far as possible in my general lectures and seminars on theoretical mechanics" [4]. In this vein he also lectured on hydrodynamics in the winter semester 1899/1900. By the same time, Klein began to edit the mechanics part of the *Encyclopedia* which included an article on hydrodynamics (authored by A. E. H. Love and published in 1901) and on hydraulics (authored by Ph. Forchheimer and published in 1905).

Klein's lecture on hydrodynamics was a sequel to preceeding lectures on point mechanics and the mechanics of rigid bodies. "Although we stress the mathematical aspects of our subjects compared to the physical aspects, there will be no lack of prospects in different directions," Klein alerted his students about the broad scope of this lecture. Originally he intended to split it in a general and a special lecture. The latter was dedicated to the application of automorphic functions, as Klein revealed in the introduction to the general lecture. “The context of this special lecture with the general lecture on deformable bodies is due to the applicability of automorphic functions to problems in mechanics. Many mechanical problems lead to elliptic functions and can therefore be regarded as solved” [5]. Klein had to cancel the special lecture on automorphic functions this time for other reasons, but the intent reveals his focus on mathematics rather than technology as his major concern. The use of automorphic functions in fluid mechanics is restricted to special two-dimensional flow arrangements, so that engineers would have hardly shared the excitement of mathematicians about their applicability. See, for example, [7].

Four years later, Klein dedicated his seminar to hydrodynamics. He recruited as co-organizer Karl Schwarzschild, who had been appointed in 1901 as professor of astronomy and director of the Göttingen obervatory. The preceding lecture served as a reference, but the form of the seminar allowed Klein and Schwarzschild to deal with specific problem areas without the need of a systematic exposition. The focus on problems provoked a revealing discrimination concerning the mathematical difficulties “to understand hydraulics from the perspective of theoretical hydrodynamics,” as Klein described the goal of the seminar in his introductory talk. “From the mathematical perspective the subjects may be categorized in the following way,” he reported to the Göttingen Mathematical Society on 9 February 1904 about the seminar, “a. well defined problems, b. rather poorly defined problems, c. very badly defined problems.” Turbulence, for example, was rated to the second category. “The question how one can explain theoretically the emergence of turbulence appears still unsolved; we will hear more about the contemporary approaches in the presentations of Schwarzschild, Herglotz and Hahn,” Klein alluded to these forthcoming seminar talks. The three theorists reviewed the riddle of turbulence in pipes and channels so pervasively that they decided to published a common paper on it – although it had little to do with their other work in mathematics, physics and astronomy [1].

Another four years later, in the winter semester 1907/08, the gulf between hydrodynamics and hydraulics was once again the subject of a Göttingen seminar, this time co-organized by Klein, Emil Wiechert, Ludwig Prandtl and Carl Runge. Wiechert was head of Germany’s first university institute for geophysics founded in 1898 at Göttingen. Runge and Prandtl had been called to Göttingen in 1904 in order to direct the new institutes for applied mathematics and mechanics that Klein had established in the course of his effort to add applied fields to Göttingen university. “Connection of theory with practice (observation, experiment, construction),” Klein remarked about his intention in the beginning of the seminar, “Enormous practical interest. Practical background: the Institute for Applied Mechanics.” (30 October 1907, SUB, Cod. Ms. Klein 20 F). Among the seminarists were Prandtl’s doctoral students and Prandtl’s assistant, Theodore von Kármán, who would later rival Prandtl as a pioneer of modern fluid mechanics.

In conclusion, the application of mathematics to fluid mechanics resulted in a development which payed more attention to practice than to mathematics. Klein's original strategy to use practice as a challenge for mathematics was transformed by Prandtl into a program that led to the establishment of applied mechanics as a discipline in its own right.

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Transmitting disciplinary practice in applied mathematics? Textbooks 1900 - 1930

Tom Archibald

> "I do not believe that there is, properly speaking, such a thing as applied mathematics. There is a British Illusion to that effect."
>
> O. Veblen 1929, quoted in [1]

1. Introduction: Disciplinary space for applied mathematics

Applied mathematics today clearly has some kind of status as a discipline, whether we define discipline casually or use a broader sociological definition. This was not always the case. Yet by the early twentieth century bodies of practice had begun to be established in various fields that now function as part of applied mathematics or in close conjunction with it. Today, speaking very broadly, we may see a tripartite division of these practices and tools into scientific computing, modelling using adapted or new mathematical tools, and the mathematical analysis of those tools. To set aside stochastic and statistical modelling, these tools are usually centred around ordinary and partial differential equations, though more recently algebra and discrete mathematics make an important appearance.

How to go from this vague impression of the present to an historical analysis based on what mathematicians do? Here we focus on the transmission of bodies of knowledge and the accompanying establishment of canonical texts, possibly different in different contexts. This brings us up against the notion of disciplinarity. The notion of discipline aims to analyse scientific activity from the point of view of division of labour. It is characterized by forms in the day to day organization of research and teaching. At the teaching level, it anticipates the necessity of a distribution of specific tasks, the tools for which have to be acquired (e.g. modelling). The relationship to knowledge production comes through the tension between the acquisition of things known and the necessity of innovation to improve or extend the domain of effectiveness of the approach. The student's acquisition of knowledge is generally done in light of a (weakly specified) frontier of knowledge defined in terms of increasing cognitive mastery of a somewhat predefined object, e.g the mechanical behaviour of fluids. Disciplines to some extent partition inquiry, and for practitioners there is a general idea that a particular discipline somehow complements others in some larger field (e.g. applied mathematics versus theoretical physics).

In what follows, by looking at various aspects of applied mathematical toolkits and practices as they appear in textbooks in the period from 1900 to 1930, we seek a nascent set of practices and hence a nascent discipline.

We focus on examples that aim specifically at transmitting a corpus of methods to a variety of publics: mathematicians, physicists, students of both, and other groups also (e.g. engineers, military, ...) The books we consider were intended for, and used by, a variety of publics. In the 19th c., the toolkits provided were for "Natural Philosophy", "Mathematical Physics", and descriptors of that kind. The eventual standard repertoire of applied mathematics emerged largely from there. Beginning already in the 19th century there were many specialized treatises, eg on variational methods or potential theory, aiming at applications to varying degrees.

Two examples from the nineteenth century give us a starting point for our reflections: Thomson and Tait, and Riemann's lecture courses (later to become Riemann-Weber, and ultimately Frank-von Mises). Thomson and Tait was initially envisaged as a survey of all of natural philosophy, a project eventually abandoned, so that it never went beyond what its authors considered to be mechanics (though their view was broad). More influential for later compendia of methods were the two volumes of Riemann. The first of these, appearing under the title *Schwere, Elektrizität, und Magnetismus* dealt with the applications of potential theory (seen as the theory of the Laplace and Poisson equations). For various reasons, the volume on partial differential equation has had a more enduring legacy. Following a basic treatment of definite integrals, Fourier series, and differential equations, detailed discussions of: the heat equation with various boundary conditions; vibrations of elastic bodies, uses of potential and Lagrange's principle; and fluids, considering among other things the case where there is a velocity potential, compressible flow, and the motion of solids in incompressible fluids. A key feature of the work is to provide repertoire of methods for fundamental examples.

2. The British Illusion: Love, Lamb and Whittaker

Let's take a look at what Veblen termed the illusion that there is such a thing as applied mathematics, a position that appeared to him to be held by the British. This impression would clearly have been fostered by the Cambridge Tripos training, which in a complex Newtonian legacy continued to emphasize natural philosophy. This older legacy is not our subject here, and we we turn instead to to writers whose works were to become canonical in British-influenced contexts – as well as in those in competition with British work in these fields. These are A. E. H. Love and Horace Lamb.

"The title of what well-known mathematical book could just as easily be the title of a tragedy? Love's Elasticity." This joke attests to the fact that this was a very well-known work, as does its universal presence in library collections. A. E. H. Love (1863–1940) was 2nd Wrangler in 1885, a Fellow of St John's from 1886, taking up a professorial position at Oxford in 1899. His 1892/3 *A Treatise on the Mathematical Theory of Elasticity*, had many editions; the 4th 1927 still falls within our period. The work is very much an exposition, was widely cited and treated as classic by later writers (Muskhelishvili for example). Above all this is a work on analysis, though there is more than a nod to engineering, especially in the (brief) Chapter 4, "The relation between the mathematical theory of elasticity and technical mechanics."

In that section of the work Love discussed the limitations of the mathematical theory:

> "The theory is worked out for bodies strained gradually at a constant temperature, from an initial state of no stress to a final state which differs so little from the unstressed state that squares and products of the displacements can be neglected... [it uses] Hooke's law, and it is known that many materials used in engineering structures, e.g. cast iron, building stone, and cement, do not obey Hooke's law..."

Love also underlined the importance for engineering while acknowledging further specific limitations of the models. Assumptions such as those of strain being proportional to load, and of strain disappearing after removal of load led to poor agreement with experiment in many situations. Nonetheless Love discusses the measuring apparatus in use by engineers, for example Unwin’s extensometer, fitted with automatic recording apparatus to produce stress strain diagram. In Love’s work there are reflections on the scope of theory as well, particular in relation to discussions of factors of safety in cases of impact or sudden reversal of load.

Horace Lamb (1839–1934) was 2nd wrangler in 1872. He became Fellow at Trinity, then took a faculty position in Australia. He subsequently returned to a position in Manchester which he occupied for 35 years, from 1885–1920. Lamb was the author of several textbooks, some elementary, such as his *Higher Mechanics* 1920, though there are many others. The many editions attest to their success. Looking at *Higher Mechanics* today, it appears broadly accessible (in terms of its limited prerequisite knowledge and the pace of the development. It is of interest as having problems (that is, work to be done by the student) that go beyond routine skill-building to address that are both modern and engineering-related. This is particularly noticeable in the concluding chapters on gyrostatic problems, vibrations, and variational principles. This work, and many of Lamb’s works, contain references to international literature both for theoretical content (eg Levi-Civita and Amaldi) and for specific practical examples and thus are useful as a guide to readers of varying levels of training and competence. Lamb’s discussion of gyroscopes in this book was briefly discussed, affording a specific practical application, the marine stabilizer.

Lamb’s best-known work was of course his *Hydrodynamics* a standard for decades with editions in 1879, 1895, 1906, 1916, and 1924. It is a comprehensive “introduction” for a varied audience, and covers a still-standard if classical repertoire including vortex motion, tidal waves, surface waves, expansion waves, viscosity, and many other fundamental subjects in fluid mechanics. Alex Craik made the observation at the meeting, worth repeating, that their own introduction lectures (in the 1960s) continued to be based on Lamb, though their own contact with it began later in their careers.

Lamb explicitly considered his treatment to go beyond what he termed “mathematical method”, where the emphasis should probably be laid on the word “mathematical.” He is interested in things that are, in his expression, physically important. Lamb’s work appears to have been widely influential, particularly in Germany. This and other British works were translated into German: the 3rd edition, at Klein’s urging, in 1907 by Friedel, and the 5th under the auspices of von Mises by Dr. Elise Helly, appearing in 1931. At this point we see the older German tradition beginning to merge with the British, or at least, beginning to absorb its main results. In the translation von Mises added about 60 pages from his own work.

Having looked on the “modelling” side, we turn to analysis and computation in the person of E. T. Whittaker (1873–1956). Whittaker is yet another 2nd Wrangler. He was a Fellow of Trinity to 1906, then took a position at Trinity College Dublin until 1911, where he was also Astronomer Royal. He then moved definitely to Edinburgh. His many well-known works include Whittaker and Watson’s *Course of Modern Analysis* (cf Barrow-Green) with editions 1902, 1915, 1920,

1927. Rather like Riemann-Weber this aims at providing the essentials of analysis for the purpose of application: Fourier series, linear differential equations, asymptotics, and special functions. A more specialized work, his *Analytical Dynamics* of 1904 (second edition in 1917) was translated into German in the Springer *Grundlehren* series in 1924.

Whittaker's stint as an Astronomer left him convinced of the importance of computation, and his literary production included a joint work with Robinson, *Calculus of Observations*, based on lectures to students in the "Mathematical Observatory" of Edinburgh University 1913–1923. The work is a selection of numerical methods, including graphical methods, buts lays considerable importance on hand calculation regimes.

> The material equipment essential for a student's mathematical laboratory is very simple. Each student should have a copy of Barlow's table of squares, etc., a copy of Crelle's "Calculating Tables," and a seven-place table of logarithms. Further it is necessary to provide a stock of computing paper (i.e. paper ruled into squares by rulings a quarter of an inch apart; each square is intended to hold two digits; the rulings should be very faint, so as not to catch the eye more than is necessary to guide the alignment of the calculation), and lastly a stock of computing forms for practical Fourier analysis (those used in Chapter X of this book may be purchased).
>
> Whittaker and Robinson 1924

While this work was not translated, it was purchased abroad: the title page of the copy on the Oberwolfach collection shows the imprint of the *Luftfahrtforschungsanstalt Hermann Goering* in Braunschweig.

3. Concluding Remarks

The mention of translations draws to our attention the fact that these were subjects of international interest in which solutions of particular problems were eagerly sought as part of a repertoire for a variety of uses. These uses include industrial uses, and a nice example of both the international dimension of the field and the applications-oriented nature of the work is provided by Stephen Timoshenko (1878–1902) whose work in Russia, at Westinghouse, and at the Universities of Michigan and Stanford provide an example of international diffusion of many techniques in elasticity theory.

Do these examples move us towards identifying a nascent toolkit for applied mathematics? These examples are intended more to be suggestive of an evolution toward discipline than demonstrative of it. Clearly, the complex networks that link the works and the authors need to be mined thoroughly, and a fuller range of works needs to be explored. In particular, an assessment of the links between engineers and mathematicians, mostly indicated only collaterally in these examples, needs to be made. The role of publishers in the diffusion of work internationally also needs to be explored.

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Turing, the Riemann Zeta-Function, and the Changing Borderline between Pure and Applied Traditions in Mathematics

Leo Corry

In 1953 Turing published an article in the *Proceedings of the London Mathematical Society* with the title “Some calculations of the Riemann zeta function”. This was actually Turing’s last research paper and not one of his most influential ones. It was an investigation related with a classical problem in number theory, the Riemann Hypothesis (RH), in which Turing presented calculations performed with the help of a program running in a *general purpose computer*, the Manchester Mark I. Compared with Turing’s contributions to the foundations of theoretical computer science, artificial intelligence, code-breaking, or actual computer design, this is a much lesser known (and certainly less dramatic) chapter of his scientific and personal biography. It is of historical importance, however, once we add to the picture the interesting fact that, previously, in 1939, Turing had applied for a grant from the Royal Society to support the engineering of a *special* machine to calculate approximate values for the Riemann zeta-function on its critical line.

My lecture discusses Turing’s involvement with the Riemann Hypothesis and the implications of the different approaches followed in these two attempts, which are separated by Turing’s war-time activities and the ensuing dramatic changes in his personal and professional life, as well as in the world of computing at large. I describe the connections of these two episodes within the broader contexts of his own scientific world, as well as that of the British tradition of mathematical table-making. In particular, the lecture uses the perspective of this involvement in order to shed some new light on two main topics commonly discussed around Turing’s 1936 seminal article, where the idea of the Universal Turing Machine (UTM) was first introduced.

The first main topic has to do with the direct relationship between the idea of the UTM and the actual building of a general-purpose, program-stored, digital fast-speed computer, particularly as embodied in the so-called Von Neumann Architecture after 1945. The perspective presented in the talk emphasizes that when proposing the abstract UTM Turing *did not* have in mind the construction of a physical machine embodying this idea. The fact that his early attempts to deal with the RH were conceived in terms of a special purpose machine is at the focus of the historical evidence presented in support of this perspective.

The second main topic discussed in this context relates to the British Mathematical Tables Committee and its activities beginning in the last third of the nineteenth century, and aimed at developing new methods for calculating mathematical tables of various sorts, including in pure mathematics, and especially under the leadership of John Leslie Comrie. The perspective presented in the talk emphasizes the connection of Turing's early mathematical activities with the kind of traditions developed in the framework of the activities of the Committee. It is claimed that this connection played an important role in Turing's active interest in formalizing and then studying the limitations of the idea of computing by one, or a group of, human computers.

"What has mathematics got to do with oil?" – Van der Waerden and applied mathematics

Martina R. Schneider

Bartel L. van der Waerden (1903–1996) published almost as much in mathematics as in the history of science. His contributions to algebra, algebraic geometry and to the history of mathematics and astronomy are well known today. Less well known are his publications in applied mathematics as well as his positions related to applied mathematics.

"What has mathematics got to do with oil?" is the title of a talk given by van der Waerden in the Netherlands after World War II. At that time van der Waerden was head of the department of applied mathematics at the newly founded mathematical research center in Amsterdam and also employed at the research laboratory of the oil company Bataafsche Petroleum Maatschappij (B.P.M., later Royal Dutch Shell). After the war van der Waerden could not get a position at Dutch universities because he had worked at Leipzig University in Germany during the occupation of the Netherlands, and this was interpreted by the general public as collaboration. In 1948, however, van der Waerden was appointed as an extra-ordinary professor of pure and applied mathematics at the University of Amsterdam, a post sponsored by Dutch industry [1]. Applied mathematics was also a regular part of his teaching obligations at Zurich university from his appointment in 1951 until his retirement in 1973.

Van der Waerden's contributions to applied mathematics consist of articles and monographs on quantum mechanics, statistics, and various other topics. They make up circa 20% of his mathematical publications if the bibliography by J. Top and L. Walling [2] is taken as a basis. Thus they form a considerable part of van der Waerden's mathematical research which has only been given little attention by historians of mathematics so far.

In my own study on van der Waerden's research on quantum mechanics [3] I have found several characteristic features of van der Waerden's scientific activity in physics. One feature is that the direct contact van der Waerden had with physicists was of vital importance for getting van der Waerden started. In 1928 it was the Austrian physicist Paul Ehrenfest, professor in Leiden, who got in touch with van der Waerden to ask him questions about Weyl's 1928-book on group theory and quantum mechanics. Van der Waerden was invited by Ehrenfest to give a lecture on the topic, which was well received. Ehrenfest also asked him to develop a kind of tensor calculus for the quantum mechanical structures which Ehrenfest called spinors. This led to van der Waerden's first publication in quantum mechanics in

1929 [4]. Van der Waerden’s network of scientists played not only a decisive role for the genesis of this article, but also for most of his other publications in physics.

Another characteristic feature of van der Waerden’s research activity in physics can be described by the notion of pragmatism. In the case of the spinor calculus mentioned above, van der Waerden developed a calculus tailored to the needs of physicists. The spinor calculus was modelled on tensor calculus. It did not require much research, but rested upon results from representation theory and invariant theory. The reader of van der Waerden’s article on spinor calculus, however, learns hardly anything of these mathematical theories. Instead van der Waerden addressed himself in considerable detail to the questions of how to handle the new calculus and of how to write the relativistic wave equation of the electron in spinor notation. The same kind of pragmatism can be detected in his monograph “Die gruppentheoretische Methode in der Quantenmechanik” (1932) [5]. There van der Waerden promoted and even improved a non-group theoretic method developed by the American physicist John C. Slater. Slater had tried to find a way around group theoretic methods in quantum mechanics as these methods were quite controversial at that time.

In the talk I have explored in how far these two characteristic features of van der Waerden’s approach to physics can also be attributed to his research in statistics.

Van der Waerden started publishing articles on statistics in 1935 when he was professor at Leipzig University. This first set of papers (1935–1943) is related to medicine and bio-assay. A central question was how to deal with small samples. Most of these publications appeared in the *Berichte* of the Saxonian Academy of Sciences in Leipzig. But van der Waerden also published in practitioner’s journals like the *Klinische Wochenschrift* for medical doctors or the *Archiv für experimentelle Pathologie und Pharmakologie* for physiologists. It was only at the end of the 1930s that he learned about the “English statistical school”, in particular about distribution-free tests [6]. From the publications it is not clear how van der Waerden came about these questions. There is only one little clue: a joint-article with Martin Gildemeister, director of the Institute of Physiology in Leipzig and, like van der Waerden, member of the Saxonian Academy of Sciences.

At the beginning of the 1950s, van der Waerden developed his own distribution-free rank order test, the so-called X-test. In the articles we find traces of an exchange with David van Dantzig. Van Dantzig was a friend and former colleague of van der Waerden, and head of the department of statistics at the Dutch mathematical research center. The research center also helped to calculate the tables for the X-test [7]. It was van Dantzig who drew van der Waerden’s attention to papers on distribution-free tests by Erich Lehmann and also by other statisticians. Van der Waerden got in touch with Lehmann, and Lehmann spent his sabbatical year with van der Waerden in Zurich in 1956/57 and returned again for six months in 1959/60.

In 1957 van der Waerden published a text book on mathematical statistics in German [8]. In the preface he explained how he was drawn into the field: “Ever since the days as a student, economists, doctors, physiologists, biologists, and engineers have come to me with queries of a statistical nature." The book was based, according to van der Waerden, on a draft written in 1945 and used in a lecture course on error calculation and statistics at B.P.M. He had already announced a lecture course on probability theory at Leipzig University for the winter term 1944/45, however it is not clear whether the course did actually take place.

It was Lehmann who suggested and supported the translation into English of van der Waerden's text book on mathematical statistics. He was impressed by the 50 examples in the book of the application of statistics to different fields taken, as van der Waerden pointed out, not from theory, but from practice. For van der Waerden, the usefulness of statistics was not only restricted to the natural sciences. He also considered statistics useful in demography, economics, industrial applications, psychology, and even in history.

One example of the use of statistics in the humanities is worth mentioning, namely its application to the history of astronomy. Van der Waerden used statistical methods in the discussion of a Byzantine sun table in example 19 of his book. He also tried to convince Otto Neugebauer of the use of statistical testing for the determination of theories about Babylonian intercalations between 529–385 BCE. After an exchange of letters in 1958/59 (see Nachlass van der Waerden, ETH-archive Zurich) the latter, however, remained unconvinced.

Summing up, I have pointed out that there are some indications that the two characteristic features of van der Waerden's research in physics might also play a role in his involvement in statistics. However, further research needs to be done with respect to both of the features: i) to determine more exactly van der Waerden's network of scientists relevant to his statistical research at various places and times, and ii) to analyze how pragmatic van der Waerden's approach actually was despite his own claims in this direction.

The case study also raises the more general question of how typical van der Waerden's research activities in applied mathematics were for mathematicians at the time.

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Mathematical Modeling, Mathematical Consultants, and Mathematical Divisions in Industrial Laboratories

Renate Tobies

“Applied” Mathematics and training of mathematical consultants

Around 1900, mathematics became a constitutive element of the newly formed technical sciences (electrical engineering, mechanical engineering, etc.). It was in this context that mathematicians began to discuss, in concrete terms, what was meant by “applied mathematics”. Developments in Germany, especially at Göttingen, played a leading role in this international process. Carl Runge (1856–1927) and Rudolf Mehmke (1857–1944), driven by Felix Klein (1849–1925), arranged the *Zeitschrift für Mathematik und Physik* [Journal for Mathematics and Physics] into a journal only for applied mathematics since 1901, and explained:

> Though it remains a matter of dispute what counts as “pure” and what counts as “applied” mathematics, we hope that our readers will approve if we refrain from drawing the sharpest lines between the two subjects. In addition to the fields discussed in volumes 4–6 of the *Encyklopädie der mathematischen Wissenschaften* [Encyclopedia of the Mathematical Sciences] – namely mechanics (especially technical mechanics), theoretical physics (including mathematical chemistry and crystallography), geophysics, geodesy, astronomy – and in addition to the essential fields of probability and regressions analysis, statistics, and actuarial mathematics, we are also interested in cultivating the following disciplines: numerical analysis, approximate calculation (“approximation mathematics”), the theory of empirical formulas, descriptive geometry (in conjunction with shadow generation and perspective), and graphical analysis. It is with the methods employed in these fields, above all, that applied mathematics is executed to its fullest capacity. Moreover, we would like to devote considerable attention to the technical instruments that are used by the practitioners of these fields, including numerical and graphical tables, mechanical calculators, and graphical instruments. [1, pp. 8–9]

When Runge accepted the first full professorship of applied mathematics at the University of Göttingen in 1904, Klein abandoned his own, discipline-based understanding of the subject. Beginning with a new examination requirements that were established for future secondary school teachers in 1898, the heart of the applied mathematics program in Prussia had been the fields of descriptive geometry, technical mechanics (including graphical statics and kinematics), and the geodesy (including probability theory). When, in 1907, German supporters of applied mathematics gathered at the University of Göttingen, they issued the following formulation:

> "The essence of applied mathematics lies in the development of methods that will lead to the numerical and graphical solution of mathematical problems." ["Das Wesen der angewandten Mathematik liegt in der Ausbildung und Ausübung von Methoden zur numerischen und graphischen Durchführung mathematischer Probleme."] [4, p. 518]

Numerical, graphical, and instrumental methods put in the center of attention supported by industrialists and internationally linked. Klein inspired the book *Über die Nomographie von M. d'Ocagne* (Leipzig: B. G. Teubner, 1900) written by his former doctoral student Friedrich Schilling (1868–1950); it was published already one year after Maurice d'Ocagne's (1862–1938) *Traité de nomographie* (Paris: Gauthier-Villars, 1899). Klein also stressed John Perry's (1850–1920) *Practical Mathematics* (3d edition London 1899; German edition Wien 1903) and *Calculus for engineers* (3d edition London 1899; German edition Leipzig: B.G. Teubner 1902). Perry had propagated a laboratory method with the training of the use of tables and mechanical instruments that was developed further under Carl Runge in Göttingen. Runge had developed his procedure for solving ordinary differential equations in the 1890s, today known as Runge-Kutta procedures. He and his doctoral students published books on *Practical Analysis* (translated also in English); and as a result of his visiting professorship at Columbia University in 1909–10, Runge published his book *Graphical Methods* (1912) first in English.

The methods (numerical, graphical, instrumental) were used, trained, developed further in the framework of research seminars of applied mathematics at the University of Göttingen, where not only future secondary school teachers but also future mathematical consultants in industrial laboratories took part including scientists who later promoted applied mathematical fields in the United States: for example George Ashley Campbell (1870–1954), Stephen P. Timoshenko (1878–1972), and Roland G. D. Richardson [5, pp. 57, 64–69, 278], [3].

A New Style of Thinking in Industrial Laboratories: "Calculation instead of trial and error"

It was a novel turn of events when calculation began to hold sway over experimentation in the laboratory setting. From a chronological perspective, the new style of thinking (following Ludwik Fleck's term) entered first the laboratories at optical workshops respectively companies, followed by Companies of Heavy-Current/Power Engineering (AEG, Siemens-Schuckert, General Electric Company), communication industry especially since the 1920s, and since the 1930s aviation and steel industries also became a job market for mathematicians.

In order to achieve repeatable products advance calculation has become increasingly important. Researchers who had completed their doctorates in mathematics already in the 1890s have been recruited as mathematical consultants and headed mathematical divisions at the famous Carl Zeiss Company in Jena. The early successful international research cooperation in this field, for example between Moritz von Rohr (1868–1940), Carl Zeiss Company, and the Swedish ophthalmologist Allvar Gullstrand (1862–1930) is a largely uncharted field of research.

We know that Charles P. Steinmetz (1865–1923) headed a mathematical division at GE since 1894, the same did Reinhold Rüdenberg (1883–1961) at the Siemens-Schuckert Company since 1908; Cäcilie Fröhlich (1900–1992) who had completed a doctorate in mathematics at the University of Bonn worked as a mathematical consultant at the AEG (General Electric Company) in Berlin from 1927 to 1937, at the "Ateliers de Constructions Electriques de Charleroi" in Belgium, and later in the United States [5].

By examining the work of Iris Runge (1888–1966), the eldest daughter of Carl Runge, I was able to identify not only an esteemed and competent mathematical consultant at the Osram Company but also a group of similar researchers at Telefunken in the 1930s (a similar group had existed at the Bell Telephone Laboratories since 1928). My research enabled me to show how an industrial researcher had constructed mathematical bridges between statistics and quality control of mass production; between the physical and chemical methods of material research and the concrete problems of manufacturing conductors, filaments, bulbs, and electron tubes; and between the models of theoretical physics (optics, colorimetry, etc.) and the design of scientific instruments. It could be demonstrated, with examples, how simplified models were employed to enhance the understanding of industrial processes and production.

A general procedure for solving mathematical problems was developed and employed internationally. John R. Carson, a member of the Mathematical Research Department at the Bell Telephone Laboratories, provided an accurate description of the procedure as early as 1936:

> The art consists in seeing how to go at a problem; in knowing what simplifications and approximations are permissible while leaving the essential problem intact, in precise formulation in mathematical terms, and finally, in reducing the solution to a form immediately interpretable in physical and engineering terms. [2, p. 398]

This remains same foundational approach to problem solving that is still characteristic in industrial mathematics.

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On the Very Notion of Applied Mathematics

José Ferreirós

We have found along this meeting many expressions of the idea that basically there is only one discipline mathematics, and that the notion of applied mathematics is problematic (von Neumann). I share the view that the historian should not employ the category of "applied math" as a properly historiographical one, although of course we should study its rise and the evolution as an actor's category. Even more, I tend to think that avoiding that category is a positive contribution to the philosophical understanding of mathematics.

The aim of my talk is to clarify some differences between the contrasts pure/mixed and pure/applied as they operated in the past; to propose some basic methodological ideas about the study of applied math in practice; and to discuss briefly some famous views about the "applicability of mathematics".

Notice that, as of 1800, almost nobody seems to have thought of asking why mathematical notions are applicable, let alone suggest that their effectiveness was "unreasonable". Six generations of mathematicians later, we find Nobel Prize Eugene Wigner (professor of physics and mathematics at Princeton University) having great success with his 1960 talk on the "unreasonable effectiveness" of mathematics. He ended by saying:

> The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better or for worse, to our pleasure, even though perhaps also to our bafflement, to wide branches of learning.

I would like to suggest that one of the main reasons for this shift is cultural and institutional. In the early 20$^{\text{th}}$ century, the image of mathematics as a pure discipline, fully autonomous from anything else, and in fact hardly comparable with 'the sciences', was well established within and without the math community. This happened despite the fact that the most influential mathematicians of 1900 – Poincaré and Hilbert – did *not* believe in such a picture.

Importantly, this cultural image of mathematics was not only shared by a large community of specialists, but, one can argue, was *strongly institutionalised* in Math Depts, first in Germany, then (surprisingly perhaps) in the USA, and then elsewhere. Felix Klein (1926) said of Jacobi's school that it is "a scientifically oriented Neohumanism, which sees its objective in an inexorably rigorous cultivation of pure science," and this orientation endured by being institutionalized. [On the adaptation of mathematicians to the environment of Neohumanist Philosophy Faculties, see among others [5]; for the USA see [8].] This institutional factor is highly important.

1.

In matters like this, when the topic has to do with broad divisions in the organization of knowledge and learning, it is useful to employ the categories of *body* and *image* of knowledge [2]. The body of knowledge relevant to a certain field, or to a group of practitioners, has to do with their skills and toolkits (more on this below), while the image of their discipline or their particular field is affected by broader factors. We have seen examples of how philosophical elements can be relevant here, but there are also sociological and institutional factors, and quite importantly educational orientations which shape up the image of the discipline for a new generation.

To put it bluntly and provocatively, consider what Vladimir I. Arnol’d had to say [1]:

> Mathematics is a part of physics. Physics is an experimental science, a part of natural science. Mathematics is the part of physics where experiments are cheap. . . .
>
> In the middle of the twentieth century it was attempted to divide physics and mathematics. The consequence turned out to be catastrophic. Whole generations of mathematicians grew up without knowing half of their science and in total ignorance of any other sciences.

While the first sentences may be ironic, pointing to ‘experiments’ on paper or on the computer, the criticism raised in the second paragraph is seriously taken. Obviously, if your conception of math coincides with Arnol’d’s, issues having to do with applied math and related matters will appear in a different light. His views resonate with Fourier (1822: “Profound study of nature is the most fertile source of mathematical discoveries”) and many other mathematicians of the past.

It is useful to consider schematically the contrast between images of mathematics in the 18th and the 20th centuries. I shall simplify quite a lot, but the resulting map is clarifying when properly employed.

2.

Up to 1800 mathematics was usually defined as the Science of magnitudes: it “is nothing more that the *science of magnitudes*”, said Euler [4, p. 4]. This is the setting for the pure/mixed contrast. Notice that this perspective immediately points to the real world and to the later called ‘applications’: magnitudes are given to us everywhere in Nature (distance from the MFO to the train station; amount of water we have spent this week; etc.). Quite importantly, the *foundations* of mathematics is not itself a topic for mathematics – but rather for philosophy of metaphysics. You know well how around 1800 there was heated debate about “the metaphysics of the calculus”; also Gauss typically referred to the “metaphysics” of space or of the complex numbers [5]. Even as late as the 1880s Kronecker would still insist that a discussion of the number concept (like discussions of space or time) pertain to the “open field” of philosophy and not in the fenced space of a particular scientific discipline.

In discussions of traditional math vs. modern math, around 1900, it was not infrequently emphasized that there is a contrast between Empiricism of the former and Idealism of the latter (du Bois-Reymond, Kronecker, Baire and Borel, etc.).

This can be easily understood: in the traditional perspective, numbers are *relations* between magnitudes, their 'existence' is derivative and unproblematic. The foundational problem of mathematical existence arose with the modernistic spirit, which conceived of mathematics as perfectly autonomous and pure, and *alienated* from the sciences [6]. Autonomous, self-contained, self-justified: this was the dream (one can find here in the background many philosophical and cultural elements).

Around 1950, mathematics was usually defined to be the *Science of structures*: "as we all know, all mathematical theories can be considered as extensions of the general theory of sets" [3]. Notice that this perspective does not point to the real world, but to the internal scaffolding of structures, concepts, and objects of a now autonomous math. Here is where the contrast pure/applied was shaped. The *foundations*, in strong contrast to the previous situation, is itself a topic for mathematics – logic, set theory, proof theory – not for philosophy. Numbers, sets, functions, structures, are "pure objects" of mathematics, independent from the real world. Mathematical existence has nothing to do with real existence (consider Hilbert's famous proposal) and it has become problematic.

Another aspect that I would like to emphasize is the strong differences in the meaning of "abstract". Within the science of magnitudes, one certainly can consider magnitudes disregarding their concrete properties and concentrating on their relations; thus we arrive at the abstract parts of the discipline (pure math), while consideration of peculiar kinds of magnitudes (time, motion, etc.) is characteristic of mixed mathematics. By contrast, in the setting of modern mathematics *abstract* points to freely defined structures, which are taken to be independent from the "real world", autonomous. Applied math emerges when those structures are employed in relation to another discipline, be it physics, engineering, biology, or economics. The second meaning, unlike the first, does pose a problem of how to account for the "applicability".

3.

Several participants to the workshop have discussed aspects that one ought to consider in applied mathematics as an activity. I propose to amplify a bit the list of facets of the practice of applied mathematics that can profitably be considered: 1. The *networks* of practitioners involved, which of course need not be just mathematicians, for one is interested in detailed knowledge of their interactions with engineers, scientists, politicians. 2. The aims and *purposes* of their work, which as Lützen remarked are definitory of this practice as such (which reminds me of what happens with science vs. technology). 3. The *skills* deployed by the practitioners, also in order to maintain their interactions with other agents involved, and the *constraints* accepted, which may be stronger than in some branches of so-called pure math. 4. The *interpretation* proposed of the target situations, which again is both a central problem of these practices and a reason why certain skills and tools are needed (in recent decades one speaks here of *modeling*, but the phenomenon is broader). 5. The *toolkits* that are required, to borrow Archibald's term, i.e., results, methods, theories. 6. The *values* that are put in effect, revealed through the language employed for appraisal and evaluation (which changes significantly from branch to branch of math), e.g., when a previous contribution is deemed limited and an improvement proposed.

4.

A last part of the talk was devoted to some critical remarks on Wigner’s paper (1960), which I believe is much more quoted that read – with the effect that its contents are less influential than its mere title. On reading it again, I find it more interesting than expected, particularly some ideas having to do with limits of physical theories. But this is not what people remember. I also find that Wigner’s perspective of mathematics, which exemplifies the impact of the 20^{th} century image in its formalistic version, amounts to a severe misrepresentation of mathematical knowledge, of its historical evolution, and of its relations to the sciences. The mathematics of 1800 was deeply engaged with techniques and with natural science, it had certainly proven its usefulness, while developments since then ought to be expected – if anything – to enlarge the spectrum of applications and to facilitate them. However, there is not enough space here to deal with this issue.

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