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Applying Models in Fluid Dynamics

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Abstract. The following article treats the ”applicational turn” of modern fluid dynamics as it set in at the beginning of the 20th century with Ludwig Prandtl’s concept of the boundary layer. It seeks to show that there is much more to applying a theory in a highly mathematical field like fluid dynamics than deriving a special case from a general explanatory theory under particular antecedent conditions. In Prandtl’s case, the decisive move was to introduce a model that provided a physical/ causal conception of viscous flow at high Reynolds numbers. It facilitated an approximate solution to the Navier-Stokes equations, which in turn gave rise to many special applications. After a detailed account of Prandtl’s achievement, the article discusses the role of the physical model and its experimental and mathematical significance. It is shown that the mathematical simplification provided by the physical model greatly expanded the explanatory capacity of the theory which the Navier-Stokes equations alone could not provide.

According to the covering law account of scientific explanation, a phenomenon is explained by ”subsuming it under general laws, i.e. by showing that it occurred in accordance with these laws, in virtue of the realisation of certain specified antecedent conditions.” (Hempel 1965, 246) It has become apparent that this conception implies a ”structural identity of explanation and prediction.” (Ibid., 367) Prediction and explanation, this thesis says, differ from each other not in logical structure but only in the temporal relation between the moment when the phenomenon happens and the time when a statement about the subsumption is uttered. When an event is explained it has already happened, whereas when it is predicted its occurrence lies in the future. Everything else stays the same. Subsequent criticism has convincingly shown that this assertion of structural identity is deeply flawed and that the covering law account of explanation is responsible for this failure. If a barometric reading correctly predicts a storm, this reading cannot be taken to explain it, although it should do just that if the covering law account, along with its identity claim, were correct. The moral to be drawn is that explanation and prediction have to be distinguished.

It seems to me that there is another identity thesis that is closely related to the former and no less effective, although it is less frequently discussed. It can be called ”structural identity of explanation and application” of a theory. It says that applying a theory to a special problem, e.g. an event or a specific empirical regularity, is a matter of deriving the individual case from the fundamental principles of the theory under suitable circumstances. To apply a theory to a problem means generating a description of the case in question and showing that it is implied by the principles. When the linear oscillator is explained by Newtonian mechanics, we produce a description of it and show how it can be derived from Newton’s laws. The only difference between explanation and application lies in the way the antecedent conditions are given: when a theory is to be *applied* to a special problem, the problem itself delivers us the antecedent conditions whereas in an *explanation* of a problem some imagination is needed to come up with the appropriate circumstances for the case in question. I think that Nancy Cartwright raises the same issue when she rejects the widespread view that scientific theories ”explain by dint of the description they give of reality,” i.e. that ”once the job of describing is done, science can shut down.” (Cartwright 1983, 44)

In the following I would like to criticise the second identity thesis and contribute to a more adequate account of what it means to apply a theory to special problems. In particular, I would like to deal with the way a physical theory is applied to *engineering* problems. I will do so by concentrating on the example of modern fluid dynamics as a case study. It will become clear that special kinds of modelling are central to this discipline and that concepts of explanation and application have been developed that take the role of models into account.

1. Prandtl’s concept of the boundary layer

Fluid dynamics stands out from other engineering disciplines insofar as its modern history seems to be less ”messy” and more straightforward than, for example, the history of electrical engineering. Its modern ”applicational turn,” as one might call it, was relatively abrupt and occurred in a special and isolated local setting. The onset of modern model building in fluid dynamics can easily be dated. It came at the beginning of the 20th century with the work of Ludwig Prandtl (1875-1953), who found a way to bring together the purely empirical engineering tradition of hydraulics and the purely theoretical mathematical tradition of rational mechanics as it had developed in the 18th century. (On Prandtl’s work see Großmann 2004, Eckert 2003, the contributions of Germain, Gersten and Zierep in Meier 2000, the introduction to Schlichting 2000, Morrison 1999, as well as chapters or sections of Graebel 2001, Siekmann 1998, Gad-el-Hak 1998, Anderson 1997, Wegener 1996, Vincenti 1990, Nickel 1984, Böhme 1983, Tani 1977, Neményi 1962-66, Rouse & Ince 1957, Kármán 1954, Goldstein 1938. On Prandtl’s ambivalent political role in Nazi Germany see Eckert 2005, 130f., Trischler 1994, 78-86 and Tollmien 1987.)

Practical engineering problems in fluid dynamics – like flow around objects in fluids or gases – have to take into account the inner friction of these liquids, i.e. their viscosity. The traditional mathematical means that were brought to bear upon these problems are the Navier-Stokes equations. Major contributions to them were given by Navier in 1823, Cauchy in 1828, Poisson in 1831, Saint-Venant in 1843 and Stokes in 1845. These equations are a system of non-linear, second-order partial differential equations, which pose formidable problems and do not have a general analytical solution.

A certain simplification of these equations is achieved if one assumes incompressibility of flow, which is given for flow speeds of less than a third of the speed of sound, i.e. for almost all liquid and many gas flows in everyday and engineering experience. Yet this does not help very much to overcome the difficulties of the equations. Since the viscosity of the majority of engineering problems is small, i.e. their Reynolds-number is high, the natural thing to do was to make another idealising assumption and try to find a solution by neglecting viscosity. The Reynolds-number Re, as suggested by Osborne Reynolds in 1883, is defined as $\rho\ V D\ /\ \mu$, which is equal to $V D\ /\ v$. V is the velocity, D a characteristic geometric dimension such as body length, pipe diameter or chord of an airfoil, ρ the mass density, μ the dynamic viscosity coefficient, and v the kinematic viscosity of the fluid. This approach was all the more attractive since the motion of inviscid, incompressible, steady flow was well understood and yielded explicit solutions for some body shapes. The mathematics for this is given by the Euler equations, which can be obtained from the Navier-Stokes equations by neglecting the friction terms. From the Euler equations one can obtain the Bernoulli equation, which gives solutions to many concrete flow problems.

However, as d’Alembert had already observed in 1768, the Euler equations yield a drag of zero for a moving body in an infinite stream. Drag is the force experienced in the direction opposite to the body’s movement. A body moving through air or water would cause no force – a result, called d'Alembert's paradox, that is falsified by everyday experience, e.g. the experience of throwing a ball. So even for problems involving low viscosity, mathematics could not help. Engineers had to rely on rules of thumb and experiences obtained from experimental model-building. Around the turn of the 20th century, the British chemist and Nobel laureate Sir Cyril Norman Hinshelwood lamented that fluid dynamists were divided into hydraulic engineers who observed things that could not be explained and mathematicians who explained things that could not be observed. (See Gad-el-Hak 1998, 181)

The discrepancy between the engineering and mathematics tradition in fluid dynamics was resolved virtually with one stroke in a short paper by Prandtl in 1904. (Prandtl 1905) In this article, Prandtl gave an outline of his so-called "boundary layer" [*Grenzschicht*] theory, which separated the problem of viscous flow into two interacting components. The first component is the laminar boundary layer: a thin viscous layer near the surface of a body, in which the viscous drag is isolated. The velocity of the liquid in this layer decreases from a finite value U (the velocity of the fluid outside the layer) to zero at the surface of the body itself. The layer thus fulfils the so-called "no-slip" condition, first proposed by Daniel Bernoulli in 1738, which almost all known (real) fluids and gases satisfy. Exceptions are, for example, rarefied gas flows and non-Newtonian flows. The second component of the problem is the free flow outside of the boundary layer, which Prandtl assumed to be inviscid and irrotational and thus amenable to the Euler equations and Helmholtz's principles.

Prandtl showed, albeit in a qualitative way, how the phenomena of flow separation and transition to turbulence can be explained by the interaction of the two components. As a result of surface friction, the boundary layer starts to develop vortices, separates from the surface, and pushes its way through the outer flow. The separation marks a transition from a smooth, laminar state of the layer with low drag to a chaotic, turbulent one with higher drag. The problem of reducing drag is thus transformed into the problem of suppressing or delaying flow separation. For the first time, there was hope that the principles governing the problem might after all be found. And indeed, Prandtl's approach also proved decisive in his later analysis of lift and drag of airfoils and of vortical and turbulent flow.

Although it is very thin in liquids like water and air, the boundary layer has a considerable effect on the free flow because of its strong velocity gradient. The higher the Reynolds-number, i.e. the longer the body in the flow direction or the faster the velocity of the outer flow, the thinner the boundary layer becomes. The movement of a body in a fluid, e.g. a flying airplane, or the flow around an object, e.g. the flow of a river against a pier, can now be seen as the result of an interaction between the two components. There are thus two intertwined moves that are decisive for Prandtl's approach: first, to divide the situation into two separate components; second, to assign a substantial causal role to the boundary layer and to confine the viscous effect to it. One can say that modern fluid dynamics arose from a fusion of the boundary layer concept and ideal-flow theory with all its splendid mathematics.

The old separation of hydraulics and rational mathematics survived, however, in a subtle but effective controversy between a purely mathematical approach to fluid dynamics and an applied one, as exemplified by the disciplinary confrontation between Garrett Birkhoff (1911-1996) and Theodore von Kármán (1881-1963) in the 1940s and 50s. Birkhoff, a specialist in modern algebra and especially lattice theory, did not see the original sin of earlier mathematical fluid dynamics in the neglect of viscosity, as Kármán, the legendary master-pupil of Prandtl, did, but in the lack of "deductive rigor" among engineers. (See Vincenti & Bloor 2003)

Prandtl's model made it possible to obtain approximate mathematical solutions for non-linear viscous flows and to predict the flow under changing circumstances. It has already been pointed out that some solutions for linear viscous flows, such as fully-developed laminar pipe and channel flows, as well as creeping flows, were known long before Prandtl. In the case of laminar stationary flow, the momentum and energy equations are parabolic and can therefore be calculated more easily than in the Navier-Stokes case. The number of equations and unknown variables is thus reduced for the viscous part and the applicability of the Euler equations maintained for the outer flow. Prandtl thereby arrived at the following basic differential equations for stationary flow in two dimensions:

$$\rho\,(u\,\partial u/\partial x + v\,\partial u/\partial y) + dp/dx = k \cdot \partial^2 u/\partial y^2$$

$$\partial u/\partial x + \partial v/\partial y = 0$$

Boundary conditions: for $y = 0$: $u = 0$, $v = 0$
for $y = \infty$: $\mathrm{u} = U\,(x)$

u and v are the velocity components in x and y direction respectively, ρ the density of the fluid, p its pressure, k its friction (coefficient of dynamic viscosity; nowadays written as μ). If dp/dx and the development of u in the outer layer are known, there exists a simple numerical approximation procedure to calculate the solution. Prandtl showed in his paper how to use a standard numerical technique to calculate the drag of water flow along a flat thin plate as caused by friction on the surface of a body. In 1907, Prandtl's student Heinrich Blasius (1883-1970) was able to extend this to cylinders. He reduced the above equations to an ordinary third-order differential equation. (Blasius 1908. Cp. Hager 2003 for a detailed discussion of Blasius' work.) Since many engineering problems require the reduction of drag, e.g. on planes and ships, Prandtl's approach became all the more attractive.

Prandtl also presented experimental, albeit only qualitative, evidence for his proposal. The experiments were carried out in a small channel in which the water could be set in motion by means of a paddle-wheel. The original piece still exists and is on exhibition in the Deutsches Museum in Munich. (Eckert 2003) He put a cylinder into the flow in which mica was suspended in order to visualize vortices and other deformations. Mica, or micaceous iron ore, is "a mineral consisting of microscopically small, reddish and very lustrous scales," as Prandtl explains. (Prandtl 1927, 761) Prandtl hit upon this substance by accident around 1900. The cylinder had a small slit at the point where flow separation occurred. The fact that flow separation disappeared when water was pumped out of the cylinder and the boundary layer thus sucked off was proof for the hypothesis that there is in fact a boundary layer and that its interaction with the outer flow is responsible for the observed phenomena. If the water had not been pumped out, separation of the flow from the cylinder wall would have occurred, i.e. the boundary layer would have acted out its causal effectiveness.

Gustave Eiffel found in 1912 that if the Reynolds number of a flow is increased beyond a certain limit, a sudden delay of separation and a reduction of drag sets in. Prandtl could explain this by the sudden change of the laminar nature of the boundary layer to a turbulent one and was again able to show how this could be used to control drag. In 1921, Prandtl's student Karl Pohlhausen (1892-1980) showed in his dissertation that in many cases of flow the velocity distribution can be represented by relatively simple polynomials. He used the so-called "integral form" of the boundary layer equations as introduced by Theodore von Kármán the same year. (Pohlhausen 1921, Kármán 1921) These and similar developments made the boundary layer theory more and more attractive to working engineers. (For a discussion of Pohlhausen's work, see Millsaps 1984)

International reception of Prandtl's approach, however, was slow. It began to come in the late 1920s and early 1930s and was not complete until after World War II. Michael Eckert has shown that American aeronautical institutes had become familiar with Prandtl's airfoil theory not long after the end of World War I, and that the Englishman Hermann Glauert was so impressed by Prandtl's work at a visit to Göttingen in 1921 that he subsequently managed to persuade his colleagues at home of the advantages of Prandtl's airfoil theory. (Eckert 2005, 125) Herrmann Schlichting suggests that Prandtl's "Wilbur Wright Memorial Lecture," which he delivered in 1927 at a meeting of the Royal Aircraft Society, where he also showed a film of a cylinder in a flow, brought the breakthrough. (Schlichting 2000, xxi; Prandtl 1927) The first comprehensive review in English of the boundary-layer tradition appeared in 1935. (Prandtl 1935)

In the 1950s and 60s, S. Kaplun, P. A. Lagerstrom and M. Van Dyke developed a powerful mathematical technique for deriving asymptotic expansions of Navier-Stokes solutions. This so-called "method of matched asymptotics," which built upon results attained by K. O. Friedrichs and Prandtl in the 1940s, assumes that inner and outer solutions of the flow are connected – that is, that in their domains they share an overlapping region in which the solutions are equally valid. (See Ting 2000)

It should be mentioned that Prandtl's boundary layer concept also proved fruitful for the study of heat transfer. Ernst R. G. Eckert (1904-2004) hit upon this idea in the early 1930s and summed it up in his influential *Einführung in den Wärme- und Stoffaustausch* of 1949, which came out in English translation a year later (Eckert 1949. On Eckert's work and career, see Dawson 1988.). In 1946, both Eckert and Karl Pohlhausen were recruited under the program "Project Paperclip," which brought hundreds of German scientists and engineers to the United States to support military research and governmental and industrial laboratories. The operation had a huge impact on American aerospace and weapons technology over the following decades. (Hunt 1991)

2. The role of Prandtl's physical model

What exactly is the aim of Prandtl's model and how does it serve it? As I have already pointed out, one purpose of Prandtl's model – or rather twofold model, one for the inner and another for the outer flow – is to simplify the mathematics and to make the partial differential equations applicable to cases that are interesting from an engineering point of view. The other goals, which go hand in hand with the first, are to broaden or deepen understanding of the physical phenomena and to make new testable predictions about them.

The important question to be raised now from a philosophical point of view is how these goals are achieved in Prandtl's model. Before I try an answer to this, I would like to invoke the distinction, proposed by Heinrich Hertz in the 1880s and 1890s, between a "theory" and its underlying "physical representation" [*physische Vorstellung*] (See Heidelberger 1998, 18). By "representation," Hertz meant the picture or physical idea of the intended objects of the theory. He also used the expressions "physical interpretation," "physical meaning," "images in thought" and "intuition" as alternatives to "representation." The form we give our representations, Hertz writes, "is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured." (Hertz 1899, 1)

From the "representation" of a theory, Hertz also distinguishes the theory's "presentation" or "expression" [*Darstellung*], i.e. the mathematical devices and concrete visual aids used to present it. Hertz had developed these distinctions in dealing with Maxwell's electrodynamics and its contemporary rivals. He took into account that a theory of a certain domain can be compatible with different physical representations. A follower of Helmholtz's representation of electrodynamics in terms of potentials and action at a distance could accept Maxwell's equations without being forced to accept Maxwell's mature representation of them in terms of electromagnetic fields. "The representation of the theory in Maxwell's own work, its representation as a limiting case of Helmholtz's theory, and its representation in the present dissertations [i.e. his own representation] have substantially the same inner significance [= mathematical content].” (Hertz 1893, 21)

Can we take Hertz’s conception over into fluid dynamics and use his concept of representation to characterise Prandtl’s physical model? Not without a certain modification. Prandtl’s model is *not* a physical representation of the Navier-Stokes equations, but of a theory whose mathematical content is an *approximation* to these equations. In a more general way, then, we can say that a physical model of a set of continuity equations is either a physical representation of these equations themselves or of a mathematical structure that approximates the equations.

In Prandtl’s case, this would amount to the following: once a continuum approximation and a quasi-equilibrium approximation are assumed, the Navier-Stokes equations do not by themselves suggest how they are to be represented physically. Such a representation is added through Prandtl’s assumption that object flow has to be conceived as the interaction of viscous and inviscid flow. This model is consistent and experimentally confirmed, but it is not a representation of the Navier-Stokes equations as such. Instead, it represents an approximation to the Navier-Stokes equations that is applicable to practical engineering problems.

Using this terminology, we can rephrase the question of exactly how Prandtl achieves his goals, breaking it up into two parts: 1. How does Prandtl’s model differ either from received physical representations of the basic equations or from representations of approximations to them? 2. How does it affect the theory and its presentation?

Michael Redhead has introduced a distinction between two kinds of models in physics: those that are ”enriched” theories and those that are ”impoverished” ones. (Redhead 1980) Sergio Sismondo has pointed out that this dichotomy cannot be exclusive since a model can be both, an enriched and an impoverished theory at the same time. (Sismondo 1999, 253f.) Instead of saying that a model develops a *theory* further by enriching or impoverishing its assumptions, I prefer to say, in accordance with Hertz, that a model is not a theory in itself but a *representation* of a theory. Thus, a new model of a theory modifies a received representation of a theory by enriching or impoverishing it.

This suggestion can be spelled out for Prandtl’s case in the following way: the physical representation normally accompanying the Navier-Stokes equations is enriched by the concept of the boundary layer. (This answers the first question.) The enrichment leads to a simplification (impoverishment) of the theory itself, or, if you prefer, of its mathematical presentation, by substituting an approximation for it that changes the elliptic character of the Navier-Stokes equations into a more easily handled parabolic character. (This is part of the answer to the second question.) It must be stressed that enrichment, on the one hand, cannot be seen as the mere addition of a new factor to an otherwise unaffected theory, nor impoverishment, on the other hand, as the mere neglect or elimination of useless theoretic elements, without which the core of the theory remains intact. Moreover, these two processes cannot be seen as correcting a theory by substituting true elements for wrong ones. A much more apt description of this modelling process would invoke Thomas Kuhn’s expression and call it ”theory articulation.” In Winsberg’s suitable words, this is the ”nontrivial process of bringing a theoretical structure into resonance with some phenomena.” (Winsberg 1999, 286) In contrast to Kuhn, however, and in accordance with Prandtl’s case, this process cannot be conceived just as the spelling out of what is already implicit in the paradigm. It seems rather that in mature sciences theory articulation is a form of genuinely new knowledge production. (Cp. Winsberg 1999)

As a consequence, applying a theory to concrete engineering situations – i.e. making it deliver a description of certain processes in its domain – is not just a matter of bringing a descriptive content to the fore that is already inherent and implicit in the theory. Rather, it is a creative process whereby a model of an approximation to the theory is found that makes it possible to solve a problem which otherwise would remain unsolved. This makes Prandtl's move a nontrivial enterprise.

Making the mathematics tractable is one effect (and one purpose) of Prandtl's model. It also plays the role of broadening and deepening our understanding of fluid flow. What this means should be spelled out in more detail. Carl G. Hempel explicitly proposed the view that broadening and deepening understanding has to be conceived of as an improvement of a covering law explanation. A new explanation achieves an *increase in breadth* in relation to a precursor if the theory covers "a wider range of occurrences than do the empirical laws previously established." And it *deepens* our understanding if at least one of the following conditions is fulfilled: 1. "It reveals the different regularities exhibited by a variety of phenomena [...] as manifestations of a few basic laws." 2. It describes the empirical generalizations known so far as good approximations of a new fundamental theory within a certain limited range. (Hempel 1965, 345) One gets the impression that in developing these ideas Hempel generalized the standard account of the Kepler-Newton relationship, on the one hand, and the Newton-Einstein relation on the other. Newton's laws encompass Kepler's laws and many other previously unconnected ones (= broadening of understanding), and Newton's laws approximate the solutions of special relativity theory for small velocities, i.e. within a limited range (= deepening of understanding).

On this account, Prandtl's model cannot be described as providing a more profound understanding of flow than its predecessors. The new regularities of the boundary layer have *not* been deduced from the Navier-Stokes equations. And, although considering the boundary layer as a limiting case might have played a role in Prandtl's mind (as suggested in Prandtl 1948, 1606), neither has the model been deduced from the Navier-Stokes equations as an approximate case within a certain limited range. A claim like this would not do justice to the ingenuity with which Prandtl resolved d'Alembert's paradox. Yet, there is some truth in Hempel's suggestion. He was right that a theory provides a better explanation when it increases the coherence of a domain, but he was wrong when he thought that this is only achieved through laws.

There is also an alternative view according to which a phenomenon is understood by invoking or identifying a mechanism that produces it (Machamer, Darden & Craver 2000; Cushing 1998, 338-342), or by showing how it fits into a pattern of causal processes and their interactions. (Salmon 1984) Would it help to reject the covering law account and adopt the mechanism view of scientific understanding? This would certainly bring out a feature of Prandtl's model that is neglected by the covering law account. The key idea is indeed to account for the phenomena in terms of a hitherto unidentified causal process, namely the interaction between the boundary layer and the outer flow. Prandtl decomposed the flow process into separate localised causal components.

Yet this achievement would improve understanding only to a very limited degree if it were not backed up by the simplification of the Navier-Stokes equations, i.e. if it did not give us a hint as to how the parameters concerned are quantitatively related to each other. This "unificatory" aspect of Prandtl's model is not automatically supplied by detailing a causal mechanism. So it is true that the mechanism-view is better than the covering law account in judging Prandtl's model, but it gives only half the story.

There is another alternative available for explicating what it means to understand a phenomenon. This is the semantic conception of theory, which identifies theory itself with a population of models that are homomorphically related to systems in the real world. At first, this might seem attractive for Prandtl's case, since it allows us to see the success of his theory in the descriptive adequacy of its basic model. Improving our understanding of a phenomenon would thus mean giving a model of it that has more features in common with the system in the real world than earlier accounts. A deeper look reveals, however, that descriptive adequacy cannot be enough. It is also relevant *how* this adequacy is achieved, indeed crucially so. If a model were made to fit a phenomenon by an *ad hoc* move, we would reject it even if it were descriptively more adequate than an alternative. So, in the end, the semantic conception does not tell us how Prandtl's model achieves scientific understanding. Consequently, the semantic conception relegates the question of how a theory achieves explanatory success to the cognitive sciences, which would have to decide by empirical research into the behaviour of scientists whether a theory is explanatory or not. As Giere writes, the question ”is not to be judged by philosophical standards.” (Giere 1990, 105) So again, only half the story is told: According to the semantic view, Prandtl's model gives a better description of the phenomena because it provides a more effective way of dealing with the differential equations, but this view overlooks the causal character of the model.

What exactly is it, then, that is responsible for deepening our understanding in the case of Prandtl? The answer to this question must lie somewhere in between the covering law account and the mechanism conception. Prandtl's model introduces a coherence that previously could not be attained *without* appealing to overarching laws, and it provides a causal story into which the phenomena can be fitted *without* forgetting about unification. Prandtl's model has a lot in common with what Thomas Kuhn called an ”exemplar”: it provides a ”concrete problem-solution” that makes it possible to view other cases of application as analogous to the original problem. ”Scientists solve puzzles by modelling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations.” (Kuhn 1970, 189f.)

It is interesting to see how contemporaries received Prandtl's work. It seems that they especially valued the coherence and fruitfulness of the new approach. By ”coherence” I mean the property of making few or no *ad hoc* assumptions. Hence, Blasius, who was mentioned earlier, stressed the greater coherence of Prandtl's theory in comparison with the theory of his predecessor Hermann von Helmholtz. The major advance is seen in the coherent explanation of flow separation: it is true that Helmholtz explained this phenomenon by juxtaposing two potential (i.e. inviscid and rotation-free) flows, namely a moving and a stationary one, and by assuming that pressure decreases to zero at their point of contact, but his theory is not able to take viscosity into account. (Blasius 1908, 2. On Helmholtz's theory, see Darrigol 1998 and Epple 1999, ch. 4.)

In particular, it does not consider the no-slip condition, and therefore cannot explain the onset of vortices after the separation point. In addition, the zero pressure postulated by Helmholtz at the meeting point of the two potential flows, the so-called discontinuity surface, does not exist. In contrast, Prandtl's explanation, Blasius writes, ”does not operate with an *ad hoc* assumption as Helmholtz's theory of rays does, but only with the representations [*Vorstellungen*] which form the basis of our hydrodynamic equations.” (Blasius 1908, 4). In a similar way, Prandtl had stressed in his first contribution that ”the most important result of these [= his] investigations for application” is the explanation of flow separation. (Prandtl 1905, 578) By overcoming the *ad hoc* character of Helmholtz's theory, Prandtl's theory promised greater coherence and analogous solutions to other cases of application that puzzled his contemporaries.

Is Prandtl's search for coherence a search for unification? Is understanding brought about by Prandtl's model due to its explanatory unity? If unity were taken to mean sparsity of the laws used, as is the case with the main advocates of the unification view (Friedman 1974 and Kitcher 1989), then the answer to these questions would have to be ”no.” If only the economical efficiency of the law were taken into account, the Navier-Stokes equations would be more unificatory than Prandtl's model. Yet, if unification is taken to mean a close relationship among the elements used – which one could call structural coherence – then ”unification” would indeed be the right expression to characterise Prandtl's advance over the rational mathematicians and especially over his predecessor Helmholtz. Prandtl does not achieve this unification through one or more laws, but through a physical model that yields the desired understanding with one stroke.

3. Computational and experimental models in fluid dynamics

In computational fluid dynamics, computers are used to integrate field equations numerically in order to simulate or predict fluid flow. Usually, the flow is decomposed (”discretised”) into small discrete cells that form a grid (”the mesh”), to which iterative methods are applied. The discretisation of the Navier-Stokes equations which are continuum equations can be accomplished in many ways: by means of finite difference, finite-volume, finite-element, or spectral methods etc. Each of these strategies has its own philosophy and programming style for obtaining a set of discrete algebraic or normal differential equations. By using an appropriate numerical algorithm, the evolution of the flow-field is computed by solving the discrete equations for well-defined initial and boundary conditions. This is done under certain idealising assumptions in order to keep the parameters manageable. Very often, the solution given by the computer is visualized so that flow characteristics can readily be grasped. The process of computational modelling comprises three phases: a physical modelling phase, a numerical approximation phase and a mapping phase. In a relatively new modelling strategy, the so-called Lattice-Boltzmann models, based on particles and cellular automata principles, the first and second phases are combined.

There are several reasons for the increasingly frequent use of computational models. They give approximate solutions to the Navier-Stokes equations; reduce the time span for parameter variation, design and development of devices; are cheaper than experimental modelling; and can simulate flow conditions that are not accessible to experimental testing. Sources of error for computational models can lie in numerical instabilities whereby small errors at each step accumulate, in the inaccuracy of the model, or in its imperfect verification or validation. ”Verification” is taken to mean the testing of the model in relation to existing analytical solutions or against a known computational solution that can serve as a benchmark, encapsulating the physics of the actual problems to be solved in as many aspects as possible. ”Validation,” on the other hand, is confirmation by experimental means. Since every computational model must incorporate some idealisation in order to be efficient, this is no easy task.

Turbulent flow is a special case that cannot be approached analytically. In order to make up for this, special terms have to be introduced that are often based on purely empirical knowledge. Insight into turbulence has been attained so far only by experimenting, be it with physical or with numerical means. Because of its high expenditure, direct numerical simulation (DNS) (simulation for vanishing cells, without averaging) has for most problems only been achieved up to a relatively modest Reynolds-number. Japanese calculations on the Earth-Simulator in Yokohama seem to have reached over 10.000. A supercomputer with 10^{18} floating-point operations per second would be needed to simulate a complete airplane via DNS. (Gad-el-Hak 1998, 182f.) Presently, 136 teraflops ($136 \cdot 10^{12}$ flops) have been reached. These numbers are rapidly changing as computers become exponentially more powerful.

With the aid of this overview of computational modelling in fluid dynamics, we may address the question of how computational models differ from physical ones as well as the question as to what the term ”model” means when talking of computational models. Remember that we characterised a physical model as a physical representation of a theory whose mathematical content is an approximation to (part of) the basic continuity equations of the theory. The most natural definition that comes to mind would be the following: a computational model of a theory is the mathematical representation of the decomposition of the continuous domain into discrete elements and its implementation in producing approximate numerical solutions.

Although computational modelling has greatly reduced the need for experimental modelling, the latter is still indispensable in fluid dynamics. (Siekmann 1998) We have already pointed out that mastering turbulence necessarily demands the use of empirical elements. But even the most powerful computer program has to be validated empirically at some point or other, directly or indirectly, and the boundary and initial conditions must be determined. Indeed, the demand for greater exactness of experimental methods to match the high resolution power of computational models has increased drastically. This demand has been met by sophisticated new measuring devices, such as non-invasive flow velocity meters with high temporal resolution, which are based on the Doppler effect and which implement laser technology, as well as miniature devices for measuring pressure.

Very recently, experimental observation – or, rather, its interplay with theory and mathematics – has enjoyed unexpected triumph and promises a new approach to solving the hard problem of fluid dynamics, namely turbulence. On the basis of computational studies and ideas drawn from dynamical systems theory, a model for the transition to turbulence in pipe flow has been developed. It is based on the conception of unstable travelling waves. These waves have been observed with surprising clarity and seem to agree with numerical studies. (Hof et al. 2004, Busse 2004) This breakthrough nourishes the hope that a full understanding of the transition problem is possible after all and that theoretical fluid dynamics will be able to influence and control turbulence transitions in the not too distant future.

To round off our discussion of models, we should reflect a bit more on experimental models. Can the use of the term ”model” in this context be adjusted to match the use of the terms ”physical model” and ”computational model” that I have suggested? I think it can. An experimental model is the concrete material representation of the physical model. The representation is to be achieved in such a way that it enables the manipulation and control of the parameters. Think of Prandtl sucking away the boundary layer: the experimental set-up makes use of the insight provided by the physical model, namely to decrease drag and reduce wake.

In her recent treatment of the Prandtl story, Margaret Morrison claims that Prandtl’s water tunnel, in ”itself a kind of physical model,” played the decisive role in Prandtl’s development of boundary layer theory. She suggests a sequence of events according to which Prandtl arrived at his results. There is, first, the ”visualization of fluid flows” in the water tunnel that enabled Prandtl to distinguish ”different areas in the fluid” and to ”represent and explain various aspects of the flow.” (Morrison 1999, 53f.) ”He could physically show that the fluid was in some sense divisible into two separate parts, the boundary layer [...] and the other [one].” (Ibid., 61) ”From there,” Morrison continues, ”he developed the physical/ conceptual model of the fluid incorporating a boundary layer.” (A little later she calls this model also a ”phenomenological” one.) Not until the next and third step does mathematical theory come in; according to Morrison: ”The way the boundary layer was conceived allowed him [Prandtl] to formulate a mathematical model that integrated the Navier-Stokes equations and the equations of motion for ideal fluids.” (Ibid., 53f.)

This particular order of succession is important for Morrison, because it ensures that Prandtl’s model is an ”autonomous” agent: Prandtl’s model, as Morrison states, ”was constructed and functions independently of both [theories, i. e. the Navier-Stokes equations and the classical Euler theory] giving it an autonomous role in the understanding and explanation of viscous flows.” ”It is models rather than abstract theory that represent and explain the behaviour of physical systems.” (Ibid., 39; 60) If the water tunnel had not served ”as the source for the phenomenological model” (ibid., 54), but only as a confirmation *after* the development of the mathematical model, i.e. after the simplification of the Navier-Stokes equations, the model would have to be deemed subordinate to theory and could not claim any autonomy over its domain.

Thus formulated, Morrison's assertion involves both a factual claim about the historical order of events and a claim concerning the 'logic of discovery', so to speak. As to the historical side of the story, it is, as usual, more complex than imagined by philosophers. In a short autobiographical note, Prandtl reports that when working as an engineer in a Nürnberg factory in 1900/01, he had to deal with a problem of pressure loss in an air pipe that was used to suck away dust and filings. (Prandtl 1948, 1605f. Cp. Eckert 2003 for additional information.) When he inserted a conically widened pipe in order to regain pressure, he noticed the separation of the air flow from the wall, which produced an increase in drag and therefore energy. He found a practical solution for the problem but he could not get it out of his mind and wanted to understand it theoretically as well. When in 1901 he became professor at Hannover and subsequently at Göttingen, he set out to study the phenomenon of flow separation in a more systematic way. It was at this time that he constructed his water channel in order to study flow separation more closely.

It is evident from his (preserved) notebooks of the time that he simultaneously tackled the problem from a mathematical point of view. In the recently released, previously unpublished abstract of his 1904 talk (Eckert 2003, 159), Prandtl cites the explanation of "discontinuity surfaces (vortex sheets) on continuously curved surface limits" as the most important result of his presentation. This shows that he judged his contribution to be primarily theoretical and mathematical, namely, as an advance over Helmholtz's heavily mathematical proposal. Additional evidence for this is adduced from the fact that he presented his results at a congress of mathematicians, not of engineers. So, from a historical point of view it is at least doubtful that, as Morrison claims, "the approximations used in the solutions come not from a direct *simplification* of the mathematics of the theory but from the *phenomenology* of the fluid flow as represented by the model." (Morrison 1999, 59)

Morrison's claim also has a systematic side to it, which relates to the logic of discovery. The idea behind it seems to be that the phenomena made visible in the flume somehow suggest *by themselves* the phenomenology, so that the phenomenological model can more or less be *read off directly* from the visualization. Understanding is "produced by observing the behaviour of fluid in the tank," Morrison writes. (Morrison 1999, 61) It is, however, strongly to be doubted that Prandtl was really able to visualize the boundary layer and the area outside of it as clearly separated regions. There is not the slightest hint in this direction in his 1905 paper. To visualize a boundary layer would have been much more technically demanding than employing the water channel. What the channel *could* show was the wake behind the cylinder, but this had been visualized long before Prandtl. (See e.g. Darrigol 2002 for part of the story.) The first known photographs or reports on visualizations of boundary layers by Prandtl and his collaborators seem to be from 1927. (See Prandtl 1927, 764f., figs. 21-26; cp. also his description on p. 762, as well as Prandtl & Tietjens 1925 and Prandtl 1927a. See Homsy et al. 2000 for a film by Prandtl of flow around a cylinder, which was probably the one shown at the 1927 "Wilbur Wright Memorial Lecture" mentioned above.) Thus, boundary layers were truly theoretical entities. Those who do not read German are warned to take Prandtl 1927, the translation of Prandtl 1905, at face value. It says that in figures 1-4 "the separating or boundary layer, which passes off from the edge, is apparent." (Prandtl 1927, 8) The German original, however, merely says "surface of separation [*Trennungsfläche*]," not "layer of separation" or anything of the sort. (Prandtl 1905, 581).

It is much more reasonable to assume that Prandtl used the water channel in the typical manner of an engineer, as a means to *control* the factors at hand: that is, the onset of the wake and the development of drag generally. But this means that Prandtl already had the physical concept of the boundary layer at his disposal. How else could he have developed the idea of sucking away liquid in the region where separation develops? Only the boundary layer as the primary reason for the fluid's viscosity can suggest that it should be possible to delay or even to prevent separation by hindering the accumulation of fluid caused by the reversal of flow into this region! So the concept of the boundary layer was clearly theory-laden!

The importance of visualization did not lie in proving the difference between layer and outer flow, but in demonstrating a reduction of wake through suction! So, Goldstein and his collaborators, as authoritative commentators on boundary layer theory, are right when they say the following about Prandtl's suction experiment: ”This first essay in boundary layer control was undertaken by Prandtl – amongst other experiments – with the sole purpose of providing support for his theoretical arguments.” (Goldstein 1938, II, 529f.)

All this suggests therefore a different order of events than the one put forward by Morrison. In view of the points mentioned, the following chronological succession is much more reasonable:

1. formation of the physical model of a boundary layer, by whatever train of thought; probably also by simplifying the mathematics of Navier-Stokes;
2. articulation of the boundary layer theory through control experiments that enriched and developed the phenomenology and simultaneously through theoretico-mathematical modelling.

Does this imply that Prandtl's model is not as autonomous as Morrison wants it to be? My interpretation of Prandtl's experiment shows that, at least in Prandtl's mode of engineering, *both* mathematical theory *and* experiment act in the service of the physical model, or, to put it in Hertzian terms, of the physical representation. As his one-time assistant Theodore von Kármán later wrote about Prandtl: he was ”endowed with rare vision for the understanding of physical phenomena and unusual ability in putting them into relatively simple mathematical form. His control of mathematical methods and tricks was limited; many of his collaborators and followers surpassed him in solving difficult mathematical problems. But his ability to establish systems of simplified equations which expressed essential physical relations and dropped the non-essentials was unique [...] even when compared with his great predecessors in the field of mechanics.” (Kármán 1954, 37)

I can wholly agree with Morrison when she writes that it ”would have been impossible from [...] theory alone” to develop a mathematical model of the case. But this is because the term ”theory” is ambiguous here. The Navier-Stokes equations already belong to what Morrison and Cartwright call the ”phenomenological” level and not to the ”fundamental” or ”abstract” laws of the theory that are true only ”of the objects in the model” and are ”explanatory” of the phenomena. (Cartwright 1983, 17; Morrison 1999) If anything is explanatory in Prandtl's case, it is the physical conception, but not the fundamental theory. The mathematics used for describing the case result from the juxtaposition of two different *phenomenological* approaches. It is true that the resulting model is autonomous, but not because of an epistemological priority over the mathematical theory; rather, because there is no really fundamental theory which could dispute the model's right for autonomy! The flow field equations are but a frame that can be filled out in many different ways. This is typical for applications in engineering unlike those of physics, although it seems that physics is becoming more and more like engineering in this respect. At least for the case of the boundary layer, the Morrison-Cartwright-Duhem picture, with its distinction between fundamental theory and phenomenological law, is a Procrustean bed.

4. Conclusion

It should be obvious by now that applying a theory in fluid dynamics is not just a matter of deriving a special case from a general theory under particular antecedent conditions. Rather, it means finding a physical model that can lead to an approximate solution of the Navier-Stokes equations, which are themselves approximate. As I have characterised the three model types in fluid dynamics, the physical model is accorded a central position. Both the computational and the experimental model have to rely on it at some point in order to do their work properly, even though their contact with the physical model may be rare or inconspicuous.

In this context, the question arises how the representational function usually accorded to a theory is distributed between the general theory and the physical model. There are two possibilities for interpreting the situation: One could say that the Navier-Stokes equations describe reality, but that we do not have direct access to this description for individual concrete cases, and therefore must be content with an approximation given in a physical model. This view makes the model the handmaiden of the theory and stresses its mediating position between, on the one hand, theory existing prior to the model, and, on the other hand, experience. We would be equally right, though, to say that the differential equations are just part of a formal technique for letting physical models speak quantitatively. In this case, the physical model is seen as an autonomous agent that calls the continuity equations to its service and uses them in a purely instrumentalist way. On this view, there is no more to theory than what all models have in common. In short, the dichotomy is a bit like that between Plato's *ante rem* realism regarding universals and Aristotle's *in re* realism as it arises in the philosophy of mathematics.

Fortunately, we do not have to solve a problem from the philosophy of mathematics here. For engineering science, we should decide this question in the following way. We should look in which direction realism tends, i.e., we should find out how the inferences to the best explanation are drawn in the practice of engineers and scientists: Do their conclusions deal with the existence of the theoretical entities postulated by the theory or with the existence of those postulated by the model? I think that, at least in Prandtl's case, the scale definitely tips to the Aristotelian side, i.e. the side of the models. Prandtl argues first for the existence of the boundary layer and the existence of a perturbation process responsible for the separation of the flow. Only after confidence in this result has been established is it meaningful to ask how it can be formulated in and be adjusted to the frame of the continuity equations by systematically neglecting certain parameters in them. So Prandtl's model is not a fictional or idealised tool that enables the theory to be applied. It is the other way around: The model tells us something about the world, whereas the theory – in the approximated form of the Navier-Stokes equations – is so abstract and malleable that it does not deliver any definite information about the concrete case at all, i.e. it does not even lie about it.

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