MODELING AND EXPERIMENTING[1]

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Abstract:

Experimental activity is traditionally identified with testing the empirical implications or numerical simulations of models against data. In critical reaction to the 'tribunal view' on experiments, this essay will show the constructive contribution of experimental activity to the processes of modeling and simulating. Based on the analysis of a case in fluid mechanics, it will focus specifically on two aspects. The first is the controversial specification of the relevant parameters of a phenomenon. The second is conceptual innovation, with a redefinition of concepts central to the understanding of the phenomenon.

On the tribunal view on experimentation, experiments are the place where empirical implications or numerical simulations of theoretical models are confronted with outcomes of measurements, the data. But that supposes that it is clear what the correct theoretical implications, the correct simulation conditions, and the correct data are. By contrast and in reaction to the 'tribunal view' on experiments, this essay will show that experimental activity, first of all, plays an essential role in a task of clarification. It will show the elaboration of an experimental system as an interactive, creative, open-ended process, and highlight its constructive contribution to the processes of modeling and simulating. This constructive role of experimental activity will be explored through the analysis of a particular episode in 20th century fluid mechanics, presented in section 2, focusing specifically on two aspects. The first, discussed in sections 3 to 6, will be introduced with a controversy regarding the characteristics of the phenomenon under investigation (section 3). The function of experimental activity will be to identify the conditions in which outcomes of measurement qualify as data that a putative model of the phenomenon would have to fit (sections 4 and 5) or a simulation would have to reproduce (section 6). The second, discussed in section 7, is conceptual clarification and innovation. In that episode, a long controversy over the identification of some structural characteristics of the target phenomenon would finally lead to questioning implicit presuppositions about the conditions of correct measurement. The answer to these questions through experimentation would take the form of a redefinition of concepts central to the understanding of the phenomenon and the conditions of its occurrence.

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1. Wake, frequency and controversy

1.1 The problem

Science provides us or aims to provide us with models of phenomena. For instance: a model of the dynamics of the wake that can be seen in figure 1. This figure shows the wake of a flow behind a cylinder when the velocity of the upstream flow reaches a certain critical value. The cylinder is on the left, seen from above- the flow, going from left to right, is visualized in the plane perpendicular to the axis of the cylinder, and the wake is formed by vortices emitted alternatively on each side of the cylinder and carried away with the downstream flow.

Figure 1: Wake behind a cylinder, seen from above.

The formation of such a wake is a very common phenomenon, happening with air or liquid going over a bluff (non-streamlined) body which can be a pole, a rock, an island. The range of application of a better theoretical understanding of wakes spreads from meteorology to the stability of bridges or platforms, from the design of cars and airplane wings to that of helicopter vanes and more generally, includes a variety of cases of development of periodic instabilities and transition towards chaotic behavior. Due to the variety of fluid structures and flow regimes that can be observed in this flow, the flow past a cylinder has served for nearly a century as a model for fundamental studies of external flows and has been characterized as "a kaleidoscope of challenging fluid phenomena" (Morkovin 1964; Karniadakis and Triantafyllou 1989)

As common as it is and as simple as it looks, this wake, and the attempt to construct a theoretical model of it, triggered an enormous number of studies, and no less controversy. I will focus on *one*, seemingly simple, question that was the object of a debate involving experimental as well as numerical and theoretical studies in fluid mechanics in the second half of the 20th century. This question concerns the evolution of the shedding frequency of the vortices with the Reynolds number (Re), the dimensionless number representing the velocity of the flow upstream before it reaches the obstacle[2], as Re is increased within a certain interval beyond the critical value: *is the variation of the shedding frequency with Re a continuous linear variation or is there some discontinuity?*

Again, one should not be misled by the simplicity of the question. The answer to this question is crucial not only to understanding the development of the wake, but also to the general understanding of the development of fluid instabilities with all the applications one can imagine in meteorology or aeronautics. In particular, as we will see, the evolution of the frequency will be suspected of being related to the development of three-dimensional patterns of shedding, which are known to have a direct influence on the tendency for a structure to vibrate in a flow (Miller & Williamson 1994).

1.2 Modeling, experimenting and relevant parameters

Looking into the details of how this question was answered, we will see a challenge to a view that

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[2] The definition of the Reynolds number is Re= Ud/ν where U is the velocity upstream of the flow, d is the diameter of the cylinder and ν the viscosity of the fluid. For a given fluid and cylinder, increase in Re expresses increase in the velocity upstream.

reduces the role of experimental activity in the practice of modeling to the assessment of theoretical models. It has been aptly noticed that experimental activity may also be exploratory (e.g. Steinle 2002). The following case study is an illustration of the way in which experimental activity is exploratory, but there are two temptations easily prompted by the notion of exploration that must be resisted. The first is to take exploration as implying a lack of specific direction or constraint. Exploratory experimental activity may be motivated and guided by a specific, clearly formulated question to be answered about a specific phenomenon. What makes it exploratory is that experimental activity can be seen retrospectively as a process of systematic investigation of the effects of different factors so as to identify the relations between different physical quantities that characterize the phenomenon under study. The second temptation to resist is to identify exploratory experimentation with a *type* of experimentation. Steinle explicitly avoids the first one but clearly succumbs to the second by contrasting exploratory experimentation and theory testing experimentation as two types of experimentation characterized by different types of question to be answered and different types of experimental set-up used. The following case study illustrates the difficulty to sustain a contrast in type. A series of experimental procedures were conducted to clarify the origin of an unexpected effect obtained in the measurement of some physical quantities characterizing the wake. Identifying the origin of these effects was needed to decide whether the effects were really characteristic of the phenomenon, the wake, or merely artifacts. These procedures could not yet be testing a theory or a model of the wake, since it was not yet clear what would count as touchstone for a putative theoretical model.

But that this process was not about testing a theory or a model of the wake does not mean that it was not about testing at all. Steinle characterizes theory-testing experimentation by its goal "to test a clearly formulated expected effect". Such a test may well be part of a larger exploratory procedure, and an experimental investigation which as whole can be regarded as a procedure of systematic variation needs to be composed of a series of tests of clearly formulated effects. What the experimenter sees as the goal of his/her experiment does not need to be what retrospectively, in view of what followed, will be seen as what was really at issue. In the following case study the individuals that participate in the process had no idea they were contributing to a process of conceptual transformation of the understanding of the wake. Moreover, an exploratory process may be realized through several relevantly different experimental set-ups, as happens in the case that follows. Each experiment looks like one that Steinle associates with theory testing, but put together they form a system that has the flexibility that he associates with exploratory experimentation. Rather than a difference in type between exploratory and testing, we should speak of a difference in function. The two functions are not epistemologically on a par: any empirical test necessarily has an exploratory procedure as its conditions of possibility to specify the empirical conditions in which informative, relevant measurements can be obtained.

One well-known reason why the outcomes of measurement may not be informative about the phenomenon under study is related to the proper use of the measuring instruments. The indication given by a hot-wire anemometer used to measure the velocity of a fluid in conditions where the temperature of the fluid changes will not be a relevant information. It would simply not be a measurement of velocity because the indication on the hot-wire anemometer would change with changes in temperature of the fluid independently of changes in velocity. This is why, as Margaret Morrison (2009) has recently emphasized, we need a good model of the apparatus used as measuring instrument. The model alerts the experimenter

to possible sources of instrumental artifacts. But instrumental artifact is not the sort of problem that I will be concerned with. It will be admitted, in this case, that when the experimenters are reporting some measurement of frequency, the instrument functions properly and in the proper conditions of use: the instrument measures the quantity it is meant to measure, the shedding frequency. The question is whether the values of the frequency are characterizing the phenomenon or not. It is not a problem with the model of the instrument but rather with identifying what part of the experimental set-up has an effect on the values of the shedding frequency and whether this effect should or should not count as an interference with the phenomenon.

To decide on whether some result is the product of an interference or characterizes the phenomenon under study is, in fact, to draw a line around the phenomenon that defines what it is and what experimental conditions need to be satisfied for the measurement of a given quantity characterizing the phenomenon.

1.3 Separating the problem of relevance

In the case under study, conflicting measurements will generate a controversy about whether the evolution of the shedding frequency with the upstream velocity of the wake is linear or instead displays a discontinuity. As long as this question is not answered, an assessment of a putative model of the wake, via either its analytical implication or a simulation, does not seem possible since it is not yet clear what the data are that such a model would have to account for. What is missing is the touchstone for such assessment.

What makes the case of the wake particularly interesting, however, is that it has been the target of two different types of modeling procedure. One, presented in section 4, is based on the theory of dynamical systems and builds on the periodic motion generated by the alternative emission of vortices on each side of the cylinder. It is this type of modeling that is immediately hampered by the lack of consensus on the identification of the empirical characteristics of the wake. But the wake is also a fluid mechanical phenomenon and as such falls in the domain of application of the Navier-Stokes fundamental equations. So the simulation of these equations, it was thought, should tell us what the wake is 'really' like and in particular whether or not there is a discontinuity in the evolution of the frequency with the velocity upstream. This claim could not be upheld. That is not due to a problem with the Navier Stokes equations nor with the discrete model that is actually simulated, the simulation model.

The problem is with the normative (even if implicitly) assumption built in the simulation regarding the conditions in which the phenomenon normally or ideally occurs. The question is about where to draw the line, within the factors that have an effect on the phenomenon, separating those that generate artifacts or interferences and those that are characterizing the phenomenon. Morrison (2009) argues for the classification of numerical simulation as experiments on the basis of the role of models in (traditional) experimental measurements. But experimenting with a physical system is not just making measurements; it is also, and maybe primarily, identifying the influential factors and determining the conditions in which an instrumental procedure counts as a measurement of a given characteristic of the phenomenon.

Philosophical studies on experiments have mainly focused on questions of instrumental reliability (Bogen and Woodward 1988), reproducibility, or replicability (Franklin 1986, Radder 2003) and taken for

granted the identification of which factors of the experimental set-up 'should' and which 'should not' have an effect on the functioning of the instrument. Then the experimental problem is reduced to identifying which do have an effect and to shield those among them which should not. Epistemologically more fundamental is the problem of identifying whether a certain effect should count as an artifact or as characteristic of the phenomenon at all, and on what basis.

The analysis of this function of experimental activity will call for the introduction of the concept of *relevant parameters*. I will use the term 'parameter' (rather than 'factor') to refer to a quantifiable characteristic of the experimental conditions that makes a difference in the outcomes of measurements. A relevant parameter is such that this difference is regarded as relevant to the understanding of the phenomenon, that is, as having to be taken into account rather than eliminated. What will count as relevant data will depend on what is recognized as relevant parameters of the system. But relevant parameters do not wear their identity on their sleeves: neither that they are parameters at all, nor that they are *relevant* parameters.

The philosophical problem at the centre of this article is 'the problem of identification of the relevant parameters'. I take it to be in large part a normative problem. In practice it takes the form of a problem of interpretation of outcomes of measurement, as artifact or as characteristic of the phenomenon under study. So we will be concerned with data, data-models, and the way in which the problems of interpretation they raise are dealt with in relation to measurement. But it is not a concern with how a data-model is made out of raw data, that is, how raw data are selected, treated, analyzed etc. Whatever one may think about what 'raw' could mean, to stay with this sort of question would still be taking for granted the identification of the experimental conditions of measurement under which the data that are representative of the phenomenon can be obtained. And it is precisely the identification of these conditions that we are here concerned with, together with the identification of the relevant parameters that it implies.

2. Origin of the controversy: the discontinuity

2.1 Experimental studies

The starting point of the difficulties is generally identified with the publication in 1954 of Anatol Roshko's dissertation "On the Development of Turbulent Wakes from Vortex Streets", a very detailed experimental study of the wake, still a reference in fluid mechanics, which:

1) shows on the basis of measurements that for values of Re between 40 and 150, called the stable range, "regular vortex sheets are formed and no turbulent motion is developed", whereas between 150 and 300 turbulent velocity fluctuations accompany the periodic formation of vortices.

2) gives, for the stable range, an empirical formula of the linear variation of the shedding frequency with the Re.

These results had been anticipated and were shortly confirmed by other experimental studies. What makes it however, retrospectively, the starting point of the difficulties is that in 1959, a new experimental study of the wake is published that calls into question the adequacy of the distinction between only two ranges of shedding, the stable and the turbulent, and directly contradicts Roshko's results regarding the evolution of the shedding frequency with the Reynolds number.

Regarding the latter point, the author of the publication (Tritton 1959) argues for the existence of a discontinuity in the velocity-frequency curve by showing the "frequency plotted against the velocity for three different runs". He recognizes that "the discontinuity does not come in just the same position relative to the points each time" and he recognizes that "if all the runs were shown on a single Reynolds number vs. Strouhal number plot [frequency] the discontinuity would probably not be apparent" but takes that to be the reason why "it has not been noticed by other workers" (566).

Regarding the former point, the distinction between two ranges of shedding, one stable the other turbulent, Tritton argues, on the basis of visualizations of the wake that had not been made by Roshko, that the dynamics of the wake is not what Roshko's identification of the main ranges of shedding suggested. Beyond the discontinuity, that is, for values of Re greater than the one for which the discontinuity occurs, the shedding of the vortices *along* the cylinder is not simultaneous, the invariance by translation is broken: the imaginary lines joining side by side vortices along the cylinder, are not parallel to the axis of the cylinder; they are oblique. And that cast serious doubt on Roshko's previous results.

2.2 The conflict

Roshko's simple distinction between two ranges of shedding suggested that the dynamics of the wake in the stable range would be two-dimensional, contained in the plane perpendicular to the cylinder. "In the stable range", he wrote, "the vortex street has a periodic spanwise structure" (1954:1), suggesting that the vortices emitted on the same side of the cylinder are emitted simultaneously and that the line of vortices, if observed, would be parallel to the axis of the cylinder. That the successive lines of vortices are or are not parallel to the axis of the cylinder translates in terms of the dimension of the dynamics of the wake: parallel lines of vortices corresponds to a 2-dimensional (2D) dynamics of the wake, whereas non-parallel lines of vortices testifies to the existence of a dynamics in the direction of the cylinder, which would make the total dynamics of the wake 3-dimensional (3D). But 3D effects on the dynamics were thought to be associated with the development of turbulence, which according to Roshko took place beyond the stable range.

This conflict between Roshko's and Tritton's experimental results will start a controversy of 30 years about "whether the discontinuity [and the oblique shedding] is an intrinsic, fluid-mechanic phenomenon, irrespective of the experimental set-up" (Williamson 1989: 580). And it is the way in which this controversy developed and was resolved that I am about to analyze. I just quoted one of the participants in the debate. More than just a participant, he will offer an unexpected solution to resolve the controversy that will transform the understanding of the problem.

3. Conflicting interpretations of the discontinuity

Numerous publications followed those by Tritton and Roshko arguing for one side or the other. Rather than giving an exhaustive account of the literature, I will focus on studies that explored original explanations of the observed discontinuity, presenting evidence for these explanations, and often motivating, in their turn, new sets of experimentation and arguments. What is philosophically remarkable here is that the panel of explanations and supportive evidence, that were advanced, pretty well exhausted the different forms of justification of the reliability or significance of experimental observations, like reproducibility, replicability, controlled and quantifiable modification of the observed effects, without

however being able to put an end to the polemic.

The reason, as I will show, is that some basic assumptions regarding the conditions of measurement were implicitly made. Calling these assumptions into doubt will later on lead to the refinement and redefinition of concepts central to the understanding of the phenomenon and the conditions of its occurrence.

3.1 Claim: The discontinuity is an artifact

In 1969, Tritton's claims for a nonlinear evolution of the shedding frequency were objected to on the basis of new experimental results, which supported an explanation of the discontinuity and the 3D effects in terms of effects of non-uniformities. The main sources of non-uniformities were identified as being in the upstream flow and irregularities in the diameter of the cylinders. In effect, some experiments conducted by Gaster (1969) showed that increasing artificially the non-uniformities in the flow, or irregularities in the cylinder diameter, had the effect of increasing the discontinuities and in fact generated additional discontinuities.

What made Gaster confident that non-uniformities were responsible for the discontinuity is the observation that reducing the span of the cylinder, and reducing thereby the total amount of irregularity along the cylinder, had the effect of suppressing the 3D dynamics along the cylinder, allowing a parallel shedding, with lines of vortices parallel to the axis of the cylinder. These results strongly suggested that the discontinuities and the 3D effects presented by Tritton as features of the wake should rather be regarded as an artifact of the experimental set-up.

Tritton had supported his argument that the discontinuity could not be an artifact by the reproducibility of the discontinuity with different set-ups. But if the factor responsible for the discontinuity is so basic that it is present in all the set-ups used by Tritton, then this reproducibility is exactly what one would expect. And the irregularity of the diameter of the cylinder is such a factor. So by showing the influence on the measurements and observations of such a basic feature of the experimental set-up, Gaster could counter Tritton's argument from reproducibility. Or so it could seem.

In fact, Gaster's results related to the effect of the length of the cylinder would prove significant, but for reasons quite different from what he thought. It is true that he showed the fragility of the argument from reproducibility. But the causal argument for the role of irregularities, based on the observed consequences of a variation in length on the discontinuity and 3D dynamics will prove to be flawed as well. On the one hand, what happens when the span is reduced, no more discontinuity and 2D dynamics, will be explained independently of the decrease in the amount of irregularities; and on the other hand, the increase of the span, followed with additional discontinuities and 3D dynamics, will be shown not to be necessary for the existence of a discontinuity and 3D dynamics. But I am running ahead.

3.2 Claim: The discontinuity is not an artifact

In 1971, new studies were made by Tritton where again he argued in favor of the 'reality' of the discontinuity. He then proposed to understand the discontinuity as a transition between two distinct instabilities requiring two different explanations of the development of the wake but without offering more to support their existence than the need to account for the discontinuity and without saying more about these explanations than what could be observed in the experiments.

This did not prevent new experimental arguments for the opposite interpretation of the

discontinuity, this time as resulting from micro-vibrations of the cylinder itself as the velocity of the flow increases (van Atta & Gharib, 1987). Tritton's explanation probably seems too ad-hoc to be right. But, on the other hand, that new discontinuities arise with an increase of non-uniformities does not show that without non-uniformities there would be no discontinuity. The observations made when the length of the cylinder is reduced seemed to partly fill the gap: not only are there more discontinuities when there are more non-uniformities, but there are less discontinuities when there are less non-uniformities. But even then, as I mentioned, Gaster was too hasty in identifying reduction of the length with reduction of the non-uniformities due to irregularities of the diameter and another interpretation will be given of the relation between length and mode of the shedding vortices.

And finally, importantly, no attempt to attribute the origin of the discontinuity to some aspects of the experimental set-up yielded a convincing account of the specific value or range of values of Re at which the discontinuity appears. That was one of Tritton's strongest points for requiring an explanation in terms of two instabilities involving a point of transition.

In this section, I have presented several experiments that were made to clarify the effects of different characteristics of the experimental set-up on the phenomenon under study, the evolution of the frequency of the wake with the Reynolds number of the flow. No experimental results were yet compelling enough to close the controversy about the form of this evolution. But several important points can already be made at this point.

First, each of these experiments was specifically directed at testing one particular factor and specifically designed for this task. But each of them is also, retrospectively, part of a process which, as a whole, constitutes a systematic investigation of the effect of different parameters akin to what Steinle (2002) identifies as characteristic of exploratory experimentation. So testing and exploring should not be seen as two types of experimentation, but as a distinction in function. And what the function of an experiment turns out to be depends in part on what happens after it.

Secondly, there was apparently no theoretical modeling of the phenomenon involved in this experimental activity. Still, as will become clear in the next section, we are already engaged with an activity of modeling. The investigation of the effects of different factors is embedded in a dispute about whether the discontinuity observed in the evolution of the frequency was or was not merely an artifact: what is at issue is what a model would have to account for, which form of evolution of the frequency, and what it would have to take into account, which experimental factors, in order to qualify as a model of the phenomenon.

4. Enter the Landau model

4.1 Model implications vs. experimental measurements

It is not until 1984 that a model of the wake was proposed to account for its temporal dynamics, i.e. the temporal evolution of the amplitude of the vortices and of the frequency at which they are emitted (Mathis et al. 1984). The model in question is obtained by applying to the wake situation the general model proposed in 1944 by Landau to describe the development of a periodic instability --which he viewed as the first step towards turbulence. It was then introduced as 'the Landau model of the wake':

$$\mathbf{dU/dt} = (\sigma_r + i\sigma_i)\mathbf{U} - (l_r + il_i)\,|\mathbf{U}|^2\,\mathbf{U} \quad \text{(equation 1)}$$

where **U** is the complex amplitude of the wake, and $(\sigma_r + i\sigma_i)$ and $(l_r + il_i)$ are resp. the linear and non-linear coefficients.

From this model can be derived the evolution of the real component of the amplitude of the instability with the control parameter, here the Re, and its critical value Re_c

$$U^2_{max} \ (=\sigma_r / l_r) \ \square \ (Re - Re_c) \qquad \text{(equation 2)}$$

The measurements of the amplitude that were made showed that, in this respect, the model works beautifully- even better than expected. So for the evolution of the amplitude, at least, one and the same model can account for the development of the instability on the whole range of the Reynolds number. This result contradicts Tritton's claim that two different instabilities are at play on two ranges of Reynolds number. From the same model can also be derived the evolution of the frequency with the Reynolds number and it shows a linear variation, with no discontinuity.

When measurements are made on the frequency, however, once more, what they show is the existence of a discontinuity. And additional measurements made along the cylinder indicate the existence of a 3D dynamics, an oblique shedding. The results are quite clear. What is not clear is what to do with them.

4.2 Interpreting the result of the comparison

Does the discrepancy between the model's prediction of the evolution of the frequency and the outcomes of measurement show or even indicate that the Landau model is not an adequate model for the wake? It will depend on whether that discontinuity has to be accounted for by a model of the wake. If the discontinuity is an artifact, the model of the wake not only does not have to account for it, but should not account for it. On the other hand, if it is an intrinsic feature of the wake, a model that does not account for it cannot, in that context, count as a model of the wake.

Ronald Giere writes that "If what is going on in the real world is similar in structure to the model of the world then the data and the prediction from the model should agree". (Giere 2005, 30) But what are the data in question? The problem is not one of data analysis or of construction of what is usually referred to, after Suppes (1962), as 'models of the data'. Even if some procedures of analysis are assumed to be in place, and a data model is produced, like the data-model of the evolution of the shedding frequency with Re, we are still left with the open question of whether this data-model is one that the model of the wake *should* agree with.

5. Intrinsic characteristics and relevant parameters

The predictions of a theoretical model of the wake will have to agree with the data only if these data are informative about the features of the wake that the theoretical model aims to provide information about, instead of being non-informative or maybe even misleading[3], as an artifact is. Just as the theoretical aim is not just any theoretical model but a theoretical model *of* the dynamics of the wake, so it is not just any data-model but a data-model *of* the dynamics of the wake that experimental measurements aim at.

As quoted above, Williamson described the controversy as a search to determine '*whether the*

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[3] Thanks to Paul Teller (private correspondence) for pointing that out.

discontinuity is an intrinsic, fluid-mechanic phenomenon, irrespective of the experimental set-up'. The idea of being irrespective of the experimental set-up seems to offer an empirical criterion to distinguish genuine data, ones that are informative about the target phenomenon, from non-informative data, including artifacts. If the discontinuity is intrinsic it should not depend on the experimental set-up, so if it is shown to depend on the experimental set-up, then it is not intrinsic. This is obviously what motivated the experimental studies of the effect of an increase of the non-uniformities in the flow or in the diameter, as well as of the effect of making a cylinder vibrate. In each case, the idea is to show that the discontinuity is generated by some specific features of the experimental set-up and consequently is not a feature of the wake itself.

It is not sufficient, however, to show that the discontinuity is only the effect of non-uniformities or vibrations. For that, it should be shown that without non-uniformities or vibrations, there is no discontinuity. That is, indeed, as we will see, the challenge that some numerical studies were going to try to address.

But the notion 'irrespective of the experimental set-up' raises a fundamental question that will prove crucial to the interpretation of the simulations that were carried out. "Irrespective of the experimental set-up" cannot be taken to mean "completely independent" of the characteristics of the experimental set-up. After all, the shedding itself does depend on the value of the velocity of the upstream flow, and other characteristics of the experimental set-up, like the diameter of the cylinder or the viscosity of the fluid, do influence the dynamics of the wake. The influence of these measurable characteristics will be part of our understanding of the wake; they will be taken into account through the Reynolds number, a control parameter of the system. The effect of a change in value of Re counts as an intrinsic feature of the wake and has to be accounted for by a model of the wake. The Reynolds number is therefore a *relevant parameter* of the system.

The experimental investigation of the wake takes place against a theoretical background where lines have already been drawn between what is to count as relevant parameters and what as source of mere interference and artifact. All the factors whose effect was carefully investigated in experiments presented in section 3, like non-uniformities or vibrations of the cylinder, were regarded as possible sources of artifact. If they turned out to have an effect, it would not have to be accounted for by a model of the wake, and they themselves would not have to be taken into account by the model. Whether they had an effect was what had to be established, not their status as non-relevant parameters.

The next section will bring to light the crucial, even though implicit, normative function of the contour of these lines in relation with simulation. And the last section will show that this contour is not written in stone. Their alteration, even if it is empirically justified, needs to be seen, first of all, as a normative transformation of the investigation, with a new conception of what is relevant and what the phenomenon under study is.

6. Simulation of the wake

To demonstrate that there are more discontinuities when the non-uniformities and vibrations are increased is not enough, as we saw, to show that the discontinuity is merely an effect of non-uniformities or vibrations of the cylinder, and therefore is an artifact. What should be shown is that when there is *no*

non-uniformity or *no* vibration, there is no discontinuity. But this is not an easy thing to show. A flowing fluid as well as the diameter of a cylinder keeps a certain level of non-uniformity however carefully they are prepared. This is where one would like to make a thought experiment starting with 'imagine a flow with no non-uniformities…'. Fortunately, the situation of the wake, by the end of the eighties, lent itself to the modern alternative: the numerical simulation, which one should think will be less subject to ambiguous interpretation. But that is not so. The ambiguity stems from how the line is drawn that separates the characteristics of the experimental set up qualifying as relevant parameters from the others.

6.1 What the simulation shows

A simulation of the equations of Navier Stokes (NS), fundamental equations in fluid mechanics, was performed to find out how the flow behind a cylinder develops when there are no non-uniformities of any sort and no vibration. The results of the simulation were presented as pointing to a definite answer to the question of the nature of the discontinuity (Karniadakis & Triantafyllou, 1989). And the answer was that the evolution of the frequency with Re is linear, with no discontinuity.

These results certainly show that the occurrence of the discontinuity in the experiments results from the influence of some factors that are not taken into account as parameters of the system in NS. And the parameters of NS are those the effect of which is constitutive of fluid-mechanical phenomena. So, if one trusts the method used for the simulation (a spectral-element method, used successfully in previous studies) and does not envisage calling into question the validity of the fundamental equations, it is most likely that the effect of these factors constitutes an artifact and should be, consequently, neutralized[4].

6.2. Interpreting the results of the simulation

This conclusion about the discontinuity only holds, however, under certain assumptions. Imagine that the simulation is not an imitation of the phenomenon we are interested in; then the results are not relevant to the understanding of this phenomenon. This is where the problem of identification of the relevant parameters sneaks in. When one speaks of simulating the fundamental equations, this is not exactly right in at least in two main respects. First of all, the simulation requires the construction of a system of discrete equations and a method of discretization for time and space to obtain the discrete model that is actually run by the computer, the simulation model (Lenhard, 2007). Lenhard has shown how the construction of the simulation model may become a modeling process 'in its own right', when the agreement with the observations of the phenomenon that the simulation intends to imitate is the norm that guides and regulates the construction. The main normative requirement being the successful imitation, the simulation model may be as far from the theoretical model as what is called a phenomenological model may be.

But there is something else that may be overlooked when one speaks of simulating the fundamental equations. It is something that is independent of the way in which the simulation model is

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[4] What counts as artifact is relative to the definition of the target of the experimental investigation. If vibrations of the cylinder were shown to have an effect on the outcome of measurement of the frequency, it would be regarded as an artifact if the target of the measurement is the 'fluid mechanics phenomenon' of development of the wake. But the effect of the vibrations of the cylinder on the development of the wake is also a common subject of study where the phenomenon is that of aeroelastic coupling between the vortex street and the cylinder vibrations, and the effect of the vibrations is what is measured.

obtained. What will turn out to be critical here is that the fundamental equations are abstract (Giere 2010; Cartwright 1999) and going from the fundamental equations to the simulation of a target phenomenon must involve some specifications that determine what particular situation is the target of the simulation. At closer inspection then, what raises doubt as to the significance of the result of the simulation is the geometry of the simulated situation: it is a 2D geometry representing a plane containing a cross section of the cylinder.

Remember that in this case there is no observation that can play the role of guide or regulator. The simulation is meant to tell what the observation should be, what the phenomenon is really like, whether the discontinuity is part of it or not. But how could this simulation of the development of a flow in a 2D plane tell what it is like when a flow goes around a cylinder? In the latter case, the flow develops in a space that contains not only the plane perpendicular to the axis of the cylinder but also the plane that contains the axis of the cylinder.

There is an answer to this question: suppose that, with respect to the phenomenon under study, the frequency of shedding of the vortices forming the wake, all points on the cylinder are equivalent to one another, that the same thing happens everywhere. Then, no need to simulate the wake in each point of the cylinder; any cross section will suffice. What the 2D simulation shows then is how the wake develops, according to NS equations, in conditions where all the points on the cylinder are interchangeable. But why should we think that all the points are interchangeable? The presence of the ends obviously creates an asymmetry contradicting the assumptions of the simulation!

To this question too there is an answer: suppose that a cylinder that is long enough can be regarded as infinite, and that an infinite cylinder can be regarded as cylinder that has no end. If there is no end, then we are in the situation where all points are interchangeable. All that is needed to satisfy this assumption of an infinite cylinder is that, for a long enough cylinder, what happens in the middle part of the cylinder be independent from what happens at or near the ends. And it can then be admitted that the 2D simulation will, at least, show what should happen in a 'long enough' cylinder, far enough from the ends.

Taking the simulation as relevant is, consequently, taking the ends of the cylinder as not being relevant to the understanding of the fluid mechanical features of the wake, amplitude or frequency. The ends of the cylinder are treated in the same way as non-uniformities of the flow or vibrations of the cylinder: if they have an effect on the outcomes of measurement this effect will be classified as artifact and should be neutralized. *The ends are taken not to be a relevant parameter of the system.*

7. Experimental contribution to conceptual understanding

The applicability of the results of the simulation rests on the assumption that the effects that the ends of a cylinder of finite length may have on the dynamics of the wake are not intrinsic characteristics of this dynamics. This assumption will be, temporarily, supported by some measurements that have shown that for a cylinder that is long enough, the frequency of shedding in the middle of the cylinder is different from that found near the ends of the cylinder. But that should not mislead us into thinking that the assumption was an empirical assumption.

This assumption is clearly normative in that the normal conditions of development of the wake, where it has its 'pure' form, are taken to be those where the cylinder is sufficiently like one with no end. Conditions under which the ends would have an effect on the measurements would not count as right conditions of measurement.

In addition, the distinction between cylinder with ends and cylinder like one with no end is taken to hinge on the length of the cylinder. The way to neutralize the effect of the ends, it is assumed, is to have a sufficient length.

These two assumptions will be called into question in a thoroughgoing experimental study of the evolution of the shedding frequency of the wake (Williamson 1989). This study was to represent a turning point on the issue of the discontinuity and the development of 3D effects.

7.1 Checking the influence of the non-uniformities

To begin with, a series of measurements was carried out to test the attribution of the discontinuity and 3D effects to the existence of non-uniformities or vibrations. On the basis, on the one hand, of measurements comparing the spectra of the wake and of the cylinder vibrations and on the other hand, of measurements made, first, with rates of turbulence and flow uniformity lower than those usually obtained, and then with 'the cylinder towed through the quiescent fluid' (instead of being fixed in a moving flow), both attributions were rejected.

7.2 Checking the influence of the ends

The next and main part of the investigation focuses on the study of the influence of the ends of the cylinder.

Measurements of the shedding frequency with a probe moving along the span of the cylinder showed the existence of different regions characterized by different shedding frequency. In particular, a region of lower frequency was found near the ends.

More precisely, for a cylinder with an aspect ratio (the ratio L/D of the length to the diameter) beyond a specific value, the frequency near the ends is different from the frequency in the central region. "This suggests", Williamson writes, "that the vortex shedding in the central regions of the span is unaffected by the *direct* influence from the end conditions" (590, italics added). Note however, it will be of crucial importance, that Williamson only recognizes the absence of a *direct* influence.

For smaller values of the aspect ratio, however, the frequency of shedding is found everywhere the same as that found near the ends, as the size of these regions is now sufficient to cover the whole span. Remember that Gaster interpreted the reduction of the span of the cylinder in terms of reduction of the non-uniformities of the diameter and took that to be the explanation for the shedding being parallel in this condition. Williamson will propose the existence of a single frequency over the whole span as alternative explanation. But before getting there, another set of measurements and observations is still needed, leading to a new conceptual understanding of the notion of ends, independent of the notion of length, and to the inclusion of the ends among the relevant parameters of the system.

7.3 Redefining the notion of ends

Why did Williamson underline the absence only of a *direct* influence of the ends on the wake in the central region? In the case where there is a difference in frequency between the ends and the central

part of the cylinder, visualizations of the temporal development of the wake along the cylinder were made. They showed that, initially, the lines of vortices traveling downstream are parallel to the cylinder, and that progressively the parallel pattern is transformed into a stable oblique pattern which propagates from the ends of the cylinder towards the central region. These observations suggested that the ends did have some influence on the way in which the wake develops in the central region. But that does not mean that this influence, indirect in that it is not on the value of the frequency itself, *should* be part of our understanding of the wake. The decisive measurements and observations in favor of a new conception of the ends, their role, and consequently the wake itself are still to come.

Early in the experimentation, it was noticed that the repeatability of the measurements was greatly improved by the use of endplates, little disks fixed at the ends of the cylinder perpendicular to the axis of the cylinder, to shield the span of the cylinder from the boundary layer along the test-section walls. So far, all the observations and measurements had been made with endplates *perpendicular* to the axis of the cylinder, in the same way as in previous experimental studies. Some further measurements, however, were made for different values of the angle between the axis of the cylinder and the plates. And it was then realized that, for a certain angle, the shedding becomes parallel, that is, the dynamics becomes 2D, and the discontinuity disappears even though *the length did not change*.

After further measurements, the explanation that was eventually given for the effect of change of angle was the following. Changing the angle of the plates has the effect of changing the *pressure* conditions responsible for the existence of a region of lower frequency towards the ends. When there is such a region of lower frequency, a phase difference propagates from the ends towards the central region and this propagation creates the pattern of oblique shedding. For a certain interval of angles of the endplates, when the pressure and the vortex frequency match those values over the rest of the span, there is no region of lower frequency, no propagation of phase difference, and the shedding is parallel. And the discontinuity only appears in the oblique mode of shedding and is found to correspond to the transition of one oblique pattern to another with a slightly different geometry.

7.4 New relevant parameter and reconception of the wake

Williamson takes his results to "show that [the oblique and parallel patterns] are both intrinsic and are simply solutions to different problems, because the boundary conditions are different" (579). In effect, the two forms of shedding simply correspond to different values of the angle between the endplates and the axis of the cylinder. If no special status is bestowed on certain values of this angle by contrast with others, there is no reason to take only one of the shedding pattern as being normal or intrinsic. In this new perspective, the parallel and the oblique pattern are not two distinct phenomena with only one being the normal form of the wake, but two possible configurations of the flow corresponding to different values of a parameter of the experimental system, two possible solutions for the same system in different conditions.

But this new way of seeing implies that the two assumptions, on which the relevance of the simulation depended, must be rejected. First, a new parameter should be added to the set of relevant parameters of the system; and this parameter characterizes the end conditions of the cylinder. That means that the effect that the end conditions have on the development of the wake is now part of the structural characteristics of the wake, rather than being a source of interference or artifact. Second, this parameter is

independent of the length of the cylinder. The concept of end needs to be redefined in terms of pressure difference and value of the angle of the end plates that determined the value of this pressure difference.

By integrating this parameter among the set of relevant parameters the gain is one of conceptual unification: what were seen as two distinct phenomena have been unified under the same description. Steinle (2002) is right to associate exploratory experimentation with the introduction of new concepts that allow the formulation of empirical general regularities. But what generalities are the relevant ones is not an empirical claim.

To integrate the ends among the relevant factors, through the definition of a new relevant parameter and not to bestow a special status on a particular range of value of the angle are normative transformations of the investigation. There is a price to pay. On this understanding of the wake, the Landau model, only able, as we saw, to predict a linear variation of the frequency with the Reynolds number, and consequently only to account for a parallel pattern, does not account for the phenomenon. A different model will be needed able to account both for parallel and oblique patterns.

Conclusion

The elaboration of an experimental system, I said to begin, is an interactive, creative, open-ended process and contributes constructively to the processes of modeling and simulating.
The constructive contribution is mediated by the identification of the relevant parameters. The relevant parameters are characteristics of the experimental system such that their variation not only has an effect on the phenomenon but this effect is constitutive of the phenomenon, instead of being an artifact. The identification of the relevant parameters is required for determining the conditions in which measurements provide the empirical touchstone of a putative model of the phenomenon. Before that, a putative model is non-testable. The specification of the relevant parameters involves a systematic empirical investigation of the effects of different factors, but the line that is drawn between which effects are relevant and which are not, is normative. The effects of the relevant parameters are those a model of the phenomenon should account for.

When the route to the origin of certain effects is scattered with ambiguities and controversies, the simulation of fundamental equations may seem to offer a sure way to determine whether a certain effect is constitutive of the phenomenon. But, as we saw, only if some normative assumptions as to what factors need to be taken into account are built in the simulation. By establishing the relevance of additional factors, experimental activity may contribute to the reformulation of what factors need to be represented in the simulation.

We had an interactive process in that both the predictions of a still non-testable model and the results of a prejudiced simulation contributed to shaping the experimental search for the relevant parameters. The new relevant parameter that was introduced in the conception of the phenomenon amounted to a conceptual innovation. It is why I speak of a creative process.

And it is open-ended. As a new model was formulated in response to the reconception of the phenomenon, already, the exactitude of some of Williamson's measurements was called into question on the basis of an analysis of the solutions of this model. New measurements were to follow, and new simulations, and a modified version of the model, and so it goes on.

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