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Prediction Techniques in Classical Physics

The role of dimensional analysis

Second edition with short notes on Modern Physics

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In memory of my father Franco Jossa

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Introduction

The reasons of this book

This book was inspired by some thoughts that I developed in a previous work[1]. These thoughts, however, have been evolved over time, leading to a goal quite different from the one of the above-mentioned book, which has required a much more broad and differentiated effort.

Here I do not look only at the occasions to give space, in the specificity of a discipline, to the simplifying role of ordinary language. Instead, I am interested to identify a whole series of *simplification techniques for the prediction of scientific results*, as I will tell you soon. And, really, these techniques have become the main reason of this book.

Regarding ordinary language, I now mainly use it with the precise function of guide for prediction, as more specifically you will see in the second chapter. Finally, I extend here the role of dimensional analysis, emphasizing its correlation with the above-mentioned techniques and with ordinary language.

After this, I must be sure that the reference to ordinary language does not cause misunderstandings. This reference, in fact, seems to imply certain themes already studied in-depth, on which I do not want to return. Therefore, I have to defend myself against this risk, and I will do this by starting to state the issues that *not* will be object of my reflection.

I do not want to discuss whether it is possible, or not, the translation of the formalized language of Physics in ordinary language. It would be a return to an all-too-debated issue. Just remember, with G. Battimelli[2], as a famous figure in the history of Physics, *Michael Far-*

1. Jossa, P. "*Linguaggio ordinario e ragionamento puro nella Meccanica strutturale*" (*Ordinary language and pure reasoning in Structural Mechanics*), Aracne Publishing, June 2011.

2. Battimelli, G. "*Pillole pedagogiche ovvero I tormenti dell'insegnante di Fisica", (pedagogical pills or the torments of the teacher of Physics*), www.mercati esplosivi.com 2013.

aday, wrote, already in 1857, to an equally distinguished colleague, his compatriot *James Clerk Maxwell*: "There is one thing that I would like to ask you. When a mathematician is committed to investigate actions and physical results and comes to his own conclusions, it is not possible to formulate these conclusions in common language, with the same thoroughness, clarity and definiteness that in mathematical formulas? ... ". Besides, I am not interested to investigate, at a general level, on the existing relation between ordinary and mathematical language. This is another widely discussed issue. Moreover, it seems to me that there is now widespread agreement in judging that a complementary relation between these two languages exists, with strengths and weaknesses, as appropriate, for each of them.

I prefer, vice versa, to support my reflection on a motto that has guided, for many years, my teaching of Structural Engineering in the Faculties of Architecture of Naples and Reggio Calabria. The motto was: *"What can be done should not be taught."* You should read in it the intention of teaching a specialized discipline maximizing the students' activity, so obeying to a *constructivist line*, conglomerate of different positions in the students training.

This choice I practiced in classroom, accompanying it with pauses for reflection. With it I wanted to solicit the initiative, encouraging students to use the means at their disposal. However, that choice was not only the *qualitative reasoning* about the use of acquired knowledge, but it was also solicitation to perform autonomous constructions. A goal maybe difficult, but certainly not impossible to achieve, especially if we are able to produce *self-confidence,* with recognition of the naturalness and the simplicity of many issues.

The reader will notice that the last sentence broadens the discourse, because the words "natural" and "simplicity" connote the mode of action of *common sense*.

However, here, again, I have to say that I am not interested in returning to the question, also this discussed at length, of the potentiality of common sense. I merely point out that common sense is the dowry that more explicit the initiative, because of its natural tendency to look in all directions. In this regard, let me add, and I will reiterate it later, that I refer here to the common sense of a person with a certain level not only of culture, but also of specialist knowledge.

The last clarification comes from the fact that what I will say has *a character in some way preliminary and complementary to a systemat-*

ic study of some aspects of Classical Physics, i.e., to a study that normally obeys to the forms proposed by manuals. It is then obvious that I can hardly arouse the interest of a reader away from the culture of science, although, as they say, hope is the last to die.

So far, I have connected the memory of students in classroom with the figure of the reader, with reference to possible opportunities of activity and initiative. I must now be more precise about what I want to do. To this end, it is perhaps appropriate to bring attention, once again, to the few words that I have remembered of the Faraday's speech.

Faraday focuses on the transmission of the conclusions of a research work, a problem that usually, it must be assumed, causes difficulties and time-consuming for the achievement of a result. He therefore waives to describe the discovery path. This is an obvious fact, since discovery is essentially individual, and follows not predictable paths that are privilege of a few. He merely refers to the question, as I said, whether it is possible the translation of the language of mathematics in the language of all, implying the ability of the reader to understand the translation.

I, vice versa, have no knowledge, as obvious, of what have been the real discovery paths. Therefore, *I prefer to place myself in some way in parallel with them.* A location that it is not ambition of reconstruction, because reconstruction is the task of historians and because the historical reconstruction, although sometimes of great interest, is too often forced to lose effectiveness, due to necessary descriptions of returns and errors. Nor yet I have ambition to comparison. It would be an underestimation of the importance and of the originality of certain results, with the advantage of working ex post. This is what, for example, I reiterate when I predict some aspects of Maxwell's equations.

What I would like to do it is to verify if it is possible to find new simple ways to conquer a result. With the belief that in such a way we exclude the risk of impoverishing the study of Science with the reduction of the discourse in a rigid conclusive frame, *mere acknowledgement of previous successes.* And here, I must say again to Battimelli[3] that the boredom that some scientific textbooks convey is not always due to a certain poverty of formalized languages, but it can sometimes

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3. Battimelli, G. "*Linguaggi scientifici e linguaggi dei manuali:il caso della Fisica*", (S*cientific languages and languages of manuals: the case of Physics*), www.phys.uniroma1.it 2013.

depend on a too systematic organization of the various arguments, which results only in learning, with absence of any autonomous construction.

In short, I think that *we must take advantage of working ex post, by preceding the systematic acquisition of knowledge by one stage of prediction: i.e. from the logical structures, sometimes hidden by developments, and from the facts of common experience that can contribute to explanation.* All this, as I will say, looking continuously at possible simplified paths. A prediction which does not want to describe the mode of formation of a specific scientific result, but tries to grasp, in whole or in part, a possible path of recognition. It is clearly implied in this operation, the ambition to equip the reader so that he could grasp the conceptual simplicity and the naturalness that are usually at the base of a formulation, of a problem, of a theorem and of other aspects of Classical Physics.

E. E. Toulouse[4], a psychologist of the Psychology Laboratory of the School of Higher Studies in Paris, recall in a book of the year 1910 entitled *Henri Poincaré,* that Poincaré did not care much about rigor, disliking logic. He judged that logic is not a way to invent, but a way for structuring ideas. Moreover, he believed that logic can limit ideas. Toulouse also reminds that the way of Poincaré to deal with a problem was to try solving it in a manner as complete as possible in his mind, and then to translate it into a well organized paper.

Now, it is obvious that we cannot take for example a genius as Poincaré. However, it is a fact that logic is not intuition and is not discovery; and that the immersion, without deviations, in mathematical developments may produce automatisms, limiting the effectiveness of constructive thought. Furthermore, it is evident that the search for unconventional approaches to problems can stimulate the mental activity. Finally, we must not forget that, if it is only highlighted the instrumental nature of a problem, then there is risk to lose the concept and the rationality supporting it.

Dewey says: "The concept has also the feature of allowing the anticipated solution of problems." [5] Therefore, it is also in searching the

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4. Toulouse, E. "*Enquete medico psychologique sur la superiorité intellectuelle: Henry Poincaré*", (*Inquiry Medico psychologique on the intellectual superiority*: *Henry Poincaré*), E. Flammarion, Paris, 1910.
5. DEWEY, J. "*Logic: the Theory of Inquiry*", Holt, Rinehart and Winston, 1938.

basic concepts that are implicit in a problem that I want to give my little contribution. I think that the anticipation, when possible, of a concept, acting it naturally upstream of specificities, not only improves the organization of knowledge, but it is also a medium that can give reasons of developments. Rather, it is often also resolution.

For example, the concept of *uniformity,* applied to the motion of the water in a long channel with sections all equal to each other (paragraph 5.17), is an observation that enables immediate results, of course, for those who have a minimum of knowledge in terms of energy dissipation and equilibrium. Alternatively, we know that the concept of *symmetry* leads to immediate simplifications of many models. See the case, verily trivial, of the polar symmetry of a ring structure in paragraph 5.15; and see some determinations of the electric field with the only use of Gauss law. Moreover, it is enough to reflect on the concept of *variation* for deducing, with the moment of momentum, the law of the dependence of the resistance from the density and from the velocity, in the fast motion of an incompressible fluid. Also, the concept of *separation* between two sets of situations shows the existence of *critical values,* as it is for the axial load on a bar and for the speed of the water in channels. Finally, the concept of *perturbation* immediately gives, with the help of dimensional analysis, the expression of the speed of sound.

In further support of what I said, I believe that logical structures and common sense are not generally tools separated from each other, to use, either, depending on the occasions. They, in my view, contribute jointly for a purpose. For example, I think, perhaps repeating, that the innate ability, in man, to process concepts, is connected with another innate gift: common sense (with the meaning indicated above), and that we cannot break the reason sharply from common sense. Therefore, I am not in line with those who believe that common sense, not only does not benefit to scientific knowledge, but also causes distortions. I prefer, conversely, Quine[6]: "Science is an extension of common sense," or Mach[7] , "Every single individual ... in his grow to full consciousness, is ready for a world view that he has not deliberately helped to build. Everyone must start from here ". Finally Duhem[8]: "The unprovable assumption, immediate and obvious, on

6. QUINE, W.V.O., *Two Dogmas of empiricism*, Philosophical Review, vol. LX, 1, 1951.
7. MACH, E., "*Die Mechanic in ihrer Entwickelung historisch dargestellt 1883*", Italian translation: "*la meccanica nel suo sviluppo storico critico"*, Bollati Boringhieri, 1977, (*The Science of Mechanics*), Chicago – London, The open Court Publishing Co. 1919.
8. DUHEM , P., *La théorie physique: son objet et sa structure* , Marcel Rivière, Paris 1906. (*The aim and structure of physical theory*, Princeton University Press, Princeton, 1954.

which the power of rationality is founded and that ensures every rational reflection is common sense."

I now come to the crucial point of my book.

It is for me an evidence that the word *"simple"* has always played a leading role in scientific research, at least in scientific research related to problems of Classical Physics, namely to problems dealing with the macroscopic scale.

With this, I mean that the discovery paths are generally successful when we opt for the easier way, which is also, usually, the way with the greatest *explanatory power.*

This belief agrees completely with the goal so far shown. I look now at its reasons.

Often, those who read a scientific manual are not in a research phase, but rather tend to acquire certain outcomes and some tools, avoiding, or better deferring, the initiative. Therefore, for the purpose that I pursue, it is not enough to urge the reader to use the means at his disposal. It must be done something more.

The more to do is to take care to *simplify,* as much as possible, the path towards the result; a simplification that sometimes we must even force, by accepting all the risks that a forcing may produce. Anyway, I have mitigated these risks, using the limitations of the *prediction,* where the fascination of this word for the achievement of a result, is tempered by its very nature, essentially unrelated to certainty. Therefore, I chose to place the simplifications in the paragraphs of the *predictions*, before those of *developments.*

Of course, as mentioned, the combination of the two words: *simplification* and *prediction*, has the meaning of *simplifications for the prediction.* Hereafter, to be short, I speak only of *prediction techniques.*

The commitment around various prediction techniques, with the many meanings that I assign to the word "simplification", is the prevailing aspect of this book. These techniques, as I now will show, are sometimes equipped with a strong explanatory power. Therefore, I must dwell on some of them, with specific descriptions.

A first prediction technique is to pay attention to particular cases, even away from the pursued objective. We know that the particular can promote generalizations. They are the processes of induction: one of the two main paths, for Aristotle, through which we form our beliefs.

However, here the attention to "particular" has another meaning. It derives from the fact that many specific cases have the advantage of implicitly containing the general ones. It follows that the achievement, starting from the particular, of a result of general validity, does not involve the uncertainties of a real inductive process, but it is immediate and certain. We have, for instance, a situation like this when the result obtained in a concrete problem that contains the velocity vector of a fluid is extended, for the generality of the magnitude and the direction of this vector, to prove a general theorem that operates on generic vectors.

Another prediction technique that I have often used it occurs when we can determine an unknown entity, such as the direction of a vector, without demonstrations. This happens, for example, if there is only one direction with a character of *singularity,* as it is the direction of the normal to an equipotential surface at a point. So it is with the definition of the gradient, with the direction of heat flow, and finally with the singularity of the principal stress directions in some classical problems of elasticity.

Here, it is appropriate to reiterate the strength of these correspondences. They replace, in my opinion, the same demonstrations, especially when they do not allow alternatives. And in fact, it is my belief that such a replacement function exists in many prediction techniques that I mentioned and that I will say soon.

We have cases of singularity when we study the elastic impact between two rigid spherical masses. In particular, the singularity, in this case, of a reference system that moves with the gravity center of the masses, almost a symmetry situation, can produce immediate and definitive predictions.

We have singularities in some thermodynamic problems. In the problems of elasticity of the semi-plane and the semi-space, we easily recognize the directional singularities as the main directions of effort. Some solutions that use singularity, without other indications, result also in the study of stationary functions, in particular in the search of a maximum or a minimum. We will see two examples about in the study of geometric optics and in the distinction between slow and fast currents in the uniform motion of water in channels.

The many correspondences that exist between stationarity of functions and singularities in general (whether they are values or laws) have led me to devote a whole chapter to this topic, as I shall say later. In that chapter, you will notice that these correspondences have two important properties.

The first: if very simple functions have maxima, minima or stationary values, these values correspond to particularly evident singularities. If, instead, the simplicity of the functions decreases, it also reduces the evidence of the correspondences.

The case of the triangle is significant. The point that has the property of minimizing the sum of its distances from the vertices of a triangle is a remarkable and typically singular point (the three segments that join the point to the vertices form equal angles among them). The point that minimizes the sum of the squares of these distances is the centre of gravity of the triangle, of course, still a remarkable and singular point. The minimum of the sum of the cubes of the distances of a point from the vertices is a function certainly less simple, and in fact, the corresponding point has no evident properties. Finally, the minimum of the sum of the nth powers of the distances of a point from the vertices of the triangle, with *n very large,* turns back to be a simple function. In fact, the point that provides this minimum is the *circumcenter*, meeting point of the axes of the triangle, therefore again a remarkable and singular point.

The second: in many cases, the singularity is unique. Trivial example: the rectangle of given area with minimum perimeter admits a unique singularity, the square. However, the uniqueness applies also to singularities corresponding to the stationarity of certain functionals. These correspondences can take on the role of a real demonstration process. This implies substantial simplifications or, if you prefer, effective predictions.

About singularities, see also, in the following, "The applications".

You can say. All of this is questionable and it not always produces reliable results. Nothing to say. However, not always it is necessary to resort to classical demonstrations, especially if the prediction is effective and beneficial, with obvious advantages for understanding.

An interesting prediction tool comes from a postulate that I proposed: *in the absence of indications about the directions of vectors, conditions of parallelism or of orthogonality apply.* This leads to useful deductions in Magnetostatics and in the study of Maxwell's equations.

In particular, we can combine the postulate with simple considerations on the impossibility that conditions of parallelism between waves apply. This allows obtaining immediate indications about the existence of orthogonality conditions between the directions of oscillation of the electric and the magnetic field vectors and between these directions and the direction of propagation of the waves.