Intended and Unintended Mathematics: The Case of the Lagrange Multipliers

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Abstract

We can distinguish between two different ways in which mathematics is applied in science: when mathematics is introduced and developed in the context of a particular scientific application; when mathematics is used in the context of a particular scientific application but it has been developed independently from that application. Nevertheless, there might also exist intermediate cases in which mathematics is developed independently from an application but it is nonetheless introduced in the context of that particular application. In this paper I present a case-study, that of the Lagrange multipliers, which concerns such type of intermediate application. I offer a reconstruction of how Lagrange developed the method of multipliers and I argue that the philosophical significance of this case-study analysis is twofold. In the context of the applicability debate, my historically-driven considerations pull towards the reasonable effectiveness of mathematics in science. Secondly, I maintain that the practice of applying the same mathematical result in different scientific settings can be regarded as a form of crosschecking that contributes to the objectivity of a mathematical result.

1. Introduction

The broad issue of accounting for the successful application of mathematics in the empirical sciences, often referred to as 'the applicability problem', has puzzled philosophers since Plato's time and it is far from being dismissed as a redundant topic. Indeed, despite its impressive pedigree, the applicability problem is still central to philosophy of science and philosophy of mathematics (for a survey see Steiner 1998, Pincock 2010, Bueno and French 2018, Panza and Molinini forthcoming). The cause of this interest is not only due to the thought-provoking character that many philosophers credit to the effectiveness of mathematics in science (i.e., its successful applicability), but also to the ramifications that the applicability issue has in other debates which have recently received a renewed attention, as for instance that concerning the role of idealizations and mathematical models in science or that over the explanatory function of mathematics in the empirical sciences.[1]

Although the 'miraculous' effectiveness of mathematics is receiving a great deal of attention, a survey of the literature on the topic reveals that philosophical analysis have not given enough importance to a particular aspect of the applicability problem.[2] This aspect concerns the way in which mathematics gets involved in application. The main aim of my article is to shed light on such facet of applicability and evaluate its import in the context of the applicability problem and in relation to discussions about the objectivity of mathematical knowledge.

In Section 2 I introduce a distinction between two different ways in which mathematics is used in application: when the mathematical resources that are necessary to study a scientific phenomenon are not available and a mathematical theorem, concept or method is developed in the context of a particular scientific application (I call 'intended mathematics' the mathematics introduced in this way); when mathematics is not specifically introduced for application but later, at a further historical moment, we discover that we can apply it with success (I call 'unintended mathematics' the mathematics that finds

[1] An overview of the connections that the applicability problem has with several ongoing debates in philosophy of science and philosophy of mathematics is offered in Panza and Molinini forthcoming.

[2] An exception is Steiner 2005, to which I shall return below.

a successful application but that has been developed independently from that particular application).[3] The dividing line between intended and unintended mathematics, however, can be very subtle in many cases. Moreover, some applications of mathematics in the empirical sciences may be considered as involving mathematical resources that are unintended, since they are developed within a purely mathematical framework, but that are nonetheless introduced in a specific context of application. One of these intermediate cases will be considered here and concerns Lagrange's multipliers' method, whose general idea can be traced back to Lagrange's 1780 memoir on the libration of the Moon (Lagrange 1780) and whose explicit presentation appears in Lagrange's *Mécanique analytique*.
In Section 3 I focus on the case of the multipliers' method and I offer a reconstruction of how Lagrange presented it in his *Mécanique analytique*. Although the method of Lagrange multipliers has been embedded in contemporary mathematics (notably in multivariable calculus and constrained optimization) and is nowadays considered as a genuine (i.e., justified entirely within mathematics) piece of mathematical knowledge, in his *Mécanique analytique* Lagrange introduced the multipliers' method in the context of statics, in order to describe the equilibrium of a system of points subject to some constraints.
In section 4, by analyzing the import of the Lagrange multiplier's case in the context of the applicability debate, I show how my considerations reinforce some considerations that have been offered elsewhere and that support the *reasonable* effectiveness of mathematics in science. Indeed, due to the way in which mathematical frameworks as for instance that of Lagrange multipliers are introduced in application, it is natural to see how some mathematical results can be applied with success (and, as I shall suggest, even reapplied successfully in a different empirical setting that exhibits a similar structure). Secondly, in Section 5, I focus on the notion of objectivity and I suggest that the practice of reapplying the same mathematical result in different mathematical and scientific settings should be regarded as a form of (internal and external) crosschecking that contributes to the objectivity of a mathematical result. My final considerations will point to an even broader claim: it is not the objectivity of mathematics that makes its application successful, but the success of mathematics in application (and reapplication) that contributes to the objectivity of mathematical knowledge.

2. Intended and unintended mathematics

Intended mathematics is mathematics introduced *ex novo* and developed in the context of a particular scientific application. Examples include the Dirac delta function, as introduced by Paul Dirac in 1926 in his formulation of quantum mechanics (Dirac 1930), and the Fourier series introduced by Fourier in his *Théorie analytique de la chaleur* (1822) to solve the heat equation. A less studied example is given by the Dirichlet principle, which was introduced in physics (more precisely, in potential theory) in the middle of the 19th century (Monna 1975).[4] Unintended mathematics is mathematics that is (successfully) applied in the context of a particular scientific application but that has been developed independently from that particular application. Group theory, for instance, was applied with success in quantum mechanics in the later 1920s and early 1930s. Nevertheless, the mathematical theory of groups was developed long before its introduction into quantum physics. Differential equations as applied in population dynamics (a particular branch of ecology) to study prey-predator systems provide another example of unintended mathematics. This is especially clear if examine the birth of biomathematics that took place in the 1920s (cf. Israel 1988). In order to describe the dynamics of biological systems, scientists such as Vito Volterra and Alfred James Lotka used mathematical resources that had been already developed in pure mathematics. Mathematics was successful in predicting the variations and

[3] A similar distinction has been made by Mark Steiner. In the next section I will consider Steiner's proposal and I will spell out two respects in which my proposal is different from Steiner's.
[4] I thank José Ferreirós for having drawn my attention to the case of the Dirichlet's principle.

fluctuations in the number of individuals in cohabiting animal species, however it was not developed for that particular application.[5]

There are two clarifications that concern the distinction introduced above and that, I think, is important to address here. First, with my proposal I do not want to distinguish between two different 'kinds' of mathematics, which are subject to different standards of justification or which are the object of distinct practices. Mathematics is one and its body includes intended and unintended mathematics. Rather, my point is that it is relevant to differentiate between *how* some pieces of mathematical knowledge are introduced in the context of application. But this does not mean that I do not see these pieces of mathematics as components of a unique body of knowledge called mathematics. Second, I want to point up that my distinction between intended and unintended mathematics should not be taken rigidly. And this especially because even when a new piece of mathematics (a method, a mathematical object or a theorem) is introduced *ex novo* in application, there is some mathematical method, object or concept that is used in formulating and developing the new result. For instance, in the development of Fourier series, Fourier used sines and cosines to study the heat flow, but these functions were already available to him. Moreover, he also made use of other mathematical results (e.g., integrals) that had been previously developed by D'Alembert, Euler, Daniel Bernoulli and other scholars before him (cf. Davis and Hersh 1982). In this sense, it would be therefore reasonable to observe that intended mathematics can be potentially reduced to unintended mathematics. After all, this is a lesson that may emerge from a fine-grained historical analysis of the development of mathematics. I acknowledge this observation and I agree that, unless we want to consider very basic arithmetical and geometrical mathematical knowledge (something that I won't deal with in this study), the distinction is not as clear-cut as traced in the previous paragraph and intended mathematics may be seen as involving some pre-developed bits of mathematics. On the other hand, however, I think that an examination of scientific practice easily reveals that there exist cases where some mathematical results are fully developed in application (as in the Dirac's case) and cases where some already developed mathematics finds a successful application to an empirical setting (as in the example of group theory applied in quantum mechanics). This is, I think, a mere fact that provides a ground for introducing the distinction.

But my motivation to introduce the distinction above also stems from another observation. Very often mathematics is introduced in application on the basis of some empirical considerations and, more importantly, the mathematical resources that are introduced in this fashion make no mathematical sense at the time of the application.[6] In such cases, the role of the empirical setting is to "produce an intuitive 'natural' context for various abstract mathematical constructions" (Atiyah *et al.* 2010, p. 915). Consider, for instance, the case of the delta Dirac. The delta Dirac was introduced by Dirac to represent the mass density function of a point particle of mass 1 situated at the origin. Mathematically speaking, the delta function was defined on the real line so that it is zero everywhere except at the origin, with integral equals to 1. However, no function with these properties can be defined on the classical real line.[7] The Dirac 'function' was therefore introduced for application as a forcing, by keeping ''the physics to the forefront'' (Dirac 1930; cf. also Bueno 2005), and it was accepted as a legitimate piece of mathematical knowledge only after its interpretation as a distribution (Urquhart, 2008).[8] This case is essentially

[5] Obviously, some work was done to find the best way of modeling the biological scenario in terms of a system of differential equations. But this does not affect the point I want to stress here, namely that the mathematical resources employed by Volterra and Lotke were already available and they were not introduced *ex novo* in the modeling process.

[6] The process by which such 'massaged' mathematics is introduced has been called re-normalisation (cf. Steiner 1992 and Maddy 1997, Ch. 6).

[7] More formally, the delta Dirac is not a function in the ordinary mathematical sense because if a function is zero everywhere except at one point, and the integral of this function over its entire domain of definition exists, then the value of this integral is necessarily equal to zero (this is not the case for the delta Dirac, which has integral equal to 1).

[8] The delta function was interpreted as a distribution by Laurent Schwartz, who observed: "I believe I heard of the Dirac function for the first time in my second year at the ENS. I remember taking a course, together with my friend Marrot, which absolutely disgusted us, but it is true that those formulas were so crazy from the mathematical point of view that there was simply no question of accepting them. It didn't even seem possible to conceive of a justification. These reflections date back to 1935, and in 1944, nine years later, I discovered distributions. The original reflections remained with me, and became part

different from the scenario in which we apply a genuine piece of mathematics to a new setting, as for instance when we apply the resources of the theory of differential equations to study the dynamics of a population.

Even if examples such as that of the Fourier series show that there may not be an absolute measure to discriminate between intended and unintended mathematics, the distinction is well present in scientific practice and deserves some attention. Moreover, as the case of the delta Dirac shows, we have scenarios in which a mathematical resource is introduced in application without a clear mathematical justification. Obviously, the history of mathematics shows how in such cases the rigorous mathematical justification is the object of later analysis, and eventually the mathematicians will give a perfect sense to the mathematics that made no sense at the time of the introduction (as in the delta Dirac case). Nevertheless, the existence of such cases in scientific practice provides a robust rationale for considering as relevant a distinction between intended and unintended mathematics.

Before illustrating the example of Lagrange multipliers, which I take as representative of an intermediate case involving unintended mathematics that is nonetheless introduced in a specific context of application, in the final part of this section I want to shortly address the following question: Is my proposal to distinguish between intended and unintended mathematics a novelty in the literature over the applicability problem?

A survey of the literature concerning the applicability problem reveals that a similar proposal has been put forward by Mark Steiner in his Steiner 2005. Steiner distinguishes between what he calls *canonical* and *non canonical* empirical applications of a mathematical theory. Canonical applications are those applications of mathematics in which "the [mathematical] theory was developed in the first place to describe the application" (Steiner 2005, p. 627), while non-canonical applications are applications that are developed independently from the particular application in which they are introduced. Example adduced by Steiner include the use of the differential calculus as developed by Newton to describe accelerated motion (canonical application) and the application of the Apollonian theory of conic sections by Kepler to planetary orbits (non-canonical application).[9] It is therefore easy to see how Steiner's canonical and non canonical applications correspond to what I have named intended and unintended (applications of) mathematics. What, then, is the difference between Steiner's proposal and mine? Am I just rephrasing Steiner's view?

There are two important differences between Steiner's account and my proposal. The first one concerns what we consider as the mathematical content of the application. According to Steiner, what is developed in a canonical application is a mathematical *theory*. Nevertheless, and the introduction of the delta Dirac function serves as a good example, it is not always the case that what is developed *ex novo* in the context of an empirical application is a mathematical theory (actually, this is very rare). What is often introduced in canonical applications are mathematical methods and objects. Moreover, these pieces of mathematics are not necessarily formulated within an already present mathematical theory, and this is an aspect that makes them particularly interesting in the context of the applicability problem. The delta Dirac function, for instance, did not fit any available mathematical theory at the time of its introduction (the theory of distributions, in which the delta function acquired mathematical significance, came later). Being more general than Steiner's, my characterization of intended mathematics is therefore better suited to account for these scenarios found in scientific practice.

A second difference between my approach and Steiner's has to do with the importance that is given to the empirical context in which the application takes place. Steiner maintains that "a good deal of what passes for applications of mathematics in physics, is really the application of mathematics to itself" (Steiner 2005, p. 647). So, for him, canonical applications can be generally reduced to non canonical

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of the accumulated material I was talking about earlier, which remained in a corner of my mind, only to explode suddenly the night I discovered distributions, in November 1944. This at least can be deduced from the whole story: it's a good thing that theoretical physicists do not wait for mathematical justification before going ahead with their theories!" (Schwartz 2001, p. 218).

[9] It is important to note that Steiner's purported cases of canonical and non-canonical applications are not accepted unanimously by historians and philosophers of science. For instance, many scholars would not agree on the claim that differential calculus was introduced by Newton *in order to* describe accelerated motion. Although interesting, I won't address those criticisms here and I limit my treatment to a presentation of Steiner's proposal.

applications and the empirical context in which the application holds should be given less importance than the pure mathematical context. I disagree. I think that, even if the boundary of what is mathematical and what is empirical is very subtle in many applications (especially in modern mathematical physics), the empirical context in which the application holds is really central to the *philosophical* problem of applicability. Thus the problem of applied mathematics cannot be rendered mainly in terms of internal applicability (i.e., application of some parts of mathematics to another parts of mathematics), as Steiner seems to believe. With the help of an historically-driven analysis, I maintain, sometimes it is possible to sufficiently disentangle the pure mathematical content from the empirical content to analyze the particular kind of application under investigation. The discussion offered in this paper is intended to support this intuition, and I shall elaborate further on this point below.
The goal of the present section was to motivate the need of distinguishing between two different ways in which mathematics is introduced in application. Moreover, I showed how my proposal differs from the only similar account that has been offered in the literature, namely that put forward by Mark Steiner. In the next section I shall selectively focus on a case, that of Lagrange's introduction of the multiplier's method, which can be seen as an hybrid case in that involves (unintended) mathematics that is introduced independently from any particular application but that is nonetheless developed within a very specific context of application. This case study will serve as a backdrop for the philosophical analysis contained in sections 4 and 5.

3. The method of Lagrange multipliers

The mathematical method of Lagrange multipliers is a powerful tool for solving optimization problems with constraints and it is largely employed in physics, engineering and economics. The way in which this method is applied to find the extrema of a function subject to a constraint is fairly simple and it can be illustrated with a short example. Consider we want to maximize or minimize the function f(x,y) which is subject to the constraint g(x,y) = k. First, we generate the Lagrange function L(x,y,λ) = f(x,y) − λ[g(x,y) − k], where λ is called 'Lagrange multiplier'. This function is composed of the function to be optimized combined with the constraint function. Then we find the partial derivatives with respect to each variable x, y and the Lagrange multiplier λ, namely $L_x = \partial L/\partial x$, $L_y = \partial L/\partial y$ and $L_\lambda = \partial L/\partial\lambda$. Next, we set each of the partial derivatives equal to zero to get $L_x = 0$, $L_y = 0$ and $L_\lambda = 0$ (setting the partial derivatives to zero amounts to find the critical points of L: $\nabla_{x,y,\lambda}L(x,y,\lambda) = 0$). Finally, we solve the (3 equations in 3 variables) system to find x^* and y^* (the optimal values of x, y). $f(x^*,y^*)$ will be the max or min value of f(x,y) subject to the constraint g(x,y) = k. Generalized to a function $f(\mathbf{x})$ of n variables subject to m constraints $g_1,\dots,g_m$, the method of multipliers amounts to solving $n + m$ equations in $n + m$ unknowns. Using a modern notation, the method can be expressed in the following way:

$$\nabla L(x_1,\dots,x_n,\lambda_1,\dots,\lambda_m) = 0 \Leftrightarrow \begin{cases} \nabla f(\boldsymbol{x}) - \sum_{i=1}^{m} \lambda_k \nabla g_k(\boldsymbol{x}) = 0 \\ g_1(\boldsymbol{x}) = 0 \\ \dots \\ g_m(\boldsymbol{x}) = 0 \end{cases} \quad (1)$$

A simple application of the Lagrange multipliers in physics is given by the following problem: find the maximum range for a cannonball that is fired from the ground level (the range is the distance between the launch point and the landing point, where the projectile hits the ground). In this scenario, the function to be optimized is the range function $R(v_x, v_y) = 2v_x v_y/g$, where v_x and v_y are the horizontal and vertical components of the velocity. The constraint is given by the kinetic energy of the cannonball $E = m(v_x^2 + v_y^2)/2$, which is fixed (m is the mass of the cannonball). If we apply the method of Lagrange multipliers, we find that the maximum range is $R(v_x^*,v_y^*) = (2/mg)E$, where the optimal values are $v_x^* = v_y^* = \sqrt{E/m}$

(this result corresponds to the most commonly found result in terms of the launch-angle: $\theta = 45°$). The success of the method is due to its power to account for the fact that an empirical state of affairs (the maximum range for the cannonball) is realized under certain conditions (a certain velocity and a fixed energy). It is interesting to note that the physical meaning of $\lambda = 2/mg$ is the rate of increase of maximum range $R(v_x^*,v_y^*)$, which is the optimized quantity, with energy E: $\lambda = dR(v_x^*,v_y^*)/dE$ (Karabulut 2006). In general, in fact, the Lagrange multiplier is interpreted as the rate of change of the solution to the constrained optimization problem as the constraint varies. The value of the multiplier therefore provides an answer to the following question: How does the solution change as the constraint changes?

Although the genesis of the Lagrange's multipliers method can be tracked back to Lagrange's 1780 memoir on the libration of the Moon (Panza 2003, p. 149), Lagrange explicitly introduces the multipliers *en tant que* method in Part I, Section IV, of his *Mécanique analytique*, to investigate problems of static equilibrium (Lagrange 1788 & 1811).[10] Lagrange's procedure of introducing the multipliers' method in *Mécanique analytique* has been subject of analysis by historians of science, but for the purposes of this paper I shall briefly reconstruct his strategy here.

Lagrange starts considering a system of material points to which forces P, Q, R, …, are applied. The directions -lines of application- of such forces are named p, q, r, …, and dp, dq, dr, …, are the variations, or increments, of these lines due to an infinitesimal change in the positions in the direction of P, Q, etc. (dp, dq, dr, …, are named 'virtual velocities'). It is important to note that Lagrange uses 'd' to denote variations. Nevertheless, he does that only because he is not considering differentials too. When both of them come at use (from Section IV, Article 10), he changes his notation by using lower case delta 'δ' to denote variations and leaves 'd' for differentials, prescribing to rewrite his previous formulas by replacing 'd' with delta. For this reason, I will use 'δ' (instead of 'd') from the beginning, in conformity with Lagrange's own indications. Thus, the system of points is in equilibrium if:

$$P\delta p + Q\delta q + R\delta r + \ldots = 0 \quad (2)$$

where the quantities $P\delta p$, $Q\delta q$, …, are called by Lagrange the 'moments' (*momens*) of the forces P, Q, R, etc. (Lagrange 1811, p. 29).

Equation (2) expresses in analytical terms the principle of virtual velocities, whose first formulation appears in a letter by Johann Bernoulli to Pierre Varignon.[11] Bernoulli named 'virtual velocities' the projections of the fictitious velocities on the direction of the forces and observed how, in any machine in equilibrium, the algebraic sum of the energies of the applied forces must vanish (where energy is defined as the product of force by virtual velocity). Lagrange endorses Bernoulli's idea and offers a generalized statement of the principle:

> Si un système quelconque de tant de corps ou points que l'on veut, tirés chacun par des puissances quelconques, est en équilibre, et qu'on donne à ce système un petit mouvement quelconque, en vertu duquel chaque point parcoure un espace infiniment petit qui exprimera sa vitesse virtuelle, la somme des puissances multipliées chacune par l'espace que le point où elle est appliquée, parcourt suivant la direction de cette même puissance, sera toujours égale à zéro, en regardant comme positifs les petits espaces parcourus dans le sens des puissances, et comme négatifs les espaces parcourus dans un sens opposé. (Lagrange 1811, p. 22) [12]

From a modern standpoint, the principle of virtual velocities corresponds to what we call 'the principle of virtual work'. This principle states that once a system has reached equilibrium, the further expenditure

[10] In some passages of his memoir on the libration of the Moon (e.g., Lagrange 1780, pp. 16-17, 20-21, 25-26) Lagrange puts forward the idea of a method for the elimination of dependent variables that can be identified with what he will explicitly introduce, in *Mécanique analytique,* as the multiplier's method.

[11] The letter is reported by Varignon in his *Nouvelle mécanique ou statique* (Varignon 1725, tome 2, pp. 174-176) and it is dated 1717.

[12] For the purpose of rigor, in this study I quote Lagrange's original passages as they appear in the second edition of *Mécanique analytique* (Lagrange 1811). For an English translation, see Lagrange 1997.

of work is impossible. Consequently, if we assume that the system is given a virtual displacement, the increment in the quantity of work must be zero.[13] In equation (2), the quantities Pδp, Qδq, Rδr, etc…, which are the products of the forces and their relative virtual velocity (i.e., the 'moments of the forces' in Lagrange's terminology), can therefore be seen as the virtual works done by the forces P, Q, R, etc.[14]
In section IV Lagrange introduces the method of multipliers. The method is introduced to make independent to each other the variables that appear in equation (2), with the advantage of bypassing the complex calculations that may be required when using the theory of elimination of linear equations. More precisely, Lagrange considers the equilibrium of a system submitted to some constraints L=0, M=0, N=0, …, where L, M, N, …, are functions of several position variables (x, y, z, x′, y′, z′, etc.). When differentiated, the constraint equations give δL=0, δM=0, δN=0, etc. Note, however, that also the quantities Pδp, Qδq, etc. that occur in (2) depend on position variables. Thus, these variables are not all independent to each other. And the same holds for the variations δp, δq, etc. ('differentials'), which also appear in equation (2). To make the variables independent (i.e., to eliminate the dependency between the variables) is to reduce equation (2) to an equation involving only independent variables, from which the values of the position variables for the body at equilibrium can be obtained (indeed, this is Lagrange's goal). But performing such elimination through the theory of elimination of linear equations could be a laborious task because of the complex calculations that may be required. That is why Lagrange introduces a way of making this elimination easier, which is the multipliers' method and which according to Lagrange is equivalent to the procedure provided by the theory of elimination of linear equations ("la même méthode sous une forme plus simple", as he writes at the beginning of Section IV).
According to Lagrange, the elimination of dependent variables can be achieved if we adopt the following method: we consider the real numbers λ, μ, ν, …, etc. and, after having multiplied each of these numbers by a differentiated constraint equation (which gives: λδL = 0, μδM = 0, νδN = 0, etc.), we sum all these terms to the left side of equation (2) and we equate to zero. This is what he states in the following passage:

> On prendra la somme des *momens* de toutes les puissances qui doivent être en équilibre (sect. II, art. 5), et on y ajoutera les différentes fonctions différentielles qui doivent être nulles par les conditions du problème, après avoir multiplié chacune de ces fonctions par un coefficient indéterminé; on égalera le tout à zéro, et l'on aura ainsi une équation différentielle qu'on traitera comme une équation ordinaire *de maximis et minimis*, et d'où l'on tirera autant d'équations particulières finies qu'il y aura de variables; ces équations étant ensuite débarrassées, par l'élimination, des coefficiens indéterminés, donneront toutes les conditions nécessaires pour l'équilibre. (Lagrange 1811, pp. 75-76)

Mathematically, what Lagrange has found is the 'general equation of equilibrium' (*équation générale de l'équilibre*):

$$P\delta p + Q\delta q + R\delta r + \ldots + \lambda\delta L + \mu\delta M + \nu\delta N + \ldots = 0 \qquad (3)$$

in which Pδp, Qδq, Rδr, …, are the virtual works realized by the agent forces (or 'moments' of the agent forces, as Lagrange calls them) and λδL, μδM, νδN, …, are the virtual works realized by the reaction of the constraints, where λ, μ, ν, …, are undetermined coefficients. Indeed, if λ, μ, ν, …, are treated as forces, then the expressions λδL, μδM, νδN, …, represent the virtual works (*momens*) that are realized by these forces:

[13] The expression 'principle of virtual work' became dominant only in the mid-nineteenth century, after the engineering concept of work had been recognized as a central concept of mechanics (Darrigol 2014, p. 13; on the history of the principle of virtual work see also Capecchi 2012).
[14] The term 'moment' will be adopted for a long time. It began to be replaced by 'virtual work' only after Coriolis introduced this terminology in his treatise *Du calcul de l'effet des machines* (1829). In what follows I shall use 'virtual work' instead of 'moment', since this makes the treatment clearer from a modern point of view.

> Je remarque maintenant que les termes $\lambda\delta L$, $\mu\delta M$, etc. de l'équation générale de l'équilibre, peuvent être aussi regardés comme représentant les momens de différentes forces appliquées au même système. (Lagrange 1811, pp. 76-77)

At this point Lagrange observes that, from a mathematical point of view, the difficulty consists in determining the values of the undetermined coefficients λ, μ, ν, etc. Nevertheless, he comments, this can always be done because the number of equations that results from both the equations for constraints ($L = 0$, $M = 0$, $N = 0$, …) and the general equation of equilibrium ($P\delta p/\delta x + Q\delta q/\delta x + ... + \lambda\delta L/\delta x + \mu\delta M/\delta x = 0$; $P\delta p/\delta y + Q\delta q/\delta y + ... + \lambda\delta L/\delta y + \mu\delta M/\delta y = 0$; …) is the same as the number of coordinates:

> Toute la difficulté consistera donc à éliminer de ces dernières équations, les indéterminées λ, μ, ν, etc. ; or c'est ce qu'on pourra toujours exécuter par les moyens connus […] les équations dont il s'agit [$L = 0$, $M = 0$, $N = 0$, …], jointes à ces dernières [$P\delta p/\delta x + Q\delta q/\delta x + ... + \lambda\delta L/\delta x + \mu\delta M/\delta x = 0$; $P\delta p/\delta y + Q\delta q/\delta y + ... + \lambda\delta L/\delta y + \mu\delta M/\delta y = 0$; ...], seront toujours en même nombre que les coordonnées de tous les corps; par conséquent, elles suffiront pour déterminer ces coordonnées, et faire connaître la position que chaque corps doit prendre pour être en équilibre. (Lagrange 1811, p. 76)

Thus Lagrange points out that the values of the multipliers, namely the solutions to the system of equations given by the equations for constraints plus the general equation of equilibrium, can always be found. The reason is, again, that the number of equations is equal to the number of unknowns. What Lagrange has introduced as a method to eliminate dependent variables is the mathematical method of multipliers that, notation aside, is tantamount to what is expressed by equation (1) introduced at the beginning of this section.

4. The reasonable effectiveness of mathematics

The mathematical method of Lagrange multipliers is not only a powerful tool in physics and mathematics but it is also used with success in a wide range of scientific contexts, as for instance in biology and economics. This scenario is a trigger for the philosophical problem of applicability: how can we account for the successful applicability of the multiplier's method? I will address this question in the present section. Nevertheless, in order to do that, I shall first elaborate more on Lagrange's strategy seen in the previous section.

There is a crucial aspect of Lagrange's introduction of the multipliers' method that I have not explicitly considered in the previous section. This aspect has to do with the way in which Lagrange justifies the mathematical method. Does Lagrange provide a *mathematical* justification for it? Is the method justified on a different basis? Answering this question is crucial in the context of the applicability problem. The justification procedure may in fact turn out to be a constitutive element of the application process, and its study may shed light on those features that contribute to the success of mathematics in a specific application.

As I have shown in the previous section, the multiplier's method is applied by Lagrange to the equation provided by the principle of virtual work (called by him 'principle of virtual velocities'). The great importance that Lagrange gives to this principle leads him to offer two demonstrations of the principle of virtual velocities: the first in Vol. I of the 2nd edition of *Mécanique analytique* (1811) and the second in the 2nd edition of his *Théorie des fonctions analytiques* (1813). Nevertheless, both Lagrange's proofs are based on another principle, the 'principle of the pulleys', which is recognized by Lagrange as a more fundamental principle:

> Quant a la nature du principe des vitesses virtuelles, il faut convenir qu'il n'est pas assez évident par lui-même pour pouvoir être érigé en principe primitif; mais on peut le regarder comme l'expression générale des lois de l'équilibre, déduites des deux principes que nous venons d'exposer. Aussi dans les démonstrations qu'on a données de ce principe, on l'a toujours fait dépendre de ceux-ci, par des moyens plus ou moins directs. Mais il y a en Statique un autre principe général et indépendant du levier et de la composition des forces, quoique les mécaniciens l'y rapportent communément, lequel parait être le fondement naturel du principe des vitesses virtuelles; on peut l'appeler le *principe des poulies*. (Lagrange 1811, p. 23)

The principle of the pulleys, which is considered by Lagrange as more 'clear' (*évident*) that the principle of virtual velocities, states that given a system composed by two pulleys, one fixed and the other movable, around which an inextensible rope is wrapped, the relationship between power P and resistance R is 1/n, with n the number of whipping situated on either side of the pulley.[15] It is this principle that, according to Lagrange, provides a justification of the principle of virtual work.
An interesting aspect of the principle of the pulleys (and the same holds for the principle of virtual work) is that, as pointed out by René Dugas in his *History of Mechanics*, the principle can be considered as empirical in character:

> Lagrange's [first] demonstration is based on physical facts -on certain principles of pulleys and strings. (Dugas 1988, p. 336)

The same point, in a different flavor, was made by Carl Gustav Jacob Jacobi in his *Cours de Mécanique analytique* (1847/48). More generally, Jacobi criticized Lagrange's program of giving mechanics a purely mathematical foundation and making it a new branch of analysis. The problem is that what Lagrange sees as first, indisputable principles, have no such infallible status because they are based on certain propositions that are assumed to correspond to something in nature (as for instance the proposition that there exist inextensible ropes). Thus, for Jacobi, they cannot provide an incorruptible foundation for framing mechanics within an axiomatic-deductive system:[16]

> From the point of view of pure mathematics, these [mechanical] laws cannot be demonstrated. [they are] mere conventions, yet they are assumed to correspond to nature . . . There are, properly speaking, no demonstrations of these propositions, they can only be made plausible; all existing demonstrations always presume more or less because mathematics cannot invent how the relations of systems of points depend on each other. (Jacobi 1847/48, p. 3; cf. also Pulte 1998, p. 179)

From the previous observations, it therefore seems that there is a *prima facie* reason for arguing that the method of multipliers, which became to be widely used by physicists and mathematicians in problems of constrained optimization since Lagrange, was originally introduced by Lagrange on the basis of empirical considerations (where these considerations specifically concern the principle of virtual work and the principle of the pulleys). This observation, however, is too quick. Indeed, as it emerges from Lagrange's treatment, the method of multipliers is perfectly independent of the principle of virtual velocities: the former is applied to the equation provided by the latter (i.e., equation (2)), and thus the multiplier's method does not *depend* on the principle of virtual velocities. Even if we suppose that the

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[15] For a more detailed illustration of the principle of the pulleys and Lagrange's treatment of it see Capecchi 2012, p. 260, and Dugas 1988, pp. 334-335. According to Capecchi, Lagrange considers the principle of the pulleys as 'self-evident' (Capecchi translates 'évident' with 'self-evident' and therefore, for him, Lagrange is giving a particular epistemic status to the principle of the pulleys). Differently from Capecchi, I translate 'évident' with 'clear', since Lagrange does not seem to use 'évident' with the epistemic force that we attach to 'self-evident' (I am grateful to one referee for drawing my attention to this point).

[16] Helmut Pulte has defined 'mechanical Euclideanism' the ideal of reducing mechanics to an axiomatic-deductive system (Pulte 1998). Lagrange's program of reducing mechanics to analysis provides an example of mechanical Euclideanism. As Pulte shows, the project of reducing mechanics to mathematics belongs to a tradition that was already present when the first edition of Lagrange's *Mécanique analytique* was published.

principle of virtual velocities has an empirical nature, as René Dugas does, its empirical character is in no way transmitted to the multipliers' method, which is a purely mathematical method for elimination of dependent variables and whose justification is purely mathematical. The justification offered by Lagrange is vague, but it is nonetheless mathematically sound: the method is equivalent to the procedure of elimination of dependent variable provided by the theory of linear equations, and therefore the result can be easily proved within such theory. As Lagrange observes:

> Il n'est pas difficile de prouver par la théorie de l'élimination des équations linéaires, qu'on aura les mêmes résultats si on ajoute simplement à la formule dont il s'agit [equation (2)], les différentes équations de condition δL=0, δM=0, δN=0, etc., multipliées chacune par un coefficient indéterminé. (Lagrange 1811, p. 75) [17]

The method of Lagrange multipliers therefore provides an exemplar of what I have named unintended mathematics. It is indeed a mathematical method fully developed within mathematics. Nevertheless, there is an aspect that makes the case of Lagrange's multipliers different from the cases of unintended mathematics that I have considered in Section 2. The difference lies in the fact that, contrarily to what happens in cases of unintended mathematics such as that of group theory, the mathematics of multipliers is introduced in the context of a particular scientific application (a mechanical application: the study of static equilibrium of a system subject to some constraints). This makes the case of multipliers an intermediate case, namely a case of unintended mathematics which is nonetheless partly intended in the sense that the purely mathematical method is introduced in a context that is not purely mathematical.[18] More importantly, Lagrange introduced the multipliers by *tracking the application*. By 'tracking the application' I mean here that in his mathematical treatment Lagrange also gives a rendering of what is the applicative (and therefore non-mathematical) content of the mathematics he is introducing. In other words, although working within mathematics, he nonetheless follows in tandem (he *tracks*) the content of application. This is very clear from what he says at pages 76-77, where he remarks that if the system is at equilibrium, then the multipliers λ, μ, ν, …, can be interpreted as forces, and the expressions λδL, μδM, νδN, …, can be seen as the virtual works realized by the reaction of the constraints that are operating on the system.[19]

But how these considerations can be read in the context of the applicability problem? And, more specifically, can we use these considerations to argue that the successful application of multipliers in Lagrange is not unreasonable? I see, at this point, two possible ways to defend the claim that the successful application of the multipliers to the mechanical scenario considered by Lagrange can be accounted for and therefore can be made reasonable. The first route would be to agree with Dugas' remarks about the empirical nature of the principle of the pulleys and maintain that the successful application of the multiplier's method is grounded in the possibility to apply such principle, with the proviso that the physical system is studied at equilibrium. Indeed, if the system is at equilibrium, and if the expressions λδL, μδM, νδN, …, are seen as the virtual works realized by the reaction of the constraints that are operating on the system, it may be claimed that the general equation of equilibrium (3) is just a formulation of the principle of virtual work for a system subject to some constraints: the total work done by all forces (virtual and real forces) on a system in static equilibrium is zero for a set of infinitesimally small displacements. If we adopt this standpoint, we may consider that the Lagrange's method of multipliers depends on the principle of virtual work and its successful applicability is granted by a principle that, as Dugas claims, is empirical and not mathematical.

[17] A more rigorous justification came soon after the publication of the 2nd edition of *Mécanique analytique* and can be found in lesson 11 of Cauchy's lessons on infinitesimal calculus (Cauchy 1823). I am indebted to two anonymous referees for having helped me to clarify the content of this section and to give a better rendering of the way in which the multiplier's method is introduced and justified on purely mathematical grounds.

[18] Note that the multiplier's case is not even a case of intended mathematics, as the multipliers are not developed by keeping the content of the application at the forefront, as it happens in cases such as that of the delta Dirac.

[19] I have already quoted the passage in Section 3. For the sake of easy reading, I offer it again here: "Je remarque maintenant que les termes λδL, μδM, etc. de l'équation générale de l'équilibre, peuvent être aussi regardés comme représentant les momens de différentes forces appliquées au même système" (Lagrange 1811, pp. 76-77).

Although very attractive, I think that such a view is not as robust as it may appear. A first difficulty lies in the claim that the principle of the pulleys is *empirical*. Indeed, while the *system* of pulleys considered by Lagrange is an empirical one, it may be observed that Lagrange does not draw the *principle* from the observation of such a system but, rather, he *imposes* it to such a system.[20] Moreover, it can be also said that many idealizations are in place when considering Lagrange's system of pulleys, and these idealizations clearly do not correspond to anything that can be observed (or even found) in nature. Third, as I have noted above, the principle of virtual velocities (and so the principle of pulleys) is independent from the method of multipliers, and therefore the former cannot be said to be indispensable to the latter. For these reasons, I think that this first standpoint about the successful applicability of mathematics in Lagrange's case is not particularly attractive and I shall opt for a second, less ambitious, view.[21]

I have already stressed how the case of multipliers is an intermediate case of application because, although unintended, it is nonetheless a case in which the mathematics is developed in an applicative context. More particularly, I have pointed to the fact that Lagrange provides a tracking of the content of the application by identifying a correspondence (a 'mapping') between the mathematical entities that figure in the multipliers' method (the multipliers λ, μ, ν, …) and the empirical entities that may be considered to correspond to them (multipliers interpreted as *forces* acting on the system). Now, even if this tracking does not guarantee *per se* the success of the application, it ensures that the mathematics of multipliers has an interpretation in the model of the empirical setting under analysis.[22] But this point is crucial in the context of the applicability problem. Indeed, if such a correspondence can be fully established, and if the mathematics that is involved in the application is successful within mathematics, then the mathematical method will find its successful counterpart in the corresponding application too. This is what happens in Lagrange's case. First, the mathematics developed by Lagrange is secured on purely mathematical grounds since the method of elimination of dependent variables is consistent with other mathematical techniques that can be used (the method is even *equivalent* to the procedure provided by the theory of elimination of linear equations). Moreover, we are guaranteed that the solutions to the equation of equilibrium can always be found because the number of equations will be always equal to the number of unknowns. Third, the solutions to the equation of equilibrium have a counterpart in the application because they are the values of the position variables on which the constraint functions L, M, N, etc. also depend. But this ensures, with the proviso that the system is at equilibrium, the *successful* applicability of mathematics: the multipliers method always permits to find the solutions, namely the values of the position variables that provide the position of the body, for the specific class of mechanical problems Lagrange is considering (i.e., problems of static equilibrium of a system subject to some constraints).[23]

The effectiveness of mathematics in Lagrange's treatment of bodies at equilibrium is therefore not *un*reasonable, as some philosophers would be tempted to say (without taking account an historical reconstruction). It is true that accounting for the success of the method of multipliers requires a detailed reconstruction of how Lagrange introduced it. But once such a reconstruction is made, it is easy to see what makes the multipliers' method successful in its application to static problems (again, the interesting point in the context of the applicability problem concerns the analysis of *how* Lagrange presented the mathematical method, in tandem with the tracking of the application-setting). Nevertheless, as I noted above, the method applies with success also to other scientific settings that (seem to) have very little to

[20] Comments provided by one referee have been particularly helpful to clarify this point. Let me note, however, that the status (empirical or not) of the principle of the pulleys in Lagrange is not uncontroversial among historians of mechanics.

[21] It seems to me important to clarify that I am not claiming that such a position could not be defended and should be ruled out. Rather, I am pointing to some difficulties of it and because of these difficulties I am opting for a more moderate philosophical standpoint.

[22] It may be objected that the forces that Lagrange sees as corresponding to the multipliers are *virtual*, and that therefore these forces do not correspond to any *actual* force that is operating on the system. It should be noted, however, that as far as the system is considered at equilibrium there will be no difference, from a physical point of view, in treating virtual forces as real forces.

[23] My feeling is that this idea can also be accounted for in terms of the *inferential conception of the applicability of mathematics* advocated by Otávio Bueno and Mark Colyvan (Bueno and Colyvan 2011). However interesting, I shall not pursue this issue here for reasons of space and I leave it for future work.

share with the mechanical scenarios for which it was developed by Lagrange. For instance, it applies with success in dynamics, when we find the maximum range of the cannonball launch, or in economics, when it is necessary to find a maximum production level for a manufacturer's production that is modeled by a particular function, or even in chemistry, to determine the equilibrium composition of large chemical systems subject to generalized linear constraints. Should we consider this extended success a miracle? What do the problems of static equilibrium studied by Lagrange have in common with such problems? How can we account for the success of mathematics in these scenarios?

Accounting for *all* these successful applications is a complex issue that would deserve a separate study. Nevertheless, I shall sketch here a possible (though neither definite nor definitive) answer. It is reasonable to think that, in order to account for the success of the Lagrange's multiplier's method in empirical settings that seems to share little with the mechanical setting considered by Lagrange, we should find something that these settings actually have in common. The key observation is that what all these different applications share is the fact that the empirical system under study is experiencing some constraints and can be treated as a system which is, ideally, at equilibrium. Nevertheless, this observation must be complemented with the consideration that the constraints and the type of equilibrium that are involved in these applications are not necessarily of mechanical nature, as for instance when we consider a budget constraint in economics or a chemical equilibrium in a chemical reaction (in fact, it is exactly the nature of the constraints and the type of equilibrium involved that make those scenarios different among them). In these cases, which are labelled cases of 'constrained optimization', the method of multipliers successfully applies because, although different with respect to the type of constraints and equilibrium they are subject to, all the cases can be seen as instances of a more abstract setting, namely that in which we have a system subject to some constraints that can be treated in a situation of ideal equilibrium.[24] Consider, for instance, the example of the cannonball launch I offered in the previous section. In that case, if we bypass the actual significance of the variables that influence the system (velocity, energy) and we consider the system as a purely abstract system that depends on some variable (v_x , v_y) and some constraint (E), we can apply the multipliers' method as in the mechanical example given by Lagrange. This amounts to see the system as a generic system at equilibrium, subject to a constraint force (which would correspond to the energy E), for which a mapping between mathematics and the model of the empirical system can be established. When the system is treated at equilibrium, the solutions to the equation of equilibrium can always be found and they can be a maximum or a minimum (the maximum corresponds to the maximum range while the minimum coincides with the launch point, where the energy is zero; note that in both cases the dynamical system is seen as a system at equilibrium). Thus the same mathematical treatment provided by Lagrange can be applied with success to a different empirical setting.[25]

This conclusion may be not very surprising for many (after all, the mathematics of multipliers applies to constrained optimization problems exactly because these problems are problems of constrained optimization). Nevertheless, once it is read in tandem with the analysis of Lagrange's introduction of the multiplier's rule, it goes far beyond the naive claim 'mathematics works because it works for those problems for which it works'. The analysis provided above shows why the mathematics of the multipliers' method works for the problems studied by Lagrange and for a class of empirical applications that, although different from the setting originally considered by Lagrange, exhibit the same abstract structure (thus the possibility to reapply the multipliers' method). Moreover, it also suggests that it would be too simplistic to depict the success of this method in the empirical sciences as the success of a piece of pure mathematics that is applied outside mathematics. Many philosophers aim to consider the successful application of mathematics in this way, by focusing on already developed and

[24] Obviously, I am not claiming that all cases of unintended mathematics involve constrained optimization and equilibrium considerations. My intuition is that the investigation of the method of Lagrange multipliers provides insights on a specific type of application and other successful applications require different analysis.

[25] I suspect that applications of the multipliers in other empirical domains, as for instance in economics, could receive a similar analysis. Nevertheless, a presentation of these cases would require a more elaborated discussion that I am not addressing here for reasons of space.

justified mathematics rather than addressing theory building. But historical considerations, at least in cases such as that of the multipliers' method, seems to offer a more fine-grained version of the story.
Before moving to the final section of the paper, in which I will trace a potential connection between the notion of objectify and the (successful) applicability of mathematics, let me note how the considerations above seem to reinforce other claims that have been put forward in the context of the applicability problem. These claims can be summarized in the following slogan: Once we adopt an historical standpoint, there's nothing miraculous in the fact that some mathematical frameworks apply with success in science. For instance, Ivor Grattan-Guinness observes:

> Much mathematics, at all levels, was brought into being by worldly demands, so that its frequent effectiveness there is not so surprising […] It is necessary to emphasize this feature of mathematics, because, especially since the middle of the 19th century, a snobbish attitude developed among substantial parts of the growing mathematical community to prefer pure over applied mathematics ('dirty mathematics' to Berliners, for example). As a consequence the impression has grown that mathematics is always, or at least often, developed independent of the natural sciences, or indeed anything else; thus its undoubted effectiveness is indeed mysterious. (Grattan-Guinness 2008, p. 7)

And, similarly, José Ferreirós points out how

> We have to resist the temptation of picturing math as totally independent from other activities [...] From the perspective of my practice-oriented approach, the problem of "applicability" ceases to be posed as external to mathematical knowledge, and becomes internal to its analysis. This is a move toward the dissolution of the problem, since in some respects mathematical frameworks (all mathematical frameworks) are designed to be applicable, and so there is little to explain —that's nothing unreasonable. (Ferreirós 2015, p. 15)

5. Successful applicability and objectivity

In this section I want to shortly address a topic that, despite being viewed as a standalone theme in the philosophy of mathematics, has been often discussed in relation to the applicability problem. The topic in question concerns the 'objective' character of mathematical knowledge. Various philosophers of mathematics claim that the successful applicability of mathematics is a symptom of its objectivity, where the notion of objectivity is built upon the idea of 'ontological independence' (Kölbel 2002). On this reading of objectivity, what is objective is taken to be independent (or to exist independently) of human thought, as opposed to something that depends (or whose existence is not independent) of human intellectual activity. Thus, in the case of mathematics, to consider the content of mathematical knowledge as objective is to identify mathematics as a body of mind-independent absolute and necessary truths.
For most platonists, the successful application of mathematics supports a realist commitment to the mathematical entities and theories used in application. And this because the successful mathematics involved in application is regarded as indispensable to the application itself.[26] Thus the success of mathematics in application is a manifestation of its objectivity, and the objectivity of mathematics is what would account for its effectiveness in science. Call this standpoint a realist-account of application. Nevertheless, the realist position is not the only option to go and some anti-realist accounts of application have been proposed. Many of these attempts have been proposed by nominalists, but not all of them are nominalistic (cf. Bueno 2016). Furthermore, there are also accounts of application that are

[26] This attitude is reflected in the role that applied mathematics plays, according to the platonist, in the enhanced (or explanatory) indispensability argument (cf. Baker 2009).

neutral on the realism/anti-realism issue in the philosophy of mathematics. Bueno and Colyvan's inferential conception of application, for instance, can be adopted by both realist and anti-realist parties to account for the successful application of mathematics (Bueno and Colyvan 2011). In these non-realist accounts of application, the linkage between the objective character of mathematics and its success in application is not as central as it is in the realist accounts of application. For instance, Otávio Bueno and Steven French maintain that the role of mathematics in applications is merely heuristic, although they point out how "that does not mean that it [mathematics] lacks objective content" (Bueno and French 2018, p. 139).

What I want to sketch here is a different attitude towards talk of objectivity of mathematical knowledge. More precisely, I suggest a notion of objectivity that is not ontologically loaded as that endorsed in the realist accounts and that could make justice of the way in which mathematical and scientific practices evolve (where this evolution includes the success but also the failure of mathematics in internal and external applications). I shall refer to this notion as 'weak objectivity' to differentiate it from the strong objectivity introduced above. The main idea is to drop the belief that it is the (strong) objectivity of mathematics that makes the application successful, while still preserving the view that it is the success of mathematics in application that contributes to the (weak) objectivity of mathematical knowledge.

Shaping *ex novo* a notion of objectivity of mathematics that is not dependent on a view of mathematics as a body of mind-independent absolute and necessary truths may be very difficult. Luckily enough, some authors have paved the way towards this direction of investigation. For instance, in his recent book *Mathematical Knowledge and the Interplay of Practices* (2015), José Ferreirós has provided a story of how mathematical knowledge may be considered objective without necessarily bringing up an aprioristic view of mathematics. Ferreirós' account is given in terms of 'practices' internal and external to mathematics. Practices are based on different activities and therefore they should be considered as distinct. Examples of practices include the practice of counting, measuring, drawing, manipulating objects (these practices are called by Ferreirós 'technical practices' and are considered as more elementary, or proto-mathematical, because rooted in our particular cognitive abilities)[27], the practice of calculating and that of using a symbolic framework. Practices are agent-based because they are performed by human agents, nonetheless they are not subjective and relative only to the individual because they are adopted and shared within the mathematical community. Furthermore, they are interconnected and they mutually interact. It is precisely in this interaction that theoretical mathematical frameworks are defined (from this standpoint, mathematics is therefore seen as the result of a sequence of different and interconnected practices). And it is this interaction that, according to Ferreirós, constraints mathematical knowledge and confers to it its objective (i.e., its intersubjective and stable) character:

> we have working knowledge of several different practices and strata of knowledge, together with their systematic interconnections. This causes links that restrict the admissible —for instance, when a new framework is being developed— and that are responsible for much of the objectivity of mathematical results and developments. The interplay of practices acts as a constraint and a guide. (Ferreirós 2015, p. 39)

One reason for bringing up some of Ferreirós' ideas on objectivity and practices in the context of this paper is that they provide a trigger to extend my discussion of applied mathematics and link it to the notion of objectivity of mathematics. And this can be made on two different, although interconnected, levels of analysis.

First, results such as that obtained by Lagrange for the multiplier's rule can be considered as the product of different practices that, taken together, interplay and constrain the mathematical outcome. For instance, the use of the principle of virtual work (called 'principle of virtual velocities' by Lagrange)

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[27] The opinion that some parts of mathematics, as for instance elementary geometry, are grounded in basic cognitive skills is shared by some philosophers of mathematics (cf. Giaquinto 2007). Giuseppe Longo has named 'cognitive foundation of mathematics' the project of accounting for the intersubjective and conceptually-stable character of mathematics in terms of early cognitive processes (Longo 2003).

can be seen as defining a practice in mathematics *and* in mechanics.[28] This practice was not exclusive of Lagrange but was well established before Lagrange himself. Moreover, in order to introduce the famous result for his multipliers, Lagrange also employed other practices that were well accepted within the mathematical and scientific community of his time. For instance, he used a particular symbolic and theoretical framework for the mathematics he used (e.g., differential calculus and algebra). All these practices and the relative working knowledge that came with them converged to the development of the new mathematical method of multipliers and, more importantly, contributed to its objectivity. It is in fact the combination of these different (accepted) practices that, using Ferreirós' words, "restrict the admissible".

Second, it may be observed that the practice of successfully applying mathematics internally (within mathematics) and externally (in the empirical sciences) can also be considered as a separate practice, which itself contributes to the objectivity of mathematics. The interesting cases that I have in mind are cases in which a mathematical result is used with success, in empirical or even in mathematical applications, before it is proved rigorously in pure mathematics. Clearly, this is not the case with the multipliers' method, since as I showed Lagrange does provide a mathematical justification when he introduces it in *Mécanique analytique.* On the other hand, I have already mentioned the case of the delta function. And I underlined when I presented it in Section 2, this 'function' received a mathematical justification only later its introduction in application (actually, the delta function was not only mathematically unjustified when it was introduced by Dirac; it was also regarded as a non-*bona-fide* mathematical object). Nevertheless, the applications of the delta Dirac in physics were successful and this success led many physicists and mathematicians to regard the delta Dirac as a legitimate mathematical object even before its mathematical status was reconsidered. Other cases may be mentioned here, but the insight is that the successive *and* successful reapplication of mathematics in the empirical sciences and in mathematics also defines a practice, or better a sequence of practices (be they internal or external to mathematics). All these practices, when successful, contribute to the objectivity of the result itself. It is a form of reinforcement through crosschecking: the successful reapplications (internal and external to mathematics) of a mathematical result contribute to the objectivity of the result because they 'constrain further' the result within a (mathematical or empirical) setting that is not necessarily the setting in which that piece of mathematics was originally developed.[29]

In the present section I have maintained that there is an organic and still unexamined connection between applied mathematics and the notion of objectivity of mathematical knowledge. And this connection can be investigated by adopting an account like that proposed by Ferreirós. I haven't pushed further this idea, which would require a more detailed treatment, but I have just sketched a direction of investigation. The underlying intuition is, again, that it is not the objectivity of mathematics that makes its application successful, but the success of mathematics in application (and reapplication) that contributes to the (weak) objectivity of mathematical knowledge. That said, I want to be clear about what I am not claiming here. I am not suggesting that the weak sense of objectivity that I am considering should replace the strong objectivity endorsed by some philosophers. The possibility is still open for the mathematical realist to show that weak objectivity is just a manifestation of the strong objectivity of mathematics (indeed, weak objectivity can be seen as a necessary but not sufficient condition for strong objectivity). Or, alternatively, it may be the case that objectivity comes in degrees and the weak and the strong senses of objectivity may coexist. That said, my feeling is that a properly formulated notion of weak objectivity may have the merit to better render how mathematical practices evolves and how "mathematical knowledge is corrigible and not absolute" (Putnam 1975, p. 529). It lays down, it's true. And more work is needed to give this notion a better shape. One option would be to cash out the notion in terms of what can be a cognitive basis for our mathematical knowledge (for instance along the proposal suggested in Longo 2003), in order not to be subject to the criticisms that conventionalist or

[28] Here I am oversimplifying for the sake of simplicity. Indeed, it would be more appropriate to see the principle of virtual work as the result of a sequence of practices.

[29] Michèle Friend has recently offered an account of the objectivity of mathematical knowledge in terms of the notion of crosschecking (Friend 2014). Although she does not explicitly connect her analysis to the problem of applicability, Friend's idea of crosschecking well fits within the picture I am sketching here.

empiricist account of mathematics have received in the philosophy of mathematics. Another strategy, which has been recently explored by Michèle Friend, would be to capture the notion of objectivity through that of crosschecking in mathematics (Friend 2014). Such possibilities are still open and show the fertility of a terrain that has remained largely unexplored by those philosophers of mathematics who are much more fascinated by Plato and his incorruptible heaven.

5. Conclusions

Philosophical analyses of the applicability problem have largely focused on why it is possible to study an empirical phenomenon, predict or even explain it, by using some already developed mathematics. But little attention has been devoted to how mathematics is presented and developed in empirical applications. In this paper I provided a distinction between intended and unintended mathematics and I focused on a case-study that can be considered as an intermediate case between the two, namely a case of unintended mathematics that is nonetheless introduced in an applicative context. By focusing on this case, I showed how an historically-driven approach may disclose the reasons why a specific application is successful, thus making reasonable the effectiveness of a particular piece of mathematics in application (beyond the uninteresting 'it works because it works when it works'). I have not claimed that all forms of applications conform to that analyzed here. And I acknowledge that other cases of applications may require different analysis (be they cases of intended mathematics as that of the delta Dirac, cases of unintended mathematics as group theory applied in quantum mechanics, or even intermediate cases that fall in between). Nevertheless, I argued that the philosophical payoff of studying particular cases of applied mathematics from an historical standpoint is very high in the context of the applicability problem. Indeed, by focusing on such cases we can have a better rendering of the different ways in which mathematics is introduced, developed and applied with success in scientific modeling.

In the final part of the paper I have shortly addressed the question of how the present analysis may have repercussions on the debate concerning the objectivity of mathematical knowledge. It was not my goal to offer a mature notion of 'weak objectivity' of mathematical knowledge, capable to account for the stable and intersubjective character of mathematics. Rather, I have just traced a possible path for future work. And I showed how this path is a reasonable option once we consider a specific account of mathematical knowledge like that recently proposed by José Ferreirós.

Finding a general account of what makes applied mathematics successful is hard. And this especially because the applicability problem may not have an univocal solution but a full range of solutions, depending on the context of application that is explored. This may require a lot of philosophical work. This work, I suspect, cannot be pursued without seriously taking into account an historical outlook on how mathematics interplays with the empirical sciences. My modest aim here was to make a little step in that direction.

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References

Atiyah, M., Dijkgraaf, R. and Hitchin, N., (2010). Geometry and physics. *Phil. Trans. R. Soc. A,* 368(1914), 913-926.

Baker, A. (2009). Mathematical explanation in science. *British Journal of Philosophy of Science*, 60, 611-633.

Bueno, O. (2005). Dirac and the dispensability of mathematics. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 36(3), 465-490.

Bueno, O. and Colyvan, M. (2011). An inferential conception of the application of mathematics. *Noûs*, 45(2), 345-374.

Bueno, O. and French, S. (2018). *Applying Mathematics: Immersion, Inference, Interpretation.* Oxford: Oxford University Press.

Capecchi, D. (2012). *History of Virtual Work Laws: A History of Mechanics Prospective (Science Networks. Historical Studies, Vol. 42)*. Milano: Springer.

Cauchy, A. L. (1823). *Résumé des leçons données a l'école royale polytechnique sur le calcul infinitesimal*. *Tome premier*, Paris, 1823 in *Oeuvres* (2), 4, 5-261.

Darrigol, O. (2014), *Physics and Necessity: Rationalist Pursuits from the Cartesian Past to the Quantum Present*. Oxford: Oxford University Press.

Davis, P. J., and Hersh, R. (1982). *The Mathematical Experience.* Boston: Houghton Mifflin.

Dirac, P. A. M. (1930). *The Principles of Quantum Mechanics*. Oxford: Clarendon Press.

Dugas, R. (1988). *A History of Mechanics*. New York: Dover (trans. J. R. Maddox).

Ferreirós, J. (2015). *Mathematical Knowledge and the Interplay of Practices*. Princeton: Princeton University Press.

Friend, M. (2014). *Pluralism in mathematics: A new position in philosophy of mathematics*. Logic, epistemology and the unity of science, 32. Dordrecht: Springer.

Giaquinto, M. (2007). *Visual Thinking in Mathematics*. Oxford: Oxford University Press.

Grattan-Guinness, I. (2008). Solving Wigner's mystery: The reasonable (though perhaps limited) effectiveness of mathematics in the natural sciences. *The Mathematical Intelligencer*, 30(3), 7-17.

Israel, G. (1988). On the Contribution of Volterra and Lotka to the Development of Modern Biomathematics, *History and Philosophy of the Life Sciences*, 10(1), 37-49.

Jacobi, C. G. J. (1847/48), *Vorlesungen uber analytische Mechanik*. Berlin, ed. Helmut Pulte, Dokumente zur Geschichte der Mathematik, vol. 8, Braunschweig/Wiesbaden: Vieweg, 1996.

Karabulut, H. (2006). The physical meaning of Lagrange multipliers. *European Journal of Physics*, 27(4), 709-718.

Kölbel, M. (2002). *Truth Without Objectivity*. London: Routledge.

Lagrange, J. L. (1780) Théorie de la libration de la Lune et des autres phénomènes qui dépendent de la figure non sphérique de cette planète. *Nouveaux mémoires de l'Académie royale des sciences et belles-lettres de Berlin*, année 1780 (pub. 1782), classe de Math., pp. 203-309; in J. L. Lagrange, *Œuvres de Lagrange*, publiées par les soins de M. J.-A. Serret et G: Darboux, Gauthier-Villars, Paris, 1867- 1892, V, 5-122.

Lagrange, J. L. (1788) *Mécanique Analytique.* 1st ed. Paris*:* La Veuve Desaint.

Lagrange, J. L. (1811). *Mécanique Analytique.* 2nd ed. Paris: Courcier, 1811-1815 (2 vols.).

Lagrange, J. L. (1997). *Analytical Mechanics.* Translated and edited by A. Boissonnade and V. N. Vagliente, V.N. Translated from the *Mécanique Analytique, nouvelle édition* of 1811. Boston Studies in the Philosophy and History of Science, Vol 191, Springer.

Longo, G. (2003). The Constructed Objectivity of Mathematics and the Cognitive Subject. In Mugur-Schächter M., van der Merwe A. (Eds.), *Quantum Mechanics, Mathematics, Cognition and Action. Fundamental Theories of Physics* (pp. 433-463). Dordrecht: Springer.

Maddy, P. (1997). *Naturalism in Mathematics*. Oxford: Oxford University Press.

Monna, A. F. (1975). *Dirichlet's Principle: A Mathematical Comedy of Errors and Its Influence on the Development of Analysis*. Utrecht, Netherlands: Osothoek, Scheltema, and Holkema.

Panza, M. (2003). The Origins of Analytic Mechanics in the 18th century. In Jahnke, H. N. (Ed.), *A History of Analysis* (pp. 137-153). Providence, RI: American Mathematical Society and London Mathematical Society.

Panza, M. and Molinini, D. (forthcoming) Le problème de l'application des mathématiques. In Wagner, P., Panza, P., Poggiolesi, F. and Arana A. (Eds.), *Précis de Philosophie des Mathématiques et de la Logique.* Paris: Editions de la Sorbonne.

Pincock, Christopher (2010). *The Applicability of Mathematics*. Internet Encyclopedia of Philosophy.

Pulte, H. (1998). Jacobi's criticism of Lagrange: The changing role of mathematics in the foundations of classical mechanics. *Historia Mathematica*, 25(2), 154-184.

Putnam, H. (1975). What is mathematical truth? *Historia Mathematica*, 2(4), 529-533.

Schwartz, L. (2001). *A Mathematician Grappling with his Century.* New York: Birkhäuser. Translated from the French by Leila Schneps.

Steiner, M. (1992). Mathematical rigor in physics. In M. Detlefsen (Ed.), *Proof and Knowledge in Mathematics* (pp. 158-170). New York: Routledge.

Steiner, M. (1998). *The applicability of mathematics as a philosophical problem*. Cambridge: Harvard University Press.

Steiner, M. (2005). Mathematics — application and applicability. In S. Shapiro (Ed.), *The Oxford handbook of philosophy of mathematics and logic* (pp. 625-650). Oxford: Oxford University Press.

Urquhart, A. (2008). Mathematics and physics: Strategies of assimilation. In P. Mancosu (Ed.), *The Philosophy of Mathematical Practice* (pp. 588-620). Oxford: Oxford University Press.

Varignon, P. (1725). *Nouvelle mécanique, ou Statique, dont le projet fut donné en 1687*, Paris: Jombert.