MATHEMATICAL RIGOR AND PROOF

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Abstract. Mathematical proof is the primary form of justification for mathematical knowledge, but in order to count as a *proper* justification for a piece of mathematical knowledge, a mathematical proof must be *rigorous*. What does it mean then for a mathematical proof to be rigorous? According to what I shall call the *standard view*, a mathematical proof is *rigorous* if and only if it can be *routinely translated* into a formal proof. The standard view is almost an orthodoxy among contemporary mathematicians, and is endorsed by many logicians and philosophers, but it has also been heavily criticized in the philosophy of mathematics literature. Progress on the debate between the proponents and opponents of the standard view is, however, currently blocked by a major obstacle, namely, the absence of a precise formulation of it. To remedy this deficiency, I undertake in this paper to provide a precise formulation and a thorough evaluation of the standard view of mathematical rigor. The upshot of this study is that the standard view is more robust to criticisms than it transpires from the various arguments advanced against it, but that it also requires a certain conception of how mathematical proofs are judged to be rigorous in mathematical practice, a conception that can be challenged on empirical grounds by exhibiting rigor judgments of mathematical proofs in mathematical practice conflicting with it.

§1. Introduction. Mathematical proof is the primary form of justification of mathematical knowledge. But in order to count as a *proper* mathematical proof, and thereby to function *properly* as a justification for a piece of mathematical knowledge, a mathematical proof must be *rigorous*. The philosopher and logician John P. Burgess, in his book entitled *Rigor & Structure* (Burgess, 2015), put it as follows:

> The quality whose presence in a purported proof makes it a genuine proof by present-day journal standards, and whose absence makes the proof spurious in a way that if discovered will call for retraction, is called *rigor*. (Burgess, 2015, p. 2)

Any account of mathematical knowledge that does not provide a satisfactory characterization of rigor as a quality of mathematical proof necessarily fails to capture an essential aspect of the justification of mathematical knowledge, and for this reason shall be considered as inherently incomplete. Providing a philosophical account of what it means for a mathematical proof to be rigorous thus constitutes a central task for the epistemology of mathematics.

It may be argued that the issue was solved almost a century ago with the revolutionary logical and philosophical developments happening in the foundations of mathematics. In this regard, it is often considered that the notion of *formal proof*, together with the

doi:10.1017/S1755020319000443

identification of a set of axioms from which all of ordinary mathematics could be deduced, provide all the necessary elements for characterizing what it means for a mathematical proof to be rigorous. Such a characterization has been formulated by the mathematician Saunders Mac Lane:

> A Mathematical proof is rigorous when it is (or could be) written out in the first order predicate language $L(\in)$ as a sequence of inferences from the axioms ZFC, each inference made according to one of the stated rules. (Mac Lane, 1986, p. 377)

According to this view, a mathematical proof P is *rigorous* if and only if P complies to the standards of *formal proof* in one of the accepted formal deductive systems for the foundations of mathematics. This characterization, however, sets the standards too high. It is widely acknowledged that the mathematical proofs to be found in ordinary mathematical practice deviate significantly from the standards of formal proof.[1] For this reason, adopting such a characterization of rigor in an account of mathematical knowledge would have for direct consequence that the vast majority of mathematical knowledge we presumably have would not qualify as such, since it is justified by mathematical proofs that do not comply to the standards of formal proof.

Although the above characterization does not capture what it means for a mathematical proof to be rigorous in mathematical practice, it does set an *ideal* or *absolute* standard of rigor. Insofar as this ideal cannot be attained in practice, it has been proposed that it could still be reached *in principle*. Thus, Mac Lane pursued the above passage as follows:

> To be sure, practically no one actually bothers to write out such formal proofs. In practice, a proof is a sketch, in sufficient detail to make possible a routine translation of this sketch into a formal proof. When a proof is in doubt, its repair is usually just a partial approximation of the fully formal version. (Mac Lane, 1986, p. 377)

According to this view, a mathematical proof P is *rigorous* if and only if P can be *routinely translated* into a formal proof. The view presumably originates from Mac Lane's Göttingen dissertation entitled *Abgekürzte Beweise in Logikkalkul* (Mac Lane, 1934), and has been disseminated in the mathematical community with the first book of Bourbaki's *Élements de Mathématique* (Bourbaki, 1970). This latter treatise contains, at the very beginning of its introduction, the following similar expression of the view:

> In practice, the mathematician who wishes to satisfy himself of the perfect correctness or "rigour" of a proof or a theory hardly ever has recourse to one or another of the complete formalizations available nowadays, nor even usually to the incomplete and partial formalizations provided by algebraic and other calculi. In general he is content to bring the exposition to a point where his experience and mathematical flair tell him that translation into formal language would be no more than an exercise of patience (though doubtless a very tedious one). If, as happens again and again, doubts arise as to the correctness of the text under

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[1] As the mathematician Thomas Hales put it: "The ultimate standard of proof is a formal proof, which is nothing other than an unbroken chain of logical inferences from an explicit set of axioms. While this may be the mathematical ideal of proof, actual mathematical practice generally deviates significantly from the ideal" (Hales, 2012, p. x).

> consideration, they concern ultimately the possibility of translating it unambiguously into such a formalized language [...] [T]he process of rectification, sooner or later, invariably consists in the construction of texts which come closer and closer to a formalized text until, in the general opinion of mathematicians, it would be superfluous to go any further in this direction. (Bourbaki, 1970, p. 8)

This view constitutes almost an orthodoxy among contemporary mathematicians—probably as a direct influence of Bourbaki's heritage—and I shall therefore refer to it as the *standard view of mathematical rigor* (henceforth, the *standard view*).[2]

The standard view is endorsed today by many philosophers, logicians, and mathematicians—see, e.g., Avigad (2006), Burgess (2015), and Weir (2016)[3], among others.[4] But the view has also been heavily criticized in the literature, most notably by Robinson (1997), Hersh (1997), Detlefsen (2009), Antonutti Marfori (2010), Larvor (2012, 2016), and Tanswell (2015). Determining whether the standard view should be maintained, revised, or rejected is today one of the most pressing issues regarding the nature of mathematical rigor and proof.

The debate between the proponents and opponents of the standard view suffers, however, from a deficiency that threatens to block any significant progress, that is, the absence of a precise formulation of the standard view. As a consequence, this debate runs the risk of resting upon confusions of what the view actually means. The present work purports to remedy this deficiency by providing a precise formulation of the standard view. This will make it possible, in turn, to conduct a proper examination of the arguments against and in favor of it. The aim of this paper is thus to provide a precise formulation and a thorough evaluation of the standard view of mathematical rigor.

In this project, it will be of primary importance to introduce a distinction between what we shall call a *descriptive* account and a *normative* account of mathematical rigor. The distinction can be stated as follows: a *descriptive account* of mathematical rigor provides a characterization of the mechanisms by which mathematical proofs are judged to be rigorous in mathematical practice; a *normative account* of mathematical rigor stipulates one or more conditions that a mathematical proof ought to satisfy in order to qualify as rigorous.

Taken at face value, the standard view provides a normative account of mathematical rigor, where the condition that a mathematical proof P ought to satisfy in order to qualify

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[2] The same terminology is adopted by Antonutti Marfori (2010), while Detlefsen (2009) refers to it as the *common view*.

[3] Weir (2016) endorses an amended version of the standard view in which the notion of formal proof is not restricted to finite proofs but allows for "infinitary derivations" as well (Weir, 2016, p. 25).

[4] Azzouni (2004, 2006, 2009, 2013) has defended a view that he has called the *derivation-indicator view* and in which mathematical proofs *indicate* formal derivations. The derivation-indicator view bears some similarities to the standard view in that it accounts for the rigor of mathematical proofs through a certain relation to formal proofs, but it also distinguishes itself from Mac Lane's and Bourbaki's original formulation by rejecting the idea that mathematical proofs are *abbreviations* or *sketches* of formal proofs (see Azzouni (2006, pp. 148–150) and Azzouni (2013, p. 248)). In this paper, I will focus on the standard view since this is the view that has been driving the contemporary discussions on mathematical rigor and proof. A detailed comparison of the standard view and the derivation-indicator view is called for to do full justice to the subtleties of Azzouni's view.

as rigorous is that P can be routinely translated into a formal proof.[5] The motivation behind this condition can be read directly from the passages of Mac Lane and Bourbaki quoted above, and originates from the issue that arises when one wishes to maintain formal proof as the ideal of proof while realizing that this ideal is not reachable in practice (for the simple reason that the length of formal proofs would render them unmanageable for any human being). The condition that P can be routinely translated into a formal proof offers some sort of a middle ground to solve this issue: it provides a less demanding condition for qualifying mathematical proofs as rigorous which, one might hope, could be met in practice, while still maintaining a certain connection with formal proofs, i.e., with the ideal of proof.

Yet, if the standard view would only amount to a normative account of mathematical rigor, it would not provide much of an epistemological grip, for stating a normative condition is harmless until one somehow commits to it in practice. This is why Mac Lane and Bourbaki *do* consider that the above normative condition *does* bind rigor judgments of mathematical proofs in mathematical practice—this is manifest in the passages quoted previously, where both Mac Lane's and Bourbaki's descriptions of the standard view are preceded by the phrase "in practice". This means that, from their perspectives, one can legitimately qualify a mathematical proof P as rigorous *only* when one possesses some *grounds* for holding that P can be routinely translated into a formal proof. But how, then, according to this conception, can one ever be able to legitimately qualify a mathematical proof P as rigorous in mathematical practice? The most natural way to make sense of this, it seems, is to think of the proponents of the standard view as possessing an implicit conception of the mechanisms by which mathematical proofs are judged to be rigorous in mathematical practice—i.e., as possessing an implicit *descriptive account* of mathematical rigor—together with some reasons for holding that whenever a mathematical proof P has been judged as rigorous according to these mechanisms, P can be routinely translated into a formal proof, i.e., P satisfies the above normative condition. This idea lies at the basis of the present attempt to provide a precise formulation of the standard view.

Thus, we shall take the standard view as embedding *both* a descriptive account *and* a normative account of mathematical rigor, and as stating a certain relation between them. We shall refer to the descriptive account as the *descriptive part* of the standard view, and say that a mathematical proof P is $rigorous_D$ if and only if P would be judged to be rigorous according to the mechanisms inherent to this descriptive account. We shall refer to the normative account as the *normative part* of the standard view, and say that a mathematical proof P is $rigorous_N$ if and only if P can be routinely translated into a formal proof. The relation between the descriptive part and the normative part of the standard view expresses then a substantial philosophical thesis, namely that the practice *conforms* to the normative condition stated in the normative part. We shall refer to this as the *conformity thesis*, and shall state it at follows: for any mathematical proof P, P is $rigorous_D$ *implies* that P is $rigorous_N$.

From this perspective, a proponent of the standard view must hold (1) a precise conception of what it means for a mathematical proof to be $rigorous_D$, (2) a precise conception of what it means for a mathematical proof to be $rigorous_N$, (3) some reasons for holding

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[5] The standard view cannot be meaningfully read as a descriptive account of mathematical rigor, for it does not say anything on how mathematical proofs are judged to be rigorous in mathematical practice. As we shall see later on, several of the arguments against the standard view originates from a reading of the standard view as providing a descriptive account of mathematical rigor.

the conformity thesis. This suggests, in turn, a three-step methodology to reach a precise formulation of the standard view: (1) specify the *descriptive part* of the standard view, i.e., characterize what it means for a mathematical proof to be rigorous$_D$; (2) specify the *normative part* of the standard view, i.e., characterize what it means for a mathematical proof to be rigorous$_N$; (3) identify the reasons for holding the *conformity thesis*. This is precisely the methodology to be adopted in this paper in order to provide a precise formulation of the standard view of mathematical rigor.

Before moving further, it is important to say explicitly at the outset, and to keep in mind all along, what the standard view is meant to accomplish. The *raison d'être* of the standard view is to be found in its capacity of dealing with the facts that, on the one hand, formal proof is considered to be the contemporary ideal of proof in present-day mathematics, but on the other hand, this ideal is not reachable in practice. What the standard view provides is a *tie* between the *practice* of proof and the *ideal* of proof, thus allowing to maintain the contemporary ideal of proof while admitting that it cannot be reached in practice. It is precisely in this tie that lies the philosophical core of the standard view. It is also for this reason that the debate between the proponents and the opponents of the standard view requires dedicated attention, for if the standard view is shown to be philosophically untenable, this would have for direct consequence to break the tie between the practice and the ideal of proof. And in the absence of a viable alternative to restore such a tie, this would force to give up, or at least revise, the contemporary ideal of proof. Such an issue would then be of primary importance not only for the philosophy of mathematics, but for the contemporary practice of mathematics itself.

The paper is organized as follows. §2 comes back to the historical roots of the standard view in the works of Mac Lane and Bourbaki. §3 proposes a general schema for the formulation of any descriptive account of mathematical rigor, specifying thereby what is to be expected of a descriptive account of mathematical rigor. §4, §5, and §6, are concerned with the three elements of the standard view—the descriptive part, the normative part, and the conformity thesis—which, taken together, provide a precise formulation of the standard view. §7 evaluates, from the point of view of this formulation, the main arguments that have been advanced against the standard view. §8 develops and assesses an argument in favor of the standard view based on an approach originally proposed by Mark Steiner (1975). §9 ends this paper by wrapping up the main conclusions of our study.

§2. Historical roots: Mac Lane and Bourbaki on mathematical rigor. In order to provide a precise formulation of the standard view, it is necessary to first come back to its historical roots, that is, to its original formulations by Mac Lane and Bourbaki. It is then interesting to notice that neither Mac Lane nor Bourbaki had the intention to provide a philosophical account of mathematical rigor as a quality of mathematical proof. Rather, their conceptions of the rigor of mathematical proofs, as expressed in the passages quoted in the introduction, appear as a *consequence* of the general projects they were undertaking. In order to understand Mac Lane's and Bourbaki's original formulations of the standard view, we shall, in this section, review the essential elements of these projects as presented by Mac Lane in his Göttingen dissertation entitled *Abgekürzte Beweise im Logikkalkul* ("Abbreviated Proofs in the Calculus of Logic") (Mac Lane, 1934) as well as in the associated paper (Mac Lane, 1935), and by Bourbaki in the first book of the *Éléments de Mathématique* entitled *Théorie des Ensembles* (Bourbaki, 1970). We shall then see how their formulations of the standard view follow naturally from the projects they were undertaking.

2.1. Mac Lane on mathematical rigor.   Although Mac Lane is often invoked in the philosophical literature as a main proponent of the standard view, and the quote from Mac Lane (1986) reported in the introduction is often considered as the archetypal formulation of the standard view, it is only rarely mentioned that the standard view finds its origins in Mac Lane's Göttingen dissertation in mathematical logic, which was precisely concerned with the analysis of the structure of mathematical proofs. And yet, as we shall now see, being acquainted with the content of Mac Lane's dissertation (Mac Lane, 1934), as well as with the associated paper (Mac Lane, 1935), turns out to be *essential* in order to understand the formulation of the standard view expressed in Mac Lane (1986), and in particular to understand Mac Lane's conception of the notion of *routine translation* central to it.

The main goal of Mac Lane's dissertation was to develop, within the field of mathematical logic, a richer theory of the structure of mathematical proofs.[6] One part of his doctoral project was then dedicated to providing a precise analysis of the inferential steps constituting ordinary mathematical proofs—what we shall call *mathematical inferences*.[7] To this end, Mac Lane's starting point was the view that mathematical inferences can be seen as specific combinations of the elementary types of inferences usually investigated in mathematical logic at the time. An analysis of mathematical inferences could then be obtained by identifying and characterizing such combinations:

> It is well known that all the steps of a proof may be reduced to combinations of the two following elementary processes:
>
> 1. *Inference:* If the theorems p and $p \supset q$ are known to be true, then we can assert the proposition q.
>
> 2. *Replacement:* If the theorem $\phi(x)$ involving the free variable x is known to be true, then we can assert the proposition $\phi(c)$ which arises from ϕ by replacing x everywhere by some one symbol c. This symbol c may be a constant, a variable, or a combination of constants and variables, but all its values must be within the range of the variable x.
>
> The actual steps taken in the course of most mathematical proofs are not single instances of these two rules, but are rather complex combinations of them. [...] If mathematical logic is to be developed into a powerful method, it cannot content itself with these two elementary operations alone, but it must advance to the definition of their most important combinations. (Mac Lane, 1935, p. 122)

Most of Mac Lane's dissertation was thus dedicated to the development of a formal machinery aiming at analyzing those combinations, an enterprise nicely summarized by Mac Lane himself in the following passage:

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[6] Mac Lane (1935) remarks that: "Classical mathematical logic has [...] given a complete and adequate description of the structure of mathematical theorems, but is has solved only the most elementary problems connected with the structure of mathematical proof" (Mac Lane, 1935, p. 121).

[7] In his review of Mac Lane's philosophy of mathematics, Colin McLarty points out that since his first encounter with foundational issues through the reading of Hausdorff's 1914 monograph on set theory (Hausdorff, 1914): "Mac Lane has [...] urged that logic should not merely study inference in principle, but the inferences made daily by mathematicians" (McLarty, 2007, p. 89). For an analysis of the differences between mathematical inferences—inferences as commonly carried out in mathematical practice—and logical inferences—inferences as described by the model of formal proof—see Hamami (2018).

The thesis [...] observes that long stretches of formal proofs (written, say, in the style of *Principia*) are indeed trivial, and can be reconstructed by following well-recognized general rules. The thesis develops standard metamathematical terminology to describe formal expressions—as certain strings of symbols, suitably arranged. This is followed by a meticulous description of what it means to substitute y (or something more complex) for x in an expression. This description let me state exactly what it would mean to determine that one expression is a special case of another.

On this basis, I described exactly a number of the routine steps in a proof, giving each a label, as for example:

Inf schrumpf: To prove a theorem $L \supset P$, search for a prior theorem of the form $M \supset N$, where L is a "special case" of M and P the corresponding special case of N.

Sub inf schrumpf: Given a prior theorem $M \supset N$, one can conclude that $L \supset L'$, where L' is obtained from L by replacing every "positive" component of the form M by a new component N.

Sub Def: Substitute the definitions.

Identität: Use one of the standard identities of algebra (or of the propositional calculus).

Sub Theorem # 20.43: Use the cited theorem, in the (only) possible way.

x = C fixieren: Given a premise $(\exists x)L(x)$, assert $L(C)$ for some suitable "constant" C.

Halborn: Move a quantifier $\exists x$ or $\forall x$ to the front of an expression.

All told, the thesis gives twenty or twenty-five of such rules (listed at the start of Chapter VII), and then observes that many proofs can be "abbreviated" by listing in order the rules to be applied. In this sense, the thesis gives a formal definition of a routine proof. (Mac Lane, 1979, p. 65)

The central idea of Mac Lane's dissertation is thus to introduce what we shall call *higher-level rules of inference*—what Mac Lane refers to as "general rules" in the previous quote.[8] Those higher-level rules of inference correspond to specific combinations of the elementary rules of inference of the formal deductive system one is considering. For each higher-level rule of inference identified, Mac Lane specifies in his dissertation the specific combination it corresponds to in terms of the rules and theorems of *Principia Mathematica*. This means that to each higher-level rule of inference is associated an algorithmic procedure that makes it possible to transform any application instance of the rule into a sequence of inferences complying to the rules of the considered formal deductive system, for it suffices to replace it by the combination of elementary rules of inference it corresponds to. It is with respect to these algorithmic procedures that Mac Lane uses the term 'routine': a mathematical inference is *routine* if it corresponds to an instance of a higher-level rule of inference for

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8 Some rules, such as *Inf schrumpf*, have a structure different from a traditional rule of inference, insofar as they may encompass one or more search procedures.

which there exists an algorithmic procedure allowing to transform the given mathematical inference into a sequence of applications of elementary rules of inference; a mathematical proof is *routine* if it is composed of routine mathematical inferences.

In this way, Mac Lane offers a formal framework in which it is possible to represent any given routine mathematical proof as a particular sequence of applications of higher-level rules of inference. These higher-level rules of inference constitute thus a means to abbreviate or condense formal proofs so as to obtain proof descriptions that come much closer to the way routine mathematical proof are presented in ordinary mathematical practice:

> In summary, the thesis observed that many proofs in mathematics are essentially *routine*—and that one can carefully write even a complete description of each type of routine step, so that the formal proof of the theorem, written in detail, can be replaced by the much shorter description of these steps. (Mac Lane, 1979, p. 66)

It is now easy to see why Mac Lane came to conceive of a rigorous mathematical proof as one that can be *routinely translated* into a formal proof: insofar as he considers that the (routine) mathematical inferences comprising a (routine) mathematical proof are all instances of higher-level rules of inference, one can then appeal to their associated algorithmic procedures to turn each (routine) mathematical inference into a sequence of inferences complying to the elementary rules of inference of the formal deductive system under consideration, and thus to translate the original (routine) mathematical proof into a formal proof. The procedure of *routine translation* is thus entirely specified by the set of algorithmic procedures underlying the higher-level rules of inference, and simply consists in replacing each application of a higher-level rule of inference by the combination of elementary rules of inference it corresponds to.

2.2. Bourbaki on mathematical rigor.

The mathematical text at the origin of the large-scale diffusion of the standard view within the mathematical community is presumably Bourbaki's *Éléments de Mathématique*, and more specifically the first book of the treatise entitled *Théorie des Ensembles* (Bourbaki, 1970), which contains most of Bourbaki's considerations on rigor and foundational issues. The Bourbaki's quote reported at the beginning, and expressing the most common formulation of the standard view, was indeed extracted from the second page of the introduction to the *Théorie des Ensembles*. In order to understand this formulation of the standard view, we shall now come back on the more general perspective undertaken by Bourbaki in the first book of the *Éléments de Mathématique*.

For the purpose of the present discussion, it is important to first recall two of the main goals of Bourbaki's enterprise. First, Bourbaki aims to rebuild the whole edifice of mathematics in the manner of Euclid's *Elements*, that is, to establish each mathematical result deductively using resources previously obtained in the treatise, which can be traced back ultimately to a given set of primitive principles or axioms stated at the very beginning. Second, Bourbaki aims to adopt a proof practice that could claim to the highest level of rigor attainable, and which rests on a particular use of formalized languages. This second goal is discussed in the very opening of the introduction to the *Théorie des Ensembles*, where it is first noticed that:

> By analysis of the mechanism of proofs in suitably chosen mathematical texts, it has been possible to discern the structure underlying both

vocabulary and syntax. This analysis has led to the conclusion that a sufficiently explicit mathematical text could be expressed in a conventional language containing only a small number of fixed "words", assembled according to a syntax consisting of a small number of unbreakable rules: such a text is said to be *formalized*. (Bourbaki, 1970, p. 7)

Although the mere possibility of formalizing existing mathematical texts does not necessarily imply that ordinary mathematical practice should be directly concerned with formalized languages, Bourbaki argues that the "conscious practice" of the axiomatic method does require a certain epistemological relation with formalized languages:

> Just as the art of speaking a language correctly precedes the invention of grammar, so the axiomatic method had been practised long before the invention of formalized languages; but its conscious practice can rest only on the knowledge of the general principles governing such languages and their relationship with current mathematical texts. In this Book our first object is to describe such a language, together with an exposition of general principles which could be applied to many other similar languages; however, one of these languages will always be sufficient for our purposes. (Bourbaki, 1970, p. 9)

The issue of describing such a formalized language is, of course, directly connected to the other goal of Bourbaki's enterprise mentioned above, namely to provide a general foundational framework within which the whole of mathematics could be represented and deduced. As is well-known, Bourbaki adopted as a foundational framework a (certain version of) the theory of sets:[9]

> For whereas in the past it was thought that every branch of mathematics depended on its own particular intuitions which provided its concepts and prime truths, nowadays it is known to be possible, logically speaking, to derive practically the whole of known mathematics from a single source, the Theory of Sets. Thus it is sufficient for our purposes to describe the principles of a single formalized language, to indicate how the Theory of Sets could be written in this language, and then to show how the various branches of mathematics, to the extent that we are concerned with them in this series, fit into this framework. (Bourbaki, 1970, p. 9)

Bourbaki's initial impulse was thus to rebuild the whole edifice of mathematics within a foundational framework consisting of a formalized version of the theory of sets.

  Of course, carrying out such a project faces some daunting practical difficulties. Bourbaki acknowledges this and puts forward some solutions to make the project feasible:

> If formalized mathematics were as simple as the game of chess, then once our chosen formalized language had been described there would remain only the task of writing out our proofs in this language, just as the author of a chess manual writes down in his notation the games he proposes to teach, accompanied by commentaries as necessary. But the matter is far from being as simple as that, and no great experience is necessary to perceive that such a project is absolutely unrealizable: the

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[9] For a discussion of Bourbaki's theory of sets, see Anacona, Arboleda, & Pérez-Fernández (2014).

tiniest proof at the beginning of the Theory of Sets would already require several hundreds of signs for its complete formalization. Hence, from Book I of this series onwards, it is imperative to condense the formalized text by the introduction of a fairly large number of new words (called *abbreviating symbols*) and additional rules of syntax (called *deductive criteria*). By doing this we obtain languages which are much more manageable than the formalized language in its strict sense. Any mathematician will agree that these condensed languages can be considered as merely shorthand transcriptions of the original formalized language. (Bourbaki, 1970, p. 10)

Bourbaki thus adopts a strategy for abbreviating or condensing formal proofs which is similar to Mac Lane's and which is based on the introduction of *higher-level rules of inference* called *deductive criteria*.

In order to precisely state what *deductive criteria* are, we shall first recall a few technical aspects of the foundational framework developed in the *Théorie des Ensembles* (Bourbaki, 1970). First of all, Bourbaki begins with the definition of a formalized language by introducing an *alphabet*, defined as a set of *signs*, and by considering *assemblies*, which are sequences of signs from the alphabet—what we now call 'formulas'. Among the assemblies that are formed according to a specified set of rules—what we now call 'well-formed formulas'—Bourbaki distinguishes between the *terms*, which represent objects, and the *relations*, which represents assertions (Bourbaki, 1970, p. 20). Bourbaki defines then the notion of a *demonstrative text* or *proof* in a theory $\mathcal{T}$, which follows essentially the definition of a formal proof in a Hilbert proof system (Bourbaki, 1970, p. 25). Bourbaki finally defines the notion of a *theorem* in $\mathcal{T}$ as a relation that appears in a proof in $\mathcal{T}$ (Bourbaki, 1970, p. 25).

We now have all the elements to state precisely what a deductive criterion is for Bourbaki: a deductive criterion is a *rule*, that takes the form of a *schema*, and which states that if such and such relations are *theorems* in a theory $\mathcal{T}$, then another relation is also a *theorem* in $\mathcal{T}$. Two representative examples of deductive criteria are the followings:[10]

> **C1** (Syllogism). Let **A** and **B** be relations in a theory $\mathcal{T}$. If **A** and **A** $\Rightarrow$ **B** are theorems in $\mathcal{T}$, then **B** is a theorem in $\mathcal{T}$. (Bourbaki, 1970, p. 25)

> **C61** (Principle of Induction). Let $R\lfloor n \rfloor$ be a relation in a theory $\mathcal{T}$ (where n is not a constant of $\mathcal{T}$). Suppose that the relation
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> $$R\lfloor 0 \rfloor \text{ and } (\forall n)((n \text{ is an integer and } R\lfloor n \rfloor) \Rightarrow R\lfloor n+1 \rfloor)$$
>
> is a theorem in $\mathcal{T}$. Under these conditions the relation
>
> $$(\forall n)((n \text{ is an integer}) \Rightarrow R\lfloor n \rfloor)$$
>
> is a theorem in $\mathcal{T}$. (Bourbaki, 1970, p. 168)

It is important to notice that a deductive criterion is a *meta-theorem*, and thereby requires a proof in the meta-theory. Such deductive criteria correspond thus to what we call in modern terminology 'derived rules of inference'.

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[10] Bourbaki (1970) introduces in total 63 deductive criteria.

Thus, Bourbaki's solution to abbreviate formal proofs is essentially the same as the one proposed by Mac Lane, for it consists in introducing higher-level rules of inference—the deductive criteria—which allow to abbreviate or condense formal proofs by writing them under the form of lists of such higher-rules of inference together with their arguments. Such a strategy, together with the use of abbreviating symbols and the so-called *abus de langage*, allows to represent mathematical proofs within this formal framework in the way they are commonly presented in ordinary mathematical practice. Furthermore, the meta-mathematical machinery developed by Bourbaki assures that, to every such condensed or abbreviated proof, corresponds a formal proof or demonstrative text as defined in (Bourbaki, 1970, p. 25). These two important points are expressed clearly in the following passages:

> We shall therefore very quickly abandon formalized mathematics, but not before we have carefully traced the path which leads back to it. The first "abuses of language" thus introduced will allow us to write the rest of this series (and in particular the Summary of Results of Book I) in the same way as all mathematical texts are written in practice, that is to say partly in ordinary language and partly in formulae which constitute partial, particular, and incomplete formalizations, the best-known examples of which are the formulae of algebraic calculation. (Bourbaki, 1970, p. 11)

> Thus, written in accordance with the axiomatic method and keeping always in view, as it were on the horizon, the possibility of a complete formalization, our series lays claim to perfect rigour: a claim which is not in the least contradicted by the preceding considerations, nor by the need to correct errors which slip into the text from time to time. (Bourbaki, 1970, p. 12)

It is now easy to understand the formulation of the standard view as expressed in the Bourbaki's quote reported in the introduction. From Bourbaki's perspective, in order for the mathematician to evaluate the correctness or rigor of a mathematical proof, it suffices for him to verify that each mathematical inference in the mathematical proof corresponds to a legitimate application of a higher-level rule of inference—i.e., a deductive criteria. The meta-mathematical machinery developed in the *Théorie des Ensembles* allows then to give a precise sense to the idea that the mathematician "is content to bring the exposition to a point where his experience and mathematical flair tell him that translation into formal language would be no more than an exercise of patience (though doubtless a very tedious one)" (Bourbaki, 1970, p. 8): insofar as the validity of each higher-level rules of inference is established through a meta-mathematical argument assuring that such rules preserve the notion of 'theoremhood' as defined by Bourbaki, one is ensured that if an ordinary mathematical proof can be written under the form of a list of application of higher-rules of inference together with their arguments, then there necessarily exists a formal proof corresponding to it which can be obtained by replacing each such application by the sequence of inferences it abbreviates. Producing such a formal proof is a task that is, however, beyond the reach of any human being.

2.3. Wrapping up. Although Mac Lane and Bourbaki offered the first formulation of the standard view, their primary objectives were not to provide a characterization of mathematical rigor as a quality of mathematical proof, nor did they have the intention to

promote a direct use of formal proofs in ordinary mathematical practice. Their respective goals lay elsewhere: Mac Lane aimed to develop a richer analysis of the structure of mathematical proofs within the field of mathematical logic, while Bourbaki aimed to secure the foundations of his mathematical treatise by developing a meta-mathematical machinery allowing to reach the highest level of rigor practically attainable by maintaining a certain epistemological relation between the standards of formal proof and the way proofs are written in the treatise.

For these reasons, and as we have just seen, the standard view as expressed by Mac Lane and Bourbaki is better conceived as a *consequence* of their respective enterprises. More specifically, the view follows from two central tenets common to the general approaches adopted by Mac Lane and Bourbaki towards their respective goals, namely:

1. Judgments of the validity of mathematical inferences in mathematical practice can be conceived as relying on higher-level rules of inference that are generated from lower-level rules of inference and propositions from background knowledge;

2. These higher-level rules of inference can ultimately be generated from the set of rules of inference and axioms of a formal deductive system adequate to serve as the foundations of mathematics.

The connection with mathematical practice is then to be found in the first tenet, which contains a view on how mathematical inferences in mathematical proofs are judged to be valid in mathematical practice, and which thereby presupposes a certain descriptive account of mathematical rigor. Although all the elements of such an account are present in substance in the work of Mac Lane and Bourbaki, those elements are embedded in technical developments, making it hard to understand why Mac Lane and Bourbaki see in their works a descriptive account of mathematical rigor, and *a fortiori* to identify what this account could consist in.

As we mentioned in the beginning, our first step in providing a precise formulation of the standard view will be to specify the descriptive part of the standard view. This task amounts then to reconstructing the descriptive account of mathematical rigor potentially present in the work of Mac Lane and Bourbaki. Before we can do so, however, it will be useful to reflect on what exactly is to be expected of a descriptive account of mathematical rigor.

§3. Preliminaries: How to formulate a descriptive account of mathematical rigor.
A descriptive account of mathematical rigor shall provide a characterization of the process by which mathematical proofs are judged to be rigorous in mathematical practice, i.e., by which the quality of being rigorous is attributed to mathematical proofs in mathematical practice. We shall refer to this process as *verification*, and say that a mathematical proof has been *verified* whenever it has successful undergone this verification process. Any descriptive account of mathematical rigor shall then take the form of the following schema:

A mathematical proof P is rigorous$_\mathcal{M}$
$$\Leftrightarrow$$
P can be verified by a typical agent in mathematical practice $\mathcal{M}$, using the resources commonly available to the agents engaged in $\mathcal{M}$.

Since a mathematical proof is a composite entity consisting of a sequence of elementary steps of deduction—as mentioned in the previous section, we shall refer to these elementary steps as *mathematical inferences*—verifying a mathematical proof amounts to

verifying all the mathematical inferences that comprise it. The previous schema becomes then:

A mathematical proof P is rigorous$_\mathcal{M}$

$\Leftrightarrow$

Every mathematical inference I in P can be verified by a typical agent in mathematical practice $\mathcal{M}$, using the resources commonly available to the agents engaged in $\mathcal{M}$.[11]

From this perspective, providing a characterization of mathematical rigor amounts to identifying the process by which mathematical inferences are *verified*—i.e., judged to be *valid*—in mathematical practice. At this stage, we can refine further the above schema by observing that, when faced with the task of verifying a mathematical inference in a mathematical proof, a typical agent is often led to introduce intermediate steps of deduction between the premises and the conclusion. This is a very common and banal observation, one which is for instance described by Yehuda Rav in the following passage:

> In reading a paper or monograph it often happens—as everyone knows too well—that one arrives at an impasse, not seeing why a certain claim *B* is to follow from claim *A*, as its author affirms. [. . .] Thus, in trying to understand the author's claim, one picks up paper and pencil and tries to fill in the gaps. After some reflection on the background theory, the meaning of the terms and using one's general knowledge of the topic, including eventually some symbol manipulation, on sees a path from *A* to A_1, from A_1 to A_2, . . . , and finally from A_n to *B*. (Rav, 1999, p. 14)

To integrate this aspect, we introduce the notion of *immediate* mathematical inference: a mathematical inference is *im*mediate for a given agent if she can evaluate it as valid without introducing *inter*mediate steps of deduction. This suggests decomposing the process of verifying a mathematical inference into two phases: the first phase consisting in *decomposing* the mathematical inference into a sequence of immediate mathematical inferences; the second phase consisting in *verifying* each immediate mathematical inference in the sequence.[12] With respect to a mathematical practice $\mathcal{M}$, if we denote by $\mathcal{D}_\mathcal{M}$ the set of processes available to the agent to decompose a mathematical inference into a sequence of immediate mathematical inferences, and by $\mathcal{V}_\mathcal{M}$ the set of processes available to the agent to evaluate immediate mathematical inferences, we obtain the following schema:

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[11] It is assumed here that all the premisses involved in the mathematical inferences of P are *legitimate*, that is, they are either conclusions of previous inferences, mathematical propositions from background knowledge, or assumptions to be discharged later on in P. If a premiss does not fall into one of these three categories, then it should be considered as the conclusion of a mathematical inference.

[12] It should be noted that, in practice, an additional process is usually preceding these two phases in the verification of a mathematical inference, which consists in identifying the *premisses* of the considered mathematical inference. This process is necessary insofar as in written mathematical proofs, premisses of mathematical inferences are sometimes left *implicit*, in which case it is left to the agent to recover them. Although this process of premiss identification is essential to the verification of mathematical inferences in mathematical proofs, we shall not attempt to analyze it further since it is not directly connected to the issues we are primarily concerned with. Throughout this paper, we shall thus assume that, whenever an agent is engaging into the verification of a mathematical inference, she has previously identified all its relevant premisses.

A mathematical proof P is rigorous$_{\mathcal{M}}$

⇔

For every mathematical inference I in P, there exist[13] $D \in \mathcal{D}_{\mathcal{M}}$ and $V_1, \ldots, V_n \in \mathcal{V}_{\mathcal{M}}$ such that (1) $D(I) = \langle I_1, \ldots, I_n \rangle$ and (2) $V_i(I_i) = \mathsf{valid}$ for all $i \in [\![1, n]\!]$.

We shall refer to this schema as the *DV schema*, and to $\mathcal{D}_{\mathcal{M}}$ and $\mathcal{V}_{\mathcal{M}}$ as the sets of *decomposition* and *verification processes*.

It is my contention that any descriptive account of mathematical rigor shall take the form of a specification of the DV schema, i.e., of a specification of the sets of decomposition and verification processes. In order to specify the descriptive part of the standard view, we shall, in the next section, specify the DV schema associated to it, i.e., identify the sets of decomposition and verification processes associated to the standard view.

§4. The standard view of mathematical rigor: Descriptive part. We are now in a position to specify the descriptive part of the standard view, i.e., to provide a precise formulation of the descriptive account of mathematical rigor embedded in the standard view—in the terminology introduced at the beginning, this amounts to characterizing what it means for a mathematical proof to be rigorous$_D$. As we have just seen, any descriptive account of mathematical rigor shall take the form of a specification of the DV schema. To specify the descriptive part of the standard view amounts then to specifying the DV schema associated to it, a schema that takes the following form:

A mathematical proof P is rigorous$_D$

⇔

For every mathematical inference I in P, there exist $D \in \mathcal{D}^{\star}$ and $V_1, \ldots, V_n \in \mathcal{V}^{\star}$ such that (1) $D(I) = \langle I_1, \ldots, I_n \rangle$ and (2) $V_i(I_i) = \mathsf{valid}$ for all $i \in [\![1, n]\!]$.[14]

where $\mathcal{D}^{\star}$ and $\mathcal{V}^{\star}$ correspond to the sets of decomposition processes and verification processes associated to the standard view. As we noted in §2, although Mac Lane and Bourbaki seem to see in their works a descriptive account of mathematical rigor, this account is nowhere made explicit as such. Our task in this section will be to reconstruct this account, by specifying the sets of processes $\mathcal{D}^{\star}$ and $\mathcal{V}^{\star}$, and this based on the core elements of the standard view as originally conceived by Mac Lane and Bourbaki.

4.1. The set of decomposition processes $\mathcal{D}^{\star}$. A decomposition process is called for whenever a mathematical agent encounters a mathematical inference in a mathematical proof that she cannot judge to be valid without introducing intermediate steps of deduction between the premises and the conclusion. As an illustration of this phenomenon, consider, for instance, the following mathematical proof of the irrationality of $\sqrt{2}$ taken from the fourth edition of Hardy and Wright's *An Introduction to the Theory of Numbers* (Hardy & Wright, 1975, pp. 39–40):

THEOREM 4.1 (Pythagoras' Theorem). $\sqrt{2}$ *is irrational.*

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[13] There is here a computational content in the phrase 'there exist', for we shall assume that, if there exist such $D \in \mathcal{D}_{\mathcal{M}}$ and $V_1, \ldots, V_n \in \mathcal{V}_{\mathcal{M}}$, a typical agent engaged in $\mathcal{M}$ should be able to identify them.

[14] For readability reasons, we will omit from now on references to the considered mathematical practice $\mathcal{M}$. One should nonetheless keep in mind that the sets $\mathcal{D}^{\star}$ and $\mathcal{V}^{\star}$, as well as the quality of being rigorous$_D$, are always relative to a given mathematical practice $\mathcal{M}$.

Proof. The traditional proof ascribed to Pythagoras runs as follows. If $\sqrt{2}$ is rational, then the equation

$$a^2 = 2b^2$$

is soluble in integers a, b with $(a, b) = 1$. Hence a^2 is even, and therefore a is even. If $a = 2c$, then $4c^2 = 2b^2$, $2c^2 = b^2$, and b is also even, contrary to the hypothesis that $(a, b) = 1$. $\square$

For the beginning college student in number theory following Hardy and Wright's book, many mathematical inferences in this proof will appear immediate, insofar as they concern elementary properties of the natural numbers which are normally already known from high-school mathematics. However, one mathematical inference might not appear so immediate, namely the one with premiss "a^2 is even" and conclusion "a is even". In this case, the student will engage into a decomposition process in order to introduce intermediate steps of deduction between the premiss and the conclusion, that is, a sequence of immediate mathematical inferences which will allow her to verify that the conclusion "a is even" indeed follows from the premiss "a^2 is even". As we already saw, this phenomenon of 'filling in the details' is almost always present when a mathematical agent is verifying a mathematical proof.

What is the nature of these decomposition processes? First of all, notice that we can represent any mathematical inference in a mathematical proof in the following way:

$$P_1, \ldots, P_n \Rightarrow C$$

where $P_1, \ldots, P_n$ are the premisses of the inference, and C its conclusion. As remarked by Avigad (2008, p. 333), whenever a mathematical agent cannot verify immediately a mathematical inference of the form $P_1, \ldots, P_n \Rightarrow C$, she is facing a situation identical to the one of proving the mathematical proposition "if $P_1, \ldots, P_n$, then C". In the above example, the student not able to verify the mathematical inference with premiss "a^2 is even" and conclusion "a is even" is then facing the task of proving the mathematical proposition "if a^2 is even, then a is even". It follows that the decomposition process required to turn the mathematical inference $P_1, \ldots, P_n \Rightarrow C$ into a sequence of immediate mathematical inferences is identical to the proof search process required to prove the mathematical proposition "if $P_1, \ldots, P_n$, then C". Decomposition processes are therefore *proof search processes*.

There are, however, restrictions on which proof search processes can be admitted as decomposition processes. Such restrictions are necessary to avoid that mathematical proofs that are patently underdeveloped be counted as rigorous by our characterization—e.g., if a certain mathematical inference in a mathematical proof corresponds to the application of a lemma that would take a few days to prove by a typical mathematical agent, we would not want this mathematical proof to qualify as rigorous. These restrictions correspond to the conditions under which it is considered admissible to leave what Fallis (2003) has called *enthymematic gaps* in written mathematical proofs. According to Fallis, the main reason why mathematicians leave enthymematic gaps in written mathematical proofs is to facilitate communication:

> The point of publishing a proof in a journal or presenting it at a conference
> is to communicate that proof to other mathematicians. [...] Somewhat
> surprisingly, the most efficient way for the mathematician to do this is not
> by laying out the entire sequence of propositions in excruciating detail.
> Instead, the mathematician just tries to include "sufficient information

so that the informed reader (or hearer) could reconstruct a perfect proof
from the enthymeme" (Lehman, 1980, p. 35). [...] His readers can simply
"fill in the missing assumptions from the common store of background
knowledge" (Lehman, 1980, p. 36). (Fallis, 2003, p. 55)[15]

Based on the constraints for leaving enthymematic gaps in written mathematical proofs, we
can identify two conditions for a proof search process to count as a decomposition process.
First, the proof search process should be part of the common background knowledge of
the mathematical agents engaged in the considered mathematical practice, so that the
agent leaving an enthymematic gap in a mathematical proof is assured that the gap can
be filled in by her peers. Second, the proof search process should be susceptible to fill
in the enthymematic gap in a 'reasonable amount of time', otherwise the mathematical
proof would contain an inadmissible gap that should be eliminated by providing additional
intermediate steps of deduction, in which case the proof should not be counted as rigorous.
This leads to the following specification of the set of decomposition processes $\mathcal{D}^\star$:

> The set of decomposition processes $\mathcal{D}^\star$ is given by the set of proof search
> processes which are (1) part of the common background knowledge of
> the agents engaged in mathematical practice $\mathcal{M}$ and (2) susceptible to
> prove mathematical propositions in a reasonable amount of time.

It should be noted that this specification of $\mathcal{D}^\star$ is independent of the specifics of the
standard view, and is very likely to be part of any characterization of rigor as a quality
of mathematical proofs. The heart of the standard view is rather to be found in the set of
verification processes $\mathcal{V}^\star$ that we now turn to.

4.2. The set of verification processes $\mathcal{V}^\star$.   On pain of infinite regress, the process of
decomposition that the agent is engaged in while evaluating the validity of a mathematical
inference must stop at some point. This happens precisely when the agent reaches *imme-
diate* mathematical inferences, that is, inferences that can be judged to be valid without
decomposing them into further mathematical inferences. We shall now say how immediate
mathematical inferences are judged to be valid according to the standard view, that is, we
shall specify the set of verification processes $\mathcal{V}^\star$.

As we saw in §2, the solution put forward by Mac Lane and Bourbaki rests on the
introduction of *higher-level rules of inference* (henceforth, *hl-rules*). In our reconstruction,
a hl-rule is entirely determined by its *inference schema*, which is a pair composed of a
premiss schema and a *conclusion schema* consisting, respectively, of a set of schemas for
the premisses and a schema for the conclusion. Here, a schema is a template or pattern
composed of placeholders and of symbols from the vocabulary of the language of the
mathematical practice $\mathcal{M}$, together with some specifications on how the placeholders are
to be filled in to generate mathematical propositions in the language of $\mathcal{M}$, propositions
which are then called *instances* of the schema.[16] As an illustration, the inference schema

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[15] A similar statement is made by Bourbaki: "Sometimes we shall use ordinary language more loosely,
by voluntary abuses of language, by the pure and simple omission of passages which the reader
can safely be assumed to be able to restore easily for himself, and by indications which cannot be
translated into formalized language and which are designed to help the reader to reconstruct the
complete text" (Bourbaki, 1970, p. 11).

[16] More generally, Corcoran (2014) defines a schema as consisting of two things: (1) a *template-text*
or *schema-template* which is "a syntactic string composed of significant words and/or symbols
and also of blanks or other placeholders", and (2) a *side condition* which specifies "how the blanks

for modus ponens is given by:

$$P, \ P \to Q \ \Rightarrow \ Q,$$

where P and Q are placeholders for mathematical propositions, while the inference schema for mathematical induction can be given by:[17]

$$H(0), \ H(X) \to H(X+1) \ \Rightarrow \ H(Y),$$

where H is a placeholder for an expression involving an arbitrary variable ranging over $\mathbb{N}$, and X and Y are placeholders for arbitrary variables ranging over $\mathbb{N}$. We shall then say that an *immediate* mathematical inference is *valid* whenever it corresponds to an *instance* of a *hl-rule*. This means that to each hl-rule R is associated a verification process V_R defined by:[18]

$$V_R(I) = \mathsf{valid} \ \Leftrightarrow \ I \text{ is an instance of the hl-rule } R.$$

The set of verification processes $\mathcal{V}^\star$ associated to the standard view is thus composed of all the verification processes associated to the hl-rules that the typical agent engaged in mathematical practice $\mathcal{M}$ possesses.

Characterizing the set $\mathcal{V}^\star$ amounts then to specifying the set of hl-rules that a typical agent engaged in $\mathcal{M}$ possesses. To this end, our proposal is to characterize $\mathcal{V}^\star$ as the set of hl-rules that a typical agent in $\mathcal{M}$ has acquired in the course of the common training she received in order to qualify as a proper member of $\mathcal{M}$.[19] Our approach will then consist in providing a simple, idealized model of such a training—that we shall refer to as the *training model*—and in characterizing the set of verification processes $\mathcal{V}^\star$ as the set of hl-rules the agent possesses once her training has been completed.

In the training model, we shall represent the situation of the agent at time t of her training by the pair (K_t, R_t) where K_t is the set of *mathematical propositions* representing the mathematical knowledge that the agent possesses at time t, and R_t is the set of *hl-rules* that the agent possesses at time t. The initial situation of the agent—at the beginning of her training—is represented by the pair (K_0, R_0), while the final situation of the agent—once her training has been completed—is represented by the pair (K_T, R_T), the set of verification processes $\mathcal{V}^\star$ being then given by the set of hl-rules R_T. In order to complete the description of the training model, we now need to specify (1) the initial situation (K_0, R_0), and (2) the processes by which K_t and R_t can be augmented, i.e., how the agent passes from (K_t, R_t) to (K_{t+1}, R_{t+1}).

The initial situation (K_0, R_0) corresponds to the ordinary situation that any mathematical student finds herself at the beginning of her training in mathematical practice $\mathcal{M}$. K_0 is the set of mathematical propositions that the agent is accepting without proof at the beginning of her training. To figure out what K_0 is for a given mathematical practice, it suffices to identify the various mathematical propositions that the student is required to accept without proof, a task that can be carried out concretely by simply looking at some of the

(placeholders, variables or ellipses) are to be filled to obtain instances". Notice that our notion of inference schema corresponds exactly to what Corcoran (2014) calls an *argument-text schema*.

[17] There are different ways one could model the hl-rule corresponding to mathematical induction in mathematical practice. One could, for instance, add universal quantifiers for the second premiss, or for the conclusion, or both.

[18] In the following, we shall often identify a hl-rule with its associated verification process, and talk freely of hl-rules as verification processes.

[19] On the importance of taking into account the mathematical training of the agents when analyzing how proofs are judged to be rigorous in mathematical practice, see also Tatton-Brown (2019).

typical textbooks in the considered mathematical practice. For instance, the mathematical student taking an introductory course in number theory at the university level is typically required to accept without proof some informal versions of the Peano axioms, some basic propositions of naive set theory, and maybe various elementary properties of the natural numbers known from elementary school and high-school mathematics. Sometimes, one witnesses some variations with respect to the set of propositions that the student is required to accept at the outset. A typical example is given by trainings in mathematical analysis, where some textbooks might require the student to accept without proof all the elementary properties of $\mathbb{N}$, $\mathbb{Q}$, $\mathbb{R}$, and $\mathbb{C}$, while others might only require to accept the Peano axioms, and establish all such elementary properties through proofs (e.g., Landau, 1930). Modulo such variations, it is, nevertheless, relatively easy to identify the mathematical propositions that are accepted without proof in a typical training in mathematical practice $\mathcal{M}$, and this is what the set of mathematical propositions K_0 represents. We shall refer to K_0 as the sets of *primitive axioms* of the agent.

R_0 is the set of rules of inference that the agent is equipped with at the beginning of her training. Usually, the set of rules of inference R_0 that the agent is allowed to use from the start of her training is not made explicit in the course of a mathematical training, but is rather considered to be a form of know-how that the learning agent is supposed to grasp by observing and mimicking her trainer's proof practice, and by practicing it herself through exercises that are in turn criticized and corrected by the trainer. Some textbook authors, however, do take specific care of providing an explicit training in the practice of mathematical proofs. For instance, Rosen (2012) dedicates a whole chapter of his book to teach the basics of mathematical proofs, while other have written entire books aiming to teach specifically the writing and reading of mathematical proofs (see, e.g., Velleman, 2006; Solow, 2014; Chartrand et al., 2018). It is not hard, however, to identify the rules of inference that an agent is required to accept at the beginning of her training for those are essentially basic rules of elementary logical reasoning necessary to reason with mathematical propositions, that is, rules of inference for reasoning with the various propositional connectives, as well as rules of inference for reasoning with quantified mathematical propositions, together with various combinations of those. We shall refer to R_0 as the sets of *primitive rules of inference* of the agent.

We shall now say how the set of mathematical propositions K_t that the agent possesses at time t can be augmented. This is straightforward:

> Whenever an agent in situation (K_t, R_t) at time t has derived a mathematical proposition C from a set of mathematical propositions $P_1, \ldots, P_n \in K_t$ through a sequence of applications of hl-rules from R_t, and by eventually using additional mathematical propositions from K_t, she is entitled to add C to her set of mathematical propositions K_t.

If the agent chooses to do so, she is then brought in a situation at time $t + 1$ in which $K_{t+1} := K_t \cup \{C\}$. We shall then say that the agent has acquired a *proof certificate* for C. Furthermore, the agent can always add a *definition* to the set K_t at any time t.

Finally, it remains to say how the set of hl-rules R_t that the agent possesses at time t can be augmented. Mac Lane (1935) has a simple answer to this issue, which is expressed in the following passage:

> In general, whenever a group of elementary processes of proof occurs repeatedly in the course of many proofs, it is desirable to formulate this group of steps once for all as a new process. Much of the ordinary

> education in mathematics consists in training students to recognize such processes at a glance, and as whole, rather than as composite. (Mac Lane, 1935, p. 123)

In the terminology introduced in this section, Mac Lane's solution of how a new hl-rule can be added to R_t at time t can be formulated as follows:

> Whenever an agent in situation (K_t, R_t) at time t has derived a mathematical proposition C from a set of mathematical propositions $P_1, \ldots, P_n$ through a sequence of applications of hl-rules from R_t, and by eventually using additional mathematical propositions from K_t, she is entitled to add to her set of hl-rules R_t the new rule:
>
> $$P_1, \ldots, P_n \Rightarrow C$$
>
> where $P_1, \ldots, P_n, C$ correspond to the mathematical propositions $P_1, \ldots, P_n, C$ in which the free variables $x_1, \ldots, x_k$ occurring in them are replaced by the place-holders $X_1, \ldots, X_k$ of the same type.

If the agent chooses to do so, she is then brought in a situation at time $t + 1$ in which $R_{t+1} := R_t \cup \{P_1, \ldots, P_n \Rightarrow C\}$. We shall then say that the agent has acquired a *rule certificate* for the new rule $P_1, \ldots, P_n \Rightarrow C$.

It is interesting to observe that, through this process, many theorems and definitions can easily be turned into hl-rules.[20] As an illustration, consider again the mathematical inference with premiss "a^2 is even" and conclusion "a is even" from Hardy and Wright's proof, and imagine now that the authors would have established the following lemma prior to presenting the proof of the irrationality of $\sqrt{2}$:

$$\forall n \, (n^2 \text{ is even} \to n \text{ is even}) \quad (L)$$

If the agent at time t is such that $L \in K_t$, then she can turn L into a hl-rule by first deriving the conclusion "x is even" from the premiss "x^2 is even" in the following way:

P	x^2 is even			
I_1	x^2 is even $\to$ x is even	$\forall$-elimination	*from*	L
C	x is even	modus ponens	*from*	I_1

and then adding the following hl-rule to R_t:

$$X^2 \text{ is even} \Rightarrow X \text{ is even}$$

where X is a placeholder for an expression denoting a natural number. If the agent would possess the above hl-rule, she will then be in a situation to recognize the mathematical inference with premiss "a^2 is even" and conclusion "a is even" as immediately valid, for this mathematical inference is an instance of the above rule.

Similarly, a definition such as the following definition of even number:

$$\forall n \, (n \text{ is even} \leftrightarrow \exists k \text{ such that } n = 2k) \quad (D)$$

can be turned into a hl-rule by first deriving the conclusion "$x = 2y$" from "x is even" in the following way:

---

[20] For a recent technical implementation of this idea, see the deductive system proposed by Sieg & Walsh (2018) in their 'natural formalization' of the Cantor-Bernstein Theorem (Sieg & Walsh, 2018, sec. 3).

P	x is even			
I_1	x is even $\rightarrow \exists k$ such that $x = 2k$	$\forall$-elimination	*from*	D
I_2	$\exists k$ such that $x = 2k$	modus ponens	*from*	P and I_1
C	$x = 2y$	$\exists$-elimination	*from*	I_2

and then adding the following hl-rule to R_t:

$$X \text{ is even} \implies X = 2Y$$

where X and Y are placeholders for expressions denoting natural numbers. If the agent would possess the above rule, she will then be in a situation to recognize as immediately valid the mathematical inference with premiss "a is even" and conclusion "$a = 2c$".

To sum up, the set of verification processes $\mathcal{V}^\star$ is given by the set of hl-rules that a typical agent in $\mathcal{M}$ has acquired in the course of her training in $\mathcal{M}$. The training model just developed provides then the necessary elements to entirely characterize the set of verification processes $\mathcal{V}^\star$.

4.3. Concluding remarks. By providing a full specification of the sets of processes $\mathcal{D}^\star$ and $\mathcal{V}^\star$, we have specified entirely the DV schema associated to the standard view, and we have thereby reached a precise formulation of the descriptive part of the standard view. It is important to stress that the descriptive account of mathematical rigor embedded in the standard view does not appeal *either* to the notion of formal proof, *or* to the one of routine translation. As a consequence, one can perfectly endorse this descriptive account, without endorsing the standard view, i.e., without endorsing either the normative part of the standard view, or the conformity thesis. Furthermore, the specifications of the sets of processes $\mathcal{D}^\star$ and $\mathcal{V}^\star$ being themselves independent from each other, one can accept either of these two components while rejecting the other. This means that this descriptive account of mathematical rigor can thus be considered *by itself*, independently from its role and presence in the standard view of mathematical rigor, and is as such of independent value and interest.

§5. **The standard view of mathematical rigor: Normative part.** We shall now specify the normative part of the standard view, i.e., characterize what it means for a mathematical proof to be rigorous$_N$. As stated in the introduction:

A mathematical proof P is rigorous$_N$
$$\Leftrightarrow$$
P can be *routinely translated* into a formal proof.

Our main task in this section will be to specify this characterization by providing a precise conception of the notion of *routine translation* central to it. This raises two main questions: How does the *translation* proceed? How should the term *routine* be interpreted?

Regarding the first question, our proposal is to think of the routine translation as a sequence of successive translations between proofs at different levels of granularity. More specifically, we will consider *four levels of granularity*, and will conceive of the routine translation as a sequence of *three successive translations* from the coarsest to the finest level of granularity. Regarding the second question, we shall interpret the term 'routine' as being equivalent to the term 'algorithmic' (or 'mechanical', 'automatic'), which is, I contend, the intended meaning of the term in Mac Lane's and Bourbaki's original conceptions of the standard view. Thus, we shall conceive of the routine translation as an algorithmic procedure that takes as input an ordinary mathematical proof and turns it into a full formal

proof, and we shall specify it by providing the algorithmic procedures corresponding to the three successive translations composing it.

5.1. Four levels of granularity.   The routine translation takes as input mathematical proofs as commonly presented in the ordinary mathematical texts of a given mathematical practice. This is the coarsest level of granularity we shall consider, and we shall refer to it as the *vernacular level*:

> *Vernacular level*: A *vernacular-level* proof P is a sequence of inferences as commonly presented in the ordinary mathematical texts of mathematical practice $\mathcal{M}$.

As we saw in §3, the inferences of a vernacular-level proof cannot always be directly verified by a typical agent in a given mathematical practice, in which case the agent enters into some decomposition processes in order to turn every such inference in the proof into a sequence of *immediate* inferences. The second level of granularity we shall consider is the one at which a proof is only composed of *immediate* inferences, and every mathematical proposition used as a premiss is either the conclusion of a previous inference, a mathematical proposition from background knowledge, or an assumption to be discharged later on, a level of granularity we shall refer to as the *higher level*:

> *Higher level*: A *higher-level* proof P_{hl} is a sequence of inferences such that (1) every inference in P_{hl} is an instance of an hl-rule in $\mathrm{R_T}$, and (2) every mathematical proposition occurring as a premiss of an inference in P_{hl} is either the conclusion of a previous inference in P_{hl}, a mathematical proposition from $\mathrm{K_T}$, or an assumption to be discharged later on in P_{hl}.[21]

In the previous section, we characterized immediate inferences as corresponding to instances of higher-level rules of inference, and we explained in the training model how higher-level rules of inference can be generated from primitive rules of inference and axioms. The third level of granularity is the one at which a proof is only composed of inferences licensed by primitive rules of inference, and every mathematical proposition used as a premiss is either the conclusion of a previous inference, a primitive axiom, or an assumption to be discharged later on, a level of granularity we shall refer to as the *intermediate level*:

> *Intermediate level*: An *intermediate-level* proof P_{il} is a sequence of inferences such that (1) every inference in P_{il} is an instance of a primitive rule of inference in $\mathrm{R_0}$, and (2) every mathematical proposition occurring as a premiss of an inference in P_{il} is either the conclusion of a previous inference in P_{il}, a primitive axiom from $\mathrm{K_0}$, or an assumption to be discharged later on in P_{il}.[22]

Finally, the last and finest level of granularity we shall consider is the one of formal proof—which is the level of granularity at which proofs are yield by the routine translation—a level we shall refer to as the *lower level*:

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[21] Recall that $\mathrm{K_T}$ and $\mathrm{R_T}$ refer respectively to the set of mathematical propositions that the agent knows and the set of hl-rules that the agent possesses once she has completed her training in the considered mathematical practice.

[22] Recall that $\mathrm{K_0}$ and $\mathrm{R_0}$ refer respectively to the set of mathematical propositions that the agent knows and the set of hl-rules that the agent possesses at the very beginning of her training in the considered mathematical practice.

> *Lower level*: A *lower-level* proof $P_{\parallel}$ is a sequence of inferences such that (1) every in-
> ference in $P_{\parallel}$ is an instance of a rule of inference in R_{Γ}, and (2) every mathematical
> proposition occurring as a premiss of an inference in $P_{\parallel}$ is either the conclusion of
> a previous inference in $P_{\parallel}$, an axiom from K_{Γ}, or an assumption to be discharged
> later on in $P_{\parallel}$.

Here Γ designates a formal deductive system adequate to serve as the foundations of
mathematics, and R_{Γ} and K_{Γ} designate respectively the rules of inference and axioms
of Γ.

We shall then conceive of the routine translation as an algorithmic procedure that takes
as input a proof at the vernacular level, and yields as output a translation of it at the lower
level, and which consists in a sequence of *three successive translations*: first from the
vernacular level to the higher level, then from the higher level to the intermediate level,
and finally from the intermediate level to the lower level. We now turn to the specifications
of the three algorithmic procedures corresponding to these three translations.

5.2. Three successive translations. The first translation, from the vernacular level
to the higher level, corresponds exactly to the first phase of the process that a typical
mathematical agent engages in when judging the rigor of a mathematical proof, namely the
decomposition of each inference in the proof that cannot be verified directly into a sequence
of immediate mathematical inferences. The algorithmic procedure $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ corresponding to
this translation consists thus, for each such inference, in first identifying a decomposition
process that would turn the inference into a sequence of immediate inferences, then carry-
ing out this decomposition process, and finally replacing the inference in the proof by the
outcome of the decomposition process:

> *Algorithmic procedure $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$*: For each inference I in P which is not immediate, the
> algorithmic procedure $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ proceeds in three steps:
>
> 1. It identifies a decomposition process $D \in \mathcal{D}^{\star}$ such that (1) $D(I) = \langle I_1, \ldots, I_n \rangle$ and (2) there exist $V_1, \ldots, V_n \in \mathcal{V}^{\star}$ such that $V_i(I_i) = \mathsf{valid}$ for all $i \in [\![1, n]\!]$,
> 2. It decomposes I into the sequence of inferences $\langle I_1, \ldots, I_n \rangle$ using the decom-
>   position process D,
> 3. It replaces I in P by the sequence of inferences $\langle I_1, \ldots, I_n \rangle$.

The second translation, from the higher level to the intermediate level, exploits the
way hl-rules in R_T and mathematical propositions in K_T are generated in the training
model from primitive rules of inference in R_0 and primitive axioms in K_0. The algorith-
mic procedure $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ corresponding to this second translation consists then in keeping
unpacking the hl-rules and the mathematical propositions in the proof into the more basic
components they were built from, and this up to the point where only remain primitive
rules of inference, primitive axioms, and assumptions to be discharged, a procedure that
can be described as follows:

> *Algorithmic procedure $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$*: The algorithmic procedure $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ proceeds in two
> steps:
>
> 1. It keeps replacing each application of a hl-rule in P_{hl} by the sequence of ap-
>   plications of hl-rules in its rule certificate, and each mathematical proposition
>   in P_{hl} by the sequence of applications of hl-rules in its proof certificate, until
>   all inferences in P_{hl} are instances of primitive rules of inference from R_0 and

all mathematical propositions occurring as premisses of inferences in P_{hl} are either definitions, conclusions of previous inferences, primitive axioms from K_0, or assumptions to be discharged,

2. It keeps replacing each occurrence of a defined expression in the mathematical propositions of P_{hl} by its definition, until all mathematical propositions in P_{hl} only contain primitive expressions from the language of K_0, and then withdraw from P_{hl} all mathematical propositions that correspond to definitions.

The third translation, from the intermediate level to the lower level, is there to bridge the gap between the primitive rules of inference and primitive axioms, and the rules of inference and axioms of the formal deductive system Γ. The algorithmic procedure $\mathcal{T}_{il \to ll}$ corresponding to this third translation can be described as follows:

Algorithmic procedure $\mathcal{T}_{il \to ll}$: The algorithmic procedure $\mathcal{T}_{il \to ll}$ proceeds in three steps:

1. It replaces each mathematical proposition in P_{il} by its translation into the formal language of Γ where each primitive expression is replaced by its definition in Γ,

2. It replaces each occurrence in P_{il} of a primitive axiom from K_0 by a proof of it in Γ,

3. It replaces each application in P_{il} of a primitive rule of inference from R_0 by a corresponding sequence of applications of rules of inference in Γ.

Do we have any reason to believe that such an algorithmic procedure could exist? Regarding step 1, although few attempts have been made to provide an explicit algorithmic procedure for translating mathematical propositions in the vernacular mathematical language into formulas of a formal language, there does not seem to be any obstacle present here that could prevent to do so since, as Wiedijk (2008) remarks: "Writing text in a stylized formal language is easy" (Wiedijk, 2008, p. 1414).[23] Regarding step 2, that any primitive axiom from K_0—i.e., any mathematical proposition accepted without proof in the various branches of contemporary mathematics—can be represented and proved within a formal deductive system adequate to serve as the foundations of mathematics is something that is known at least since the beginning of the twentieth century, as Hilbert wrote in 1920 with respect to Zermelo's axiom system for set theory:

> The theory which results from the development of the consequences of this axiom system encompasses all mathematical theories (like number theory, analysis, geometry), in the sense that the relations which obtain between the objects of these mathematical disciplines are represented in a perfectly corresponding way by relations which obtain within a subdomain of Zermelo's set theory. (Hilbert, 2013, p. 292)

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[23] According to Wiedijk (2008), it is for this reason that "it is *not* important to have proof assistants be able to process existing mathematical texts" (Wiedijk, 2008, p. 1414). This might explain why few efforts have been invested to develop technologies in order to translate vernacular mathematical language into formal ones, the cost of doing so compared to the potential low gain that could result from it might simply not be worth the effort.

Regarding step 3, given that the set of primitive rules of inference R_0 amounts to basic rules of elementary logical reasoning, it ought to be possible to translate any application of these rules into corresponding sequences of applications of rules of inference in R_Γ, involving eventually further formulas that could be derived in Γ.

But the best reason we have to believe that such an algorithmic procedure as $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ could exist is that, in fact, procedures of this kind *do* exist. More specifically, the main proof assistants currently used in the field of formal verification possess the necessary resources to convert any intermediate-level proof P_{il}—where each mathematical proposition in P_{il} has been replaced by its translation into the formal language of the considered proof assistant— into a lower-level proof P_{ll} within the formal deductive system they are built on, as Avigad notices:

> To date, a substantial body of definitions and theorems from undergraduate mathematics has been formalized, and there are good libraries for elementary number theory, real and complex analysis, point-set topology, measure-theoretic probability, abstract algebra, Galois theory, and so on. (Avigad, 2018, p. 685)

Of course, one will need to supplement any intermediate-level proof P_{il} with further instructions to obtain a proof script that can be verified by such proof assistants. But given that all that is required for steps 2 and 3 of $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ is the ability to replace any primitive axiom from K_0 by a proof of it in Γ and any application of a primitive rule of inference in R_0 into a corresponding sequence of applications of rules of inference in Γ, it suffices to provide once and for all a proof script that can verify any primitive axiom from K_0 and that can turn any primitive rule of inference in R_0 into a rule of the considered system, in order to obtain an algorithmic procedure able to carry out the steps 2 and 3 of $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$.

Having defined the algorithmic procedures $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$, $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$, and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$, we now possess all the elements to define precisely the notion of routine translation.

5.3. The routine translation. The *routine translation*[24] is given by the algorithmic procedure RT consisting of the composition of the three algorithmic procedures $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$, $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$, and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$, that is:

$$\mathsf{RT}: P \xmapsto{\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}} P_{\mathsf{hl}} \xmapsto{\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}} P_{\mathsf{il}} \xmapsto{\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}} P_{\mathsf{ll}} \qquad \text{or} \qquad \mathsf{RT} = \mathcal{T}_{\mathsf{il}\to\mathsf{ll}} \circ \mathcal{T}_{\mathsf{hl}\to\mathsf{il}} \circ \mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}.$$

This allows us to state precisely what it means to say that a mathematical proof P can be *routinely translated* into a formal proof in terms of the capacity of the algorithmic procedure RT to *succeed* in turning the mathematical proof P into a formal proof:[25]

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[24] I am talking here about 'the' routine translation, but it would be better to talk about a *family* of routine translations, as there could be many variations at the level of the three algorithmic procedures $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$, $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$, and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$. For instance, in the algorithmic procedure $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$, different choices could be made regarding the decomposition processes used to decompose the various inferences under consideration, and of course the algorithmic procedure $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ is entirely dependent on the formal deductive system Γ one is adopting. What matters for the present discussion is the *general structure* of this routine translation, not its specific implementations.

[25] To say that a mathematical proof P *can be* routinely translated into a formal proof is an *existential* statement, and should be interpreted as saying that *there exists* a routine translation able to turn P into a formal proof. In this reconstruction of the normative part of the standard view, I am specifying this existential statement by exhibiting the routine translation that, I think, the proponents of the standard view have in mind.

P can be *routinely translated* into a formal proof
$$\Leftrightarrow$$
RT would *succeed* in translating P into a formal proof.

Insofar as RT is the composition of the three algorithmic procedure $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$, $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$, and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$, we can specify the second part of this equivalence further as follows:

RT would *succeed* in translating P into a formal proof
$$\Leftrightarrow$$
$\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ would *succeed* in translating P into a higher-level proof P_{hl},
and $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ would *succeed* in translating P_{hl} into an intermediate-level proof P_{il},
and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ would *succeed* in translating P_{il} into a lower-level proof P_{ll}.

We thus obtain that:

P can be *routinely translated* into a formal proof
$$\Leftrightarrow$$
$\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ would *succeed* in translating P into a higher-level proof P_{hl},
and $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ would *succeed* in translating P_{hl} into an intermediate-level proof P_{il},
and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ would *succeed* in translating P_{il} into a lower-level proof P_{ll}.

This completes our specification of the normative part of the standard view, that is, our characterization of what it means for a mathematical proof P to be rigorous$_N$. We can now turn to the examination of the last component of the standard view: the conformity thesis.

§6. The standard view of mathematical rigor: Conformity thesis. The *conformity thesis* relates the descriptive part and the normative part of the standard view, and states that, for any mathematical proof P:

$$P \text{ is rigorous}_D \implies P \text{ is rigorous}_N.$$

In this section, we will show that this implication holds for the accounts of what it means for a mathematical proof P to be rigorous$_D$ and rigorous$_N$ developed in the two previous sections.

Let P be a mathematical proof and let us assume that P is rigorous$_D$. We want to show that P is rigorous$_N$. To this end, we will argue that the successive application of the three algorithmic procedures $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$, $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$, and $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ would succeed in turning P into a formal proof.

Since P is rigorous$_D$, we have that for every mathematical inference I in P, there exist $D \in \mathcal{D}^\star$ and $V_1, \ldots, V_n \in \mathcal{V}^\star$ such that (1) $D(I) = \langle I_1, \ldots, I_n \rangle$ and (2) $V_i(I_i) = \mathsf{valid}$ for all $i \in [\![1, n]\!]$, and furthermore that a typical agent in mathematical practice $\mathcal{M}$ would be able to identify such decomposition processes for each I in P. It follows from this that the first step of $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ would succeed in identifying suitable decomposition processes for all the inferences in P that are not immediate, the second step would succeed in decomposing these inferences into sequences of immediate inferences since decomposition processes are algorithmic procedures, and finally the third step would succeed in replacing them in P by their decompositions. Furthermore, the proof P_{hl} yielded by $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ is indeed a higher-level proof, since all its inferences are immediate, and all the premises of its inferences are either conclusions of previous inferences, mathematical propositions from background knowledge, or assumptions to be discharged later on in P_{hl}. Thus, $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ would succeed in translating P into a higher-level proof P_{hl}.

Since P_hl is a higher-level proof, this means that P_hl is a sequence of inferences such that (1) every inference in P_hl is an instance of an hl-rule in $\mathrm{R_T}$, and (2) every mathematical proposition occurring as a premiss of an inference in P_hl is either the conclusion of a previous inference in P_hl, a mathematical proposition from $\mathrm{K_T}$, or an assumption to be discharged later on in P_hl. Since P is rigorous$_D$, the process by which the pair $(\mathrm{R_T}, \mathrm{K_T})$ is obtained is the one described in the training model, and so we can replace each application of a hl-rule in P_hl by the sequence of applications of hl-rules in its rule certificate, and each mathematical proposition in P_hl by the sequence of applications of hl-rules in its proof certificate. In doing so, we obtain a proof in which all inferences are instances of hl-rules from R_t and all mathematical propositions occurring as premises of inferences are either definitions, conclusions of previous inferences, mathematical propositions from K_t, or assumptions to be discharged, and this for some $t < \mathrm{T}$. Such a process can be repeated up to a point where all inferences in the proof are instances of primitive rules of inference from $\mathrm{R_0}$ and all mathematical propositions occurring as premises of inferences are either definitions, conclusions of previous inferences, primitive axioms from $\mathrm{K_0}$, or assumptions to be discharged. Furthermore, since this process would consist of at most T iterations, we are assured that it will terminate. It follows from this that the first step of $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ would succeed. In the proof resulting from this first step, we can then replace each occurrence of a defined expression in the mathematical propositions of the resulting proof by its definition, and repeat this process until all mathematical propositions in the proof only contain primitive expressions from the language of $\mathrm{K_0}$. We can finally withdraw from the proof all mathematical propositions that correspond to definitions, for all these definitions would have been turned into tautologies by the process just described. The proof P_hl yielded by $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ is then an intermediate-level proof, since all its inferences are instances of primitive rules of inference, and all the premises of its inferences are either conclusions of previous inferences or primitive axioms. Thus, $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ would succeed in translating P_hl into an intermediate-level proof P_il.

Since P_il is an intermediate-level proof, this means that P_il is a sequence of inferences such that (1) every inference in P_il is an instance of a primitive rule of inference in $\mathrm{R_0}$, and (2) every mathematical proposition occurring as a premiss of an inference in P_il is either the conclusion of a previous inference in P_il, a primitive axiom from $\mathrm{K_0}$, or an assumption to be discharged later on in P_il. We have already argued in the previous section that one could specify the algorithmic procedure $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ in such a way that it would be able to turn any intermediate-level proof P_il into a lower-level proof P_ll. It follows from this that such a specified algorithmic procedure $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ would succeed in translating P_il into a lower-level proof P_ll.

We have argued that $\mathcal{T}_{\mathsf{vl}\to\mathsf{hl}}$ would succeed in translating P into a higher-level proof P_hl, that $\mathcal{T}_{\mathsf{hl}\to\mathsf{il}}$ would succeed in translating P_hl into an intermediate-level proof P_il, and that $\mathcal{T}_{\mathsf{il}\to\mathsf{ll}}$ would succeed in translating P_il into a lower-level proof P_ll. This means that RT would succeed in translating P into a formal proof, and that P can be routinely translated into a formal proof. We have thus shown that, for any mathematical proof P, P is rigorous$_D$ $\Rightarrow$ P is rigorous$_N$, namely that the conformity thesis holds for the accounts of what it means for a mathematical proof P to be rigorous$_D$ and rigorous$_N$ developed in the two previous sections.

This completes our reconstruction of the standard view of mathematical rigor, thus providing us with a precise formulation of it. We are now in a position to engage in a thorough evaluation of the standard view, a task that we will undertake in the two following sections by examining, in turn, the arguments against and in favor of it.

§7. The arguments against the standard view.   The standard view has been heavily criticized in the literature. In this section, we will examine the main arguments that have been advanced against the standard view in light of the formulation of it proposed in the three previous sections.

One of the first rejections of the standard view comes from John A. Robinson who, after having restated the standard view in its common form, wrote the following:

> This explanation of the rigorousness of rigorous unformalized proofs amounts to saying that informal proofs really are, so to speak, no more than sketches or outlines of formal proofs. But on closer examination this view seems unsatisfactory, and is rejected by most mathematicians.
>
> In actual mathematical work, formal proofs are rarely if ever used. Moreover, the unformalized proofs which are the common currency of real mathematics are judged to be rigorous (or not) directly, on the basis of criteria which are intuitive and semantic—not simply based on syntactic form alone. Although construction of a corresponding formal proof is rarely in practice undertaken, one sometimes attempts it anyway, if only for the sake of the exercise, or perhaps for the sake of submitting it to a computer proof-checking system. Formalization of a given informal proof then often turns out to be surprisingly difficult. The translation from informal to formal is by no means merely a matter of routine. In most cases it requires considerable ingenuity, and has the feel of a fresh and separate mathematical problem in itself. In some cases the formalization is so elusive as to seem to be impossible. (Robinson, 1997, p. 54)

In this passage, Robinson advances three reasons to reject the standard view. First, he notices that formal proofs are rarely used in ordinary mathematical practice. To my knowledge, no ones has ever contested this, and the standard view does not say or imply that mathematicians are or ought to use formal proofs in practice, so this does not constitute a reason to reject it.

Second, Robinson claims that judgments of the rigor of ordinary mathematical proofs are based, in practice, on 'intuitive' and 'semantic' criteria, and not only on 'syntactic' ones. For this to constitute a reason to reject the standard view, one would need to (1) define what is meant by the properties of being 'intuitive' and 'semantic', (2) provide an argument that judgments of rigor in practice are based on criteria that have these properties, and (3) explain why this would prevent the conformity thesis from holding, that is, why if a mathematical proof is judged to be rigorous based on 'intuitive' and 'semantic' criteria, it might not or could not meet the normative requirement that it be routinely translatable into a formal proof. Fleshing out these three points is a necessary condition for this to constitute a potential reason to reject the standard view.

The third reason is the most interesting one. It can be reformulated as follows: (P_1) the standard view says that if a mathematical proof is rigorous, then it can be routinely translated into a formal proof, but (P_2) when one tries to carry out concretely such a translation, this turns out to be "surprisingly difficult" and "by no means merely a matter of routine", so this means that (C) there is something problematic with the implication in P_1. This argument, however, rests on a confusion on what the term 'routine' means. One can distinguish at least two senses in which a process could be qualified as routine: in the first sense, a process is *routine* if it consists in an algorithmic procedure, in which case routine is synonymous with the terms 'algorithmic' or 'mechanical'; in the second sense, a process is *routine* if performing it does not present any difficulty. In the above passage,

Robinson uses the term routine in the second sense, as witnessed by his juxtaposition of the ideas that translation from informal to formal is "surprisingly difficult" and "by no means merely a matter of routine". If the term 'routine' in P_1 and P_2 is interpreted in the second sense, then the conclusion of the argument indeed holds. But the intended meaning of the term 'routine' as it appears in the standard view, and so as it appears in P_1, is that of the first sense, not the second one, as we established in §5. If we interpret the term 'routine' in P_1 and P_2 respectively in the first and second sense, then we have that P_1 and P_2 holds but that C does not follow from them. The reason is that a process can be routine in the first sense but not in the second one. To see this, it suffices to think of any particularly complicated algorithm which would be particularly hard to perform, such as the one used in the proof of the four color theorem. Indeed, this is precisely how Bourbaki sees the matter with respect to the process of routine translation when he says that the mathematician is "content to bring the exposition to a point where his experience and mathematical flair tell him that translation into formal language would be no more than an exercise of patience (though doubtless a very tedious one)" (Bourbaki, 1970, p. 8), for he recognizes that although such a translation "would be no more than an exercise of patience"—which is the case of any algorithmic procedure—carrying it out concretely would turn out to be "very tedious".

In a review of contemporary philosophical developments on the nature and significance of mathematical proofs, Detlefsen addresses the issue of the relation between rigor and formalization, and provides the following argument against the standard view:

> Mathematical proofs are not commonly formalized, either at the time they're presented or afterwards. Neither are they generally presented in a way that makes their formalizations either apparent or routine. This notwithstanding, they are commonly presented in a way that *does* make their *rigor* clear—if not at the start, then at least by the time they're widely circulated among peers and/or students. There are thus indications that rigor and formalization are independent concerns.
>
> This is not the common view, however. On that view, nonformalized proofs are typically close enough to formalized proofs to make the fact of formalizability clear and the remaining work of formalization routine. (Detlefsen, 2009, p. 17)

This argument has the following structure: (P_1) mathematical proofs are not "presented in a way that makes their formalizations either apparent or routine", but (P_2) they are "presented in a way that *does* make their *rigor* clear", so (C) "rigor and formalization are independent concerns". One can identify three issues with this argument.

First, the argument presupposes a reading of the standard view as providing a descriptive account of mathematical rigor. According to this interpretation, what the standard view is saying is that judging whether a mathematical proof is rigorous amounts to judging whether it can be formalized, that is, whether it can be translated into a formal proof. If one adopts such a descriptive reading, then the argument does provide a reason to reject the standard view, for if the rigor of a mathematical proof can be judged from an ordinary presentation of it while its formalizability cannot, then surely judging a mathematical proof as rigorous cannot be based on a judgment of its formalizability. However, as we mentioned at the beginning, the standard view cannot be meaningfully read as providing a descriptive account of mathematical rigor, and as we can see from our reconstruction, the standard view does not require that mathematical agents be able to judge directly from an ordinary presentation of a mathematical proof whether it can be formalized. Indeed,

Bourbaki himself recognizes that "the tiniest proof at the beginning of the Theory of Sets would already require several hundreds of signs for its complete formalization", so it is hard to see how, under these conditions, he could hold a view requiring that mathematical agents be able to judge the formalizability of mathematical proofs directly from their presentations.

Second, the argument interprets the term 'routine' as being synonymous to the terms 'apparent' or 'clear'. This is not, however, the intended interpretation of the term as it appears in the standard view. Rather, as we saw in the previous sections, the term 'routine' should be interpreted as being equivalent to 'algorithmic' or 'mechanical'. Under this interpretation, translation of a mathematical proof into a formal proof could perfectly be routine while it might neither be apparent nor clear that such a translation could be carried out.

Third, the argument presupposes that the only possible connection between rigor and formalization necessarily passes by the capacity to judge rigor and formalizability directly from the ordinary presentation of mathematical proofs. There are, however, other ways to establish such a connection. As we have seen in the previous sections, the standard view achieves a connection between rigor and formalization through a tripartite combination of a descriptive account of mathematical rigor, a normative account of mathematical rigor, and a philosophical thesis relating the two.

In §5 of his article on the nature of informal proofs, Larvor (2012) identifies several reasons to reject the standard view. One is particularly directed at Mac Lane's own formulation of the view:

> Saunders Mac Lane, reflecting on mathematical rigour, claimed that, "In practice, a proof is a sketch, in sufficient detail to make possible a routine translation of this sketch into a formal proof." (Mac Lane, 1986, p. 377). By 'formal proof', Mac Lane means a proof that is not content-dependent: "...the test for the correctness of a proposed proof is by formal criteria and *not by reference to the subject matter at issue*" (Mac Lane, 1986, p. 378; emphasis added). However, the proofs that mathematicians create and deploy typically make inferences that exploit local features of the subject-matter in hand. Euclid's proof of the infinitude of primes employs the fact that if a natural number m (>1) divides another, n, it cannot divide $n + 1$. (Larvor, 2012, p. 724)

There are two issues with this argument. First, the argument interprets Mac Lane as saying that, in practice, the correctness of mathematical proofs is assessed by "formal criteria" in a similar way as the correctness of formal proofs are. But the second quote of Mac Lane is talking about formal proofs, and is part of a paragraph discussing the standard of *absolute* rigor; it is not about the criteria used to assess the correctness of mathematical proofs in practice, that is, the "sketches" discussed in the first quote. Indeed, as we saw in §2, Mac Lane does not consider that the verification of mathematical proofs in practice is similar to the verification of formal proofs. Rather, he considers that the verification of proofs in practice proceeds via "well-recognized general rules" (Mac Lane, 1979, p. 65)— what we have called *higher-level rules of inference* or *hl-rules*. Second, such hl-rules do rely sometimes (indeed often) on the "local features of the subject-matter in hand", in the sense that some (indeed most) hl-rules have a restricted scope of application. To see this, it suffices to consider, for instance, the hl-rule X^2 is even $\Rightarrow X$ is even that we saw in §4—in the context of Hardy and Wright's proof of the irrationality of $\sqrt{2}$—and which can only be applied to an expression denoting a natural number (another typical example is the hl-rule

corresponding to mathematical induction). This is also exactly what is happening with the example of Euclid's proof of the infinitude of primes used to illustrate the argument, for what is at stake in this case is simply the use of the hl-rule:

$$X > 1, \ X \text{ divides } Y \ \Rightarrow \ X \text{ does not divide } Y + 1$$

where X and Y are placeholders for expressions denoting natural numbers, and which is also a hl-rule with a restricted scope of application. In sum, the observation that the verification of mathematical inferences in mathematical proofs can sometime "exploit local features of the subject-matter in hand" is perfectly compatible with the standard view as reconstructed here.

Another reason to reject the standard view advanced by Larvor (2012) has to do with the notion of routine translation:

> Philosophers of mathematical practice have had plenty to say about the short-comings of the view that 'real' proofs are sketches of derivations. One of the lessons of Lakatos (1976) is that translating a mathematical argument into a more formal idiom transforms it. By the time it is fully formalised [. . .], it is no longer the same piece of reasoning. Such translations are not 'routine' (to pick up Mac Lane's word); rather, *traduttore, traditore*. (Larvor, 2012, p. 725)

What this argument is saying is that the translation of a mathematical proof into a formal proof cannot be routine because such a translation "transforms" the mathematical argument in the mathematical proof one is starting with, and as a consequence the result "is no longer the same piece of reasoning". The argument presupposes then an interpretation of the term 'routine' according to which a routine translation should necessarily preserve the reasoning under consideration. However, this is not the intended meaning of the term 'routine' as it occurs in the standard view, and there are no requirements in the standard view that a routine translation of a mathematical proof into a formal proof should preserve the reasoning under consideration (at least in the sense of 'preserve' that matters for this argument). It may be that the argument originates from an interpretation of the notion of translation as being similar to a linguistic translation for which the primary requirement is precisely that the translation preserves the meaning of the sentences under consideration. But the notion of translation in the standard view is quite different from the one of linguistic translation. As noted by Burgess (2015), who reports a metaphor originally proposed by the mathematician and computer scientist Gil Kalai, a better analogy would be with the process of *compilation*, that is, with the 'translation' of a computer program written in a high-level programming language into machine language.

In another contribution, Larvor has proposed an argument against the standard view which he considers to be independent of what can be said, from a psychological or sociological perspective, on "how human mathematicians individually and collectively come to understand and confirm proofs" (Larvor, 2016, p. 402), i.e., "independent of questions about human cognitive and social functioning" (Larvor, 2016, p. 403). The argument is presented as follows:

> Let P be a mathematician's proof for a theorem C. Then it follows from the Derivation Recipe model that P is not as it stands (before any translation) a proof of C, but is rather an argument to convince the reader that:

C': there is a suitable formal system S such that $\vdash_S \gamma$, where γ is the formula in S corresponding to C.

For the sake of clarity: this is not what proponents of the Derivation Recipe model say; rather it is what the Derivation Recipe model amounts to once we recognise that the existence of a suitable derivation is itself a mathematical claim. The Derivation Recipe model requires that P must be, as it stands, before any translation, a compelling, rigorous argument (epistemically equivalent to a proof) of a mathematical conclusion, namely, C'. [...]

How can P work as a proof of C'? If it is just a recipe for a derivation, this would initiate an obvious regress. So, the Derivation Recipe picture must be that P, gappy, informal and intuitive as it may be, is an adequate proof of the mathematical claim C', whereas it is not an adequate proof of the mathematical claim C. (Larvor, 2016, p. 403)

As I analyze it, this argument works in two steps: first, it starts from two premises stating that (P_1) according to the standard view, for P to be a proof of C, P must be an argument for C' and (P_2) C' is a mathematical conclusion, and derives from them the conclusion that (C) according to the standard view, for P to be a proof of C, P must be a proof of C'; second, it observes that the conclusion C is absurd, because it leads either to an infinite regress, or to the strange claim that P would be an adequate proof of C' but not an adequate proof of C. From our formulation of the standard view, the issue with this argument is that P_1 is too strong. More specifically, what P_1 is saying, in the terminology we introduced, is that for being able to judge a mathematical proof P as being rigorous$_D$, i.e., as a rigorous mathematical proof from the point of view of mathematical practice, a mathematical agent must first establish that P is rigorous$_N$. But in our formulation, establishing that P is rigorous$_N$ is *not* a necessary requirement for judging P as being rigorous$_D$, and it is indeed not a requirement at all. On the contrary, the standard view works the other way around: it is by judging P as rigorous$_D$ *and* by holding the standard view that one can come to the judgment that P is rigorous$_N$, and thus obtain a ground for C'. Thus, our formulation of the standard view does not lead to the infinite regress pointed out by Larvor, a regress which would prevent mathematical agents to ever be able to judge a proof as being rigorous in mathematical practice.

In a paper discussing the notions of informal proofs, mathematical rigor, and mathematical knowledge, Antonutti Marfori (2010) provides several arguments against the standard view. Many of them are similar to the ones just discussed, and so will not be tackled again here. One argument is, however, quite different from the previous ones in that it is *social* in nature:

[T]he large convergence of the mathematical community on what makes for an adequate proof looks as mysterious as the success of mathematical practice in the light of the consideration that formalisation is seldom appealed to in order to resolve controversies. This indicates that formalisation and rigour are independent concerns of mathematicians, and that there must be some notion of informal rigour that normatively governs practitioners' work. (Antonutti Marfori, 2010, p. 267)

This argument rightly observes that mathematicians usually converge quite quickly in their judgments of whether a purported proof constitutes or not a rigorous mathematical proof

for a given mathematical proposition. But for this to appear as mysterious, and thus to constitute a challenge for the standard view, one needs to adopt a descriptive reading of the standard view, that is, a reading in which formalization would play a role in the process by which mathematical proofs are judged to be rigorous in practice. As we have repeatedly emphasized, such a descriptive reading should be rejected. Indeed, in our reconstruction, the notion of formalization does not play a role in how the standard view conceives of the verification of mathematical proofs in practice (see §4). Now, does it follow from this sociological fact that "formalisation and rigour are independent concerns"? What our reconstruction shows is that these are concerns that can be *separated*, as witnessed by our separation between the descriptive part—which does not appeal to the notion of formalization—and the normative part of the standard view. But it does not follow from this that one cannot hold a view relating the two. In particular, this does not prevent the two to be connected via the conformity thesis as it is the case our reconstruction of the standard view. Finally, it should be noted that our reconstruction agrees perfectly with the idea that "there must be some notion of informal rigour that normatively governs practitioners' work". Indeed, the descriptive part of the standard view precisely aims to capture the mechanisms that govern judgments of rigor in practice, and thus constitutes a potential candidate for an account of informal rigor as advocated by Antonutti Marfori (2010).

Tanswell (2015) has developed a general argument against what he calls 'derivationist' views, i.e., views for which "the rigour and correctness of informal proofs is taken to be dependent (in some sense) on associated formal proofs" (Tanswell, 2015, p. 296). The argument is directed at one particular derivationist view, that of Azzouni (2004), where informal proofs are taken to 'indicate' formal proofs, but is meant to be a challenge to any derivationist view. The heart of the argument goes as follows:

> [According to derivationist views] it is the underlying formal proofs that are meant to be ensuring the rigour and correctness of informal proofs, but if there are multiple different formal proofs simultaneously being depended upon this undermines the effectiveness of the explanation the derivation-indicator account gives. For example, what is there then to stop an informal proof from corresponding to both one correct and one incorrect formal proof? [. . .] Once it is conceded that there are multiple different, non-equivalent, formal proofs underlying some informal proof, we can immediately ask why it is these particular ones that are selected and what ensures that it is only correct and rigorous formal proofs that are picked out. (Tanswell, 2015, p. 302)

The argument is based on an obvious requirement of any derivationist view, namely that the dependence relation of informal proofs on formal proofs be specified. Now, if any informal proof would always depend on one, and only one, formal proof, then there would be no issues in determining the dependence relation. But as Tanswell rightly points out, when one is engaging in actually turning an informal proof into a formal one—e.g., with the help of a proof assistant—one realizes that there are often different ways to proceed, which would result in different and non-equivalent formal proofs—Tanswell refers to this phenomenon as 'overgeneration'. It follows that derivationist views ought to address the two key questions mentioned in the above quote, namely (1) "why it is these particular [formal proofs] that are selected" and (2) "what ensures that it is only correct and rigorous formal proofs that are picked out". In other words, what derivationist views shall provide

is "an explanation of how exactly the informal proof can be used to pick out some formal proof or proofs" (Tanswell, 2015, p. 298). Insofar as the standard view qualifies as a derivationist view in Tanswell's sense, and furthermore fully embraces the overgeneration phenomenon, it is directly concerned by these requirements and shall provide answers to the questions just raised. But, as it turns out, a large part of our reconstruction of the standard view was indeed directed to these questions. In particular, the standard view provides a direct explanation as to how an informal proof 'picks' specific formal proofs: the specific formal proofs to be picked are precisely those that can be obtained by routinely translating the initial informal proof. Insofar as the notion of routine translation has been entirely specified in §5, this provides a direct answer to question (1). It also provides an answer to question (2) since, as we saw in §6, a routine translation, when applied to an informal proof that has (correctly) been judged to be rigorous in practice, necessarily yields a (correct) formal proof. Thus, the standard view, in its present reconstruction, is not concerned by the overgeneration problem.

The absence of a precise formulation of the standard view in the literature was a major obstacle to a proper assessment of its strengths and weaknesses. Our reconstruction of the view has made it possible in this section to carry out a more fine-grained evaluation of the main arguments that have been advanced against it. From the perspective of our own formulation of the standard view, the main arguments proposed against it rests on one or more of the following misinterpretations of the view: (1) a confusion on the meaning of the term 'routine'; (2) a reading of the standard view as providing a descriptive account of mathematical rigor; (3) a presupposition that any connection between rigor and formalizability necessarily passes by a direct judgment of these qualities from the ordinary presentation of mathematical proofs; (4) an interpretation of the standard view as stating that the rigor of mathematical proofs in practice is assessed by formal criteria; (5) an interpretation of the standard view as requiring that for judging a proof to be rigorous$_D$, an agent must first establish that the the proof be rigorous$_N$; (6) an assessment that the standard view does not specify the dependence relation between an ordinary mathematical proof and the formal proof that could be obtained through a routine translation. It should be noted that these interpretations were perfectly legitimate given the previous formulations of the standard view available in the literature. As we mentioned in §2, it is not surprising that the standard view has been left underspecified since the view was only a consequence of the projects pursued by Mac Lane and Bourbaki, and that these two authors did not have as a primary objective to provide a characterization of mathematical rigor as a quality of mathematical proof. Finally, the fact that the arguments reported here are found wanting does not mean, of course, that our formulation of the standard view is immune to any criticism. It shows, however, that the standard view, when properly construed, is more robust than previously thought.

§8. An argument in favor of the standard view. Although the standard view is endorsed by many philosophers and logicians and is almost considered as an orthodoxy among contemporary mathematicians, it is surprisingly hard to find an articulated defense of it in the literature and to pinpoint arguments specifically advanced to support it.[26] This might be explained by the absence of a precise formulation of the standard view, which

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[26] Hersh (1997) holds this against the standard view. While examining the assertion that "*Any correct practical proof can be filled in to be a correct theoretical proof*" (Hersh, 1997, p. 154), Hersh observes that this assertion is "commonly accepted", but he remarks that he has seen "no practical

makes it hard to figure out how exactly the view is supposed to be defended, and what is to be expected of arguments aiming to support it. The reconstruction of the view provided in this work allows to overcome this difficulty, making it possible to identify more precisely what is required to defend it. In the previous sections, we have specified what it means for a mathematical proof to be rigorous$_D$ and rigorous$_N$, and we have already argued for the conformity thesis. But for the standard view to succeed in what it has been designed for— i.e., to establish a tie between the practice and the ideal of proof—there is still a central element that needs to be argued for, namely that the descriptive account of mathematical rigor embedded in the standard view—what we have called the *descriptive part* of the standard view—is indeed a faithful model of how mathematical proofs are judged to be rigorous in practice. This claim is particularly hard to argue for, since it is essentially an *empirical claim* about mathematical practice.[27] Yet, an indirect argument for it can still be provided on the basis of an approach originally proposed by Mark Steiner (1975). The aim of this section is to construct this argument.

In his book entitled *Mathematical Knowledge*, Steiner undertakes an analysis of the concept of "knowing a proof" (Steiner, 1975, chap. 3, sec. 3). He proposes the following characterization:

> [A] mathematician is said to know a proof of *S*, if, working with a logician who supplies no premises, he could produce a formal proof of *P* (i.e., the *wff* which expresses *S*) [. . .]. (Steiner, 1975, p. 100)

The logician acts here as a "midwife" (Steiner's term) whose task is "to make explicit only those premises and arguments that were implicit in the mathematician's initial [proof of *S*]" (Steiner, 1975, pp. 100–101). To this end, the logician engages in a specific *dialogue* with the mathematician. This dialogue should be epistemically conservative in the sense that it should not import or reveal "more knowledge than the mathematician had before" (Steiner, 1975, p. 101). Once all the required information has been made explicit through this dialogue, to obtain a formal proof the logician must then pursue by translating the propositions of the informal argument into the considered formal language and by providing the logical steps omitted by the mathematician. The key issue here is to set rightly the "power" of the logician, for as Steiner emphasizes:

> If [the logician] is dull, his failure should not be laid at the doorstep of the mathematician's alleged ignorance. On the other hand, we cannot envision a superhuman, because such a being would discover a completed proof despite the ignorance of the mathematician. (Steiner, 1975, pp. 101–102)

For Steiner, this logician should be such that he is "brillant at analysis and symbolic manipulation" but "lacks mathematical creativity" (Steiner, 1975, p. 102). The mathematician is then said to know a proof of *S* if the logician succeeds in finding a formal proof of *P* (the formal translation of *S*) through the procedure just described.

The approach adopted by Steiner can be adapted to extract an important *datum* regarding rigor judgments of mathematical proofs in mathematical practice. To do so, we will now introduce a similar dialogue as the one imagined by Steiner, but this time the partici-

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or theoretical argument for it, other than absence of counterexamples" (Hersh, 1997, p. 154). Hersh concludes that "It may be true" but that "It's a matter of faith" (Hersh, 1997, p. 154).

[27] We will come back to this issue in the conclusion.

pants in the dialogue will not be a mathematician and a logician but *two mathematicians*. This dialogue can be construed as a *game* in which one mathematician—the *defender*—aims to defend her claim that a certain mathematical proof P is rigorous while the other mathematician—the *challenger*—aims to challenge this claim. The game takes the form of a sequence of questions asked by the challenger and answered by the defender, each question being immediately followed by a potential answer to it. The game starts with the defender putting the mathematical proof P "on the table", the proof being then expanded with the answers provided by the defender. At stage s, the challenger can ask two different types of questions regarding the proof P_s on the table: she can challenge a premiss of an inference I in P_s by asking "How do you know this premiss of I?", or she can challenge an inference I of P_s by asking "How do you know that the conclusion of I follows from its premiss(es)?". Similarly to the dialogue imagined by Steiner, the answers of the defender should be epistemically conservative, in the sense that the defender cannot draw or verify new inferences to answer the questions of the challenger and can only appeal to knowledge she had before her claim that P is rigorous, otherwise the defender could simply verify P in the course of the dialogue. In other words, the defender can only report knowledge acquired, and actions taken, *prior* to her claim that P is rigorous. Furthermore, to avoid that the game enters into an infinite loop, we should add as a constraint that the challenger cannot challenge twice the same premiss or inference, that is, each answer from the defender should be either fully accepted or fully rejected by the challenger. If at some point it happens that the defender cannot answer a question asked by the challenger, then the game stops and the challenger wins the game. The idea here is that the challenger has revealed through the dialogue a *failure* in the defender's verification of P, forcing the defender to give up her initial claim that P is rigorous.[28] If the game is pursued up to a stage s in which each premiss involved in the inferences in P_s is either a definition, the conclusion of a previous inference, a primitive axiom, or an assumption to be discharged later on in P_s, and each inference in P_s is an instance of a primitive rule of inference, then the game stops and the defender wins the game. The idea here is that the challenger is then forced to accept all the inferences in P_s, and as a consequence is forced to grant the claim to the defender that the proof P is rigorous. We will refer to this game as the *rigor game associated to P*.[29]

Now, it seems plain that the following implication holds: if a mathematician has properly judged a mathematical proof P to be rigorous, then she possesses a *winning strategy* against the challenger in the rigor game associated to P. This *dialogical implication*, as we shall call it, should be considered as a *datum* from the perspective of mathematical practice, for it simply embeds the obvious claim that if a mathematician cannot answer at least one of the challenges put forward by the challenger, then this means that either she has used a

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[28] Of course, the defender might restore later on her claim that P is rigorous by engaging into further verification.

[29] The structure of these rigor games is, of course, reminiscent of the games and dialogues developed and studied in the contexts of *game-theoretical semantics* (Hintikka & Sandu, 1997), *dialogical logic* (Keiff, 2011), the *dialogical account of deduction* proposed by Dutilh Novaes (2016), and the *prover-skeptic dialogues* introduced by Sørensen & Urzyczyn (2006). Notice, however, that the use of the rigor games here is of a very different nature than in these other works: the point is not to define or characterize notions such as truth, validity, deduction, or proof, but rather to *reveal* certain features of the process of verification that the defender has previously carried out and which serves at the basis of her claim that the mathematical proof P under consideration is rigorous.

premiss that she is not able to prove, or she has made an inference that she is not able to justify, and in both cases this reveals a failure in her verification of P. If this implication is correct, being able to satisfy this implication constitutes a *requirement* that any descriptive account of mathematical rigor shall meet in order to be acceptable.

The descriptive account of mathematical rigor embedded in the standard view does meet this requirement. To see this, first assume that a mathematician has properly judged a mathematical proof P to be rigorous$_D$ and consider a given stage s in the rigor game associated to P. Since the defender has judged P to be rigorous$_D$, she can answer all the possible challenges that the challenger can put forward at stage s: if the challenge concerns a premiss of an inference in P_s, then in the case where the premiss is a definition, the conclusion of a previous inference, or an assumption to be discharger later on in P_s, the defender can simply points this out, and otherwise the defender can reply that she possesses a *proof certificate* for this premiss, and she can then update the proof P_s with the corresponding sequence of inferences—i.e., applications of hl-rules—she used to establish it; if the challenge concerns an inference of P_s, then the defender can reply that she possesses a *rule certificate* for the hl-rule R she used to carry out this inference, and she can then update the proof P_s with the sequence of inferences—i.e., applications of hl-rules—she previously used to acquire the hl-rule R.[30] The defender possesses then a *winning strategy* against the challenger in the rigor game associated to P. It is thus noticeable that the descriptive account of mathematical rigor embedded in the standard view provides a clear picture of what the winning strategy in the dialogical implication could consist in, and this should be taken as a piece of evidence in support of the claim that it is a faithful model of how mathematical proofs are judged to be rigorous in practice. But this only constitutes an *indirect* argument for this claim insofar as nothing prevents the possibility that an alternative descriptive account of mathematical rigor would satisfy as well the dialogical implication.

Interestingly, the dialogical implication can be exploited to yield a *direct* argument in favor of the standard view. To see this, it suffices to notice that whenever a mathematician possesses a winning strategy against the challenger in the rigor game associated to P, she then has the capacity to turn P into what we have previously called an intermediate-level proof, that is, a proof in which every inference is an instance of a primitive rule of inference and every premiss is either the conclusion of a previous inference, a primitive axiom, or an assumption to be discharged (see §5.1). Assuming that the computational power of a human mathematician does not exceed the one of a Turing machine, this means that there exists an algorithmic procedure able to turn any rigorous mathematical proof P into an intermediate-level proof. Together with the considerations provided in §5.2, one can then conclude that there exists an algorithmic procedure able to turn P into a lower-level proof, that is, into a formal proof. This means that one can directly obtain from the dialogical implication a ground for the view that whenever a mathematical proof P has been judged to be rigorous, it can be routinely turned into a formal proof. However, from the perspective of the epistemology of mathematics, this argument is not entirely satisfying for it does not tell us what rigor in mathematical practice *is*, nor what rigor judgments *amount to*. The argument merely identifies a high-level property of the notion of rigor as used in practice, and exploits it to provide a ground for the existence of an algorithmic procedure able to turn any rigorous mathematical proof into a formal proof. However, if one only wants to make sure that whenever a mathematical proof has been judged to be rigorous in practice it

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[30] The definitions of the notions of *proof certificate* and *rule certificate* were provided in §4.2.

indeed meets the normative condition that it can be routinely translated into a formal proof, then this argument is sufficient by itself. This direct argument might then be the reason why the standard view enjoys such a widespread acceptance in mathematical practice.

The argument in favor of the standard view provided here is based on what we have called the *dialogical implication* which can be considered as a *datum* from mathematical practice. This is only an *indirect* argument, as nothing prevents an alternative descriptive account of mathematical rigor to also satisfy the dialogical implication. A direct argument in favor of the standard view can be obtained from the dialogical implication, but this argument treats the process by which proofs are judged to be rigorous in practice as a 'black box'. From an epistemological perspective, a more satisfying argument shall provide direct *empirical support* for the empirical claim that the descriptive account of mathematical rigor embedded in the standard view is indeed a faithful model of how proofs are judged to be rigorous in practice.

§9. Conclusion. The aim of this paper was to provide a precise formulation and a thorough evaluation of the standard view of mathematical rigor. Our reconstruction has revealed that the standard view is the combination of three components:

1. A certain conception of the mechanisms by which mathematical proofs are judged to be rigorous in mathematical practice, according to which mathematical inferences are first decomposed into immediate mathematical inferences via certain proof search processes, and immediate mathematical inferences are then verified using higher-level rules of inference. This is the *descriptive part* of the standard view.
2. A certain conception of what it means to say that a mathematical proof *P* can be *routinely translated* into a formal proof, where the notion of routine translation is conceived as the combination of three successive translations turning a mathematical proof provided at the vernacular level into one at the lower-level—i.e., into a formal proof—and where the term 'routine' is interpreted as being equivalent to 'algorithmic'. This is the *normative part* of the standard view.
3. A philosophical appraisal of the relation between the mechanisms involved to judge the rigor of mathematical proofs in practice and the ideal standards of formal proof—i.e., of the relation between the descriptive part and the normative part of the standard view—according to which whenever a mathematical proof has been judged to be rigorous in mathematical practice, it can be routinely translated into a formal proof. This is the *conformity thesis*.

Taken together, these three components provide a precise formulation of the standard view, one which can support a detailed evaluation of its strengths and weaknesses. In the previous two sections, we have examined the main arguments against and in favor of the standard view that can be found in the literature. All the arguments advanced against the standard view that we have examined were found wanting, and most of them were found to originate in what we consider to be misinterpretations of the standard view. We have then constructed an argument in favor of the standard view which aims to support a claim necessary for the standard view to work, namely that the descriptive part of the standard view is indeed a faithful model of how mathematical proofs are judged to be rigorous in practice. This argument provides support for this claim, but it is only an indirect argument insofar as it may be compatible with alternative accounts of how proofs are judged to be rigorous in practice.

Two main conclusions can be drawn from the present study. First, the standard view—in its present reconstruction—is more robust to criticisms than it has been suggested by the various papers which have opposed it. This is, of course, not to say that the standard view is immune to any challenge. Indeed, one interest of the precise formulation provided here is to open the way to a detailed scrutiny of the standard view so as to identify its eventual drawbacks and weaknesses.

Second, the standard view is still in need of further support, given that the dialogical argument only provides partial support for it. The element of the standard view which is the most open to criticisms, and which requires further evidence to support it, is the descriptive part. The crucial point to acknowledge here is that the descriptive part is, ultimately, an *empirical claim*, since it is a claim concerning the mechanisms by which mathematical proofs are judged to be rigorous *in practice*.[31] In this respect, the descriptive part needs to be supported by *empirical evidence*. What are the proper forms of empirical evidence to do so remains to be determined. But the corresponding empirical inquiry can be conducted along two paths, aiming, respectively, to *confirm* or to *refute* the standard view. In the first case, the objective will be to provide empirical evidence that the way mathematical agents verify proofs in practice conforms to the mechanisms proposed in the descriptive part. In the second case, the objective will be to identify cases of mathematical inferences for which it can be argued, based on empirical evidence, that either the verification of these mathematical inferences in practice cannot follow the mechanisms provided by the descriptive part, or that an alternative descriptive account of mathematical rigor—to be specified—is superior to the one provided by the descriptive part.

We mentioned at the beginning that the *raison d'être* of the standard view was to provide a *tie* between the *practice* and the *ideal* of proof. As Bourbaki put it:

> If formalized mathematics were as simple as the game of chess, then once our chosen formalized language had been described there would remain only the task of writing out our proofs in this language [. . .]. But the matter is far from being as simple as that, and no great experience is necessary to perceive that such a project is absolutely unrealizable: the tiniest proof at the beginning of the Theory of Sets would already require several hundreds of signs for its complete formalization. (Bourbaki, 1970, p. 10)

> We shall therefore very quickly abandon formalized mathematics, but not before we have carefully traced the path which leads back to it. (Bourbaki, 1970, p. 11)

> Thus, written in accordance with the axiomatic method and keeping always in view, as it were on the horizon, the possibility of a complete formalization, our series lays claim to perfect rigour [. . .]. (Bourbaki, 1970, p. 12)

If it can be shown that this tie cannot be maintained for some mathematical practices, then this would have for direct consequence to force a revision of the contemporary ideal of proof. Investigating the mechanisms by which mathematical proofs are judged to be

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[31] Important empirical studies on how mathematicians evaluate and validate mathematical proofs have already been carried out by mathematics education researchers, see in particular Weber (2008), Inglis & Alcock (2012), and Inglis, Mejia-Ramos, Weber, & Alcock (2013).

rigorous in various mathematical practices, and eventually identifying thereby some challenges for the standard view of mathematical rigor, shall then remain a topic of primary importance for the philosophy of mathematics.

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