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This proof which is not one: the problem of individuating mathematical proofs and its impact their evaluation

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§ 1. — Introduction.

The current philosophical literature on proofs is often (as evidenced in many of the references in this paper) concerned with identifying good proofs, where "good" may refer to various virtues or values, ranging from validity, through rigor, to aesthetic judgment. Usually, proofs are evaluated as clearly individuated entities, based on a single presentation, such as a printed text or seminar talk.

In this paper, I argue that individuating proofs is a tricky and context-dependent issue. In turn, this makes the evaluation of the properties of proofs tricky and contextual. Instead of trying to resolve this by offering individuation criteria (a project that I find less than promising), I suggest that philosophical analysis may benefit from viewing "a proof" not as a single individuated presentation, but as a network of textual and performative presentations related to each other by partial translations.

In sections 2 and 3, I demonstrate the problem of individuating proofs, first from a historical perspective and then in contemporary mathematics, using toy-examples and real-world examples. In section 4, I provide the theoretical framework that I propose as an alternative for attempting to individuate proofs. This approach is inspired by the structuralist-semiotic approach to discourse (stemming from the work of de Saussure and Lévi-Strauss, more on which below) and contextualized in current perspectives on the nature of proofs. In sections 5 and 6 I use this framework to analyze two examples of characteristics associated withs proofs: rigor and explanatory value. In a nutshell, I argue that evaluating such characteristics depends not on a single presentation of "the" proof, but on curating a corpus of proof-presentations and interpreting the relations between them, leading to potentially divergent evaluations of those characteristics. In section 7, I review historical proof practices that do not reduce proofs to an individual presentation of a proof in order to provide the framework suggested here with broader grounding and purport.

§ 2. — The problematic individuation of proofs: historical examples.

In order to think of a proof as an individual text (in a broad sense, including diagrammatic and other printed components) or performance (possibly accompanied by material aides), we require criteria of individuation that would identify whether two texts or performances of a proof (henceforth: proof-presentations) are the same. Since a proof is typically a semiotic entity, a proof cannot be identified with a singular material presentation. At a minimum, one would expect that any copy of a proof-presentation should be considered as the *same* proof.

The thorn here, however, is in how much tinkering a "copy" may involve. For example, it is not clear whether a proof-presentation that is typeset for a new publication with revised notation remains the same proof. Given that notations have cognitive and epistemic purport (Schlimm 2024), can we safely say that both versions would constitute the same proof? While current mathematical publication norms would probably identify two such proof-presentations as the same, historiographical norms suggest that the two versions may constitute different proofs. The use of algebraic notation in rewriting classical Greek geometric proofs, to take an (admittedly extreme) example, is a famous concern, which gave rise to an ongoing debate starting with Unguru (1975). Mathematicians tend to view the algebraized proof-presentations as essentially the same, whereas historians tend to consider them as essentially different. In some historical contexts, even a change of layout and spelling may suggest difference in the conceptualization of the mathematical content.

A nice example for the ambiguity of the individuation of proofs is Cauchy's demonstration of Euler's identity for polyhedra, $F + E - V = 2$, where F is the number of faces, E the number of edges, and V the number of vertices. This proof has been famously studied from historical and normative perspectives in Lakatos' *Proofs and Refutations* (1976). A crucial step in the proof, as presented by Lakatos, is imagining the polyhedral surface as made of a stretchy material, tearing a hole in one of the faces, and stretching the surface onto a plane. However, as Lakatos explicitly notes (1976, 89, fn. 3), Cauchy's original argument involved no stretchy material. Instead, Cauchy states that "taking one of [the polyhedron's] faces as base, and transporting onto this face all the other vertices without changing their number, one would obtain a plane figure composed of several polygons enclosed in a given contour" (Cauchy 1813, 77).(1)

In terms of what is happening in the proof, whether we stretch the surface or use some "transportation" of the vertices and edges that retains straight lines hardly matters at all. However, to allow "stretching" is to turn the theorem from a geometric characterization of systems of straight line-segments into a topological characterization of deformable networks of lines. By invoking the stretchy material, the rectilinear nature of polygons turns out to be irrelevant, suggesting the reformulation of the theorem in terms of curved surfaces and lines that is paradigmatic of modern topology. Arguably, since it makes such a substantial difference, Lakatos' presentation might not be considered the same as Cauchy's.

Another historical example is that of Hilbert's famous hotel. Below I quote Hilbert's version from his lectures and a related 14th century argument by mathematician-philosopher-theologian Thomas Bradwardine of the Oxford calculators. First, Hilbert's version, as translated in Kragh (2014, 8):

> We now assume that the hotel has infinitely many rooms numbered 1, 2, 3, 4, 5, ... and that each of the rooms is occupied by a single guest. All that the manager has to

(1) "en prenant une de ses faces pour base, et transportant sur cette face tous les autres sommets sans changer leur nombre, on obtiendra une figure plane composée de plusieurs polygones renfermés dans un contour donné".

> do in order to accommodate a new guest is to make sure that each of the old guests moves to a new room with the number one unit larger. In this way room 1 becomes available for the new guest. One can of course make room for any finite number of new guests in the same manner; and thus, in a world with an infinite number of houses and occupants there will be no homeless.
>
> The situation is the same with an infinite dance party where all the gentlemen have asked the ladies to dance. A new lady enters, but the organizer of the dance can easily arrange that she will not be without a partner. It is even possible to get space for an infinite number of new guests, respectively ladies [that is: partners for an infinite number of new ladies on the dance floor]. One could, for example, ask the old guest who originally occupied room number n to move to room number $2n$. In this way infinitely many rooms with odd numbers would be left free for new guests.

Next, Bradwardine's version, as translated in Thakkar (2009, 627-9), based on Bradwardine's *De Causa Dei*. In this setting, instead of rooms and guests or men and women dance partners, we have an infinite multitude A of souls and an infinite multitude B of bodies, both arranged in enumerated sequences:

> let the souls be distributed [...] in this way: the first soul to the first body, the second to the second, and so on; when the distribution is complete, each soul will have a unique body, and each body a unique soul. So these [multitudes] jointly and severally correspond equally to one another... let the first soul be given to the first body, the second to the third (or the tenth, or to one as distant as you please from the first), and the third soul to the body as distant from the second ensouled body as the latter is to the first, and so on until the whole distribution is completed in this way.
>
> This done, either all the individual souls have been distributed to bodies, or there are some souls left over. If all the individual souls have been distributed to such bodies, the whole multitude A jointly and severally corresponds equally to that part of B, and vice versa. If any soul is

> left over, then since there are only finitely many between it and the first, the bodies already taken from the multitude B are the same in number and finite; so the whole multitude B — which was supposed to be infinite — is likewise finite.

Despite the differences in setting and detail, many would consider these two arguments as essentially the same or mathematically equivalent. But the sting of this example is that while Hilbert used this argument to illustrate the properties of infinite sets, for Bradwardine this was a proof that infinite collections did not exist, as they would yield a part equal to the whole, violating a Euclidean axiom. The change of context turns a proof of a property of infinite sets into a proof that excludes their existence by contradiction. The different contexts involve different conceptualizations of what equality between whole and part could legitimately mean, which, arguably, changes the entire meaning of the apparently mathematically-similar proofs.

§ 3. — The problematic individuation of proofs: contemporary examples.

The problem, however, is not merely historical. Consider the identity $1 + 3 + \ldots + (2n - 1) = n^2$. I propose two "diagrammatic" proof-presentations (accompanied by an explanatory text), and two textual proof-presentations.

The first (Figure 1, left) is taken from the Wikipedia page "proofs without words" (Wikipedia 2024), citing Dunham (1994, 121) and is, ironically, accompanied by words. The second (Figure 1, right) is a variant that I propose for the same diagram. The first diagram is accompanied in the Wikipedia entry by the following text:

> In one corner of a grid, a single block represents 1, the first square. That can be wrapped on two sides by a strip of three blocks (the next odd number) to make a 2×2 block: 4, the second square. Adding a further five blocks makes a 3×3 block: 9, the third square. This process can be continued indefinitely.

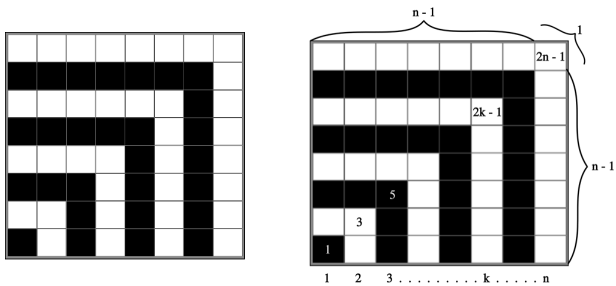

Figure 1. — Left: "Proof without words" that the sum of consecutive odd numbers is a square (Wikipedia 2024). Right: an amended version. CC BY-SA 3.0

I added the right-hand diagram because some might object that the first diagram is merely a special case, whereas the elliptic omissions and labels additions in the second diagram may render it more general. For others, however, these changes would be merely cosmetic, leaving the diagrams essentially the same.

Next are two more standard proofs for the same identity composed by myself.

Proof by induction:

First, check the case $n = 1$. Indeed, $1 = 1^2$.

Next, for $n > 1$, suppose the claim is true for all $m < n$. Let's prove it for n.

Indeed, $1+3+\ldots+(2n-1) = (1+3+\ldots+(2n-3)) + (2n-1)$.

By the induction hypothesis, this equals

$$(n-1)^2 + (2n-1) = n^2 - 2n + 1 + (2n-1) = n^2.$$

Set-theoretic Proof:

Let us use pairs of integer coordinates to designate the cells of the large square, ranging from $(1,1)$ at the bottom left to (n,n) at the top right.

On the one hand, these coordinate-pairs form the set $\{1, 2, \ldots, n\} \times \{1, 2, \ldots, n\}$, which has exactly n^2 elements.

On the other hand, this same set of coordinate-pairs can be broken into the following disjoint subsets:

$S_1 = (1,1)$
$S_2 = \{(1,2),\ (2,2),\ (2,1)\}$
...
$S_k = \{(1,k),\ (2,k),\ldots,\ (k-1,k),\ (k,k),\ (k,k-1),\ldots,(k,2),\ (k,1)\}$
...
$S_n = \{(1,n),\ (2,n),\ldots,\ (n-1,n),\ (n,n),\ (n,n-1),\ldots,\ (n,2),\ (n,1)\}$

The k-th set of has exactly $(k-1)+1+(k-1) = 2k-1$ coordinate-pairs, so the total number of coordinate-pairs is $1+3+\ldots+(2k-1)+\ldots+(2n-1)$.

Therefore, $1+3+\ldots+(2k-1)+\ldots+(2n-1) = n^2$.

Is either of these textual proof-presentations the same as the ones presented in the diagrams? Are they the same as each other?

First, one may argue that the diagrammatic proof-presentations are not proofs at all and thus cannot be the same as the textual proof-presentations. This is indeed a bone of contention among mathematicians (see, e.g., Weber and Czocher 2019). As noted above, the first diagram may be argued to be merely a special case. The second diagram attempts to gesture toward generality, but one may argue that replacing parts of the diagram by ellipses and adding some labels cannot turn a diagram into a proof.

A counter-argument may be that the inductive and set-theoretic proofs above would not lose in generality if translated to a particular case, and therefore the particular/general distinction is not an essential issue here. What I mean is, that if one replaces the symbol "n" by the symbol "17" (adding the necessary parentheses) and refrains from resolving arithmetical operations (that is, retains expressions such as $17-1$ and 17^2 rather than replacing them by 16 and 389 respectively), the proof remains the same up to a change of notation (the symbol "17" could after all, be used to denote any mathematical entity). Indeed, the translation between the particular and general notation can be made routine and even automated (a criterion that will recur when we discuss rigor below). The distinction between the particular and the general case is thus not as clear-cut as it might seem initially and depends on intentions and a cohort of manipulation practices, not just notation. If we accept that, then the apparent particularity of the diagrams might not be enough to distinguish them from the textual proofs.

Continuing with our comparison of the diagrams and the textual proofs, I note that the diagrams appear static, whereas the inductive argument is iterative. But the text accompanying the diagrams suggests an iteration, so perhaps the static diagrams have to be *seen as* dynamic (that static diagrams might be intended to be read and manipulated as dynamic is well precedented, see for example Apollonius' diagrams in Fried and Unguru 2001, 74; a more general theoretical discussion is available in Châtelet 2000, summarized in De Freitas and Sinclair 2014, ch. 3).

Another objection to identifying the diagrammatic proofs with the inductive one is that the latter fails in capturing the main visual clue of the diagrams: the visible squareness of the sum. However, if we identify geometrical squares with numerical squares and rewrite the final equation as

$$(n-1)^2 + (2n-1) = (n-1)^2 + (n-1) + (n-1) + 1 = n^2,$$

do we not in fact capture the geometric structure of the right-hand diagram arithmetically? Still, the particular spatial relations between the parts of the diagram, which are so salient visually, are not captured arithmetically.

As for the set-theoretic argument, it is designed to capture more precisely the original geometric structure. Coordinate-pairs represent the cells of the diagram, the Cartesian product represents the square, and the decomposition of the set of coordinate-pairs into a disjoint union captures the black and white gnomons in the diagrams. One might therefore argue that the set-theoretic argument is a faithful rendering of the diagrammatic proof. However, there's nothing properly square or even spatial in this system of pairs of coordinates, and while readers with the appropriate level of mathematical education today are very likely to make such an association, it is nevertheless a rather recent (17^{th} century) association. One may then object that Cartesian products, in themselves, have nothing square about them and that the set-theoretic proof and diagrammatic proof are still very distinct.

Moreover, the set-theoretic proof depends on a translation of set-operations (Cartesian product, union) into arithmetical operations (multiplication, addition), which depend, in turn, on a preliminary theory and proof apparatus. The inductive proof and diagrams do not depend on these preliminaries and so appear to be distinct from the set-theoretic proof. However, the same preliminaries of the set-theoretic proof may also be used to argue in favor of an assimilation of the set-theoretic proof and the inductive one. While there's no induction in the set-theoretic proof itself, the preliminaries that it relies on are inductive (the identification of Cartesian and arithmetical products is derived by induction from the identification of disjoint union and addition, which is also derived inductively). It may therefore be argued that the set-theoretic proof with its preliminaries and the inductive proof with its implicit use of the distributive law require essentially the same preliminaries. In that sense the inductive proof may be closer to the set-theoretic proof than it first appears to be.

Moving on from toy-examples to the real world, I note that the question whether two proofs are or are not essentially the same is not a mere philosophical speculation — it is part of mathematical practice. There are collections that present and classify different proof-presentations of well-known theorems into distinct families, where each family contains variants that are treated as essentially the same (for the Pythagorean theorem, see Loomis 1927). Moreover, when an author submits a new proof to a journal, reviewers need to decide whether it is different enough from existing proofs to merit publication.

In fact, such discussions can turn quite intense. I invoke just two examples (from Wagner's 2022, section 3, where other related examples can be found; additional examples can be found in the famous Jaffe-Quinn debate, 1993 and the responses it received in Atiyah et al., 1994). The first example is Arnold's conjecture, which was first proved by Kenji Fukaya and Kaoru Ono in 1996. Their proof was challenged in a lengthy online discussion in 2012. At the end of this discussion, Dusa McDuff and Katrin Wehrheim published a revised proof. Fukaya believed that the "new" proof did not innovate anything, and the differences were minor technicalities. Others argued that while the original approach worked, the clarifications in the revision were too substantial to be dismissed as insignificant. A more famous example is Gregory Perelman's solution of the Poincaré conjecture from 2002, challenged by Xi-Ping Zhu and Huai-Dong Cau in 2006. In this case too, the debate spanned from claiming that the original and revised proofs were essentially the same to claims that the original proof had too many gaps to be considered a valid proof at all, and that the revisions made it properly distinct.

These toy-examples and real-world examples are meant to show that whether different proofs (or "proofs") should be considered different, similar, or essentially identical is a contested and interpretation-laden issue, with potentially grave repercussions (Perelman ended up quitting mathematics, at least partly because of the debates surrounding his work, making an adverse impact on the research community). This phenomenon shows that we lack clear and consensual individuation criteria for proofs. All too often, mathematicians simply do not agree whether two proofs are the same, or on how similar two proofs actually are.

But the problem of individuating proofs may be even more ontologically radical than the above discussion shows. Even what we would consider as a single proof-presentation may be interpreted differently by different readers. This is palpable in the context of the history of mathematics, where scholars make substantial efforts not to project modern mathematical conceptions on past proofs without always reaching a consensus as to the meaning and validity of the original argument. But even two contemporary readers may understand the same proof-presentation in different ways, sometimes even disagreeing on the validity of a given proof-presentation based on their training and on the standards endorsed by their mathematical communities (Inglis et al. 2013). This may extend to a single reader at different times, or to a single reader recalling the "same" proof.[2]

§ 4. — How to deal with the problematic individuation of proofs.

Since there are no clear and universal criteria for deciding whether two proof-presentations are the same, and since even the "sameness" of a single proof-presentation may be put into question

[2] Due to these observations, the very notion of proof-presentation ends up being context-dependent itself. The problem of individuation goes all the way down, and the very notion "proof-presentation" is merely an imprecise crutch for discussing proofs at different resolutions. This notion could be replaced by a fractal notion whereby the term "a proof" can always be replaced by "a collection of proofs contextually considered to be the same" relative to the resolution of the "context". While this fractal can come to an end if we reach a singular material entity at a given point in time, this is not a useful approach for a philosophical analysis of proofs, because a singular materiality cannot be referred back to — it disappears right as it comes into being. This tension between reference and singularity is at the core of deconstruction — an approach which grew out of the structuralist-semiotic tradition (see, e.g., Derrida 1988).

relative to its different interpretations by different mathematicians, philosophers may try to establish criteria of individuation for mathematical proofs based on normative considerations and/or empirical observations. But given the many different contexts in which we might ask what makes one proof the same as another and the many different aspects of proofs that they depend on, I doubt this would be a very successful project.

I suggest, instead, not to consider a proof as a properly individuated entity. Instead, I propose to consider proofs as networks of textual and performative presentations that are considered as more or less equivalent, more or less different, according to various inter-subjective judgments that may be more or less stable across mathematical communities. Such networks have no clear boundaries, only a fuzzy core of relatively closely related actual and potential proof-presentations. This will make the evaluation of the merits of proofs fuzzy as well, but as the empirical literature cited above shows (as well as Inglis and Aberdein 2015; 2016; Mejía-Ramos et al. 2021), this fuzziness is a real phenomenon, and, according to some (notably Lakatos 1976, but see also René Thom's response in Atiah et al. 1994), a driving force of mathematics rather than a problem that needs to be resolved.

This approach follows the philosophical structuralist tradition in rejecting the understanding of signs as individuated entities with inherent identities, meanings and properties, in favor of defining them by their relations of similarity and difference in broader networks of signs (the basic ideas stem from de Saussure [1916] 2011 and were expanded to a full method of discourse analysis by, among others, Lévi Strauss in his two *Structural Anthropology* volumes [1958] 1963 and [1973] 1976; for an overview of these methods applied to mathematical discourse see Wagner 2024). To introduce the structuralist approach, think for example of the English sound "r". There are many ways to pronounce it (say, trilled or non-trilled), but they are all "the same" in the sense that the meaning of a word does not change if one replaces one received pronunciation by another (although those different pronunciations can change the meaning of a word in other languages, such as Tamil). However, the individuation of the English sound "r" as "all pronunciations that do not produce a semantic difference in English" still has fuzzy edges. Pronouncing "r" in a louder voice may indicate alarm or anger, and pronouncing certain variants of "r" may suggest a regional dialect or sociolect, which may even affect the expected meaning of words. From a structuralist point of view, the sameness of a sound, defined by whether or not it makes a difference for how we interpret what we hear, turns out to be a contextual issue.

Similarly, instead of looking for criteria to individuate proofs and decide whether two proof-presentations are the same, we can focus instead on networks of proof-presentations. Instead of thinking of each of them as an individual with its own properties, we can consider each presentation as problematically self-same (in the sense that it may be evaluated differently by different mathematicians or even by the same mathematician on different occasions) and as having its properties defined not inherently, but by contextual commutation tests, namely by asking what happens to one's evaluation of the properties of a proof-presentation when some of its components are replaced. Since for some evaluations of properties of proofs only some specific aspects matter, a proof-presentation can be contextually "the same" as (or exchangeable with) another, even if they are clearly very different — perhaps even proving different theorems (as in the Bradwardine and Hilbert examples above).(3)

In practical terms this means that instead of looking at a specific proof-presentation in order to tease out its properties, one should consider instead a bunch of related proof-presentations and extract their properties from their relations. Since what constitutes the relevant "bunch of related proof-presentations" may be deemed to encompass different corpora, since the relations between the proof-presentations in a given corpus can be interpreted in different ways (as in the examples above), and since the evaluation of the properties of a proof depends on all of the above, the same proof-presentation may end up being evaluated very differently.

While most philosophical accounts assume an unproblematic individuation of proofs, there are some recent exceptions. One is the dialogical account of proofs elaborated by Dutilh Novaes. According to this account, "a deductive proof *corresponds* to a dialogue between the person wishing to establish the conclusion (given the presumed truth of the premises), and an interlocutor who will not be easily convinced and who will bring up objections,

(3) If we go all the way with some current trends in material-semiotic approaches, as in Barad 2007, we could have as our primitive a "proving event" (a student reading a proof, a proof being presented in a colloquium) and define proof-presentations and mathematical subjects as secondary, relatively articulated structural entities emerging from such events. For this paper, however, my point can be made while bracketing such event-based ontology.

counterexamples, and requests for further clarification and precision" (Dutilh Novaes 2018, 88, my italics; see also Dutilh Novaes 2016). According to this view, a proof does not stand alone, but gains its persuasive and explanatory power from its relation to other real or imaginary performances.

Two other approaches are summarized in Tanswell's (2024) exposition of the recipe model of proofs and virtue approaches to proofs. According to the former "proofs as written artefacts become secondary to the activity and the sequence of actions that they instruct the reader to carry out" (Tanswell's 2024, 43). A proof here is not a standalone entity, but gains at least some of its properties from another potential activity that takes place outside the proof — an actual or imagined enactment of the proof.

The virtue approach considers specifically the question of rigor (but is adaptable to several other properties of proof) "as primarily an intellectual virtue of the mathematician engaged in proving activities" (Tanswell's 2024, 58). In other words, it is not the proof itself that is rigorous, but the mathematician who may have acted rigorously in producing this and other proofs. The same presentation, according to this view, may be considered as rigorous or non-rigorous based on what the mathematician who produced it did or did not do to make sure that the proof was foolproof. According to this model too, a proof can be judged only in relation to other proof-presentations (the material or mental "drafts" of the mathematician's proof-presentation).

None of these approaches relate explicitly to the question of the individuation of proofs, and all of them involve something that I did not quite imagine initially as belonging to the category of proof-presentation. However, they do support, at least obliquely, the structuralist view of proofs proposed here, in that they suggest that some properties of a proof do not reside in a given proof-presentation, but depend on how it relates to other actual or potential, real or imagined counterparts (critical dialogues in the dialogical approach, mathematical actions in the recipe approach, the proof-verification performances of the mathematician in the virtue approach).

In order to show how the suggested view of proofs *makes a difference* (otherwise, structurally speaking, it would be the same as proof-individuating approaches), I will consider its impact on a couple of properties attributed to proofs. The philosophical, historical, and pedagogical literature engages with various properties or virtues of mathematical proofs (for recent surveys see Aberdein et al. 2021; Chemla 2015; Hanna and Barbeau 2010; Morris 2024), from which I select for a brief analysis the properties of rigor and explanatory value. I will demonstrate what the investigation of these properties gains from the structuralist view of proofs as fuzzily defined relational networks.

§ 5. — Characteristics of proofs: rigor.

The so-called standard view of rigor (reviewed in Tanswell 2024, ch. 3) claims, broadly speaking, that a proof is rigorous if it is translatable to a formal proof. There are many shades to this definition. At one extreme, there is an ideal of mechanical, practically algorithmic translatability (Hamami 2022). On the other extreme, it is only required that mathematicians be convinced, based on their expert experience and/or community standards, that a translation could be produced with the assistance of a person trained in such activities (or, as is becoming increasingly relevant, with the aid of a computerized proof assistant) even if they cannot do it themselves (for a review of various nuances see Burgess and De Toffoli 2022).

In itself, this view depends on relating at least two proof-presentations: the given proof-presentation and the formal counterpart, which may or may not actually be produced. In fact, since there are many relevant formal systems in which one may try to formalize a proof and many different possible formal translations of a given proof into a given formal system, we can safely say that a rigorous proof involves more than two texts, where the salience of potential translatability between these texts may depend on the context and target audience. Even at the forefront of research, different experts may assess the salience of translation differently, as evidenced by some of the examples in section 3.

An alternative to the standard view of rigor is made explicit in Larvor's (2022) take on Avigad's (2021) explanation of how mathematicians verify rigor. Avigad accepts that formalization underwrites rigor, but since mathematicians rarely do and are often unable to formalize, they must have different methods to be convinced of the possibility of formalization. Avigad's list of methods includes: reasoning by analogy, modularizing the argument into relatively independent components, generalizing, using algebraic abstraction, collecting examples, classifying mathematical objects into families that tend to behave in similar manner, developing complementary approaches, and visualizing. Larvor's suggestion is that instead of thinking of these methods as witnesses to rigor-as-formalizability, we should think of them as the very criteria of rigor themselves.

Rigor as measured by Avigad's methods may more or less cohere extensionally with rigor as formalizability, because our criteria for what constitutes a *correct* formalization (that is, a formalization that correctly captures the argument, rather than misinterprets it) and for what constitutes a *correct* applications of Avigad's means serve to check and balance each other. Of course, one could also *add* partial formalization (and with computerized proof assistants, even full formalization) to the above list of means. One could even suggest (as I did in Wagner 2022) that in current mathematical practice formalization has a privileged position in this list of methods in the still relatively rare cases where formalization is actually pursued. But such privileged position does not obviate the other methods in Avigad's list. Indeed, if an argument that passes the test of the above means lacks a successful formalization, we can always "save" the argument by adapting our model, axioms, or rules of inference to the needs of the argument. We might then claim that it was due to lax analysis that we had never noticed the required adaptation and even describe it as always having been there implicitly (this narrative resonates with Celucci's 2022 heuristic-analytic view of mathematics).

If we follow this alternative narrative, then rigor clearly depends on practices of translating (components of) proofs-presentations into others. Analogy, generalization, abstraction, and visualization are all translations of one presentation to another, shedding light on the original presentation by their similarity. Examples and classifications translate an argument to a narrower case or extend it to a wider class. Developing complementary approaches associates rigor, interestingly, with being able to produce arguments that are *not* too similar. And modularizing a proof is translating an interconnected argument into a edifice consisting of independent blocks.

So the landscape of philosophical approaches to rigor already turns out to look at proofs in translation, that is, as members of networks of more and less similar proofs. But is this a superfluous observation that misses the essence of rigor, or one that can really tell us something philosophically insightful about it? I claim here that it is *relative to* such a family of translations that a proof-presentation fails or lives up to ideals of rigor. In other words, embedding the proof-presentation into different proof-networks or evaluating differently the translations between the members of such networks may impact our rigor evaluations.

Let us go back to our toy example of proofs of

$$1 + 3 + \ldots + (2k - 1) + \ldots + (2n - 1) = n^2.$$

Is there a straightforward translation between the diagrammatic proofs and the inductive and set-theoretic ones? For me, based on my own mathematical trajectory, the translation from the diagram to inductive and set-theoretic arguments was very straightforward — I have experience with similar arithmetic-geometric translations from classroom exercises and historical case studies. For others, such translations may require figuring out some new connections as they go along and may therefore be considered non-routine and even creative. Translating in the other direction — from argument to diagram — is also straightforward, but in many cases the resulting diagram may be too messy to serve as a convincing argument.

From the vantage point of most versions of the standard view, this would mean that all these proofs are formalizable and therefore rigorous. But note that in order to characterize the translation as routine, and thus the proofs as rigorous, I had to expand the scope of the corpus of relevant proofs to a network that contains other diagrammatic proof presentations of other arithmetical statements and rely on my own experience. Moreover, I am not sure whether the asymmetry concerning the direction of translation tells us anything useful about rigor. What we see here is that a rigor judgment depends on which corpus of arguments is included in the analysis and on how the translations between members of this corpus are evaluated. Based on the empirical studies above, believing that there is some universal common denominator or reference point for such evaluations is not realistic.

To further the analysis, let's recall what is lost and preserved in the translations and how it affects the question of rigor. One may argue that the diagrammatic proofs leave out an edifice of number-theoretic or set-theoretic properties that are crucial to the inductive and set-theoretic proofs (such as the inductive nature of numbers or the algebraic properties of arithmetic and set-theoretic operations) and that are not properly established in the less rigidly coded diagrammatic context. Relative to this loss in translation, the diagrams may be considered as less rigorous than the inductive and set-theoretic arguments.

One may counter, however, that the inductive and set-theoretic argument leave out the obviously square character of the sum and therefore miss out on what makes the proof so convincing. As Larvor argues (2012), with due reference to Lakatos (1976), something must be lost when translating an essentially informal argument, which depends on semantic derivations, into a formal, syntactic one. If we privilege this kind of loss, the diagrammatic proof may be considered more compelling than the other proofs and thus serve as a more secure gold-standard for rigor. In other words, if we could not translate the diagram into a number-theoretic or set-theoretic argument, we would probably attempt to enrich the latter frameworks to enable a translation, rather than doubt the obviously compelling and historically prevalent diagrammatic arguments. In yet other words, we grant number-theoretic and set-theoretic frameworks their status as standard for rigor because, among other things, they capture a family of diagrammatic arguments serving as the actual gold-standard that we would be loath to forgo.

One may also evaluate the question of rigor by analogy from another direction. The diagrammatic proof may be considered as too similar to other misleading diagrammatic arguments, debunking its purported rigor (again, the corpus of proofs relevant for rigor judgment here must be extended in order to frame the problem in this manner). But is it really similar enough in actually relevant ways to misleading diagrammatic proofs so as to justify suspicion? And do we not have our fair share of experience with misleading inductive and set-theoretic arguments?[(4)] And are the latter misleading arguments similar enough to the inductive and set-theoretic argument above to cast a shadow on them?

My bottom line is that a subtle evaluation of the rigor of proofs depends on looking not at a single proof-presentation, but at a family of more and less similar presentations, and on asking various questions about how they relate to each other. Whether and why a proof-presentation is or isn't considered rigorous then depends on

[(4)] An example: Show that any finite graph without isolated vertices is connected. "Proof" by induction: The case $n = 1$ is vacuously true, and the case $n = 2$ is trivial. For the inductive step, take a graph with n vertices, and add one non-isolated vertex. Since by the inductive hypothesis the graph with n vertices is connected, and the additional vertex connects to it with at least one edge, the entire graph is connected. This "proof" notwithstanding, there's an obvious counter example for consisting of two pairs of vertices, where each vertex is connected only to its counterpart in the pair.

the selection of the corpus and the evaluation of relations between its components. This, in turn, can yield conflicting answers that undermine our ability to provide strictly consensual criteria of rigor.(5)

§ 6. — Characteristics of proofs: explanation.

When considering the explanatory value of proofs, I don't need to swim against the current. The explanatory value of proofs is acknowledged to be much less robust than rigor. Moreover, as we will see, various existing philosophical reflections already see it as depending on how we relate proof-variants, rather than as an inherent property of single proof-presentation.

I will start with what is at once an obvious and somewhat evasive definition of explanation: explanation is that which generates understanding. This approach is reviewed in Inglis and Mejía-Ramos (2021), where they point out two distinct strands. The first strand is attributed to Kelp (2016), according to whom understanding is characterized by the degree of comprehensiveness and well-connectedness of knowledge. According to this view, the better one connects more pieces of knowledge in a certain domain, the better one's understanding. This resonates with Cellucci's aesthetic approach, according to which understanding is "the recognition of the fitness of the parts to each other and to the whole" (2015, 345). The second strand is attributed to Avigad, for whom understanding a proof means "the ability to supply missing inferences, draw appropriate analogies, prove related theorems and so on" (2021, 6377). Here the focus is not about relating pieces of knowledge, but on the ability to produce new ones. Of course, these two strands

(5) I am well aware that the above discussion will antagonize many of my philosopher colleagues. I am playing the old and tired game of showing how everything is contextual, multi-faceted, and complicated. Shouldn't we, as philosophers, offer clear criteria and manageable answers, even at the cost of simplification? My motivation for the above sophistry has to do with the social image of mathematics. A popular image of mathematics represents it as the model of robust, mechanical, unassailable knowledge. This leads some people to an over-valuation of mathematics as the form of knowledge that should be trusted over all others, and other people to an under-valuation of mathematics as a meaningless game of signs devoid of any relation to our inherently uncertain lives (Ernest 2018, 2020, Wagner 2023). I think that at this point in time, in order to attenuate these two problematic attitudes, it is important that in the philosophical division of labor at least some of us insist on the fragility of mathematical judgments of rigor rather than fortify the image of their robustness. Right now, when discussing mathematics, I am happy to be a sophist.

may be strongly interdependent, the latter being perhaps an operationalization of the former.

With these approaches in mind, let us reflect on the fuzzy network of proof presentations referred to as Cantor's proof of the uncountability of the reals. This is an interesting example, because while the proof is clearly acknowledged as important and striking, its explanatory value is difficult to assess. It is not clear whether this proof provides understanding or simply implements a deus-ex-machina trick that is as awesome as it is opaque.

Here are two initial versions. In both cases, I will consider real numbers in the segment $[0, 1)$ and represent them by decimal expansions without a tail of 9s to guarantee unique decimal representation.

> *Proof 1:* Let $r_1, r_2, \ldots, r_k, \ldots$ be any enumeration of real numbers in the segment $[0, 1)$, and let r_k^j be the j-th digit after the zero in the decimal expansion of r_k. Define the real number r by making its k-th digit after the zero equal to $r_k^k - 1$ if $r_k^k > 0$ and to 1 if $r_k^k = 0$. Now, the real number r is different from each of the r_k's in the k-th digit, so r is not in the enumeration. Therefore, no enumeration of the real numbers in the segment is complete.

> *Proof 2:* Take any enumeration of real numbers in $[0, 1)$, omit the initial "0.", and arrange their digits as rows in an infinite table with each row and column corresponding to a natural number, such that each cell contains one digit. Go along the diagonal, and change its digits, avoiding changing any digit into a 9. The digits along the diagonal (after adding a "0." at the beginning) form a decimal expansion that is different in at least one place (the one on the diagonal) from any real number in any of the rows. The resulting number is therefore missing from the enumeration. Therefore, no enumeration of the real numbers in the segment is complete.

Based on my past classroom experience (the reactions and question of computer science undergraduates in an introductory logic and naïve set theory course), the former proof is completely opaque, at least in such an early tertiary pedagogical setting, unless accompanied by the second.

Now, both versions would typically be considered as essentially the same. If anything, the first simply specifies and formalizes some of the features of the second. It is no surprise to anyone with any pedagogical experience that specifying and formalizing can hinder or delay understanding, so the second proof is crucial for students' ability to reproduce and narrate the argument. But if understanding depends on the ability to relate pieces of knowledge or produce variants of a given proof, then the first proof is crucial; indeed, its formalism allows to adapt the diagonal method to other cardinalities, where the image of a discrete square of digits is problematic (and even to impossibility theorems such as Gödel's and Turing's). My point is that understanding, at least in Avigad's and Kelp's senses, has to do here with one's ability to translate between the first proof and the second, whether explicitly or implicitly. For me, the emphasis is, of course, on the suggestion that understanding depends on embedding a proof-presentation in a larger corpus and evaluating the relations between its components. As a result, the explanatory value does not reside in either proof, but in translating them into each other.

However, if we consider the notion of explanation not as that which produces understanding, but as that which connects an explanans to an explanandum, we can follow the proposals of Steiner (1978) and Kitcher (1981), which have become canonical in the explanatory proof literature. Steiner's criterion for explanation is that the result evidently depend on some "characteristic property" of the entities in the theorem. For Kitcher, an explanation is a principle that unifies facts and concepts into a theory (Inglis and Mejía-Ramos 2021, 6371-2). In these settings, we should ask: What is the characteristic property that explains the result? What is the unifying theory in which these proofs fit?

These questions can be tackled by my own classroom attempt to improve my students' understanding of the proof. To do that, I came up with the idea of preceding the above proofs by the following:

> *Claim:* There are more than three sequences of three digits.
>
> *Proof:* Take any three sequences of three digits and arrange their digits as rows in a 3-by-3 table, such that each cell contains one digit. Go along the diagonal, and change each digit. The digits along the diagonal form a sequence which is different in at least one place (the one on the diagonal) from any sequence in any of the rows.

> The resulting sequence is therefore missing from the enumeration. Therefore, no enumeration of three sequences of three digits covers all sequences of three digits.[6]

Based on classroom reactions and questions, this proof clearly facilitates the understanding of Cantor's proof. It also helps us account for the explanatory value of the proof in terms of Steiner's and Kitcher's criteria. For Kitcher, we turn the argument into a method for comparing different pairs of cardinalities (the cardinality of coordinates and that of sequences with as many coordinates) underlying Cantor's power-set theorem. For Steiner, the characteristic property is that flipping digits along the diagonal of a square array of sequences produces a new sequence.

However, both answers are weak, and in terms of Kitcher's and Steiner's view probably bear witness to a weak explanatory value of the proofs. The theorem about the cardinality of power sets is not quite a "unifying theory" — it is merely one central component of a theory of cardinalities. And the "characteristic property" formulated above is so clanky and contrived, that it is hardly a characteristic property of anything mentioned in the theorem.

If I were to try and satisfy Kitcher and Steiner to the best of my understanding of their criteria, I would follow two different trajectories. For Kitcher, I would develop (as one does in such classrooms) the elementary theory of infinite cardinalities establishing the more or less canonized family of elementary set-theoretic counting arguments. So the explanatory value of the proof would emerge from embedding it in a network of other definitions and proofs that are not necessarily similar to the original proof-presentations. This result is not surprising. In their review, Mancosu and Pincock (2012, 15), characterized Kitcher's notion of explanatory proof as being "part of a small collection of argument patterns that allows the derivation of the mathematical claims that we accept". Explanation, according to this view, is a pattern that has a relatively central place in a network of arguments, not a property of any specific proof.

For Steiner, I would try to zoom in on an explanatory property that characterizes the reals. To get there, I would propose a version of Cantor's first proof of the uncountability of the reals.

(6) Obviously, this argument is an overkill for such a simple observation. However, it can be motivated by asking for a proof that provides a simple and efficient procedure for coming up with one of the missing sequences.

> *Proof:* Consider any enumeration of real numbers. Let a_1 be the smaller and b_1 the larger of the first two reals in the enumeration. Continue following the enumeration until you reach the first two reals in the enumeration inside the segment (a_1, b_1), and denote the smaller a_2 and the larger b_2. Continue in this way to produce nested segments (a_n, b_n). Note that no number in the segment (a_n, b_n) could have appeared in the enumeration before a_n and b_n.
>
> There are two possibilities. Either the process produces a last segment (a_n, b_n) and the enumeration includes at most one real number inside it, in which case the enumeration is clearly incomplete. Alternatively, the segments converge to a segment $[a, b]$, where a and b may be equal or different. Either way, the number a in this segment never appears in the enumeration. Therefore, in this case too the enumeration is incomplete.

At first glance, this proof is very different from the previous one (no decimal representations, no diagonal, a clear reliance on the topological completeness of the reals). But if we are *creatively and actively looking for an explanation* in these proofs, we can actually find a characteristic property that explains the result. In both proofs one gradually constructs a new number that does not belong to the given enumeration by accumulating partial convergent information (initial digits and containing segments respectively). This information distinguishes this new number from all previous numbers in the enumeration, and by the end of the proof, from all numbers in the enumeration. The emergent characteristic property of the reals is completeness: the topological completeness that defines the reals in the usual topology, and, emerging as surprisingly closely related to the former, the completeness of the product topology over the set of sequences of digits. At this point, I, at least, gain a sense of a well-articulated explanans.

Based on this, a Steiner-like explanation, too, emerges not from a single proof-presentation, but from a collection of related proofs — or, rather, from our tentative ability to extract a common characteristic from a family of proofs. Again, that Steiner's view derives the explanatory value of a proof from its relation to other proofs is not a surprise. Coming back to Mancosu and Pincock (2012, 16), they characterize Steiner's notion of explanation as one where a certain result clearly depends on a certain property in such a way that "the explanatory proof be part of a family of proofs where this property is varied". What Steiner had in mind here are proofs where the *failure* of the characteristic property would produce a different result, but in order to get there, one first has to extract a characteristic property, and different collections of proofs may "select" (or lead a mathematician to articulate) different characteristic properties.

The bottom line is that in all these versions of the notion of explanatory proof, it is the choice of a relevant corpus and our somewhat contingent and creative evaluation of the relations between the components of the corpus that are responsible for the emergence of understanding and explanation. Starting from the same proof-presentation, different ambient corpora and different evaluations of the relations between their members may lead to different kinds and qualities of explanation.

§ 7. — Historical practices of proofs as consisting of multiple parallel arguments.

The above discussion is set in a contemporary mathematical practice where a proof is usually thought of as a single argument leading from premises to conclusion. This single-mindedness of proofs obviously motivates attempts to individuate proofs. The last point I would like to make here is that in some historical contexts, "a proof" (to the extent that we can think of it as one thing) was not necessarily conceived as a single line of argumentation from premises to conclusion.

The first historical context is that of mathematical analysis and synthesis in Arabic geometry (building on classical Greek conceptions). Analysis assumes that a required construction is attained and, given ambient postulates, derives the conditions and tools that would enable it. Synthesis derives the construction from those conditions, tools, and ambient postulates. Analysis was associated with discovery and explanation, whereas synthesis was associated with validation.

Concerning this pair of methods, Berggren (1990, 42) wrote: "it appears that during the tenth century some important geometers came to the conclusion that a complete solution to a problem should contain not only the synthesis but also the analysis". The notion of proof or solution was therefore seen, at least by some prominent mathematicians, as not reducible to a single line of argument, whether analytic or synthetic, but consisted of complementary parallel argument.

Another context in which proofs can be said to be inherently "multiple" is that of canonical Chinese commentaries. Chemla makes a note of this phenomenon in several papers, such as (1997, 2008), where a nuanced historical analysis is provided, so I will allow myself here to take anachronistic shortcuts. We will consider here the example of adding fractions in Liu Hui's 3rd century commentary on the *Nine Chapters*, the most famous Chinese mathematical classic usually dated to the 1st century BCE or CE.

For an addition of the form $\frac{a}{c} + \frac{b}{d} = \frac{ad+bc}{cd}$, Liu Hui's argument goes along lines that we might expect. It recalls that fractions can be expanded and reduced by multiplying or dividing the numerator and denominator by some factor. Then, for fractions to add up, we need to reach a common denominator, which is obtained by multiplying the denominators by appropriate factors. In order to maintain the values of the fractions, we should multiply the numerators by the corresponding factors. Since we have a common denominator, the addition is simply that of the numerators divided by the common denominator.

However, during the proof, Liu Hui adds some additional terminology. The multiplication of denominators is called "equalizing". The corresponding operation on numerators is called "homogenizing". This seems like a redundant terminological detour in the proof, which is followed by further apparently redundant discussions of the ontological classification and transformation of objects. Read with our current expectations from proofs, the terminological detour and ontological discussion seem out of place. However, Chemla offers an insightful explanation. She shows that the use of the terms "equalize" and "homogenize" associates these fraction-related operations with other mathematical operations, grouping them together into an overarching class.

For example, the terms "equalize" and "homogenize" also appear when solving systems of linear equations. Starting from the system

$$\begin{cases} ax + cy = e \\ bx + dy = f. \end{cases}$$

The operation "equalization" is applied to the coefficients of x and "homogenization" to the other coefficients, yielding:

$$\begin{cases} bax + bcy = be \\ abx + ady = af. \end{cases}$$

Subtracting the second equation from the first, we can derive a solution.

"Equalization" and "homogenization" are further associated in Liu Hui's and in related work with other mathematical maneuvers, as well as with more general ontological principles. The purport of the proof is therefore double: to verify the procedure for adding fractions, but also to embed it within a broader mathematical-philosophical motif that underwrites its validity. For Liu Hui, these are not two separate tasks, but a unified goal of his various commentaries. His proof practice thus seamlessly integrates different kinds of arguments at different "scales". A proof is not reducible to just one of those arguments.

The final example will depend on my analysis (Wagner 2018) of the 16th century Sanskrit mathematical treatise *Kriyākramakarī* by Snakara Variyar and Narayana (edited in Sarma 1975), which belongs to the Kerala school of mathematical astronomy. My main thesis was that the *Kriyākramakarī* attempts to relate what appear to be disparate and unrelated pieces of mathematical knowledge and techniques from different mathematical domains. I mentioned that various claims are offered several distinct proofs but I failed to note that characterizing these proofs as "distinct", even though they are sometimes appended seamlessly to each other, might be anachronistic and misleading.

For example, the treatise considers what we would call Pythagorean triplets, such as $2nm$, $m^2 - n^2$, $m^2 + n^2$, where the sum of squares of two integers (or rational numbers) equals the square of another integer (or rational number) (Wagner 2018, 116-7). The argument depends on the fact that a triangle circumscribed in a circle, such that one of its sides is the diameter, is always a right-angled triangle and vice-versa. Therefore, the square of the larger side must equal the sum of squares of the others. It also depends on the following, rather intricate result concerning such triangles (a variant of formulas for the sine and cosine of the sum of two angles). This result is attributed to 14^{th} century mathematician Madhava, and I paraphrase it with anachronistic notation as follows:

> Suppose a circle with radius R circumscribes a triangle with sides A, B, and $2R$, and another triangle with sides C, D, and $2R$. Then the same circle will also circumscribe a triangle with sides $\frac{(AD+BC)}{2R}$, $\frac{(BD-AC)}{2R}$, and $2R$.

Now, consider a right-angled triangle with integer (or rational) sides n, m, and hypotenuse equal to the (not necessarily rational) diameter $\sqrt{n^2+m^2}$ of the circumscribing circle. If we apply the above rule to that triangle and itself (letting it play the role of *both* triangles in the above claim), we get a new right-angled triangle circumscribed in the same circle with sides $\frac{2nm}{\sqrt{n^2+m^2}}$, $\frac{m^2-n^2}{\sqrt{n^2+m^2}}$. Stretching the entire configuration by the factor $\sqrt{n^2+m^2}$, we obtain a right-angled triangle with sides $2nm$, m^2-n^2, and a hypotenuse equal to the diameter of the circumscribing circle, n^2+m^2.

Since one of the sides of this circumscribed triangle is the diameter, it is a right-angled triangle, and the sum of squares of the first two integers (or rational numbers) equals the square of the third. However, instead of concluding the argument with this observation, the versed proof continues seamlessly, without any hiatus, to verify the result by algebraic manipulation, expanding the squares of the first two expressions and comparing them to the square of the third (Sarma 1975, 294-5, verses 1-8). No note is made by the original authors that this is a complementary argument or that the conclusion is already proved and is now being re-proved (a subsequent algebraic argument in the following verses is clearly demarcated by the authors as a new, alternate argument).

The continuous nature of the proof may not be so surprising if we consider current literature on the epistemology of Indian mathematical proofs. Srinivas (2005), among others, noted that Indian proofs do not start with axiomatic foundations and do not attempt to pretend to attain inviolable certainty. They can rely on observing diagrams, mathematical analogies, or analogies with physical situations. In fact, many Indian logical schools preferred observation and critically scrutinized authority over inference, and inference was usually presented in the framework of an analogy (see Matilal 1998, ch. 1). However, this does not mean that Indian mathematical proofs were some sort of inductive generalizations — such proofs were evaluated by their ability to provide compelling reasons that would convince their audience. In such a setting, a "double" argument is more convincing that just one of its components. In fact, there's no reason to think about such an argument as double. Instead, it can be viewed as a duly comprehensive analysis of a certain mathematical situation in a framework where convincing is evaluated over certainty.

What these Arabic, Chinese, and Indian examples have in common is that they attest to historical practices where proofs were not articulated and individuated according to our current expectations. There are also European examples of multi-faceted proofs that reach well into the 20$^{\text{th}}$ century (such as Cantor's combination of mathematical and theological arguments and Gödel's inclusion of semantic and syntactic strands in his 1931 publication of his incompleteness proof). From such a historical-global perspective, the idea of reducing proofs to single-argument presentations appears much less natural than it would be for people trained in current mathematical practices. A structuralist network view of proofs therefore has the added value of making the philosophical account applicable to a wider mathematical context.

§ 8. — Conclusion.

Over the last few years, I have become increasingly concerned by the facility with which philosophers (including myself) have been discussing proofs as obviously individuated objects with inherent properties. In this paper, I have been making explicit my doubts concerning this view and suggested an alternative structuralist framework. In this framework, a proof attains its meaning and properties by embedding it in various corpora of proofs and interpreting the relations of translatability and distinction between elements of these corpora.

According to this view, to judge whether a proof is, for example, rigorous or explanatory, we need to situate the proof in a corpus, study how different elements in the corpus relate to each other, figure out to what extent different proof-presentations in the corpus differ or align from the point of view of relevant considerations, and then derive the resulting not-necessarily-consensual judgments about their properties. The relativity to corpus and interpretation explains why such judgments are fraught with difficulties.

Specifically in the context of rigor, I showed how some prominent philosophical approaches depend on embedding a proof in a collection of proofs and evaluating the salience of translations between members of this collection in a manner that may depend on agents and contexts. Moreover, by embedding a proof in a specific corpus with its "gold standards" for rigor, we may arrive at different rigor evaluations. Thinking about proofs as embedded in networks of textual and performative presentations captures these phenomena.

Similarly, in the context of explanation, I showed how prominent accounts of understanding and explanation depend on embedding a proof in a corpus of proofs. The examples then show how embedding the proof in different corpora and extracting different relations between the members of each corpus may tease out different kinds of explanations that fit different definitions of explanation. Evaluating the explanatory value of proofs therefore depends not on individuating proofs but on their relational place in a network.

I also showed that historical practices do not always handle each proof as a single argument starting from premises and reaching a conclusion. This means that we may think of some proofs as inherently plural in nature. In turn, this marks the assumption that proofs should be individuated — an assumption leading to problematic philosophical expectations when evaluating proofs — as a historical contingency. The structuralist-semiotic approach to proofs presented in this paper helps us resist such assumption and expectations.

I would like to end with a bit of a speculation. Proof assistants are growing in popularity among mathematicians, and some believe that automated proving is within our reach. If this ends up being the case, then the main task of mathematicians will no longer be to write proofs, but rather to interpret automatically generated proofs to gain a more relational and comprehensive mathematical understanding. This kind of interpretive practice is already an important part of mathematicians' work, but with such a sci-fi vision in mind, their status may change from being more private and back-office activities to the foreground of mathematical work. The mathematician will then be less a theorem prover and more of a hermeneutic subject, like a literary theoretician (at least until AI can perform this task as well). In that event, the structuralist-semiotic framework suggested here may become even more salient for our philosophical engagement with mathematical proofs.

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