Preprint as on June 4, 2021.

Why did Fermat believe he had 'a truly marvellous demonstration' of FLT?

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Abstract. Conventional wisdom dictates that proofs of mathematical propositions should be treated as *necessary* and *sufficient* for entailing *significant* mathematical truths *only* if the proofs are expressed in a—minimally, deemed consistent—formal mathematical theory in terms of: • Axioms/Axiom schemas • Rules of Deduction • Definitions • Lemmas • Theorems • Corollaries. Whilst Andrew Wiles' proof of FLT, which appeals essentially to geometrical properties of *real* and *complex* numbers, can be treated as meeting this criteria, it nevertheless leaves two questions unanswered: (i) Why is $x^n + y^n = z^n$ solvable only for $n < 3$? (ii) What technique might Fermat have used that led him to, even if only briefly, believe he had 'a truly marvellous demonstration' of FLT? Prevailing post-Wiles wisdom—leaving (i) essentially unaddressed—dismisses Fermat's claim as a conjecture without a plausible proof of FLT. However, we posit that providing *evidence-based* answers to both queries is *necessary* not only for treating FLT as *significant*, but also for *understanding* why FLT can be treated as a *true* arithmetical proposition. We thus argue that proving a theorem formally from explicit, and implicit, premises/axioms using currently accepted rules of deduction is a *meaningless* game, of little scientific value, in the absence of *evidence* that has already established—unambiguously—why the premises/axioms and rules of deduction can be treated, and categorically communicated, as *pre-formal* truths in Marcus Pantsar's sense. Consequently, *only evidence-based*, *pre-formal*, truth can entail formal provability; and the formal proof of any *significant* mathematical theorem *cannot* entail its *pre-formal* truth as *evidence-based*. It can *only* identify the explicit/implicit premises that have been used to *evidence* the, *already established*, *pre-formal* truth of a mathematical proposition. Hence *visualising* and *understanding* the *evidence-based*, *pre-formal*, truth of a mathematical proposition is the *only* raison d'être for *subsequently* seeking a formal proof of the proposition within a formal mathematical language (whether first-order or second order set theory, arithmetic, geometry, etc.) By this yardstick Andrew Wiles' proof of FLT fails to meet the required, *evidence-based*, criteria for entailing a true arithmetical proposition. By appealing, however, to a plausible resolution of some philosophical ambiguities concerning the relation between *evidence-based*, *pre-formal*, truth and formal provability, we offer two scenarios as to why/how Fermat could have laconically concluded in his recorded noting that FLT is a true arithmetical proposition.

Keywords. symmetrically centered configuration, evidence-based reasoning, Fermat's Last Theorem, hypercube, pre-formal mathematics, unique isomorphism. **2010 Mathematics Subject Classification.** 01-02, 03C99, 11D41, 11D99, 11G99, 11H99

1. Prologue: A 'Disembodied' Proof of FLT (A *fictional* narrative)

1.A. *When* is $x^2 + y^2 = z^2$ solvable for $x, y, z \in \mathbb{N}$?

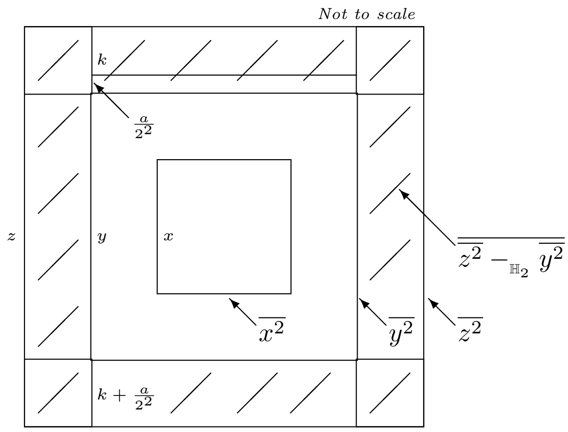

Fig.a

1. Sometime prior to 1637, Pierre de Fermat received for endorsement a jigsaw puzzle J_2 (illustrated by Fig.a), ostensibly consisting of one square tile of side y plus:

(a) 4 rectangular tiles with dimensions $y \times (k + \frac{a}{2^2})$; and

(b) 4 square tiles of side $(k + \frac{a}{2^2})$,

where (a) plus (b) had been certified by the Manufacturer as having been cut and re-assembled from a square tile of side x, such that $x^2 + y^2 = z^2$ and $z = y + 2(k + \frac{a}{2^2})$, with $x, y, z, k, a \in \mathbb{N}$.

2. However, Fermat found that the 4 square tiles of side $(k + \frac{a}{2^2})$ were missing, and requested from the Manufacturer a matching set of square tiles as per the packing list.

3. Customer Service checked with Stores, and confirmed they had a matching square tile R_2 of side $2(k+\frac{a}{2^2})$ and area $4(k + \frac{a}{2^2})^2$, which they would happily have cut into a matching set of square tiles that would complete the puzzle J_2 to Fermat's satisfaction.

4. Customer Service then directed Stores to issue the square tile R_2 to the Factory, with instructions to cut R_2 into 'smaller' square tiles.

5. The Factory cut R_2 as instructed, and despatched a set S_2 of 'smaller' square tiles to Fermat.

6. A bemused Fermat observed, however, that the Factory had, either carelessly or in ignorance, cut the square tile R_2 of side $2(k + \frac{a}{2^2})$ and volume $4(k + \frac{a}{2^2})^2$ into a set of 'smaller' square tiles that did not match the set of 4 square tiles of side $(k + \frac{a}{2^2})$ as specified in the packing list!

7. ***Beyond bemusement, Fermat also realised that the challenge in endorsing the Manufacturer's claim lay in formally defining what it would mean for the set of rectangular tiles (a) and (b) to have been cut and re-assembled from a square tile of side x.***

8. After considerable consideration, Fermat decided *any* such definition[1] should entail that: *Every* configuration of rectangular tiles resulting from cutting the set of tiles (a) and (b) into smaller rectangular tiles which, after a re-configuration, claims to *well-define* a square tile of side x must, when combined suitably with the square tile of side y, also *well-define* the square tile of side z.

9. Following a thorough investigation at the Factory, Fermat concluded he could confirm, and endorse[2], that if x, y, z were stipulated as a Pythagorean triple[3], then *any* such configuration (as in §1.A.(8)) of the 2-D tiles (a) plus (b) supplied by the Manufacturer would indeed, in the above sense, *well-define* a square tile of side x such that $x^2 + y^2 = z^2$, as certified.

1.B. *Why* $x^3 + y^3 = z^3$ is *not* solvable for $x, y, z \in \mathbb{N}$

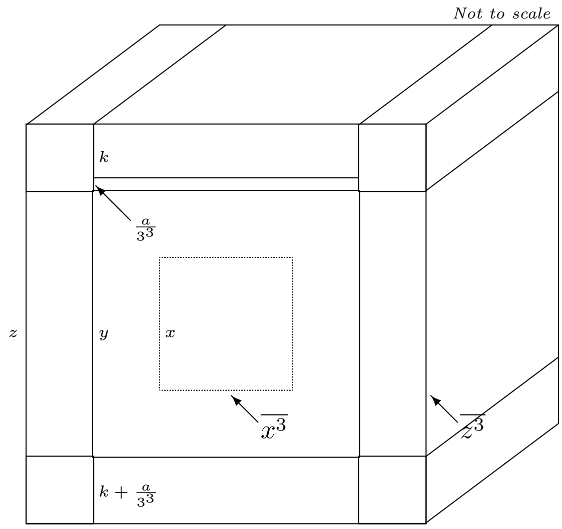

Fig.b

[1]Expressed formally in §2.A. as Definitions 1, 2, and 3. It is not obvious whether Andrew Wiles' (essentially set-theoretic) 1995 proof of FLT, as outlined by Michael Harris in [18] (see also §6.), *implicitly* admits—or needs to *explicitly* admit—corresponding definitions.

[2]Without, as was not unusual with Fermat (see [28], p.42), bothering to provide a 'proof'; but see §3.A.(a).

[3]Such as (3, 4, 5), (5, 12, 13), (161, 240, 289), etc.; of which there are an infinity of *essentially* different sets.

1. Shortly thereafter, Fermat received a further request for endorsement, this time for a LEGO blocks puzzle J_3 (illustrated by Fig.b), ostensibly consisting of one cube of side y plus:

   (a) 6 parallelepiped blocks with base y^2 and height $(k + \frac{a}{3^3})$,

   (b) 12 parallelepiped blocks with base $(k + \frac{a}{3^3})^2$ and height y, and

   (c) 8 cube blocks of side $(k + \frac{a}{3^3})$,

   where (a) plus (b) plus (c) had been certified by the Manufacturer as having been cut and re-assembled from a LEGO cube block of side x, such that $x^3 + y^3 = z^3$ and $z = y + 2(k + \frac{a}{3^3})$, with $x, y, z, k, a \in \mathbb{N}$.

2. To his exasperation, Fermat again found the 8 cube blocks of side $(k + \frac{a}{3^3})$ inexplicably missing, and irritably requested from the Manufacturer a matching set of cube blocks as per the packing list.

3. An embarrassed Customer Service checked with Stores, and confirmed they did have a matching cube block R_3 of side $2(k + \frac{a}{3^3})$ and volume $8(k + \frac{a}{3^3})^3$, which they were only too willing to have cut into a matching set of cube blocks that would complete the puzzle J_3 to Fermat's satisfaction.

4. Customer Service then directed Stores to issue the cube block R_3 to the Factory, with instructions to cut R_3 into 'smaller' cubes.

5. The Factory cut R_3 as instructed, and despatched a set S_3 of 'smaller' cubes to Fermat.

6. However Fermat observed, now more intrigued than irritated, that the Factory had, either carelessly or in ignorance, cut the cube block R_3 of side $2(k + \frac{a}{3^3})$ and volume $8(k + \frac{a}{3^3})^3$ into 9 cube blocks of side $(\frac{2}{3})(k + \frac{a}{3^3})$, instead of 8 cube blocks of side $(k + \frac{a}{3^3})$ as specified in the packing list.

7. Since the set S_3 could not complete the puzzle J_3, Fermat could not endorse that: *Every* configuration of 3-D LEGO blocks resulting from cutting the set of 3-D LEGO blocks (a) plus (b) plus (c) into smaller 3-D LEGO blocks which, after a re-configuration, claims to *well-define* a LEGO cube block of side x (as certified[4] by the Manufacturer), when combined suitably with the 3-D LEGO cube block y^3, also *well-defines* a 3-D LEGO cube block of side z.

8. Fermat concluded, moreover, that the above reasoning—which he treated as entailing that $x^3 + y^3 = z^3$ is not solvable for $x, y, z \in \mathbb{N}$—ought to hold by symmetry in the general case; and laconically laid claim in the margin of his copy of Diophantus' *Arithmetica* to 'a truly marvellous demonstration' that there were no integer solutions to $x^n + y^n = z^n$ for $n > 2$.

1.C. *Why* $x^p + y^p = z^p$ is *not* solvable for $x, y, z \in \mathbb{N}$ and any prime $p > 2$

1. Almost 400 years later, disembodied and drifting desultorily in a n-dimensional universe beyond human conception, the shades of Fermat welcomed receipt, for endorsement, of a p-dimensional LEGO puzzle J_p (with prime $p > 3,\ n \geq p \in \mathbb{N}$) from a n-D Manufacturer, ostensibly consisting of one p-D hypercube of side y^p, denoted by $\overline{y^p}$ in a p-D Euclidean space $\mathbb{H}_p$, plus:

   (a) $2.^pC_1$ p-D hyper-objects, each denoted by $\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}}$ with hyper-dimensions:

   $$(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} \underbrace{y \times_{\mathbb{H}_p} y \times_{\mathbb{H}_p} \ldots \times_{\mathbb{H}_p} y}_{(p-1)};$$

   (b) $2^2.^pC_2$ p-D hyper-objects, each denoted by $\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}}$ with hyper-dimensions:

   $$(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} (k + \frac{a}{p^p}) \times_{\mathbb{H}_p} \underbrace{y \times_{\mathbb{H}_p} y \times_{\mathbb{H}_p} \ldots \times_{\mathbb{H}_p} y}_{(p-2)};$$

   ...

   (c) 2^p p-D hypercubes, each denoted by $\overline{(k + \frac{a}{p^p})^p}$ with sides $(k + \frac{a}{p^p})$;

   where (a) plus (b) plus ... plus (c) had been certified by the n-D Manufacturer as having been cut and re-assembled from a p-D hypercube of side x, such that $x^p + y^p = z^p$ and $z = y + 2(k + \frac{a}{p^p})$, with $x, y, z, k, a \in \mathbb{N}$.

[4] And, as the jurist (see [28], p.37) in Fermat had begun to suspect, *misleadingly*.

2. The, by now fascinated, disembodied Fermat again found, as almost anticipated, that the 2^p p-D hypercubes of side $(k + \frac{a}{p^p})$ were inexplicably missing, and patiently requested from the n-D Manufacturer a matching set of p-D hypercubes as per the packing list.

3. The n-D Customer Service checked with their n-D Stores, and confirmed they did have a matching p-D hypercube R_p of side $2(k + \frac{a}{p^p})$ and hyper-volume $2^p.(k + \frac{a}{p^p})^p$, which they would happily have cut into a matching set of p-D hypercubes that would complete the puzzle J_p, and satisfy the disembodied Fermat.

4. The n-D Customer Service then directed their n-D Stores to issue the p-D hypercube R_p to their n-D Factory, with instructions to cut R_p into 'smaller' p-D hypercubes.

5. The n-D Factory cut R_p as instructed, and despatched a set S_p of 'smaller' p-D hypercubes to the disembodied Fermat.

6. The disembodied Fermat now observed that the n-D Factory too had, not entirely *unexpectedly* but yet *inexplicably*, cut the p-D hypercube R_p, of side $2(k + \frac{a}{p^p})$ and hyper-volume $2^p.(k + \frac{a}{p^p})^p$, into p^p p-D hypercubes of side $(\frac{2}{p})(k + \frac{a}{p^p})$, instead of the 2^p p-D hypercubes of side $(k + \frac{a}{p^p})$ specified in the packing list.

7. Since $2^p \not| \, p^p$, the set S_p could not complete the puzzle J_p; whence the disembodied Fermat could *not* endorse that: *Every* configuration of p-D hyper-objects resulting from 'cutting' the set of p-D hyper-objects (a) plus (b) plus ... plus (c) into 'smaller' p-D hyper-objects which, after a re-configuration, claims to *well-define* a p-D hypercube of side x, when 'combined' suitably with the p-D hypercube y^p, also *well-defines* a p-D hypercube of side z (as certified by the n-D Manufacturer).

8. Relieved after centuries of uncertainty, a disembodied Fermat concluded contentedly that, since the p-D hypercube $\overline{x^p}$ of side x could not be *well-defined* by the p-D hyper-objects (a) plus (b) plus ... plus (c), there were no integer solutions to $x^p + y^p = z^p$ for $p > 3$; thus justifying the erstwhile mortal Fermat's claim (FLT), which entailed that $x^p + y^p = z^p$ was unsolvable for $p > 2$.

9. Moreover, since the mortal Fermat had shown that $x^4 + y^4 = z^4$ is unsolvable for $x, y, z \in \mathbb{N}$ (see [28], p.98), the preceding now entailed (see [28], p.99) that $x^n + y^n = z^n$ is unsolvable for $x, y, z \in \mathbb{N}$ if $n > 2$!

2. Introduction

Fermat's Last Theorem FLT states that no three positive integers x, y, z satisfy the equation $x^n + y^n = z^n$ for any integer value of n greater than 2. FLT has been made famous, literally and literarily (see [28], p.73) beyond it's innate challenge for mathematicians, by Pierre de Fermat's posthumously revealed remarks, written around 1637 in the margin of his copy of Diophantus' *Arithmetica*:

> "It is impossible for a cube to be written as a sum of two cubes or a fourth power to be written as the sum of two fourth powers or, in general, for any number which is a power greater than the second to be written as a sum of two like powers. ... I have a truly marvellous demonstration of this proposition which this margin is too narrow to contain".
>
> *... Singh: [28], p.66, An English translation of Fermat's marginal noting in Latin.*

For 358 years, FLT remained unproven; until the 108-page proof [31]—appealing *inexplicably* to *geometrical* properties of *real* and *complex* numbers in order to prove an essentially *arithmetical* problem over the *natural numbers*—was published in 1995 by Andrew Wiles in the *Annals of Mathematics*. It proved an equivalence between *geometric* properties of elliptic curves and, seemingly disparate, modular forms that could cogently be argued[5] as entailing FLT from their *explicit* (and *implicitly* set-theoretical) premises.

What yet remains unanswered, though, is whether, and if so what, Fermat might have 'realised' he had 'briefly deluded himself' as having solved 'with an irretrievable idea' (see also [28], p.128):

[5] Detailed consideration of Wiles' proof (see [31], [11]) lies beyond the scope, and competence, of this *pre-formal* (see §5.; also [2], §1.D) perspective; which only seeks an 'elementary' understanding of why $x^n + y^n = z^n$ is solvable *only* for $n < 3$ if $x, y, z, n \in \mathbb{N}$. However we address, in §6., Michael Harris' outline (see [18], *Other publications*, #21) of the logical steps in Wiles' 'analytic' proof, in order to highlight how these 'mirror' the logical steps in the 'elementary' proof (in §3.) of this reconstruction of the putative reasoning behind Fermat's laconic marginal noting.

> "It is not known whether Fermat had actually found a valid proof for all exponents n, but it appears unlikely. Only one related proof by him has survived, namely for the case $n = 4$, as described in the section Proofs for specific exponents. While Fermat posed the cases of $n = 4$ and of $n = 3$ as challenges to his mathematical correspondents, such as Marin Mersenne, Blaise Pascal, and John Wallis, he never posed the general case. Moreover, in the last thirty years of his life, Fermat never again wrote of his "truly marvelous proof" of the general case, and never published it. Van der Poorten suggests that while the absence of a proof is insignificant, the lack of challenges means Fermat realised he did not have a proof; he quotes Weil as saying Fermat must have briefly deluded himself with an irretrievable idea.
>
> The techniques Fermat might have used in such a "marvelous proof" are unknown.
>
> *... Wikipedia: https://en.wikipedia.org/wiki/Fermat%27s_Last_Theorem, accessed 10th October 2020.*

Wiles' proof thus leaves two questions unaddressed, which we shall seek to illuminate by a reconstruction—from an inter-disciplinary, *pre-formal*[6] (see §5.), perspective—of:

(i) *What* argument or technique might Fermat have used that led him to, even if only briefly, believe he had 'a truly marvellous demonstration' of FLT?

> "Wiles's proof of Fermat's Last Theorem relies on verifying a certain conjecture born in the 1950s. The argument exploits a series of mathematical techniques developed in the last decade, some of which were invented by Wiles himself. The proof is a masterpiece of modern mathematics, which leads to the inevitable conclusion that Wiles's proof of the Last Theorem is not the same as Fermat's. Fermat wrote that his proof would not fit into the margin of his copy of Diaphantus's *Arithmetica*, and Wiles's 100 pages of dense mathematics certainly fulfills this criterion, but surely the Frenchman did not invent modular forms, the Taniyama-Shimura conjecture, Galois groups, and the Kolyvagin-Flach method centuries before anyone else.
>
> If Fermat did not have Wiles's proof, then what did he have?"
>
> *... Singh: [28], p.307.*

(ii) *Why* is $x^n + y^n = z^n$ solvable *only* for $n = 2$?[7]

A curious feature of recorded, post-Fermat, attempts to prove FLT has been the, seemingly universal[8], focus on seeking a formal proof, and understanding, of *only* (as claimed by Fermat) why $x^n + y^n = z^n$ is unsolvable for both specific, and general, values of $n > 2$ when $x, y, z, n \in \mathbb{N}$.

> **Comment**: 'Curious' since, for instance, if FLT[9] is not provable in PA, it would follow by [1], Theorem 7.1 (p.41)[10], that *no* deterministic algorithm TM could, for any *specified* $n > 2$, *evidence* that $x^n + y^n = z^n$ is unsolvable[11]. In which case, even if FLT can be *evidenced* as numeral-wise *true*[12] under a *well-defined* interpretation of PA over $\mathbb{N}$[13], seeking to understand *why* $x^n + y^n = z^n$ is unsolvable for *all* $n > 2$ may be futile. Instead, one could reasonably expect a better insight (see §3.A.) by seeking *why* $x^n + y^n = z^n$ is solvable for $n = 2$ (and trivially for $n = 1$), but not for $n = 3$.

Moreover, Michael Harris' recent claim[14] that 'Wiles' proof, complicated as it is, has a simple underlying structure that is easy to convey to a lay audience', implicitly admits that such an understanding yet remains as elusive as was reflected in Keith Devlin's 1994 observation:

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[6] The need for distinguishing between *belief-based* 'informal', and *evidence-based* 'pre-formal', reasoning is addressed by Markus Pantsar in [26]; see also §5..

[7] The Diophantine equation is, of course, trivially solvable for $n = 1$; and solvable for $n = 2$ by Pythagoras' Theorem.

[8] See [10], Chapter XXVI, pp.731-776; [5], pp.303-304; [28], pp.115-117, 126-127, & 251-252; [21], p.657, *§3.1 Germain's plan for proving Fermat's Last Theorem*; [8], *Abstract*.

[9] Strictly speaking the PA-formula, say [FLT], expressing FLT in PA.

[10] *A PA formula* $[F(x)]$ *is PA-provable if, and only if,* $[F(x)]$ *is algorithmically computable as always true in* $\mathbb{N}$.

[11] Since FLT is not then algorithmically *computable* as an always *true* arithmetical proposition by [1], Definition 2, p.37: *A number theoretical relation* $F(x)$ *is algorithmically computable if, and only if, there is an algorithm* AL_F *that can provide objective evidence for deciding the truth/falsity of each proposition in the denumerable sequence* $F(1), F(2), \ldots$.

[12] In the sense of being algorithmically *verifiable* as a *true* arithmetical proposition for any *specified* instantiation by [1], Definition 1, p.37: *A number-theoretical relation* $F(x)$ *is algorithmically verifiable if, and only if, for any given natural number* n*, there is an algorithm* $AL_{F,n}$ *which can provide objective evidence for deciding the truth/falsity of each proposition in the finite sequence* $\{F(1), F(2), \ldots, F(n)\}$.

[13] In other words, for any *specified* $n > 2$, there would be some deterministic algorithm TM_n which could *evidence* $x^n + y^n = z^n$ as unsolvable for *only* that *specified* value of n; or, equivalently, for all values $\leq n$.

[14] In [18], *Other publications*, #21; see also §6..

> "Wiles made his claim at the end of a series of three lectures he gave at a small meeting of number-theorists at the Isaac Newton Institute at Cambridge, England. The powerful new techniques he outlined in his proof, together with his own track record as a research mathematician, were enough to convince the audience that the new proof was probably correct. And, since that audience included many of the world's most highly qualified experts in the area, that was good enough for everyone else. Such was the complexity of Wiles' argument that, even with a copy of his 200-page proof, most of us would in any case have to rely on the judgement of these experts."
> *. . . Devlin: [12].*

A contributory factor obscuring such understanding could be that even definitive expositions of Wiles' reasoning—such as, for instance, [18]—may not (as, we argue in §6., they should) view the 'proof' as needing, or even being capable of, 'enhancement' by seeking to *formally* justify the *necessity* of appeal to arcane *geometrical* properties, of *real* and *complex* numbers[15], for concluding the *logical truth* of putative Diophantine solutions of, essentially, *arithmetical* propositions when such putative solutions are expressed *arithmetically* as elliptic curves, but interpreted *geometrically*.

2.A. Could *this* have been Fermat's '*truly marvellous demonstration*'?

Some insight into *why* $x^n + y^n = z^n$ can be treated *pre-formally* as true *only* for $n < 3$ follows if we note that[16]:

If $x^n + y^n = z^n$ for $x, y, z, n \in \mathbb{N}$, and $z = y + 2(k + \frac{a}{n^n})$ (see Figs.1-3), then:

(i) $x^n = 2.^nC_1(k + \frac{a}{n^n})y^{n-1} + 2^2.^nC_2(k + \frac{a}{n^n})^2y^{n-2} + \ldots + 2^n(k + \frac{a}{n^n})^n$

FLT is then equivalent to proving that the *necessary* and *sufficient* conditions which, for any *specified* $n \geq 1 \in \mathbb{N}$, admit some $y, z \geq 1 \in \mathbb{N}$ that yield a representation of x^n as in §2.A.(i), hold only for $n < 3$.

However, it is not obvious how, or even whether, such conditions are *formally* definable *within* PA (see §3.A.(b)). It is not unreasonable to assume, therefore, that Fermat could have intuited some such perspective *pre-formally* and, sensing that there may be a need to appeal beyond the ambit of arithmetical *truth* and *proof*, instead visualised *geometrically* that, for any pair of natural numbers $z > y$:

(1) We can take a string (see Fig.1), say $\overline{z}$, of length z units, cut off a central section $\overline{y}$ of length y units, and we will ***always*** (courtesy human self-evidence) have a symmetrically centered configuration, denoted by $\mathbb{C}_{Sym}(\overline{\overline{z} -_{\mathbb{H}_1} \overline{y}})$, of a 1-dimensional object, denoted by $\overline{\overline{z} -_{\mathbb{H}_1} \overline{y}}$, consisting of two separated pieces of length $k + a$ units, which can be defined as *uniquely isomorphic* under change of scale (see Definition 2):

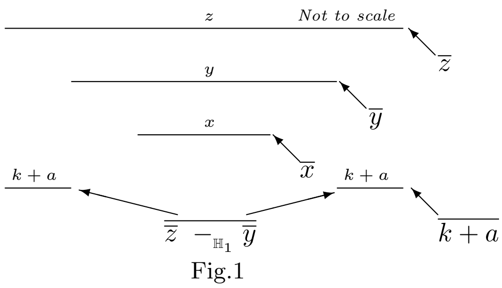

Fig.1

- by cutting into smaller units a string $\overline{x}$ of length x units, where x is also a natural number,
- and re-assembling the smaller lengths to form the symmetrically centered configuration:

  $$\mathbb{C}_{Sym}(\overline{\overline{z} -_{\mathbb{H}_1} \overline{y}}) =_{\mathbb{H}_1} 2\overline{k + a},$$

- such that any two such re-assemblies are *isomorphic* upto *uniqueness* (by Definition 2);

[15] A justification the *pre-formal* proof of FLT in §3. seeks to achieve more transparently by identifying, and generalising, the *necessary* and *sufficient* geometrical properties which entail the specific case of FLT for $n = 3$, in the *pre-formal* argument in §3.A.(b), *without* appeal to properties of *real* and *complex* numbers.

[16] Compare with the outline of Wiles' proof in Harris' lay exposition [18] (see §6., eqns. (A)-(E)), which views any putative integral solution $a, b, c \in \mathbb{N}$ of the, essentially *arithmetical*, equation $a^p + b^p = c^p$ (p an odd prime) *geometrically* as defining the elliptic curve $y^2 = x(x - a^p)(x + b^p)$, and identifies the latter's Galois representation with a unique modular form that entails a contradiction; whence $a^p + b^p \neq c^p$.

(2) We can take a square tile (see Fig.2), say $\overline{z^2}$, of side z and area z^2, cut off a central square tile $\overline{y^2}$ of side y and area y^2, and we will ***sometimes*** (courtesy Pythagoras' Theorem) have a configuration, denoted by $\mathbb{C}_{Sym}(\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}})$, of a 2-dimensional object, say $\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}}$ (shaded area in Fig.2), which can be defined as *uniquely isomorphic* under change of scale (see Definition 2):

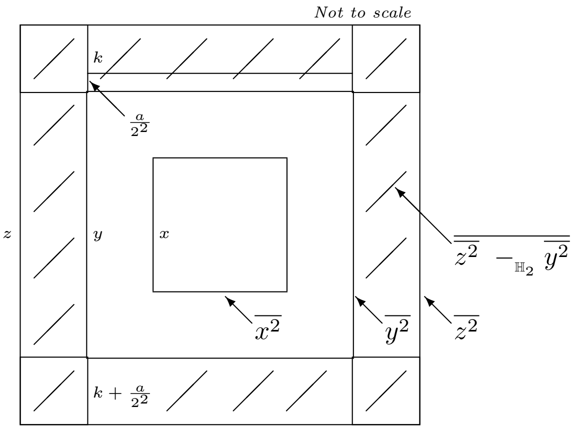

Fig.2

- by cutting into smaller square tiles a square tile $\overline{x^2}$ of side x and area x^2, where x is also a natural number,
- and re-assembling the smaller square tiles to form the symmetrically centered configuration of $\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}}$:

$$\mathbb{C}_{Sym}(\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}}) =_{\mathbb{H}_2} 4\overline{(k+\tfrac{a}{2^2}) \times_{\mathbb{H}_2} y} +_{\mathbb{H}_2} 4\overline{(k+\tfrac{a}{2^2})^2},$$

- such that any two such re-assemblies are *isomorphic* upto *uniqueness* (by Definition 2);

Comment: In other words, by Pythagoras' Theorem we *can* (see §3.A.(a)) design a jigsaw puzzle for *some* $y, z \in \mathbb{N}$ such that the square tile $\overline{y^2}$, along with *any* configuration which is *isomorphic* to $\mathbb{C}_{Sym}(\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}}) =_{\mathbb{H}_2} 4\overline{(k+\frac{a}{2^2}) \times_{\mathbb{H}_2} y} +_{\mathbb{H}_2} 4\overline{(k+\frac{a}{2^2})^2}$, could be assembled as the square tile $\overline{z^2}$.

(3) We can take a cube (see Fig.3), say $\overline{z^3}$, of side z and volume z^3, cut off a central cube $\overline{y^3}$ of side y and volume y^3, but we will ***never*** (courtesy Fermat's insight; see §1.B.(7)-(8)) have a configuration, denoted by $\mathbb{C}_{Sym}(\overline{\overline{z^3} -_{\mathbb{H}_3} \overline{y^3}})$, of a 3-dimensional object, say $\overline{\overline{z^3} -_{\mathbb{H}_3} \overline{y^3}}$, which can be defined as *uniquely isomorphic* under change of scale (see Definition 2):

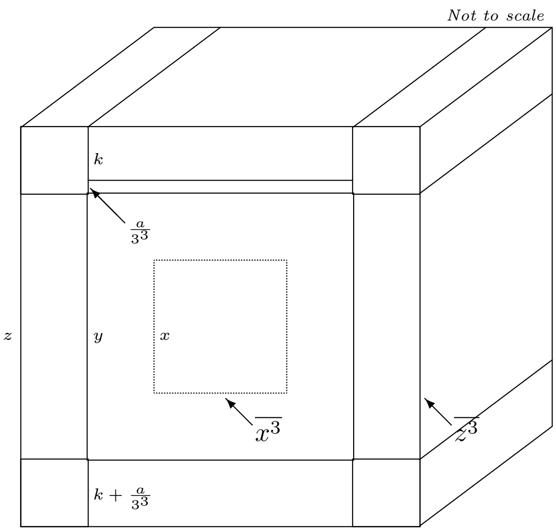

Fig.3

- by cutting into smaller cubes a cube $\overline{x^3}$ of side x and volume x^3, where x is also a natural number,

- and re-assembling the smaller cubes to form the symmetrically centered configuration of $\overline{\overline{z^3} \ -_{\mathbb{H}_3} \ \overline{y^3}}$:

$$\mathbb{C}_{Sym}(\overline{\overline{z^3} \ -_{\mathbb{H}_3} \ \overline{y^3}}) =_{\mathbb{H}_3} 6\overline{(k+\tfrac{a}{3^3}) \times_{\mathbb{H}_3} y^2} \ +_{\mathbb{H}_3} 12\overline{(k+\tfrac{a}{3^3})^2 \times_{\mathbb{H}_3} y} \ +_{\mathbb{H}_3} 8\overline{(k+\tfrac{a}{3^3})^3},$$

- such that any two such re-assemblies are *isomorphic* upto *uniqueness* (by Definition 2);

  Comment: In other words, Fermat's insight entails that we *cannot* (see §3.A.(b)) design a LEGO blocks puzzle for *any* $y, z \in \mathbb{N}$ such that the LEGO cube $\overline{y^3}$, along with *any* configuration of LEGO blocks which is *isomorphic* to $\mathbb{C}_{Sym}(\overline{\overline{z^3} \ -_{\mathbb{H}_3} \ \overline{y^3}}) =_{\mathbb{H}_3} 6\overline{(k+\tfrac{a}{3^3}) \times_{\mathbb{H}_3} y^2} \ +_{\mathbb{H}_3} 12\overline{(k+\tfrac{a}{3^3})^2 \times_{\mathbb{H}_3} y} \ +_{\mathbb{H}_3} 8\overline{(k+\tfrac{a}{3^3})^3}$, could be assembled into the LEGO cube $\overline{z^3}$.

We note that all three are particular instances of a n-dimensional mathematical object, say $\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}}$, which is *well-defined* (see Definition 3) by the following, symmetrically centered, configuration $\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}})$ of $\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}}$ if, and only if, $z^n - y^n = x^n$ for some particular set of natural numbers z, y, x, where:

$$\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}}) =_{\mathbb{H}_n} 2.{}^nC_1\overline{(k+\tfrac{a}{n^n}) \times_{\mathbb{H}_n} y^{(n-1)}} +_{\mathbb{H}_n} 2^2.{}^nC_2\overline{(k+\tfrac{a}{n^n})^2 \times_{\mathbb{H}_n} y^{(n-2)}} +_{\mathbb{H}_n} \ldots +_{\mathbb{H}_n} 2^n\overline{(k+\tfrac{a}{n^n})^n}.$$

Definition 1. (Isomorphic configuration) *Any two configurations of a n-D hyper-object $\overline{x^n} \in \mathbb{H}_n$, denoted by $(\sum_{\mathbb{H}_n})_{i=1}^j a_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^n u_{ik})}$ and $(\sum_{\mathbb{H}_n})_{i=1}^j b_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^n v_{ik})}$, where $\overline{((\prod_{\mathbb{H}_n})_{k=1}^n u_{ik})}, \overline{((\prod_{\mathbb{H}_n})_{k=1}^n v_{ik})} \in \mathbb{H}_n$, and $a_i, b_i, u_{ik}, v_{ik} \in \mathbb{N}$, are defined as isomorphic if, and only if, for any $1 \leq i \leq j \in \mathbb{N}$, $b_i = r^n a_i$ and $(\prod_{k=1}^n u_{ik}) = r^n(\prod_{k=1}^n v_{ik})$ for some rational $r > 0 \in \mathbb{Q}$*[17].

Definition 2. (Unique isomorphism) *A configuration $(\sum_{\mathbb{H}_n})_{i=1}^j a_i \overline{(\prod_{k=1}^n u_{ik})}$ of a n-D hyper-object $\overline{x^n} \in \mathbb{H}_n$ is defined as uniquely isomorphic if, and only if, for all $1 \leq i \leq j \in \mathbb{N}$, either $a_i | b_i$ or $b_i | a_i$ in any two configurations $(\sum_{\mathbb{H}_n})_{i=1}^j a_i \overline{(\prod_{k=1}^n u_{ik})}$ and $(\sum_{\mathbb{H}_n})_{i=1}^j b_i \overline{(\prod_{k=1}^n v_{ik})}$ of $\overline{x^n}$ that are isomorphic.*

Definition 3. (Well-defined object) *A n-D hyper-object $\overline{x^n}$ is well-defined by the configuration $\mathbb{C}(\overline{x^n})$ if, and only if, $\mathbb{C}(\overline{x^n})$ is uniquely isomorphic.*

For $\overline{x^n}$ to, then, admit a configuration $\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}})$ that *well-defines* $\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}}$, each term in the configuration $\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}})$ must be *uniquely isomorphic* under any change of scale by Definition 2.

However, we argue *pre-formally* in §3. that, for any natural numbers x, y, z which claim to yield a solution of $z^n - y^n = x^n$, such *unique isomorphism* is only possible for $n < 3$.

3. Could this be viewed as a *pre-formal* proof of FLT?

Proposition 3.1. *If $x^p + y^p = z^p$, where $1 < x < y < z \in \mathbb{N}$, and $p \in \mathbb{N}$ is a prime, then $p = 2$.*

Proof. 1. Consider the three, symmetrically centered, squares (2-D hypercubes) with sides x, y, z in Fig.4 for any specified natural numbers $1 < x < y < z$ which are co-prime.

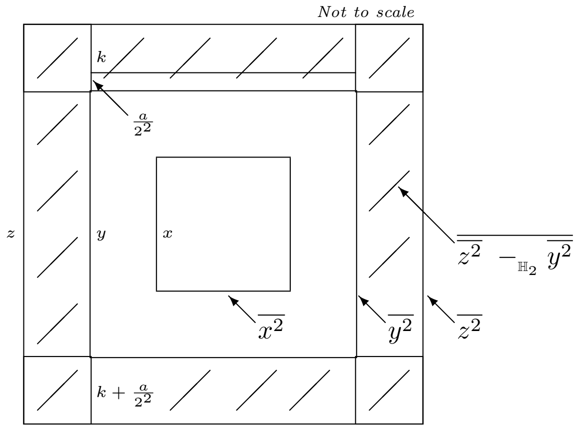

Fig.4

[17] $\mathbb{Q}$ is the structure of the rational numbers.

Then Fig.4 is a *pictorial* proof (compare [28], p.29, Fig. 4) that $x^2 + y^2 = z^2$ if, and only if, we can *physically construct* (assemble *uniquely*) a 2-D LEGO blocks (tiles) puzzle for $k > 0$ and $a \in \{0, 1, 2, 3\}$, where $k + \frac{a}{2^2} > 0$, such that:

(a) one square block (tile) of side y,

(b) plus 4 rectangular blocks (tiles) with dimensions $y \times (k + \frac{a}{2^2})$,

(c) and 4 square blocks (tiles) of side $(k + \frac{a}{2^2})$,

must combine to *well-define* a square block (tile) denoted by, say, $\overline{z^2}$, of side z, where the 2-D 'hyper-object' denoted by, say (shaded area), $\overline{\overline{z^2} \ -_{\mathbb{H}_2} \overline{y^2}}$, is *well-defined* by the symmetrically centered 'configuration' of 2-D LEGO blocks (tiles):

(i) $\mathbb{C}_{Sym}(\overline{\overline{z^2} \ -_{\mathbb{H}_2} \overline{y^2}}) =_{\mathbb{H}_2} 4\overline{(k + \frac{a}{2^2}) \times_{\mathbb{H}_2} y} \ +_{\mathbb{H}_2} 4\overline{(k + \frac{a}{2^2})^2}$.

2. Similarly, Fig.5 is a *pictorial* proof[18] that $x^3 + y^3 = z^3$ if, and only if, we can *physically construct* (assemble *uniquely*) a 3-D LEGO blocks puzzle for $k > 0$ and $a \in \{0, 1, 2, \ldots, 26\}$, where $k + \frac{a}{3^3} > 0$, such that:

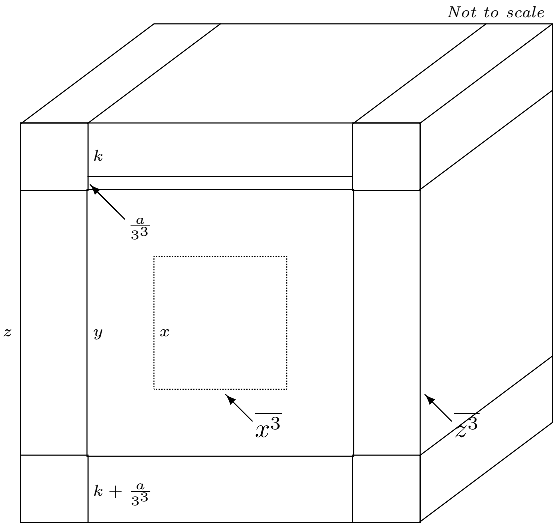

Fig.5

(a) one cube block of side y,

(b) plus 6 parallelepiped blocks with base y^2 and height $(k + \frac{a}{3^3})$,

(c) plus 12 parallelepiped blocks with base $(k + \frac{a}{3^3})^2$ and height y,

(d) plus 8 cube blocks of side $(k + \frac{a}{3^3})$,

must combine to *well-define* a cube block denoted by $\overline{z^3}$, of side z, where the 3-D 'hyper-object' denoted by $\overline{\overline{z^3} \ -_{\mathbb{H}_3} \overline{y^3}}$ is *well-defined* by the symmetrically centered 'configuration' of 3-D LEGO blocks:

(i) $\mathbb{C}_{Sym}(\overline{\overline{z^3} \ -_{\mathbb{H}_3} \overline{y^3}}) =_{\mathbb{H}_3} 6\overline{(k + \frac{a}{3^3}) \times_{\mathbb{H}_3} y^2} \ +_{\mathbb{H}_3} 12\overline{(k + \frac{a}{3^3})^2 \times_{\mathbb{H}_3} y} \ +_{\mathbb{H}_3} 8\overline{(k + \frac{a}{3^3})^3}$.

3. In the general case, if $x^p + y^p = z^p$ for $p \geq 2$, and $z = y + 2(k + \frac{a}{p^p})$, a not unreasonable appeal to a principle of symmetry such as Curie's (see [4], §2.2, Curie's principle) *suggests* that the p-D hyper-object denoted by $\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}$ *must* then be *well-defined* by the symmetrically centered 'configuration' of p-D hyper-objects denoted by:

(i) $\mathbb{C}_{Sym}(\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} 2.{}^pC_1\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}} +_{\mathbb{H}_p} 2^2.{}^pC_2\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}} +_{\mathbb{H}_p} \ldots +_{\mathbb{H}_p} 2^p\overline{(k + \frac{a}{p^p})^p}$.

[18] Compare the visual 'challenge' suggested in [28], p.31, Fig.5. Also Gerd Falting's insightful (albeit analytic) visualisation ([28], p.255, Fig.23) of $x^n + y^n = 1$ for $x, y \in \mathbb{C}, n \in \mathbb{N}$, when extended to $z^n - y^n = 1$ for $z, y \in \mathbb{C}, n \in \mathbb{N}$.

4. If we, therefore, represent:

   — the concept '*physically construct*' mathematically by the concept '*well-define*' (in the usual sense of deterministically assigning an unambiguous 'configuration', which need not, however, be *unique*); and

   — the concept '*pictorial*' by '*formal*';

   we can uniquely correspond:

   — the relation $z^p - y^p = x^p$ in a *formal* Peano Arithmetic (such as PA); and

   — the relation, $\mathbb{C}_{Sym}(\overline{\overline{z^p} -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} \mathbb{C}_{Sym}(\overline{x^p})$—in any putative, *formal*, geometry $T_{\mathbb{H}_p}$ (of the structure $\mathbb{H}_p$ of p-D hyper-objects in a p-dimensional Euclidean space which includes the cases where $p = 2, 3$)—between the p-D hyper-objects denoted by $\overline{\overline{z^p} -_{\mathbb{H}_p} \overline{y^p}}$ and $\overline{x^p}$, that is *well-defined* as *uniquely isomorphic* (see Definition 2) by the symmetrically centered 'configuration' of p-D hyper-objects:

   (i) $\mathbb{C}_{Sym}(\overline{\overline{z^p} -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} 2.^pC_1\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}} +_{\mathbb{H}_p} 2^2.^pC_2\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}} +_{\mathbb{H}_p} \ldots +_{\mathbb{H}_p} 2^p\overline{(k + \frac{a}{p^p})^p}$.

   Of course we assume here as intuitively plausible that we could *formally* define '*configuration* $\mathbb{C}(\overline{x^p})$ of a p-D hyper-object $\overline{x^p}$', '*symmetrically centered* configurations of a p-D hyper-object $\overline{x^p}$', '*isomorphic* configurations of a p-D hyper-object $\overline{x^p}$', 'hyper-volume $\mathbb{V}(\overline{x^p})$ of a p-D hyper-object $\overline{x^p}$', '$-_{\mathbb{H}_p}$', '$=_{\mathbb{H}_p}$', '$+_{\mathbb{H}_p}$', '$\times_{\mathbb{H}_p}$', '$(\sum_{\mathbb{H}_n})$', '$(\prod_{\mathbb{H}_n})$', and '$\equiv_{\mathbb{H}_p}$' in $T_{\mathbb{H}_p}$ so as to admit the pictorial interpretations §3.1 and §3.2 when $p = 2, 3$ respectively, such that §3.4(i) interprets as:

   (ii) $\overline{\overline{z^p} -_{\mathbb{H}_p} \overline{y^p}}$ denotes a p-D hyper-object that is *well-defined* (see Definition 3) in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of:

   (a) the $2.^pC_1$ p-D hyper-objects, each denoted by $\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}}$ with hyper-dimensions:

   $$(k + \tfrac{a}{p^p}) \times_{\mathbb{H}_p} \underbrace{y \times_{\mathbb{H}_p} y \times_{\mathbb{H}_p} \ldots \times_{\mathbb{H}_p} y}_{(p-1)};$$

   (b) the $2^2.^pC_2$ p-D hyper-objects, each denoted by $\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}}$ with hyper-dimensions:

   $$(k + \tfrac{a}{p^p}) \times_{\mathbb{H}_p} (k + \tfrac{a}{p^p}) \times_{\mathbb{H}_p} \underbrace{y \times_{\mathbb{H}_p} y \times_{\mathbb{H}_p} \ldots \times_{\mathbb{H}_p} y}_{(p-2)};$$

   ...

   (c) the 2^p p-D hypercubes, each denoted by $\overline{(k + \frac{a}{p^p})^p}$ with sides $(k + \frac{a}{p^p})$;

   and where, in the usual arithmetic of the natural numbers:

   (iii) $x^p = 2.^pC_1(k + \frac{a}{p^p})y^{(p-1)} + 2^2.^pC_2(k + \frac{a}{p^p})^2y^{(p-2)} + \ldots + 2^p(k + \frac{a}{p^p})^p$.

5. Since $z - y = 2(k + \frac{a}{p^p}) \in \mathbb{N}$, each term of §3.4(iii) admits only those values of $a \in \mathbb{N}$ that yield a natural number. We thus have that if §3.4(iii) *well-defines* a p-D hypercube denoted by $\overline{x^p}$ in the theory $T_{\mathbb{H}_p}$ of p-D hyper-objects, then this would correspond to the symmetrically centered 'configuration' of p-D hyper-objects defined *only* upto *isomorphism* (see Definition 1) by:

   (i) $\mathbb{C}_{Sym}(\overline{x^p}) =_{\mathbb{H}_p} 2.^pC_1(k + \frac{a}{p^p})y^{(p-1)}\overline{(u)^p} +_{\mathbb{H}_p} 2^2.^pC_2(k + \frac{a}{p^p})^2y^{(p-2)}\overline{(u)^p} +_{\mathbb{H}_p} \ldots +_{\mathbb{H}_p} 2^p(k + \frac{a}{p^p})^p\overline{(u)^p}$

   where $\overline{(u)^p}$ denotes the p-D *unit* hypercube.

6. However, for $1 \leq r \leq p$, the p-D hyper-objects defined in §3.4(ii)(a)-§3.4(ii)(c) *must* further be *well-defined*, at any rational scale $0 < s \leq 1$ of scaled down p-D hyper-objects, by the substitution:

   (i) $2^r.^pC_r\overline{(k + \frac{a}{p^p})^r \times_{\mathbb{H}_p} y^{(p-r)}} =_{\mathbb{H}_p} \frac{1}{s^p}.2^r.^pC_r\overline{((k + \frac{a}{p^p})s)^r \times_{\mathbb{H}_p} (ys)^{(p-r)}}$.

7. In particular, since $z - y = 2(k + \frac{a}{p^p}) \in \mathbb{N}$, the p-D hyper-object *well-defined* by the symmetrically centered 'configuration' of p-D hyper-objects denoted by:

   (i) the 2^p p-D hypercubes $\overline{(k + \frac{a}{p^p})^p}$ with hyper-dimensions denoted by $(k + \frac{a}{p^p})^p$, and cumulative p-D hyper-volume $2^p(k + \frac{a}{p^p})^p$, in a p-dimensional Euclidean space;

   must be capable of also being *well-defined* by the symmetrically centered 'configuration' of p-D hyper-objects denoted by:

   (ii) the p^p scaled down p-D hypercubes $\overline{((k + \frac{a}{p^p})\frac{2}{p})^p}$ with hyper-dimensions denoted by $((k + \frac{a}{p^p}) \times_{\mathbb{H}_p} (\frac{2}{p}))^p$, and cumulative p-D hyper-volume $p^p((k + \frac{a}{p^p})(\frac{2}{p}))^p = 2^p(k + \frac{a}{p^p})^p$.

   since both *well-define* the p-D hypercube:

   (iv) $\mathbb{C}_{Sym}(\overline{2^p(k + \frac{a}{p^p})^p}) =_{\mathbb{H}_p} 2^p\overline{(k + \frac{a}{p^p})^p} =_{\mathbb{H}_p} p^p\overline{((k + \frac{a}{p^p})(\frac{2}{p}))^p}$

8. Moreover, since $T_{\mathbb{H}_p}$ must admit the pictorial interpretations §3.1 and §3.2 when $p = 2, 3$ respectively—as detailed in §3.A.(a) and §3.A.(b)—then the p-D hyper-object denoted by $\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}$ is *well-defined* under interpretation in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of p-D hyper-objects §3.4(i) if, and only if, each term in §3.4(i) is *uniquely isomorphic* under any change of scale.

9. Consequently, if $\overline{\overline{z^p} -_{\mathbb{H}_p} \overline{y^p}}$ denotes a p-D hyper-object that is *well-defined* under interpretation in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of p-D hyper-objects §3.4(i), by Definition 2 we cannot have that both:

   (i) $\mathbb{C}_{Sym}(\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} 2.^pC_1\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}} +_{\mathbb{H}_p} 2^2.^pC_2\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}} +_{\mathbb{H}_p} \ldots +_{\mathbb{H}_p} 2^p\overline{(k + \frac{a}{p^p})^p}$;

   and:

   (ii) $\mathbb{C}_{Sym}(\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} 2.^pC_1\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}} +_{\mathbb{H}_p} 2^2.^pC_2\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}} +_{\mathbb{H}_p} \ldots +_{\mathbb{H}_p} p^p\overline{((k + \frac{a}{p^p})\frac{2}{p})^p}$;

   satisfy $\mathbb{C}_{Sym}(\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} \mathbb{C}_{Sym}(\overline{x^p})$, and thereby entail $z^p - y^p = x^p$, if $2^p \not| \, p^p$.

10. Hence, if the p-D hyper-object denoted by $\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}$ is *well-defined* under interpretation in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of p-D hyper-objects §3.4(i), then $p^p = 2^p$, and $p = 2$.

11. We thus have the contradiction that, for prime $p > 2$, if $z^p - y^p = x^p$:

    (i) then $z^p - y^p = 2.^pC_1(k + \frac{a}{p^p})y^{p-1} + 2^2.^pC_2(k + \frac{a}{p^p})^2y^{p-2} + \ldots + 2^p(k + \frac{a}{p^p})^p$

    (ii) but[19] $z^p - y^p \neq 2.^pC_1(k + \frac{a}{p^p})y^{p-1} + 2^2.^pC_2(k + \frac{a}{p^p})^2y^{p-2} + \ldots + p^p((k + \frac{a}{p^p})\frac{2}{p})^p$

12. However (see §3.A.(a) below), since $2^2 = 2.^2C_1 = 2^2.^2C_2$, the p-D hyper-object sought to be *well-defined* in §3.(4(i)) by the symmetrically centered 'configuration' of p-D hyper-objects:

    (i) $\mathbb{C}(\overline{\overline{z^p} \ -_{\mathbb{H}_p} \overline{y^p}}) =_{\mathbb{H}_p} 2.^pC_1\overline{(k + \frac{a}{p^p}) \times_{\mathbb{H}_p} y^{(p-1)}} +_{\mathbb{H}_p} 2^2.^pC_2\overline{(k + \frac{a}{p^p})^2 \times_{\mathbb{H}_p} y^{(p-2)}} +_{\mathbb{H}_p} \ldots +_{\mathbb{H}_p} 2^p\overline{(k + \frac{a}{p^p})^p}$,

    where $y, z \in \mathbb{N}$, does *uniquely well-define* a p-D hypercube denoted by $\overline{x^p}$ under change of scale, where $x \in \mathbb{N}$, for $p = 2$.

The proposition follows. □

Corollary 3.2. *If $x^n + y^n = z^n$, where $1 < x < y < z \in \mathbb{N}$, and $1 < n \in \mathbb{N}$, then $n = 2$.*

[19] *Reason*: Even if the hyper-volume $\mathbb{V}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}})$, sought to be *well-defined* in the particular configuration §3.(4(i)) by the n-D hyper-object denoted by $\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}}$, could be *platonically* assumed as being capable of being 'filled' with *unit* n-D hypercubes of total hyper-volume $\mathbb{V}_{Sym}(\overline{x^n})$, it could not even *platonically* be assumed as capable of being 'filled' with n-D hypercubes of side $\frac{2}{n}$, of total hyper-volume $\mathbb{V}_{Sym}(\overline{x^n})$, if n is a prime greater than 2; see §8..

Corollary 3.2 follows since, as noted by Simon Singh in [28] (p.98), by showing that $x^4+y^4 = z^4$ is unsolvable for $x, y, z \in \mathbb{N}$, Fermat had 'given mathematicians a head start' in proving FLT since, additionally:

> "To prove Fermat's Last Theorem for all values of n, one merely has to prove it for the prime values of n. All other cases are merely multiples of the prime cases and would be proved implicitly."
> *. . . Singh: [28], p.99.*

The significance of showing we cannot *well-define* the n-D hyper-object denoted by $\overline{z^n}$, such that $\mathbb{C}_{Sym}(\overline{x^n}) =_{\mathbb{H}_n} \mathbb{C}_{Sym}(\overline{\overline{z^n} \; -_{\mathbb{H}_n} \overline{y^n}})$ entails $x^n = z^n - y^n$, is that it circumvents any implicit appeal (see [28], p.126) to unique factorisation 'in number systems that extend beyond the ordinary integers':

> "In the 1840's, several mathematicians worked on a general proof which, like Miyaoka's, foundered on an unwarranted assumption: they had assumed that the unique factorization of integers into primes (such as $60 = 2 \times 2 \times 3 \times 5$) would hold for number systems that extend beyond the ordinary integers. In actuality, unique factorization is rather rare. For instance, 2×3 and $1+\sqrt{-5}$ and $1-\sqrt{-5}$ are distinct factorizations of 6 in a number system that treats $\sqrt{-5}$ as an integer."
> *. . . Cipra: [9].*

3.A. *Why* is $x^n + y^n = z^n$ solvable for $n = 2$, but not for $n = 3$

We consider the cases $n = 2$ and $n = 3$ to illustrate *why* $x^n + y^n = z^n$ can be argued *pre-formally* as solvable for $n = 2$, but unsolvable for $n > 2$; where we note that for any specified natural numbers $x, y, z, k, a \in \mathbb{N}$ as defined in §3., Proposition 3.1:

(a) If $x^2 + y^2 = z^2$ and $z - y = 2(k + \frac{a}{2^2})$ then, for instance:

  (i) the $2.^2C_1$ 2-D hyper-objects denoted by $\overline{(k + \frac{a}{2^2}) \times_{\mathbb{H}_2} y}$, with hyper-dimensions $(k + \frac{a}{2^2}) \times_{\mathbb{H}_2} y$, and cumulative 2-D hyper-volume $2.^2C_1.(k + \frac{a}{2^2})y$,

  defined in §3.4(i) are *well-defined* by (assembled *uniquely* from):

  (ii) the 2^4 scaled down 2-D hyper-objects denoted by $\overline{(k + \frac{a}{2^2})\frac{1}{2} \times_{\mathbb{H}_2} y(\frac{1}{2})}$, with hyper-dimensions $(k + \frac{a}{2^2})\frac{1}{2} \times_{\mathbb{H}_2} y(\frac{1}{2})$, and cumulative 2-D hyper-volume $2^4.(k + \frac{a}{2^2})\frac{1}{2}y(\frac{1}{2}) = 2.^2C_1.(k + \frac{a}{2^2})y$;

  whilst:

  (iii) the $2^2.^2C_2$ 2-D hypercubes denoted by $\overline{(k + \frac{a}{2^2})^2}$, with hyper-dimensions $(k + \frac{a}{2^2}) \times_{\mathbb{H}_2} (k + \frac{a}{2^2})$, and cumulative 2-D hyper-volume $2^2.^2C_2.(k + \frac{a}{2^2})^2$,

  are also *well-defined* by (assembled *uniquely* from):

  (iv) the 2^4 scaled down 2-D hypercubes denoted by $\overline{((k + \frac{a}{2^2}) \times_{\mathbb{H}_2} (\frac{1}{2}))^2}$ with hyper-dimensions $((k + \frac{a}{2^2})(\frac{1}{2})) \times_{\mathbb{H}_2} ((k + \frac{a}{2^2})(\frac{1}{2}))$, and cumulative 2-D hyper-volume $2^4.((k + \frac{a}{2^2})(\frac{1}{2}))^2 = 2^2.^2C_2.(k + \frac{a}{2^2})^2$.

(b) However, if $x^3 + y^3 = z^3$ and $z - y = 2(k + \frac{a}{3^3})$, then:

  (i) the 2^3 3-D hypercubes denoted by $\overline{(k + \frac{a}{3^3})^3}$, with hyper-dimensions $(k+\frac{a}{3^3}) \times_{\mathbb{H}_3} (k+\frac{a}{3^3}) \times_{\mathbb{H}_3} (k+\frac{a}{3^3})$, and cumulative 3-D hyper-volume $2^3.(k + \frac{a}{3^3})^3$,

  are *not* capable of being *well-defined* by (assembled *uniquely* from):

  (ii) the 3^3 scaled down 3-D hypercubes denoted by $\overline{((k + \frac{a}{3^3}) \times_{\mathbb{H}_3} (\frac{2}{3}))^3}$, with hyper-dimensions $((k + \frac{a}{3^3})(\frac{2}{3})) \times_{\mathbb{H}_3} ((k + \frac{a}{3^3})(\frac{2}{3})) \times_{\mathbb{H}_3} ((k + \frac{a}{3^3})(\frac{2}{3}))$ and cumulative 3-D hyper-volume $3^3.((k + \frac{a}{3^3})(\frac{2}{3}))^3 = 2^3.(k + \frac{a}{3^3})^3$;

  in a 3-D LEGO blocks puzzle which evidences $\mathbb{C}_{Sym}(\overline{\overline{z^3} \; -_{\mathbb{H}_3} \overline{y^3}}) =_{\mathbb{H}_3} \mathbb{C}_{Sym}(\overline{x^3})$ as *well-defined only* upto *isomorphism* (see Definition 1) in §3.4(i), since (see §1.B.(7)) we cannot assemble the 3-D hypercube denoted by $\overline{z^3}$ in the puzzle by replacing 2^3 identical 3-D hypercubes (as defined in (i)), with 3^3 scaled down, identical, 3-D hypercubes (as defined in (ii)).

Comment: In other words, we can *never* design a LEGO blocks puzzle for *any* $y, z \in \mathbb{N}$ such that the LEGO cube $\overline{y^3}$, along with *any* configuration of LEGO blocks which is *isomorphic* (see Definition 1) to $\mathbb{C}_{Sym}(\overline{\overline{z^3} \ -_{\mathbb{H}_3} \ \overline{y^3}}) =_{\mathbb{H}_3} 6\overline{(k+\frac{a}{3^3}) \times_{\mathbb{H}_3} y^2} +_{\mathbb{H}_3} 12\overline{(k+\frac{a}{3^3})^2 \times_{\mathbb{H}_3} y} +_{\mathbb{H}_3} 8\overline{(k+\frac{a}{3^3})^3}$, could be assembled into the LEGO cube $\overline{z^3}$.

Reason: If, in the above LEGO blocks puzzle, $(\sum_{\mathbb{H}_n})_{i=1}^{j} a_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^{n} u_{ik})}$ and $(\sum_{\mathbb{H}_n})_{i=1}^{j} b_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^{n} v_{ik})}$ are *any* two *well-defined* configurations that are *uniquely isomorphic* (see Definition 2) of the hypercube $\overline{x^n}$, each of which, along with the n-D hypercube $\overline{y^n}$, claim to *well-define* a hypercube $\overline{z^n}$, then it is:

- *necessary*, but not *sufficient*, that: $(\sum_{\mathbb{H}_n})_{i=1}^{j} a_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^{n} u_{ik})}$ and $(\sum_{\mathbb{H}_n})_{i=1}^{j} b_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^{n} v_{ik})}$ are *isomorphic* (by Definition 1);
- *necessary* and *sufficient* that: $(\sum_{\mathbb{H}_n})_{i=1}^{j} a_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^{n} u_{ik})}$ and $(\sum_{\mathbb{H}_n})_{i=1}^{j} b_i \overline{((\prod_{\mathbb{H}_n})_{k=1}^{n} v_{ik})}$ are *isomorphic* (by Definition 1); *and*, for all $1 \leq i \leq j$, either $a_i | b_i$ or $b_i | a_i$ (by Definition 2).

We note that, prima facie, it is not obvious whether Andrew Wiles' 1995 proof of FLT, as outlined by Michael Harris in [18] (see §6.), *implicitly* admits—or needs to *explicitly* admit—corresponding definitions.

4. Summary of the *pre-formal* proof of FLT

We have argued above that some insight, into *why* Fermat might have concluded that $x^n + y^n = z^n$ can be treated *pre-formally* as true *only* for $n < 3$, follows by reasoning that:

If $x^n + y^n = z^n$ for $x, y, z, n \in \mathbb{N}$, and $z = y + 2(k + \frac{a}{n^n})$ (see Figs.1-3), then:

(1) $x^n = 2.^nC_1(k + \frac{a}{n^n})y^{n-1} + 2^2.^nC_2(k + \frac{a}{n^n})^2 y^{n-2} + \ldots + 2^n(k + \frac{a}{n^n})^n$.

FLT is then equivalent to proving that the *necessary* and *sufficient* conditions which, for any *specified* $n \geq 1 \in \mathbb{N}$, admit some $y, z \geq 1 \in \mathbb{N}$ that yield a representation of x^n as in §4.(1), hold only for $n < 3$.

Since it is not obvious how, or even whether, such conditions are *formally* definable *within* the first-order Peano Arithmetic PA, §3. and §3.A. argue the *pre-formal* perspective that FLT is a *true* arithmetical proposition which appeals *necessarily* to the *essentially* geometrical property of *unique isomorphism* (by §2.A., Definitions 1 and 2) of the centrally symmetrical configurations of n-dimensional hyper-cubes $\overline{x^n}, \overline{y^n}, \overline{z^n}$, in the structure $\mathbb{H}_n$ of n-D hyper-objects in a n-dimensional Euclidean space, such that:

(a) Fermat's Last Theorem can be interpreted as an assertion concerning the geometrical properties of the hyper-geometric objects sought to be *well-defined* (by §2.A., Definition 3) in §3.4(ii); where

(b) If $x, y, z, n, \in \mathbb{N}$, and $z^n = x^n + y^n$, then the n-D hyper-object denoted by $\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}}$, with symmetrically centered configuration $\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}})$, is *well-defined only* if $n \leq 2$ (see §3.10); and

(c) Since it would then follow that $\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \ \overline{y^n}})$ and $\mathbb{C}_{Sym}(\overline{x^n})$ *well-define* the same n-D hyper-object, the n-D hypercube denoted by $\overline{x^n}$ is also *well-defined only* if $n \leq 2$ (see §3.10).

(d) This entails the contradiction that, for prime $p > 2$, if $z^p - y^p = x^p$:

(i) then $z^p - y^p = 2.^pC_1(k + \frac{a}{p^p})y^{p-1} + 2^2.^pC_2(k + \frac{a}{p^p})^2 y^{p-2} + \ldots + 2^p(k + \frac{a}{p^p})^p$;

(ii) but $z^p - y^p \neq 2.^pC_1(k + \frac{a}{p^p})y^{p-1} + 2^2.^pC_2(k + \frac{a}{p^p})^2 y^{p-2} + \ldots + p^p((k + \frac{a}{p^p})\frac{2}{p})^p$.

(e) In other words, for any *specified* $y, z, \in \mathbb{N}$, x^n cannot be *well-defined* in $\mathbb{N}$ by $2.^nC_1(k + \frac{a}{n^n})y^{(n-1)} + 2^2.^nC_2(k+\frac{a}{n^n})^2 y^{(n-2)} + \ldots + 2^n(k+\frac{a}{n^n})^n$ such that there is a deterministic algorithm which will *evidence* $x^n + y^n = z^n$ for any *specified* $n > 2$.

Comment: The reason it is not *pre-formally* evident that the *geometrical* property of *unique isomorphism* of the centrally symmetrical configurations of a n-dimensional hyper-cube $\overline{x^n}$ in $\mathbb{H}_n$, where $x, n \in \mathbb{N}$, can be corresponded to any *arithmetical* property of the integer x^n, is that:

If $x^n = z^n - y^n$, and $z = y + 2(k + \frac{a}{n^n})$ (see Figs.1-3), then x^n is *well-defined uniquely* in $\mathbb{N}$ by both:

(i) $x^n = 2.^nC_1(k + \frac{a}{n^n})y^{(n-1)} + 2^2.^nC_2(k + \frac{a}{n^n})^2 y^{(n-2)} + \ldots + 2^n(k + \frac{a}{n^n})^n$, *and*

(ii) $x^n = 2.^nC_1(k+\frac{a}{n^n})y^{(n-1)} + 2^2.^nC_2(k+\frac{a}{n^n})^2y^{(n-2)} + \ldots + p^n((k+\frac{a}{n^n})\frac{2}{p})^n$,

for *all* primes p, and not *only* for $p = 2$ as in the case of the n-D hyper-object in $\mathbb{H}_n$ denoted by $\overline{x^n}$, whose centrally symmetrical configurations are sought to be *well-defined* as *uniquely isomorphic* in §3.10 such that $\overline{x^n} =_{\mathbb{H}_n} (\overline{z^n} -_{\mathbb{H}_n} \overline{y^n})$.

It is conceivable that such a *pre-formal* insight could have been *intuited* by Fermat, and viewed initially as a 'truly marvellous demonstration'; but perhaps[20] one whose 'truth' in the general case he was unable to *evidence* independently of the above *pre-formal* argument just enough (lacking a seemingly common argument for sufficient special cases) to let his initial claim lie obscured, but not disowned; thus bequeathing posterity the question:

> "If Fermat did not have Wiles's proof, then what did he have?"
>
> Mathematicians are divided into two camps. The hardheaded skeptics believe that Fermat's Last Theorem was the result of a rare moment of weakness by the seventeenth century genius. They claim that, although Fermat wrote 'I have discovered a truly marvellous proof,' he had in fact found only a flawed proof. The exact nature of this flawed proof is open to debate, but it is quite possible that it may have been along the same lines as the work of Cauchy or Lamé.
>
> Other mathematicians, the romantic optimists, believe that Fermat may have had a genuine proof. Whatever this proof might have been, it would have been based on seventeenth-century techniques, . . . "
>
> *. . . Singh: [28], pp.307-308.*

5. Distinguishing between 'informal', *belief-based*, reasoning and 'pre-formal', *evidence-based*, reasoning

We note that what we have argued above in this reconstruction of Fermat's possible, 'truly marvellous demonstration' of FLT is that any proof of FLT necessarily appeals to conceptualising (as in §2.A.) Fermat's Last Theorem as a *formal* proposition concerning the *geometrical* property of *unique isomorphism* (as defined by §2.A., Definitions 1 and 2) over the structure, say $\mathbb{H}_n$, of n-D hyper-objects in a n-dimensional Euclidean space.

This, we argue, yields a *pre-formal* proof of FLT by evidencing, *without* appeal to properties of *real* and *complex* numbers, that if, for some natural numbers x, y, z, n, we can *well-define* n-D hyper-cubes $\overline{x^n}, \overline{y^n}, \overline{z^n} \in \mathbb{H}_n$ (by §2.A., Definition 3) which entail $x^n + y^n = z^n$, then $n = 2$.

The need for distinguishing between:

- *belief-based* 'informal' reasoning in the inherited, *classical*, sense of Plato's *knowledge as justified true belief* (see [27]; also [2], §5.A) which, we argue, is how prevailing wisdom views Fermat's claim; and
- *evidence-based* 'pre-formal', reasoning in the sense of Gualtiero Piccinini's *knowledge as factually grounded belief* (see [27]; also [2], §5.A), which is how we view Fermat's claim in §3.;

is addressed by Markus Pantsar in his introduction to 'Truth, Proof and Gödelian Arguments: A Defence of Tarskian Truth in Mathematics':

> "In this work I will argue that without any outer reference, mathematics as we know it could simply not be possible: it could not have developed, and it could not be learnt or practised. Sophisticated formal theories are the pinnacle of mathematics but, philosophically, they cannot be studied separately from all the non-formal background behind them.
>
> This way, what might seem like a completely formalist theory of mathematics turns out to be nothing of the sort. It could not have existed without a wide *pre-formal* background, which we will see when we examine mathematical practice in general.[3] Formal systems are not of the self-standing type that extreme formalism seems to claim. My purpose in this work is to show that the formalist program uses the actual practice of mathematics as a ladder that they later discard. This by itself is of course perfectly acceptable,

[20] In the absence of an *evidence-based* distinction between the *weaker* requirements for *evidencing* the *logical truth* of algorithmically *verifiable* arithmetical propositions (see [1], Definition 1; also [2], §7.C, Definition 18), vis à vis the *stronger* requirements for *evidencing* the *logical truth* of algorithmically *computable* arithmetical propositions (see [1], Definition 2; also [2], §7.C, Definition 20).

and it mirrors the way we strive for formal axiomatic systems in mathematics. What is not acceptable is how they refuse [to acknowledge] using the ladder.

When it comes to the question of truth and proof, this could not be any more relevant. The deflationist truth of extreme formalism equates mathematical truth with formal proof. However, as we will see, that strategy requires that we take mathematics to concern only formal systems. Once we look at the wider picture, we see that outer criteria are needed to avoid arbitrariness. Theory choice must be explained, and this requires reference outside formal systems of mathematics. Philosophers have tried to explain this by a wide array of concepts—usefulness, assertability, consistency and conservativeness, to name a few—but ultimately none of them have been satisfactory. The only plausible way to answer the problem of theory choice, I will argue, is by appealing to *truth*.

[3] What I refer to as pre-formal mathematics in this work is more often discussed as informal mathematics in literature. The choice of terminology here is based on two reasons. First, I want to stress the order in which our mathematical thinking develops. We initially grasp mathematics through informal concepts and only later acquire the corresponding formal tools. Second, the term "informal mathematics" seems to have an emerging non-philosophical meaning of mathematics in everyday life, as opposed to an academic pursuit—which is not at all the distinction that I am after here."

. . . Pantsar: [26], §1.1 General background.

"(*Extreme*) *Formalism*: to say that a mathematical sentence is true involves no reference to any entity outside formal systems. Hence, a mathematical sentence is true in a formal system S if and only if it is provable in S, and mathematical truth cannot be discussed in any other context."

. . . Pantsar: [26], §2.4 Formalism/nominalism.

We interpret Pantsar's 'pre-formal mathematics' here[21] as evidencing the philosophy that an *evidence-based*[22] definition of mathematical *truth* is a, necessarily transparent, prerequisite for determining, in a formal proof theory, which axiomatic assumptions of a formal theory underlie the truth of *pre-formal*, *evidence-based*, reasoning.

Comment: By *evidence-based* we mean the paradigm introduced in [1]; a paradigm whose philosophical significance is that it pro-actively addresses the challenge[23] which arises when an intelligence:

— whether human or mechanistic,

— accepts arithmetical propositions as *true* under an interpretation,

— either axiomatically or on the basis of *subjective* self-evidence,

— *without* any specified methodology for *objectively evidencing* such acceptance,

— in the sense of, for instance, Chetan Murthy and Martin Löb:

> "It is by now folklore . . . that one can view the *values* of a simple functional language as specifying *evidence* for propositions in a constructive logic . . . "
>
> *. . . Murthy: [23], §1 Introduction.*
>
> "Intuitively we require that for each event-describing sentence, $\phi_{o^{\iota}} n_{\iota}$ say (i.e. the concrete object denoted by n_{ι} exhibits the property expressed by $\phi_{o^{\iota}}$), there shall be an algorithm (depending on **I**, i.e. M^*) to decide the truth or falsity of that sentence."
>
> *. . . Löb: [22], p.165.*

The foundational significance of the *evidence-based* definitions of arithmetical truth, introduced in [1], lies in the fact that the first-order Peano Arithmetic PA—which, by [1], Theorem 6.7 (p.41), is *finitarily* consistent—forms the bedrock on which all formal mathematical languages that admit rational and real numbers are founded (see, for instance, Edmund Landau's classically concise exposition [19] on the foundations of analysis).

Such a perspective is also implicit in a recent paper [24] on *Proof vs Truth in Mathematics*, where Roman Murawski (as does Harris in [18]; see §6.) emphasises the critical role that "informal proofs" (which we view as corresponding to Pantsar's *pre-formal* proofs) variously play in 'mathematical research practice' for not only the *understanding*, but also the subsequent *verification* and *justification*, of *formal* proofs:

[21] See also Anand [2], §1.A, *Pre-formal mathematics*.

[22] 'Evidence-based' as defined implicitly in Anand [1], and explicitly in Anand [2], §1.D.

[23] For a brief review of such challenges, see *Feferman* [13] and [14]; also Freire [15].

"Mathematics was and still is developed in an informal way using intuition and heuristic reasonings—it is still developed in fact in the spirit of Euclid (or sometimes of Archimedes) in a *quasi*-axiomatic way. Moreover, informal reasonings appear not only in the context of discovery but also in the context of justification. Any correct methods are allowed to justify statements. Which methods are correct is decided in practice by the community of mathematicians. The ultimate aim of mathematics is "to provide correct proofs of true theorems" [2, p. 105]. In their research practice mathematicians usually do not distinguish concepts "true" and "provable" and often replace them by each other. Mathematicians used to say that a given theorem holds or that it is true and not that it is provable in such and such theory. It should be added that axioms of theories being developed are not always precisely formulated and admissible methods are not precisely described.[2]

Informal proofs used in mathematical research practice play various roles. One can distinguish among others the following roles (cf. [4], [7]):

(1) verification,

(2) explanation,

(3) systematization,

(4) discovery,

(5) intellectual challenge,

(6) communication,

(7) justification of definitions.

The most important and familiar to mathematicians is the first role. In fact only verified statements can be accepted. On the other hand a proof should not only provide a verification of a theorem but it should also explain why does it hold. Therefore mathematicians are often not satisfied by a given proof but are looking for new proofs which would have more explanatory power. Note that a proof that verifies a theorem does not have to explain why it holds. It is also worth distinguishing between proofs that convince and proofs that explain. The former should show that a statement holds or is true and can be accepted, the latter—why it is so. Of course there are proofs that both convince and explain. The explanatory proof should give an insight in the matter whereas the convincing one should be concise or general. Another distinction that can be made is the distinction between explanation and understanding. In the research practice of mathematicians simplicity is often treated as a characteristic feature of understanding. Therefore, as G.-C. Rota writes: "[i]t is an article of faith among mathematicians that after a new theorem is discovered, other, simpler proof of it will be given until a definitive proof is found" [23, p. 192].

> It is also worth quoting in this context Aschbacher who wrote:
>
> The first proof of a theorem is usually relatively complicated and unpleasant. But if the result is sufficiently important, new approaches replace and refine the original proof, usually by embedding it in a more sophisticated conceptual context, until the theorem eventually comes to be viewed as an obvious corollary of a larger theoretical construct. Thus proofs are a means for establishing what is real and what is not, but also a vehicle for arriving at a deeper understanding of mathematical reality [1, p. 2403].

As indicated above a concept of a "normal" proof used by mathematicians in their research practice (we called it "informal" proofs) is in fact vague and not precise.

. . . Murawski: [24], §2. Proof in Mathematics: Formal vs Informal, pp.11-12.

Thus the *evidence-based* perspective underlying this hypothetical reconstruction, of the possible reasoning that had motivated Fermat to record his marginal noting postulating FLT laconically, is that *pre-formal*, *evidence-based*, mathematical *truth* is *stronger* than mathematical *proof* since, contrary to the inherited relationship between mathematical *truth* and mathematical *proof* in current mathematical paradigms:

1. The *proof* of a mathematical proposition in a first-order mathematical language does not entail its *truth* in any first-order mathematical language if the proposition has no *evidence-based* interpretation;

2. The *truth* of a mathematical proposition that has a *pre-formal*, *evidence-based*, interpretation entails its *proof* in some, suitably defined, first-order mathematical language.

6. Should Wiles' *pre-formal* proof of FLT be treated putatively as sufficiently *formal*?

The significance of, and need for, explicitly explicating further Pantsar's distinction between *formal* and *pre-formal* proofs, of mathematical propositions, is highlighted by Michael Harris' recent questioning of the necessity for a foundational perspective that would justify *why* Wiles' proof of FLT may be treated putatively as a *logically true* arithmetical proposition:

> "After Wiles' breakthrough, it became common to hear talk of a new "golden age" of mathematics, especially in number theory, the field in which the Fermat problem belongs. The methods introduced by Wiles and Taylor are now part of the toolkit of number theorists, who consider the FLT story closed. But number theorists were not the only ones electrified by this story.
>
> I was reminded of this unexpectedly in 2017 when, in the space of a few days, two logicians, speaking on two continents, alluded to ways of enhancing the proof of FLT—and reported how surprised some of their colleagues were that number theorists showed no interest in their ideas.
>
> The logicians spoke the languages of their respective specialties—set theory and theoretical computer science—in expressing these ideas. The suggestions they made were intrinsically valid and may someday give rise to new questions no less interesting than Fermat's. Yet it was immediately clear to me that these questions are largely irrelevant to number theorists, and any suggestion that it might be otherwise reflects a deep misunderstanding of the nature of Wiles' proof and of the goals of number theory as a whole.
>
> The roots of this misunderstanding can be found in the simplicity of FLT's statement, which is responsible for much of its appeal: If n is any positive integer greater than 2, then it is impossible to find three positive numbers a, b and c such that
>
> $$a^n + b^n = c^n$$
>
> This sharply contrasts with what happens when n equals 2: Everyone who has studied Euclidean geometry will remember that $3^2 + 4^2 = 5^2$, that $5^2 + 12^2 = 13^2$, and so on (the list is infinite). Over the last few centuries, mathematicians repeatedly tried to explain this contrast, failing each time but leaving entire branches of mathematics in their wake. These branches include large areas of the modern number theory that Wiles drew on for his successful solution, as well as many of the fundamental ideas in every part of science touched by mathematics. Yet no one before Wiles could substantiate Fermat's original claim."
>
> *. . . Harris: [18], Other publications, #21.*

Prima facie, Harris seems to hold that 'the simplicity of FLT's statement' and, presumably, the seeming straightforwardness of his following outline of the argument underlying Wiles' proof—covering 'large areas of the modern number theory that Wiles drew on for his successful solution, as well as many of the fundamental ideas in every part of science touched by mathematics'—should suffice for establishing FLT informally (also *pre-formally* in Pantsar's sense) as a *logically true* arithmetical proposition which substantiates Fermat's original claim:

> ". . . Wiles' proof, complicated as it is, has a simple underlying structure that is easy to convey to a lay audience. Suppose that, contrary to Fermat's claim, there is a triple of positive integers a, b, c such that
>
> (A) $a^p + b^p = c^p$
>
> for some odd prime number p (it's enough to consider prime exponents). In 1985, Gerhard Frey pointed out that a, b and c could be rearranged into
>
> (B) a new equation, called an elliptic curve,
>
> with properties that were universally expected to be impossible. More precisely, it had long been known how to leverage such an elliptic curve into
>
> (C) a Galois representation,
>
> which is an infinite collection of equations that are related to the elliptic curve, and to each other, by precise rules.
>
> The links between these three steps were all well-understood in 1985. By that year, most number theorists were convinced—though proof would have to wait—that every Galois representation could be assigned, again by a precise rule,

(D) a modular form,

which is a kind of two-dimensional generalization of the familiar sine and cosine functions from trigonometry.

The final link was provided when Ken Ribet confirmed a suggestion by Jean-Pierre Serre that the properties of the modular form entailed by the form of Frey's elliptic curve implied the existence of

(E) another modular form, this one of weight 2 and level 2.

But there are no such forms. Therefore there is no modular form (D), no Galois representation (C), no equation (B), and no solution (A).

The only thing left to do was to establish the missing link between (C) and (D), which mathematicians call the modularity conjecture.

This missing link was the object of Wiles' seven-year quest. It's hard from our present vantage point to appreciate the audacity of his venture. Twenty years after Yutaka Taniyama and Goro Shimura, in the 1950s, first intimated the link between (B) and (D), via (C), mathematicians had grown convinced that this must be right. This was the hope expressed in a widely read paper by André Weil, which fit perfectly within the wildly influential Langlands program, named after the Canadian mathematician Robert P. Langlands. The connection was simply too good not to be true. But the modularity conjecture itself looked completely out of reach. Objects of type (C) and (D) were just too different."

... Harris: [18], Other publications, #21.

Comment: We note that in the reconstruction of Fermat's unrecorded, putative 'proof' of FLT in §3., instead of Harris' (B) above, we consider the arithmetical expression detailed in §2.A.:

(i) $x^n = 2.^nC_1(k+\frac{a}{n^n})y^{n-1} + 2^2.^nC_2(k+\frac{a}{n^n})^2y^{n-2} + \ldots + 2^n(k+\frac{a}{n^n})^n$

and, instead of Harris' (C) above, we consider the corresponding geometrical configuration of n-dimensional mathematical objects as defined and detailed in §2.A.:

(ii) $\mathbb{C}_{Sym}(\overline{\overline{z^n} \ -_{\mathbb{H}_n} \overline{y^n}}) =_{\mathbb{H}_n} 2.^nC_1\overline{(k+\frac{a}{n^n}) \times_{\mathbb{H}_n} y^{(n-1)}} +_{\mathbb{H}_n} 2^2.^nC_2\overline{(k+\frac{a}{n^n})^2 \times_{\mathbb{H}_n} y^{(n-2)}} +_{\mathbb{H}_n} \ldots +_{\mathbb{H}_n} 2^n\overline{(k+\frac{a}{n^n})^n}$

By extrapolating the pictorial argument for $n = 1, 2, 3$ in §2.A., and considering what is entailed by Definitions 1, 2, and 3 in the general case, we then argue *pre-formally* in §3.A. that (i) defines (ii) as *uniquely isomorphic* if, and only if, $n < 3$. We conclude that this entails FLT.

We further note that:

- whilst Wiles' *analytic* proof appeals to properties of *real* and *complex* numbers[24] for establishing that: 'the missing link between (C) and (D)' entails that 'there is no modular form (D), no Galois representation (C), no equation (B), and no solution (A)' for some odd prime p;
- the *pre-formal* proof in §3.A. is *elementary*, since it does *not* appeal to properties of *real* and *complex* numbers[25] for establishing that: for $n > 2$, (i) above does not define (ii) as *uniquely isomorphic* by Definition 2, thereby entailing that there is no solution (A) for some odd prime p.

Moreover, prima facie, it is not obvious whether Andrew Wiles' 1995 proof of FLT, as outlined above by Michael Harris, *implicitly* admits—or needs to *explicitly* admit—definitions corresponding to Definitions 1, 2, and 3.

In other words, it is not obvious whether, for prime $p > 3$, a modular form of 'weight 2' and 'level 2' exists if, and only if, the p-D object $2^p\overline{(k+\frac{a}{p^p})^p}$ is *well-defined* (by §2.A., Definition 3).

Harris acknowledges that establishing FLT as a theorem within a formal system such as the first-order Zermelo-Fraenkel set theory ZFC, or a first-order Peano Arithmetic such as PA, may be desirable in principle; since both can lay claim to admitting automated theorem proving that would, then, establish FLT additionally as an algorithmically *computable* (logical) *truth* under any *well-defined* (i.e., *evidence-based*[26]) Tarskian

[24]As do the 1896 proofs of the Prime Number Theorem by Jacques Hadamard and Charles Jean de la Vallée Poussin (see [30], Theorem 3.7, p.44).

[25]As is the case with the 1948 proofs of the Prime Number Theorem by Atle Selberg and Paul Erdős (see [16], Theorem 6, p.9).

[26]As detailed in [1], §3 (see also [2], §2.A) in the case of PA.

interpretation of the concerned formal theory:

> "Mathematical logic was developed with the hope of placing mathematics on firm foundations—as an axiomatic system, free of contradiction, that could keep reasoning from slipping into incoherence."
>
> *... Harris: [18], Other publications, #21.*

However he questions both the practical utility and theoretical necessity of such rigour in the absence of a consensus on what constitutes a mathematical language of categorical communication:

> "Although Kurt Gödel's work revealed this hope to be chimerical, many philosophers of mathematics, as well as some logicians (a small but vocal minority, according to the set theorist), still regard ZFC and the requirements listed above as a kind of constitution for mathematics.
>
> Mathematicians never write proofs this way, however. A logical analysis of Wiles' proof points to many steps that appear to disregard ZFC, and this is potentially scandalous: When mathematicians make up rules without checking their constitutionality, how can they know that everyone means the same thing?"
>
> *... Harris: [18], Other publications, #21.*

Instead, he justifies his perspective of the validity of Wiles' proof of FLT by commenting, from a professional mathematician's perspective, that:

> "More recently, in the fall of 2016, for example, 10 mathematicians gathered at the Institute for Advanced Study in Princeton, New Jersey, in a successful effort to prove a connection between elliptic curves and modular forms in a new setting. They had all followed different routes to understanding the structure of Wiles' proof, which appeared when some of them were still small children. If asked to reproduce the proof as a sequence of logical deductions, they would undoubtedly have come up with 10 different versions. Each one would resemble the (A) to (E) outline above, but would be much more finely grained.
>
> Nevertheless—and this is what is missing from the standard philosophical account of proof—each of the 10 would readily refer to their own proof as Wiles' proof. They would refer in a similar way to the proofs they studied in the expository articles or in the graduate courses they taught or attended. And though each of the 10 would have left out some details, they would all be right.
>
> What kind of thing is Wiles' proof, if it comes in so many different flavors? In philosophy of mathematics it's customary to treat a published proof as an approximation of an ideal formalized proof, capable in principle of being verified by a computer applying the rules of the formal system. Nothing outside the formal system is allowed to contaminate the ideal proof—as if every law had to carry a watermark confirming its constitutional justification.
>
> But this attitude runs deeply counter to what mathematicians themselves say about their proofs. Mathematics imposes no ideological or philosophical litmus test, but I'm convinced that most of my colleagues agree with the late Sir Michael Atiyah, who claimed that a proof is "an ultimate check—but it isn't the primary thing at all." Certainly the published proof isn't the primary thing.
>
> Wiles and the number theorists who refined and extended his ideas ... were certainly aware that a proof like the one Wiles published is not meant to be treated as a self-contained artifact. On the contrary, Wiles' proof is the point of departure for an open-ended dialogue that is too elusive and alive to be limited by foundational constraints that are alien to the subject matter."
>
> *... Harris: [18], Other publications, #21.*

From the *evidence-based* perspective of this *pre-formal* reconstruction of what Fermat might have intuited when making his marginal notation on FLT, Harris could be viewed as drawing upon his earlier perceptions of mathematical 'truth', mathematical 'knowledge', and mathematical 'intuition' for his defense that Wiles' proof can be viewed putatively as *logically true*:

> "It will therefore come as a surprise ... to many philosophers, that truth is also a secondary issue in mathematics. Of course we want to prove true theorems, but this is hardly an adequate or even useful description of our objective. Mathematicians, and scientists for that matter, judge our peers not by the truth of their work but by how interesting it is[52]. ...
>
> This point is hardly novel; Lévy-Leblond says something similar in IS (p. 39), and Dieudonné distinguishes further between "mathématiques vides" and "mathématiques significatives."[54] But it is surprising to see just how little we seem to be concerned with "truth" these days. Mathematicians rarely discuss foundational issues any more[55], so it was significant that an article by Arthur Jaffe and Frank Quinn, reaffirming

the importance of rigorous proof in the current context of strong interaction between physics and mathematics, provoked no fewer than 16 responses by eminent mathematicians, physicists, and historians. No two of the positions expressed were identical, which already should suggest caution in laying down the law on rationality, as Sokal and Bricmont (and Lévy-Leblond, see note *) seem inclined to do. But for our purposes here, what is remarkable is that almost none of the responses had much to say about "truth."[56] "Truth" was central, predictably, only to the responses of Chaitin and Glimm. Chaitin's branch of mathematics treats "truth" as a technical term, without metaphysical connotations, and Chaitin's claim to have "found mathematical truths that are true for no reason at all" suggests that it may be harder than Fredkin suspects to know just when to award his prize. Glimm's brand of truth is quite the opposite: it "lies not in the eye of the beholder, but in objective reality ... It is thus reproducible across barriers of distance, political boundaries and time."[57] Turning to the introduction to the book Quantum Physics, by Glimm and Jaffe, one finds the unusual assertion that "mathematical analysis must be included in the list of appropriate methods in the search for truth in theoretical physics." Generally speaking, the mathematics department may be the only spot on campus where belief in the reality of the external world is not only optional but frequently an annoying distraction. But this patently does not apply to mathematical physicists, and I can't help thinking it's not a coincidence that both Bricmont and Sokal are amply represented in the Glimm-Jaffe bibliography.

Philosophers and philosophically-minded sociologists concerned with mathematics seem to think their job is to explain mathematical truth. Edinburgh sociologist David Bloor and philosopher Philip Kitcher, cast for science wars purposes as an irresponsible relativist and a moderate realist, respectively,[58] have both attempted to develop empiricist accounts of mathematical knowledge[59]. (Knowledge and truth are not synonyms but they are on the same wavelength.[60]) They have their own (very different) reasons, but in so doing I'm convinced they have missed the point of mathematics. As is typical in such discussions, their examples are drawn either from mathematical logic or from mathematics no more recent than the 19th century. If the sociologist, at least, had done some field work, he couldn't have helped observing that what mathematicians seem to value most are "ideas" (not necessarily of the Platonic variety); the most respected mathematicians are those with strong "intuition." Now intuition, the philosopher assures us, is philosophically indefensible; Sokal and Bricmont add that "intuition cannot play an explicit role in the reasoning leading to the verification (or falsification) of these theories, since this process must remain independent of the subjectivity of individual scientists."[61] Fredkin's theorem-proving machine may see things that way, but what are we [t]o make of Thurston's emphasis on the "continuing desire for human understanding of a proof, in addition to knowledge that the theorem is true"?[62] We know what he means, as we know what Robert Coleman means, when, having discovered a gap in Manin's proof of Mordell's conjecture over function fields, he nevertheless writes "I believe that all this is testimony to the power and depth of Manin's intuition."[63] Is Coleman trying to slip a counterfeit coin between the context of discovery and the context of justification? Do these offhand comments touch on something genuine and profound about mathematics? Or is it just my indoctrination that makes me think so?"

... Harris: [17], Other publications, #2.

6.A. Resolving the persisting ambiguity in current paradigms on the nature of, and relation between, mathematical *truth* and mathematical *proof*

If so, although Harris' perspective faithfully reflects current paradigms on the nature of, and relation between, mathematical *truth* and mathematical *proof*, it also thereby admits a persisting ambiguity[27] in inherited paradigms that appears unsympathetic to his argumentation. Harris' reasoning may, thus, also need to accommodate a putative resolution of such ambiguity as articulated by the Complementarity Thesis:

"**Thesis 1. (Complementarity Thesis)** *Mathematical 'provability' and mathematical 'truth' need to be interdependent and complementary, 'evidence-based', assignments-by-convention towards achieving*:

(1) *The goal of proof theory, post Peano, Dedekind and Hilbert, which is*:

— *to uniquely characterise each informally defined mathematical structure S (e.g., the Peano Postulates and their associated, classical, predicate logic)*,

– *by a corresponding, formal, first-order language L, and a set P of finitary axioms/axiom schemas and rules of inference (e.g., the first-order Peano Arithmetic PA and its associated first-order logic FOL)*,

- *which assign unique provability values (provable/unprovable) to each well-formed proposition of the language L without contradiction*;

[27]See [2], §5.B, *A removable ambiguity in Brouwer-Heyting-Kolmogorov realizability.*

(2) *The goal of constructive mathematics, post Brouwer and Tarski, which must be*:

— *to assign unique, evidence-based, truth values (true/false) to each well-formed proposition of the language* L,

– *under an, unarguably constructive, well-defined interpretation* $\mathcal{I}$ *over the domain* D *of the structure* S,

- *such that the provable formulas of* L *are true under the interpretation.*"

. . . Anand: [2], §1.

The Complementarity Thesis, thus, seeks to restrict appeal in formal assignments of mathematical *truth* and mathematical *proof* to only *evidence-based* 'pre-formal', reasoning[28], rather than admit *belief-based* 'informal' reasoning as in current paradigms[29].

Consequently:

- Whilst the focus of a *formal* theory *may* be viewed as seeking to ensure that any mathematical language intended to represent our conceptual metaphors and their inter-relatedness is unambiguous, and free from contradiction;
- The focus of *pre-formal* mathematics *must* be viewed as seeking to ensure that any such representation does, indeed, uniquely identify and adequately represent such metaphors and their inter-relatedness.

Further, the epistemological perspective of the Complementarity Thesis is that logic, too, can be viewed as merely a methodological tool that seeks to formalise an intuitive human ability that pertains not to the language which seeks to express it formally, but to the cognitive sciences in which its study is rooted:

"**Definition 1 (Well-defined logic)** *A finite set* λ *of rules is a well-defined logic of a formal mathematical language* L *if, and only if,* λ *assigns unique, evidence-based, values*:

(a) *Of provability/unprovability to the well-formed formulas of* L*; and*

(b) *Of truth/falsity to the sentences of the Theory* $T(U)$ *which is defined semantically by the* λ*-interpretation of* L *over a given mathematical structure* U *that may, or may not, be well-defined; such that*

(c) *The provable formulas interpret as true in* $T(U)$.

Comment: We note that although the question of whether or not λ categorically defines a unique Theory $T(U)$ is mathematical, the question of whether, and to what extent, any Theory $T(U)$ succeeds (in the sense of Carnap's *explicatum* and *explicandum* in [7]) in faithfully representing the structure U—which, from the *evidence-based* perspective of this investigation, can be viewed as corresponding to Pantsar's *pre-formal mathematics* in [26] (§4. Formal and pre-formal mathematics)—is a philosophical question for the cognitive sciences (cf. [20]; see also [2], §25), where:

> "By the procedure of *explication* we mean the transformation of an inexact, prescientific concept, the *explicandum*, into a new exact concept, the *explicatum*. Although the explicandum cannot be given in exact terms, it should be made as clear as possible by informal explanations and examples. . . . A concept must fulfill the following requirements in order to be an adequate explicatum for a given explicandum: (1) similarity to the explicandum, (2) exactness, (3) fruitfulness, (4) simplicity." *. . . Carnap: [7], p.3 & p.5.*"
>
> *. . . Anand: [2], §1.B.*

Thus, from the *evidence-based* perspective of [1] and [2], both *pre-formal* mathematics, and *formal* mathematics, ought to be viewed more appropriately as (see [2], §1.A):

— a *necessary* set of complementary, symbolic, languages (see [2], §13),

— intended to serve Philosophy and the Natural Sciences (see [2], §13.C),

— by seeking to provide the necessary tools for adequately expressing our sensory observations—and their associated perceptions (and abstractions)—of a 'common' external world;

— corresponding to what some cognitive scientists, such as Lakoff and Núñez in [20] (see also [2], §25), term as *primary* and *secondary* 'conceptual metaphors',

[28]In the sense of Gualtiero Piccinini's *knowledge as factually grounded belief* (see [27]; also [2], §5.A).

[29]In the inherited, *classical*, sense of Plato's *knowledge as justified true belief* (see [27]; also [2], §5.A).

— in a symbolic language of unambiguous expression and, ideally, categorical communication.

Moreover, we may need to recognise explicitly that *evidence-based* reasoning (see [2], §13.E):

(a) restricts the ability of highly expressive mathematical languages, such as the first-order Zermelo-Fraenkel Set Theory ZF, to *categorically* communicate abstract concepts corresponding to Lakoff and Núñez's *secondary* conceptual metaphors in [20] (such as those involving Cantor's first limit ordinal ω[30]);

and:

(b) restricts the ability of effectively communicating mathematical languages, such as the first-order Peano Arithmetic PA, to *well-define* infinite concepts (such as ω).

6.B. The significance of *evidence-based* reasoning for Wiles' proof

Consequently, from the perspective of any discipline which claims (whether *explicitly* or *implicitly*) to appeal only to *evidence-based* reasoning, any claim that Wiles' proof can be treated as a *categorically* communicable *logical truth* may *necessarily* require its validation as a finite sequence of *formal* propositions, each of which is *necessarily* algorithmically *verifiable* (in the sense of [1], Definition 1), for any *specified* instantiation, as a *logically true* proposition under a *well-defined* Tarskian interpretation of some recursively *well-defined* set of axioms/axiom schemata and rules of deduction.

Such validation would also validate the status of Wiles' proof as *pre-formally* justified, *evidence-based*, reasoning that is a legitimate contender, even if not a claimant, to being treated as a *logically true*, rather than a *questionably true*, arithmetical proposition:

> "...How do we know Wiles' proof of Fermat's Last Theorem, completed by Taylor and Wiles, is correct? Although this particular theorem, better publicized than any in history, has been treated with unusual care by the mathematical community, whose "verdict" is developed at length in a graduate textbook of exceptionally high quality, I'd guess that no more than 5% of mathematicians have made a real effort to work through the proof[64]. Some scientists (and some mathematicians as well) apparently view Wiles and his proof as an "anachronism."[65] The general public is not entirely convinced. Why are we? Can a sociologist study this question without knowing the proof? Can mathematicians pose the question in terms sociologists would find meaningful? Knowing the truth of the matter is obviously of no help, and relativism is not the issue: it's not clear what kind of "reality" would be relevant to settling the question, but the fact that no one has found a counterexample is certainly not a good candidate. ...
>
> Few of us would choose to treat our belief that Wiles proved Fermat's last theorem as "a mythical and false ideology," but is it possible that our attempts to justify this belief always involve an element of self-delusion? And how are we to convince a skeptical outsider that this is not the case? The only reasonable answers that come to mind are empirical in nature, and specifically historical and sociological, rather than philosophical.[111] We would have to pay attention to the question of how knowledge is transmitted among mathematicians. Fermat's last theorem provides a particularly good test case. Wiles' proof generated an unprecedented[112] number of reports, survey articles, colloquium talks, working seminars, graduate courses, and mini-conferences, as well as books, newspaper and magazine articles, television reports, and other forms of communication with non-mathematicians. Not to mention the spate of announcements, designed to impress public policy-makers and the public at large, citing Wiles' work as proof that mathematics "has never been healthier.**" Has anyone been keeping track of all these incitements to belief formation, checking them for contamination by myth and false ideology?
>
> Studying questions like these provides a second answer to the thought experiment proposed above, complementary to the answer we would naturally provide based on our experience as mathematicians, and potentially just as interesting. Leaving aside romantic rhetoric, these two answers are not in competition, much less on opposite sides of a battlefield. Arriving at the second answer would be the work of sociologists. For this, full cooperation with mathematicians would be necessary. The examples just cited provide hope that such cooperation may be possible."
>
> *... Harris: [17], Other publications, #2.*

Moreover, the need for such rigour—in any *proof* of *number-theoretic* propositions that, *explicitly* or *implicitly*, appeals essentially to set-theoretical reasoning—is that (see [2], §1.A) it would also address an earlier issue raised by Harris in [17], concerning the epistomological status of set-theoretically defined real numbers:

[30] See [20], Preface, p.*xii-xiii*: "*How can human beings understand the idea of actual infinity?*"

> "More interestingly, one can ask what kind of object π was before the formal definition of real numbers. To assume the real numbers were there all along, waiting to be defined, is to adhere to a form of Platonism.[34] Dedekind wouldn't have agreed.[35] In a debate marked by the accusation that postmodern writers deny the reality of the external world, it is a peculiar move, to say the least, to make mathematical Platonism a litmus test for rationality.[36] Not that it makes any more sense simply to declare Platonism out of bounds, like Lévy-Leblond, who calls Stephen Weinberg's gloss on Sokal's comment "une absurdité, tant il est clair que la signification d'un concept quelconque est évidemment affectée par sa mise en œuvre dans un contexte nouveau!"[37] Now I find it hard to defend Platonism with a straight face, and I prefer to regard formula $\pi^2 = 6\zeta(2)$ as a creation rather than a discovery. But Platonism does correspond to the familiar experience that there is something about mathematics, and not just about other mathematicians, that precisely doesn't let us get away with saying "évidemment"![38] This experience is clearly captured by Alain Connes, a selfavowed Platonist, in his dialogue with neurobiologist J.-P. Changeux, who (to oversimplify) expects to find mathematical structures in the brain.[39] I don't think Connes (or Roger Penrose, another prominent Platonist) is confused about reality, and I have a hard time imagining a neuronal representation that does justice to the concept of π. But the ontological issues are far from settled, and while there is no reason to assume they will ever be settled, the important point is that this situation is not an obstacle to mathematics, much less to rationality.[40] The real absurdity is to claim otherwise."
>
> *. . . Harris: [17], Other publications, #2.*

Thus, from an *evidence-based* perspective, set-theoretically defined real numbers exist *merely* as axiomatically *postulated* mathematical objects[31] *only within*[32] any first-order set theory such as ZF; whilst *only* those of such numbers that can further be defined arithmetically exist as axiomatically *postulated* mathematical objects (symbols) also *within* any first-order arithmetic such as PA.

Moreover, only the latter have the *evidence-based* properties that can be communicated under a *finitary* interpretation of PA (as detailed in [1], §6, p.40), as algorithmically *verifiable* (i.e., *logical*) *truths* which can, then, be treated as *factually grounded knowledge* (in the sense of [2], §5.A) when describing properties of the *actual* universe we inhabit.

In other words:

— although ZF admits unique, set-theoretical, definitions of—and allows us to unambiguously talk about the *putative* existence of—'*ideal*' real numbers as the *putative* limits of Cauchy sequences of rational numbers in a mathematically *well-defined*, albeit Platonically conceived, *putative* set-theoretical universe, ZF has no *well-defined* Tarskian interpretation that would necessarily *evidence* a ZF theorem over the *finite ordinals* as an algorithmically *computable* truth over the *natural numbers* in the interpretation[33];

---

[31] More specifically, as symbols corresponding to what George Lakoff and Rafael Núñez describe as *secondary* conceptual metaphors in [20] (see also [2], §13.F, *Three categories of information*, and [2], §25.F, *The Veridicality of Mathematical Propositions*).

[32] As stressed by Thoralf Skolem in [29] (see also [2], §7.K., *Skolem's paradox: intended and unintended interpretations of PA*), where he draws attention to a:

> **Skolem's (apparent) paradox**: ". . . peculiar and apparently paradoxical state of affairs. By virtue of the axioms we can prove the existence of higher cardinalities, of higher number classes, and so forth. How can it be, then, that the entire domain B can already be enumerated by means of the finite positive integers? The explanation is not difficult to find. In the axiomatization, "set" does not mean an arbitrarily defined collection; the sets are nothing but objects that are connected with one another through certain relations expressed by the axioms. Hence there is no contradiction at all if a set M of the domain B is non-denumerable in the sense of the axiomatization; for this means merely that *within* B there occurs no one-to-one mapping Φ of M onto Z_o (Zermelo's number sequence). Nevertheless there exists the possibility of numbering all objects in B, and therefore also the elements of M, by means of the positive integers; of course such an enumeration too is a collection of certain pairs, but this collection is not a "set" (that is, it does not occur in the domain B)."
>
> *. . . Skolem: [29], p.295.*

[33] A striking example is that of Goodstein's Theorem, where it can be argued that, although the finite ordinals can be meta-mathematically put into a 1-1 correspondence with the natural numbers:

> "Goodstein's sequence $G_o(m_o)$ over the finite ordinals in any putative model $\mathbb{M}$ of ACA_0 *terminates* with respect to the ordinal inequality '$>_o$' even if Goodstein's sequence $G(m)$ over the natural numbers *does not terminate* with respect to the natural number inequality '$>$' in $\mathbb{M}$."
>
> *. . . Anand: [2], §18, Theorem 18.1.*

— only PA, by virtue of the Provability Theorem for PA (see [1], Theorem 7.1, p.41), admits unique, algorithmically *verifiable*, number-theoretic definitions of—and allows us to unambiguously talk about the *categorical* existence of (see [2], §7.1)—*specifiable* real numbers (see [2], Theorem 7.5), and their properties which, under a *finitary* interpretation of PA (as detailed in [1], §6, p.40), can be communicated as algorithmically *verifiable* (i.e., *logical*) *truths* which can be treated as *factually grounded knowledge* (in the sense of [2], §5.A) when describing properties of the *actual* universe we inhabit.

The fragility of uncritically accepting 'sociological validation of proofs'—particularly those that appeal necessarily to an, *essentially unverifiable*, set-theoretical axiom of infinity—in lieu of logical, *evidence-based*, validity is highlighted by Henk Barendregt and Freek Wiedijk in [6] 'The Challenge of Computer Mathematics'[34]:

> "During the course of history of mathematics proofs increased in complexity. In particular in the 19-th century some proofs could no longer be followed easily by just any other capable mathematician: one had to be a specialist. This started what has been called the sociological validation of proofs. In disciplines other than mathematics the notion of peer review is quite common. Mathematics for the Greeks had the 'democratic virtue' that anyone (even a slave) could follow a proof. This somewhat changed after the complex proofs appeared in the 19-th century that could only be checked by specialists. Nevertheless mathematics kept developing and having enough stamina one could decide to become a specialist in some area. Moreover, one did believe in the review by peers, although occasionally a mistake remained undiscovered for many years. This was the case with the erroneous proof of the Four Colour Conjecture by Kempe [1879].
>
> In the 20-th century this development went to an extreme. There is the complex proof of Fermat's Last Theorem by Wiles. At first the proof contained an error, discovered by Wiles himself, and later his new proof was checked by a team of twelve specialist referees[†]. Most mathematicians have not followed in detail the proof of Wiles, but feel confident because of the sociological verification."
>
> *. . . Barendregt and Wiedijk: [6], 1. The Nature of Mathematical Proof.*

Such 'fragility' highlights the significance of seeking an *evidence-based*, *pre-formal*, basis for not only a *deeper* understanding of formal reasoning as in Wiles' proof of FLT, but also for *identifying* as mathematically significant *only* those formal proofs that admit conceptualisation as *pre-formal* proofs.

Thus, in his recent 'Varieties of Mathematical Understanding', Jeremy Avigad acknowledges that if 'the goal of mathematics is to obtain a *conceptual* understanding of mathematical phenomena, and a *deep* understanding at that', then—particularly with the attraction and 'use of computational proof assistants to develop libraries of formally checked mathematics'—Wiles' proof of FLT is evidence that a 'really deep proof often requires more background knowledge than any one person can master' and, ipso facto, *visualise* and/or *understand*:

> "It is common to say that the goal of mathematics is to obtain a *conceptual* understanding of mathematical phenomena, and a *deep* understanding at that. The Laglands program, which seeks to develop far-reaching connections between number theory and geometry, is often held as a paradigm of conceptual depth. What makes it so? To develop some intuitions, I will draw on informal writings by Kevin Buzzard, a number theorist at Imperial College in London. In 2017, Buzzzard launched his *Xena* blog,[1] in part to document his newfound interest in the use of computational proof assistants to develop libraries of formally checked mathematics.
>
> . . .
>
> One observation is that deep mathematics is usually pretty complicated. Generally speaking, the deeper the result, the harder it is for the general public or even mathematicians not directly involved with the research to appreciate it.
>
>> So what are the mathematicians I know interested in? Well, let's take the research staff in my department at Imperial College. They are working on results about objects which in some cases take hundreds of axioms to define, or are even more complicated: sometimes even the definitions of the objects we study can only be formalised once one has proved hard theorems. For example the definition of a Shimura variety over a number field can only be made once one has proved most of the theorems in Deligne's paper on canonical models, which in turn rely on the theory of CM abelian varieties, which in turn rely on the theorems of global class field theory. That's the kind of definitions which mathematicians in my department get excited about [. . .]. I once went to an entire 24 lecture course by John Coates which assumed local

[34] As also by Melvyn B. Nathanson in [25], 'Desperately Seeking Mathematical Truth'.

> class field theory and deduced the theorems of global class field theory. I have read enough of the book by Shimura and Taniyama on CM abelian varieties to know what's going on there. I have been to study group on Deligne's paper on canonical models. So after perhaps 100 hours study absorbing the prerequisites, I was ready for the definition of a Shimura variety over a number field. And then there is still the small matter of the definition of étale cohomology. (*Xena*, July 6, 2018)

Perhaps more important than the complexity of statements and definitions is the complexity of the proofs. A really deep proof often requires more background knowledge than any one person can master.

> To completely understand a proof of FLT (let's say, for now, the proof explained in the 1995 Darmon-Diamond-Taylor paper) you will need to be a master of the techniques used by Langlands in his proof of cyclic base change (and I know people who are), and a master of Mazur's work on the Eisenstein ideal (and I know people who are). But what about the far less sexy technical stuff? To move from the complex analytic theory of modular forms to the algebraic theory of moduli spaces of elliptic curves (and I know people who know this—but I went through some of this stuff once and it's far more delicate than I had imagined, and there are parts of it where the only useful reference seems to be Brian Conrad's brain). This last example is perhaps a good example of a tedious technical issue which it's very easy to forget about, because the results are intuitive and the proofs can be technical. There are many other subtleties which one would have to fully understand because they're on the syllabus. Is there really one human being who would feel confident answering questions on all of this material? I am really not sure at all. (*Xena*, September 27, 2019)

It would be a mistake, however, to equate depth with complexity, and other postings on *Xena* make it clear that complexity is only a means to an end. Complex definitions and proofs are worth the effort when they provide answers to questions that are judged by the community to be interesting and important."
... Avigad: [3], §3, Conceptual Understanding and Depth.

Moreover, whilst acknowledging that the current emphasis on trying to grasp the totality of formal reasoning—such as all the 'subtleties' involved in Wiles' proof of FLT—could tend to 'equate depth with complexity', Avigad makes 'it clear that complexity is only a means to an end': a conceptual understanding of what is termed as 'deep' mathematics.

Thus, from the *evidence-based* perspective of this investigation, the 'deeper' significance of §3., Proposition 3.1, for the Complementarity Thesis ([2], §1, Thesis 1), is that it illustrates the symbiotic inter-dependence of formal provability and *evidence-based*, *pre-formal*, truth, since it is the lack of *well-definedness* of the *evidence-based*, arithmetical property §3.(4(i)), in the hyper-geometric representation §3.(4), of the formal arithmetical relation $x^n + y^n = z^n$, which yields the *pre-formal*, transparent, proof of Fermat's Last Theorem in §3..

In other words, one could conjecture that the challenges in, and illusory barriers to, formulating a formal proof of Fermat's Last Theorem, and in reconstructing Fermat's putative 'Lost Proof', has been rooted in a philosophy that views interpreted mathematical truth as an adjunct entailment of mathematical provability, rather than as a necessarily transparent, and equal, *evidence-based* prerequisite for determining in a formal proof theory which axiomatic assumptions underlie the truth of *pre-formal*, *evidence-based*, reasoning.

7. Conclusions

In this investigation, we have argued for the perspective that:

1. Conventional wisdom dictates that proofs of mathematical propositions should be treated as *necessary* and *sufficient* for entailing *significant* mathematical truths *only* if the proofs are expressed in a—minimally, deemed consistent—formal mathematical theory in terms of:

    - Axioms/Axiom schemas
    - Rules of Deduction
    - Definitions
    - Lemmas
    - Theorems

- Corollaries

2. Whilst Andrew Wiles' proof of FLT, which appeals essentially to geometrical properties of *real* and *complex* numbers, can be treated as meeting this criteria, it nevertheless leaves two questions unanswered:

   (i) Why is $x^n + y^n = z^n$ solvable only for $n < 3$?

   (ii) What technique might Fermat have used that led him to, even if only briefly, believe he had 'a truly marvellous demonstration' of FLT?

3. Prevailing post-Wiles wisdom—leaving (i) essentially unaddressed—dismisses Fermat's claim as a conjecture without a plausible proof of FLT.

4. However, we posit that providing *evidence-based* answers to both queries is *necessary* not only for treating FLT as *significant*, but also for *understanding* why FLT can be treated as a *true* arithmetical proposition.

5. We thus argue that:

   - proving a theorem formally from explicit, and implicit, premises/axioms using currently accepted rules of deduction is a *meaningless* game;
   - of little scientific value in the absence of *evidence* that has already established—unambiguously—why the premises/axioms and rules of deduction can be treated, and categorically communicated, as *pre-formal* truths in Marcus Pantsar's sense in [26];
   - which is not the case with either the first-order set Theory ZF, or the second-order Peano Arithmetic ACA_0, both of which admit real and complex analysis but, like all formal theories that admit infinite objects such as Cantor's transfinite ordinal ω, are only *deemed* consistent since neither has any *evidence-based* interpretation.

6. Consequently, whilst *evidence-based*, *pre-formal*, truth (such as, for instance, formally defined in [1], §3, and [2], §2.A, in the case of the first-order Peano Arithmetic PA) can entail formal provability, the formal proof of a mathematical theorem *cannot* be assumed to always entail its *pre-formal* truth as a *significant*, *evidence-based*, truth.

7. In other words, a proof sequence within a formal mathematical language:

   - can *only* identify the explicit/implicit premises that have been used to *evidence* the, *already established*, *pre-formal* truth of the mathematical proposition sought to be *unambiguously expressed*, proven and *categorically communicated* by means of the language;
   - *provided* the language has an *evidence-based* interpretation;
   - such as, for instance, the finitary, algorithmically *computable*, interpretation $\mathcal{I}_{PA(N,SC)}$ of PA detailed in [1], §6, Theorem 6.8 (*PA is consistent*).

8. Hence *visualising* and *understanding* the *evidence-based*, *pre-formal*, truth of a mathematical proposition is the *only* raison d'être for *subsequently* seeking a formal proof of the proposition within a formal mathematical language (whether first-order or second order set theory, arithmetic, geometry, etc.).

9. By this yardstick Andrew Wiles' proof of FLT fails to meet the required, *evidence-based*, criteria for entailing a true arithmetical proposition.

Accordingly, we have offered two scenarios as to why/how Fermat could have laconically concluded—in the recorded marginal noting in his copy of Diophantus' *Arithmetica*—that FLT is a true arithmetical proposition; even though, prima facie, he either did not (or could not to his own satisfaction) succeed in cogently evidencing, and recording, *why* FLT can be treated as an *evidence-based*, *pre-formal*, arithmetical truth (presumably *without* appeal to properties of *real* and *complex* numbers).

It is primarily such a putative, unrecorded, *evidence-based* reasoning underlying Fermat's laconic assertion which this investigation has sought to reconstruct (in §1. to §4.); and to justify (in §5. to §6.B.) by appeal to the resolution of some outstanding philosophical ambiguities concerning the relation between *evidence-based*, *pre-formal*, truth and formal provability, offered by the Complementarity Thesis (see [2], §1, Thesis 1).

8. Epilogue

We briefly remark, finally, that the *pre-formal* argument for FLT in §3. raises some additional philosophical issues—suggesting need for a, possibly unsuspected, distinction between properties of *continuous* measures (only definable in a first-order set theory such as ZF) and *discrete* measures (definable in a first-order arithmetic such as PA)—that could conceivably have significance for the physical sciences:

(i) In *any* physical interpretation of FLT, say as a water tank of volume z^3L (in litres), with mutually independent hollow compartments in lieu of the 3-D LEGO blocks defined in §3., Fig.5, FLT entails that we cannot fill the volume z^3L completely—and without overspill—with water volumes x^3L and y^3L, if $x, y, z \in \mathbb{N}$.

(ii) Moreover, we can also consider such an interpretation *theoretically* for any *platonic* model for $n > 3$.

(iii) Now, even if the hyper-volume $\mathbb{V}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}})$, sought to be *well-defined* in the particular configuration §3.(4(i)) by the n-D hyper-object denoted by $\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}}$, could be *platonically* assumed as being capable of being 'filled' with *unit* n-D hypercubes of total hyper-volume $\mathbb{V}_{Sym}(\overline{x^n})$, it could not even *platonically* be assumed as capable of being 'filled' with n-D hypercubes of side $\frac{2}{n}$, of total hyper-volume $\mathbb{V}_{Sym}(\overline{x^n})$, if n is a prime greater than 2 (an eventuality that would not arise with a *continuous* measure).

> **Comment** (see also §3.(11)): FLT follows since $x^p + y^p = z^p$ does not entail $(\frac{p}{2})^p(\frac{2x}{p})^p + y^p = z^p$ for prime $p > 2$!

(iv) Moreover, even if the putative hyper-volume $\mathbb{V}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}})$ 'between' the n-D hypercubes denoted by $\overline{y^n}$ and $\overline{z^n}$ in such a *platonic* configuration could always be assumed as capable of being *platonically* 'filled' with a *continuous* measure (such as that of, say, flowing water) so as to satisfy $x^n + y^n = z^n$, even *platonically* this cannot always be done with *discrete* measures (say water frozen as blocks of ice) if $n > 2$.

(v) Any proof of FLT within a putative, formal, theory such as $T_{\mathbb{H}_p}$ could, then, be interpreted as a formal expression of this, *pre-formal*, distinction between properties of *continuous* and *discrete* measures that must be reflected in the theory.

> **Comment**: A distinction that could conceivably have significance for the physical sciences, which appeal to interpretations of *well-defined*, formal, mathematical systems (such as *string theories* in particle physics) that admit n-dimensional objects in quantized mathematical structures.

(vi) In the absence of such an *informal* interpretation, it is not obvious *why*, and in *what* sense, Andrew Wiles proof of FLT can be treated as entailing a true arithmetical proposition under a *well-defined* interpretation of the first-order Peano Arithmetic PA.

(vii) *Reason*: As argued in [2], §19.C. (§19.C.a., Case 1, to 19.D.c., Case 5) any well-defined, set-theoretical, interpretation of a formal number-theoretic argument—such as, for instance, that of Wiles which must, presumably, implicitly appeal to the limits of Cauchy sequences as well-defined, set-theoretical, real numbers—need not be true *pre-formally* in the arithmetic of the natural numbers (as highlighted in the—albeit distinctly different—case of Goodstein's Theorem in [2], §18., Theorem 18.1).

References

[1] ANAND, BHUPINDER SINGH. 2016. *The truth assignments that differentiate human reasoning from mechanistic reasoning: The evidence-based argument for Lucas' Gödelian thesis.* In *Cognitive Systems Research.* Volume 40, December 2016, 35-45.
doi:10.1016/j.cogsys. 2016.02.004.

[2] . . . 2021. *The Significance of Evidence-based Reasoning in Mathematics, Mathematics Education, Philosophy, and the Natural Sciences.* Second edition, 2022, Forthcoming. (Limited First Edition archived at *PhilPapers*).
Preprint

[3] AVIGAD, JEREMY. 2021. *Varieties of Mathematical Understanding.* In *Bulletin of the American Mathematical Society* (New Series), Published electronically: February 22, 2021 (to appear in print). DOI: https://doi.org/10.1090/bull/1726
https://doi.org/10.1090/bull/1726

[4] BRADING, KATHERINE and CASTELLANI, ELENA. 2005. *Symmetries and invariances in classical physics.* CiteSeer^X, The College of Information Sciences and Technology, The Pennsylvania State University, Pennsylvania, USA.
http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.516.4606

[5] BELL, E. T. 1961. *The Last Problem.* Martino Fine Books, Eastford, CT (2017), USA.

[6] BARENDREGT, HENK and WIEDIJK, FREEK. 2005. *The challenge of computer mathematics.* In *Philosophical Transactions of The Royal Society A*, Volume 363, pp.2351-2375.
https://royalsocietypublishing.org/doi/pdf/10.1098/rsta.2005.1650

[7] CARNAP, RUDOLF. 1962. *Logical Foundations of Probability.* Second Edition 1962. The University of Chicago Press, Chicago, Illinois, U.S.A.

[8] CAI, TIANXIN; CHEN, DEYI; and ZHANG, YONG. 2015. *A new generalization of Fermat's Last Theorem.* In *Journal of Number Theory*, Volume 149, April 2015, Pages 33-45. Elsevier B. V. https://doi.org/10.1016/j.jnt.2014.09.014
https://doi.org/10.1016/j.jnt.2014.09.014

[9] CIPRA, BARRY A. 1988. *Fermat's Last Theorem Remains Unproved.* in *Science*, New Series, Vol.240, No. 4857, (Jun. 3, 1988), pp. 1275-1276, Accessed October 10, 2020. American Association for the Advancement of Science, Washington DC, USA.
https://www.jstor.org/stable/1701048

[10] DICKSON, LEONARD EUGENE. 1920. 'History of the Theory of Numbers (Volume II) Diophantine Analysis.' Carnegie Institution of Washington, Publication No. 256, Vol. II. 1950, Chelsea Publishing Company, New York, N. Y. USA.

[11] DARMON, HENRI; DIAMOND, FRED; and TAYLOR, RICHARD. 1995. *Fermat's Last Theorem.* In *Current Developments in Mathematics*, Volume 1995, pp.1-154. International Press, Inc., Somerville, Maryland, U.S.A.
https://intlpress.com/site/pub/files/_fulltext/journals/cdm/1995/1995/0001/CDM-1995-1995-0001-a001.pdf

[12] DEVLIN, KEITH. 1994. *Fermat's Last Theorem.* In *Math Horizons*, Vol. 1, No. 2 (Spring 1994), pp. 4-5. Accessed October 10, 2020. Mathematical Association of America, Washington DC, USA.
http://www.jstor.org/stable/25677961

[13] FEFERMAN, SOLOMON. 2006. *Are There Absolutely Unsolvable Problems? Gödel's Dichotomy.* Philosophia Mathematica (2006) 14 (2): 134-152.
http://math.stanford.edu/ feferman/papers/dichotomy.pdf

[14] . . . 2008. *Lieber Herr Bernays!, Lieber Herr Gödel! Gödel on finitism, constructivity and Hilbert's program.* in *Special Issue: Gödel's dialectica Interpretation* Dialectica, Volume 62, Issue 2, June 2008, pp. 245-290.
http://math.stanford.edu/ feferman/papers/bernays.pdf

[15] FREIRE, RODRIGO A.. 2018. *Interpretation and Truth in Set Theory.* In Eds. Walter Carnielli and Jacek Malinowski, *Contradictions, from Consistency to Inconsistency*, pp.183-205. Part of Trends in Logic, Studia Logica Library, SpringerLink.
https://link.springer.com/chapter/10.1007/978-3-319-98797-2_9

[16] HARDY, G. H. and WRIGHT, E. M. 1960. *An Introduction to the Theory of Numbers.* 4th edition. Clarendon Press, Oxford.

[17] HARRIS, MICHAEL. 2001. *Contexts of Justification: I Know What You Mean.* See *Other publications, #2*, on author's web-page (Downloaded 15/05/2021).
http://www.math.columbia.edu/ harris/website/publications

[18] . . . 2019. *Why the Proof of Fermat's Last Theorem Doesn't Need to Be Enhanced.* In *Quanta Magazine*, June 2019. See *Other publications, #21*, on author's web-page (Downloaded 15/05/2021).
http://www.math.columbia.edu/ harris/website/publications

[19] LANDAU, YEHEZKEL-EDMUND. 1929. *Foundations of Analysis.* 1951. Translation into English by F. Steinhardt of the German-language book *Grundlagen Der Analysis* by Edund Landau. 2nd Edn. 1960. Chelsea Publishing Company, New York.

[20] LAKOFF, GEORGE and NÚÑEZ, RAFAEL E. 2000. *Where Mathematics Comes From.* Basic Books, NY, USA.

[21] LAUBENBACHER, REINHARD and PENGELLEY, DAVID. 2010. *"Voici ce que j'ai trouvé:" Sophie Germain's grand plan to prove Fermat's Last Theorem.* In *Historia Mathematica*, Volume 37, Issue 4, November 2010, Pages 641-692. Elsevier B. V.
https://www.sciencedirect.com/science/article/pii/S0315086009001347

[22] LÖB, MARTIN H. 1959. *Constructive Truth.* In Heyting, A. (ed.) *Constructivity in Mathematics.* North-Holland, Amsterdam.

[23] MURTHY, CHETAN R. 1991. *An Evaluation Semantics for Classical Proofs.* Proceedings of Sixth IEEE Symposium on Logic in Computer Science, pp. 96-109, (also Cornell TR 91-1213), 1991.
http://www.nuprl.org/documents/Murthy/EvaluationSemantics.pdf

[24] MURAWSKI, ROMAN. 2020. *Proof vs Truth in Mathematics.* In *Studia Humana*, Volume 9:3/4 (2020), pp.10—18, University of Information, Technology and Management, Rzeszow, Poland.
http://studiahumana.com/pliki/wydania/10443-Voulme9_Issue3-4-02_paper.pdf

[25] NATHANSON, MELVYN B. 2008. *Desperately Seeking Mathematical Truth.* Opinion in the August 2008 *Notices of the American Mathematical Society*, Vol. 55, Issue 7.

[26] PANTSAR, MARKUS. 2009. *Truth, Proof and Gödelian Arguments: A Defence of Tarskian Truth in Mathematics.* In Eds. Marjaana Kopperi, Panu Raatikainen, Petri Ylikoski, and Bernt Österman, *Philosophical Studies from the University of Helsinki 23*, Department of Philosophy, University of Helsinki, Finland.
https://helda.helsinki.fi/bitstream/handle/10138/19432/truthpro.pdf?sequence=2

[27] PICCININI, GUALTIERO. 2019. *Knowledge as Factually Grounded Belief.* Unpublished manuscript (cited by permission of the author).
https://www.academia.edu/35484902/Factually_Grounded_Belief

[28] SINGH, SIMON. 1997. *Fermat's Last Theorem.* Harper Perennial, 2005, Harper Collins, London, UK.

[29] SKOLEM, THORALF. 1922. *Some remarks on axiomatized set theory.* Text of an address delivered in Helsinki before the Fifth Congress of Scandinavian Mathematicians, 4-7 August 1922. In Jean van Heijenoort. 1967. Ed. *From Frege to Gödel: A source book in Mathematical Logic, 1878 - 1931.* Harvard University Press, Cambridge, Massachusetts.

[30] TITCHMARSH, E. C. 1951. *The Theory of the Riemann Zeta-Function.* Clarendon Press, Oxford.

[31] WILES, ANDREW. 1995. *Modular Elliptic Curves and Fermat's Last Theorem.* In *Annals of Mathematics*, Second Series, Volume 141, No. 3 (May, 1995), pp.443-551 (109 pages), Princeton University, Princeton, New Jersey, USA. doi:10.2307/2118559
https://www.jstor.org/stable/2118559