An Elementary *Pre-formal* Proof of FLT

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

Bhupinder Singh Anand

Mumbai, India
bhup.anand@gmail.com
https://orcid.org/0000-0003-4290-9549

Abstract. Andrew Wiles' *analytic* proof of Fermat's Last Theorem FLT, which appeals to *geometrical* properties of *real* and *complex* numbers, 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? In this inter-disciplinary perspective of *why* FLT can be treated as a *pre-formally true* arithmetical proposition (one which, moreover, might not be provable *formally* in the first-order Peano Arithmetic PA), we admit only *elementary* (i.e., number-theoretic) reasoning, without appeal to *analytic* properties of *real* and *complex* numbers, to argue why any *formal proof* of FLT may need—as is implicitly suggested by Wiles' proof—to appeal *essentially* to *geometrical* properties of *arithmetical* propositions. Moreover, we argue that Fermat could have reasoned *informally*: (a) for ALL natural numbers $y < z$, we can cut a symmetrically centered length y from a string of length z, where the remaining, symmetrically centered, configuration is *uniquely* defined by *every* isomorphic configuration of a string of length x, where $x \in \mathbb{N}$; (b) for SOME natural numbers $y < z$, we can design a jigsaw puzzle such that removing a symmetrically centered square tile of side y from a square tile of side z, will leave a symmetrically centered configuration of regular 2-D tiles that is *uniquely* defined by *every* isomorphic configuration of a square tile of side $x \in \mathbb{N}$; and (c) for NO natural numbers $y < z$, can we design a LEGO blocks puzzle such that removing a symmetrically centered LEGO cube of side y from a LEGO cube of side z, will leave a symmetrically centered configuration of regular 3-D objects that is *uniquely* defined by *every* isomorphic configuration of a LEGO cube of side $x \in \mathbb{N}$. Fermat could then have argued, *pre-formally* and more generally, that (a)-(c) are particular instances of the arithmetical property that: if $x^n + y^n = z^n$ and $z = y + 2(k + \frac{a}{n^n})$, then FLT is equivalent to proving (not necessarily arithmetically *within* PA) the necessary and sufficient conditions which would admit the representation $x^n = z^n - y^n = 2.{}^nC_1(k+\frac{a}{n^n})y^{n-1} + 2^2.{}^nC_2(k+\frac{a}{n^n})^2y^{n-2} + \ldots + 2^n(k+\frac{a}{n^n})^n$; and conceived, consequently, that if $\overline{x^n}, \overline{y^n}, \overline{z^n}$ denote corresponding n-dimensional hyper-cubes in a Euclidean hyperspace $\mathbb{H}_n$, such that the symmetrically centered configuration of n-D hyper-objects corresponding to $z^n - y^n$, denoted by $\mathbb{C}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}}) =_{\mathbb{H}_n} 2.{}^nC_1\overline{(k+\frac{a}{n^n})y^{(n-1)}} +_{\mathbb{H}_n} 2^2.{}^nC_2\overline{(k+\frac{a}{n^n})^2y^{(n-2)}} +_{\mathbb{H}_n} \ldots +_{\mathbb{H}_n} 2^n\overline{(k+\frac{a}{n^n})^n} =_{\mathbb{H}_n} \mathbb{C}_{Sym}(\overline{x^n})$, can be *well-defined uniquely* upto *isomorphism*, then this would entail that $x^n + y^n = z^n$ if, and only if, $n < 3$.

Keywords. evidence-based reasoning, Fermat's Last Theorem, hypercube, pre-formal mathematics.

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1. 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 [20], 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: [20], p.66, An English translation of Fermat's marginal noting in Latin.*

For 358 years, FLT remained unproven; until the 108-page proof [22][1]—appealing to *geometrical* properties of *real* and *complex* numbers in order to prove an essentially *arithmetical* problem over the

[1]Detailed consideration of Wiles' 'analytic' proof lies beyond the scope, and competence, of this *evidence-based, pre-*

natural numbers—was published in 1995 by Andrew Wiles in the *Annals of Mathematics*. It proved an equivalence between, seemingly disparate, *geometric* properties of elliptic curves and modular forms that can be cogently argued (see §3.) as entailing FLT from the *specified* 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 [20], p.128):

> "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 putative reconstruction—from an inter-disciplinary, *pre-formal* (see [18]; also §2.), 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: [20], p.307.*

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

A curious feature (see [9], Chapter XXVI, pp.731-776; [4], pp.303-304; [20], pp.115-117, 126-127, & 251-252; [15], p.657, *§3.1 Germain's plan for proving Fermat's Last Theorem*; [7], *Abstract*) of recorded, post-Fermat, attempts to prove FLT has been the, seemingly universal, 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 $x, y, z, n \in \mathbb{N}$.

Moreover, Michael Harris' recent claim (in [13], *Other publications*, #21; see also §3.) 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:

> "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

formal (see [2], §1.D), perspective; which only seeks an 'elementary' understanding of why $x^n + y^n = z^n$ is provable *only* for $x, y, z, n \in \mathbb{N}$ and $n < 3$. However we address, in §3., Michael Harris' outline (see [13], *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 §2.B. of this putative reconstruction of the reasoning behind Fermat's laconic marginal noting.

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

> 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: [10].*

A possible reason could be that even definitive expositions of Wiles' reasoning—such as, for instance, [13]—may not (see §3.) view the 'proof' as capable of being essentially enhanced by *formally* justifying the *necessity* of appeal to arcane *geometrical* properties, of *real* and *complex* numbers[3], for concluding the *logical truth* of putative Diophantine solutions of, essentially, *arithmetical* propositions when such solutions are expressed *geometrically* as elliptic curves.

The fragility of uncritically accepting 'sociological validation of proofs' in lieu of *logical* validity is highlighted by Henk Barendregt and Freek Wiedijk in [5] 'The Challenge of Computer Mathematics'[4]:

> "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: [5], 1. The Nature of Mathematical Proof.*

For instance, if FLT[5] is not provable in PA, it would follow by [1], Theorem 7.1 (p.41)[6], that *no* deterministic algorithm TM could, for any *specified* $n > 2$, *evidence* that $x^n + y^n = z^n$ is unsolvable[7].

In which case, even if[8]—as entailed by Wiles' proof—FLT can be *evidenced* as numeral-wise *true*[9] under a *well-defined* interpretation of PA over $\mathbb{N}$[10], 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 §2.B.a.) by seeking *why* $x^n + y^n = z^n$ is solvable for $n = 2$ (and trivially for $n = 1$), but not for $n = 3$.

[3] A justification the *pre-formal* proof of FLT in §2.B. 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 §2.B.a.(b), *without* appeal to properties of *real* and *complex* numbers.

[4] As also by Melvyn B. Nathanson in [17], 'Desperately Seeking Mathematical Truth'.

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

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

[7] 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* $F(1), F(2), \ldots$.

[8] As in the case of Kurt Gödel's well-known 'formally undecidable' arithmetical proposition $[(\forall x)R(x)]$ (see [1], Corollary 8.3, p.42): *In any model of PA, Gödel's arithmetical formula* $[R(x)]$ *interprets as an algorithmically verifiable, but not algorithmically computable, tautology over* $\mathbb{N}$.

[9] 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* $\{F(1), F(2), \ldots, F(n)\}$.

[10] 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 $\leq n$.

2. Could *this* have been Fermat's *insight*?

Some insight into *why* $x^n + y^n = z^n$ can be treated *informally* as true *only* for $n < 3$ follows if—instead of expressing 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 an elliptic curve, and seeking to identify the latter's Galois representation with a unique modular form (cf. [22]; see also [13] and §3., eqns. (A)-(E))—we note 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), we can express:

(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 the necessary and sufficient conditions (see §2.B.a.(b)) that, for any *specified* $n \geq 1 \in \mathbb{N}$, admit some $y, z \geq 1 \in \mathbb{N}$ which yield a unique representation of x^n as above.

We shall argue that even if such a proof were *arithmetically* impossible because FLT is PA-unprovable, we could yet visualise (as in §2.A. below) Fermat's Last Theorem as a *formal* proposition concerning the *geometrical* properties of recursively *well-defined* mathematical objects in the structure, say $\mathbb{H}_n$, of n-D hyper-objects[11] in a n-dimensional Euclidean space; where the cases $n = 2, 3$ can be corresponded to the geometrical properties in physical space of the familiar LEGO blocks.

Such an insight could be viewed as yielding a *pre-formal* proof of FLT by visually evidencing, *without* appeal to properties of *real* and *complex* numbers, that if, for some natural numbers x, y, z, n, we can *well-define unique* n-D hyper-cubes $\overline{x^n}, \overline{y^n}, \overline{z^n} \in \mathbb{H}_n$ which entail $x^n + y^n = z^n$, then $n = 2$. 'Pre-formal', as detailed by Markus Pantsar in [18]:

> "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: [18], §1.1 General background.*

Moreover, we interpret Pantsar's 'pre-formal mathematics' here (see also Anand [2], §1.A, Pre-formal mathematics) as evidencing the philosophy that an *evidence-based*[12] 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.

In a recent paper [16] on *Proof vs Truth in Mathematics*, Roman Murawski too (as does Harris in [13]; see §3.) emphasises the critical role that "informal proofs" (which could be viewed 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:

> "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]):

[11] See Wikipedia: Hypercube.

[12] 'Evidence-based' as defined implicitly in Anand [1], and explicitly in Anand [2], §1.D., in the sense (see [2], §5.A) of Gualtiero Piccinini's *knowledge as factually grounded belief* (see [19]), rather than that of Plato's *knowledge as justified true belief*.

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

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

It is not unreasonable to assume that Fermat could have intuited some such *pre-formal* perspective towards mathematical *truth* and *proof*, and visualised that, for any pair of natural numbers $z > y$:

(1)

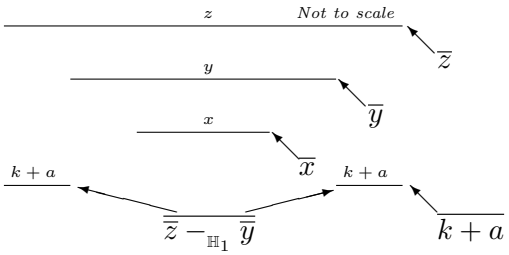

Fig.1

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 1-dimensional object consisting of two separated pieces of length $k+a$ units each, denoted by say $\overline{\overline{z} -_{\mathbb{H}_1} \overline{y}}$, which can be *uniquely* defined upto *isomorphism* under change of scale (see Definition 2):

- 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);

(2)

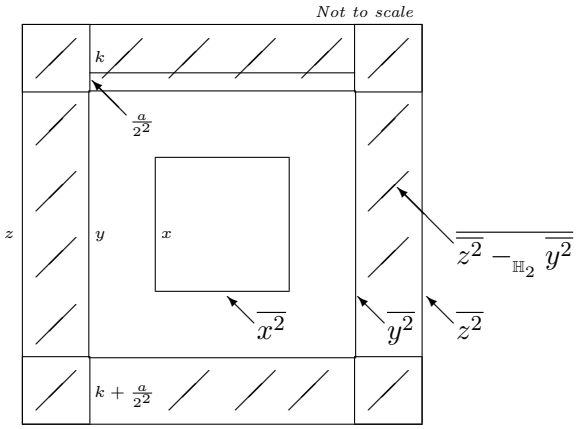

Fig.2

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 2-dimensional object, say $\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}}$ (shaded area in Fig.2), which can be *uniquely* defined upto *isomorphism* under change of scale (see Definition 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 $\overline{\overline{z^2} -_{\mathbb{H}_2} \overline{y^2}}$:

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

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

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

(3)

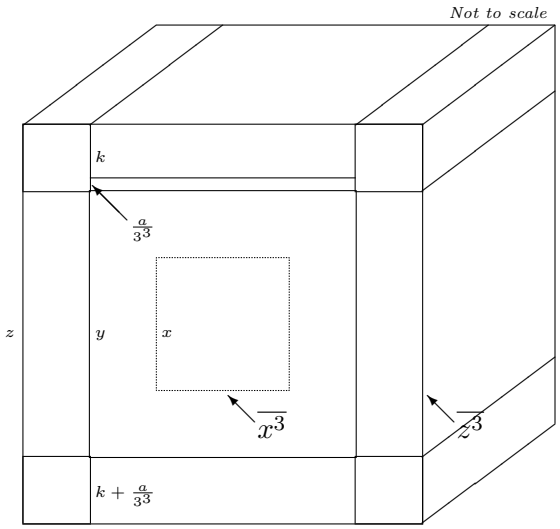

Fig.3

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) have a 3-dimensional

object, say $\overline{\overline{z^3} -_{\mathbb{H}_3} \overline{y^3}}$, which can be *uniquely* defined upto *isomorphism* under change of scale (see Definition 2):

- 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 $\overline{\overline{z^3} -_{\mathbb{H}_3} \overline{y^3}}$:

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

- 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 §2.B.a.(b)) design a LEGO blocks puzzle for *any* $y, z \in \mathbb{N}$ such that *any* configuration $\mathbb{C}(\overline{y^3})$ of the cube $\overline{y^3}$, along with *any* configuration of LEGO blocks which is *isomorphic* to $\mathbb{C}_{Sym}(\overline{\overline{z^3} -_{\mathbb{H}_3} \overline{y^3}}) =_{\mathbb{H}_3} 6\overline{(k+\frac{a}{3^3})y^2} +_{\mathbb{H}_3} 12\overline{(k+\frac{a}{3^3})^2 y} +_{\mathbb{H}_3} 8\overline{(k+\frac{a}{3^3})^3}$, could be assembled into the 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 *uniquely* defined upto *isomorphism* 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:

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

where:

Definition 1. (Isomorphic configuration) *Any two 'configurations' of a n-D hyper-object* $\overline{x^n} \in \mathbb{H}_n$*, denoted by* $\sum_{i=1}^{j} a_i\overline{(\prod_{k=1}^{n} u_{ik})}$ *and* $\sum_{i=1}^{j} b_i\overline{(\prod_{k=1}^{n} v_{ik})}$*, where* $\overline{(\prod_{k=1}^{n} u_{ik})}, \overline{(\prod_{k=1}^{n} v_{ik})} \in \mathbb{H}_n$ *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* $r > 0 \in \mathbb{Q}$[13]*.*

Definition 2. (Uniqueness) *A n-D hyper-object* $\overline{x^n}$ *is uniquely defined upto isomorphism 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'* $\sum_{i=1}^{j} a_i\overline{(\prod_{k=1}^{n} u_{ik})}$ *and* $\sum_{i=1}^{j} b_i\overline{(\prod_{k=1}^{n} v_{ik})}$ *of* $\overline{x^n}$ *that are isomorphic.*

For $\overline{x^n}$ to, then, admit a configuration $\mathbb{C}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}})$ that will *uniquely* define $\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}}$, each term in the configuration (which too is a configurations of n-D objects) must also be *uniquely* defined upto *isomorphism* under any change of scale by Definition 2.

However, we argue *pre-formally* in §2.B. 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 $n < 3$.[14]

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

Proposition 2.1. *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.

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

[14] We note that, in his commentary [13] on FLT, Michael Harris outlines Wiles' proof as arguing that (see §3.): If $a^p + b^p = c^p$ for some odd prime p and $a, b, c \in \mathbb{N}$, then there would exist 'another modular form, this one of weight 2 and level 2'; however there are no such modular forms.

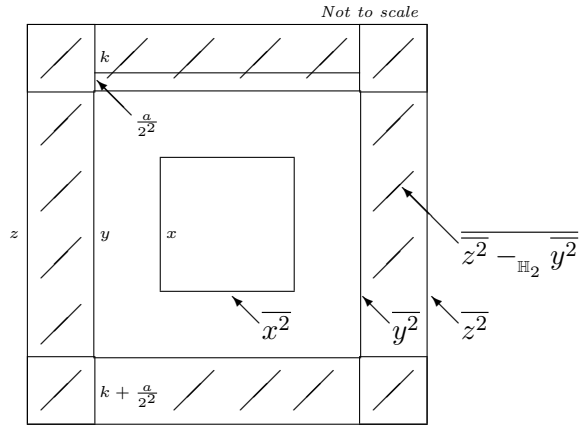

Fig.4

Then Fig.4 is a *pictorial* proof (compare [20], 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 *uniquely* defined upto *isomorphism* (by Definition 2) by the symmetrically centered 'configuration' of 2-D LEGO blocks (tiles):

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

2. Similarly, Fig.5 is a *pictorial* proof[15] 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:

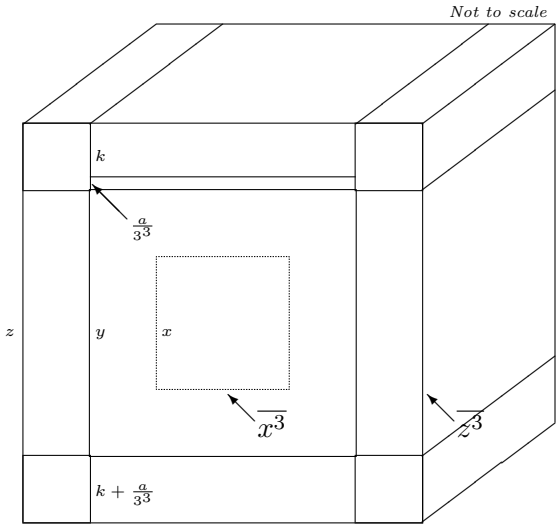

Fig.5

(a) one cube block of side y,

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

[15] Compare the visual 'challenge' suggested in [20], p.31, Fig.5. Also Gerd Falting's insightful (albeit analytic) visualisation ([20], 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 $z, y \in \mathbb{C}, n \in \mathbb{N}$.

(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 *uniquely* defined upto *isomorphism* (by Definition 2) by the symmetrically centered 'configuration' of 3-D LEGO blocks:

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

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 [3], §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 uniquely* upto *isomorphism* (by Definition 2) by the symmetrically centered 'configuration' of p-D hyper-objects denoted by:

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

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 $\mathrm{T}_{\mathbb{H}_p}$ (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 uniquely* upto *isomorphism* (see Definition 2) by the symmetrically centered 'configuration' of p-D hyper-objects:

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

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 $\overline{x^p}$', '$-_{\mathbb{H}_p}$', '$=_{\mathbb{H}_p}$', '$+_{\mathbb{H}_p}$' and '$\equiv_{\mathbb{H}_p}$' in $\mathrm{T}_{\mathbb{H}_p}$ so as to admit the pictorial interpretations §2.B.1 and §2.B.2 when $p = 2, 3$ respectively, such that §2.B.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 uniquely* upto *isomorphism* (see Definition 2) in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of:

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

$$(k + \tfrac{a}{p^p}) \times \underbrace{y \times y \times \ldots \times y}_{(p-1)};$$

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

$$(k + \tfrac{a}{p^p}) \times (k + \tfrac{a}{p^p}) \times \underbrace{y \times y \times \ldots \times y}_{(p-2)};$$

...

(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 §2.B.4(iii) admits only those values of $a \in \mathbb{N}$ that yield a natural number. We thus have that if §2.B.4(iii) *well-defines* a p-D hypercube denoted by $\overline{x^p}$ in the theory $\mathrm{T}_{\mathbb{H}_p}$ of p-D hyper-objects, then this would correspond to the symmetrically centered 'configuration' of p-D hyper-objects *well-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 §2.B.4(ii)(a)-§2.B.4(ii)(c) *must* further be *well-defined uniquely* upto *isomorphism* (see Definition 2) at any rational scale $0 < s \leq 1$ of scaled down p-D hyper-objects denoted by:

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

7. In particular, since $z-y = 2(k+\frac{a}{p^p}) \in \mathbb{N}$, the p-D hyper-object *well-defined uniquely* upto *isomorphism* (see Definition 2) 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 uniquely* upto *isomorphism* (see Definition 2) 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 $((k+\frac{a}{p^p})(\frac{2}{p}))^p$, and cumulative p-D hyper-volume $p^p((k+\frac{a}{p^p})(\frac{2}{p}))^p = 2^p(k+\frac{a}{p^p})^p$.

8. Moreover, since $\mathrm{T}_{\mathbb{H}_p}$ must admit the pictorial interpretations §2.B.1 and §2.B.2 when $p = 2, 3$ respectively—as detailed in §2.B.a.(a) and §2.B.a.(b)—then the p-D hyper-object denoted by $\overline{\overline{z^p} -_{\mathbb{H}_p} \overline{y^p}}$ is *well-defined uniquely* upto *isomorphism* (see Definition 2) under interpretation in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of p-D hyper-objects §2.B.4(i) if, and only if, each term in §2.B.4(i) is *isomorphic* (see Definition 1) 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 uniquely* upto *isomorphism* (see Definition 2) under interpretation in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of p-D hyper-objects §2.B.4(i), by Definition 2 we cannot have that both:

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

and:

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

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 uniquely* upto *isomorphism* (see Definition 2) under interpretation in $\mathbb{H}_p$ by the symmetrically centered 'configuration' of p-D hyper-objects §2.B.4(i), then $p^p = 2^p$, and $p = 2$.

11. Further (see §2.B.a.(a) below), since $2^2 = 2.^2C_1 = 2^2.^2C_2$, the p-D hyper-object sought to be *well-defined uniquely* upto *isomorphism* (see Definition 2) in §2.B.(4(i)) by the symmetrically centered 'configuration' of p-D hyper-objects:

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

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

Corollary 2.2 follows since, as noted by Simon Singh in [20] (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: [20], p.99.*

The significance of showing we cannot *well-define* the n-D hyper-object denoted by $\overline{z^n}$ *uniquely* upto *isomorphism* (see Definition 2), for $n > 2$, such that $\mathbb{C}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}}) =_{\mathbb{H}_n} \mathbb{C}_{Sym}(\overline{x^n})$ interprets as $z^n - y^n = x^n$ in $\mathbb{N}$, is that it circumvents any implicit appeal (see [20], 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: [8].*

2.B.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 §2.B., Proposition 2.1:

(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 $\overline{(k + \frac{a}{2^2}) \times y}$, with hyper-dimensions $(k + \frac{a}{2^2}) \times y$, and cumulative 2-D hyper-volume $2.^2C_1.(k + \frac{a}{2^2})y$,

    defined in §2.B.4(i) are *well-defined uniquely* upto *isomorphism* (see Definition 2) by (assembled *uniquely* from):

    (ii) the 2^4 scaled down 2-D hyper-objects denoted by $\overline{(k + \frac{a}{2^2})\frac{1}{2} \times y(\frac{1}{2})}$, with hyper-dimensions $(k + \frac{a}{2^2})\frac{1}{2} \times y(\frac{1}{2})$, 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 $(k+\frac{a}{2^2})\times(k+\frac{a}{2^2})$, and cumulative 2-D hyper-volume $2^2.{}^2C_2.(k+\frac{a}{2^2})^2$,

are also *well-defined uniquely* upto *isomorphism* (see Definition 2) by (assembled *uniquely* from):

(iv) the 2^4 scaled down 2-D hypercubes denoted by $\overline{((k+\frac{a}{2^2})(\frac{1}{2}))^2}$ with hyper-dimensions $((k+\frac{a}{2^2})(\frac{1}{2}))\times((k+\frac{a}{2^2})(\frac{1}{2}))$, 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 $(k+\frac{a}{3^3})\times(k+\frac{a}{3^3})\times(k+\frac{a}{3^3})$, and cumulative 3-D hyper-volume $2^3.(k+\frac{a}{3^3})^3$,

are capable of being *well-defined* upto *isomorphism* (see Definition 1), but *not* capable of being *well-defined uniquely* upto *isomorphism* (see Definition 2) by (assembled *uniquely* from):

(ii) the 3^3 scaled down 3-D hypercubes denoted by $\overline{((k+\frac{a}{3^3})(\frac{2}{3}))^3}$, with hyper-dimensions $((k+\frac{a}{3^3})(\frac{2}{3}))\times((k+\frac{a}{3^3})(\frac{2}{3}))\times((k+\frac{a}{3^3})(\frac{2}{3}))$ 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 §2.B.4(i), since 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 *any* configuration $\mathbb{C}(\overline{y^3})$ of the cube $\overline{y^3}$, along with *any* configuration of LEGO blocks which is *isomorphic* (see Definition 1) to $\mathbb{C}_{Sym}(\overline{\overline{z^3}-_{\mathbb{H}_3}\overline{y^3}}) =_{\mathbb{H}_3} 6\overline{(k+\frac{a}{3^3})y^2} +_{\mathbb{H}_3} 12\overline{(k+\frac{a}{3^3})^2y} +_{\mathbb{H}_3} 8\overline{(k+\frac{a}{3^3})^3}$, could be assembled into a cube $\overline{z^3}$.
>
> *Reason*: If, in the above LEGO blocks puzzle, $\sum_{i=1}^{j} a_i\overline{(\prod_{k=1}^{n} u_{ik})}$ and $\sum_{i=1}^{j} b_i\overline{(\prod_{k=1}^{n} v_{ik})}$ are *any* two *uniquely well-defined* configurations upto *isomorphism* (see Definition 2) of the hypercube $\overline{x^n}$, each of which, along with *any* configuration (see Definition 1) of the hypercube $\overline{y^n}$, could be assembled *uniquely* into a hypercube $\overline{z^n}$, then it is:
>
> - *necessary*, but not *sufficient*, that: $\sum_{i=1}^{j} a_i\overline{(\prod_{k=1}^{n} u_{ik})}$ and $\sum_{i=1}^{j} b_i\overline{(\prod_{k=1}^{n} v_{ik})}$ are *isomorphic* (by Definition 1);
> - *necessary* and *sufficient* that: $\sum_{i=1}^{j} a_i\overline{(\prod_{k=1}^{n} u_{ik})}$ and $\sum_{i=1}^{j} b_i\overline{(\prod_{k=1}^{n} v_{ik})}$ are *isomorphic* (by Definition 1); *and*, for all $1\leq i\leq j$, either $a_i|b_i$ or $b_i|a_i$.

2.B.b. Does FLT need to appeal *essentially* to *geometrical* properties of *arithmetical* propositions?

In conclusion, we note §2.B. and §2.B.a. argue the *pre-formal* perspective that FLT is a *true* arithmetical proposition which appeals *necessarily* to the *essentially* geometrical properties of *unique isomorphism* 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 uniquely* upto *isomorphism* (by Definition 2) in §2.B.4(i); where

(b) If $x, y, z, n, \in \mathbb{N}$, and $z^n = x^n + y^n$, 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 uniquely* upto *isomorphism* (by Definition 2) *only* if $n \leq 2$ (see §2.B.10); and

(c) Since it would then follow that $\mathbb{C}_{Sym}(\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}}) =_{\mathbb{H}_n} \mathbb{C}_{Sym}(\overline{x^n})$, the n-D hyper-object denoted by $\overline{x^n}$, and sought to be *well-defined* upto *isomorphism* (by Definition 1) in §2.B.5(i), such that $\overline{x^n} =_{\mathbb{H}_n} (\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}})$, is also *well-defined uniquely* (see Definition 2) *only* if $n \leq 2$ (see §2.B.10); whence

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

> **Comment**: We note §2.B. and §2.B.a. argue the *pre-formal* perspective that FLT is a *true* arithmetical proposition which appeals ***necessarily*** to the *essentially* geometrical properties of *unique isomorphism* 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, since it is not *pre-formally* evident that the *geometrical* property of *unique isomorphism* 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.
>
> *Reason*: 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) $x^n = 2.^nC_1(k + \frac{a}{n^n})y^{(n-1)} + 2^2.^nC_2(k + \frac{a}{n^n})^2y^{(n-2)} + \ldots + 2^n(k + \frac{a}{n^n})^n$, *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 $\overline{x^n}$, and sought to be *well-defined uniquely* upto *isomorphism* in §2.B.10 such that $\overline{x^n} =_{\mathbb{H}_n} (\overline{\overline{z^n} -_{\mathbb{H}_n} \overline{y^n}})$.

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[16] one whose 'truth' in the general case he was unable to *evidence* 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: [20], pp.307-308.*

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

The significance of, and need for, Pantsar's explicit distinction between *formal* and *pre-formal* proofs of mathematical propositions (see §2.) 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:

[16] 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).

> "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: [13], 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: [13], Other publications, #21.

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

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

By extrapolating the pictorial argument for $n = 1, 2, 3$ in §2.A., and considering what is entailed by Definition 1 and Definition 2 in the general case, we then argue *pre-formally* in §2.B.a. that (i) *uniquely* defines (ii) *upto isomorphism* 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[17] 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 §2.B.a. is *elementary*, since it does *not* appeal to properties of *real* and *complex* numbers[18] for establishing that: for $n > 2$, (i) above does not *uniquely* define (ii) *upto isomorphism* by Definition 2, thereby entailing that there is no solution (A) for some odd prime p.

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 admit automated theorem proving that would, then, establish FLT additionally as an algorithmically *computable* (logical) *truth* under any *well-defined* (i.e., *evidence-based*[19]) Tarskian 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: [13].

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:

[17] As do the 1896 proofs of the Prime Number Theorem by Jacques Hadamard and Charles Jean de la Vallée Poussin (see [21], Theorem 3.7, p.44).

[18] As is the case with the 1948 proofs of the Prime Number Theorem by Atle Selberg and Paul Erdős (see [11], Theorem 6, p.9).

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

> "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: [13], 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: [13], Other publications, #21.*

From the *evidence-based* perspective of this *pre-formal*, putative, 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: [13], Other publications, #2.

3.A. A putative resolution of 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 the persisting ambiguity in current paradigms on the nature of, and relation between, mathematical *truth* and mathematical *proof*, it may also need to accommodate a putative resolution of such ambiguity that appears sympathetic to his argumentation, such as:

"**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*;

(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.*

In other words:

- 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 [6]) 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 [18] (§4. Formal and pre-formal mathematics)—is a philosophical question for the cognitive sciences (cf. [14]; 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: [6], 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):

— merely a 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 [14] (see also [2], §25), term as *primary* and *secondary* 'conceptual metaphors',

— 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 [14] (such as those involving Cantor's first limit ordinal ω[20]);

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).

3.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: [13], 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 ad-

[20] See [14], Preface, p.*xii-xiii*: "*How can human beings understand the idea of actual infinity?*"

dress an earlier issue raised by Harris in [12], concerning the epistomological status of set-theoretically defined real numbers:

> "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: [12], Other publications, #2.*

Thus, from an *evidence-based* perspective, set-theoretically defined real numbers exist *merely* as axiomatically *postulated* mathematical objects[21] *only within* any first-order set theory such as ZF; whilst those of such numbers that can further be defined arithmetically exist as axiomatically *postulated* mathematical objects[22] (symbols) *only 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[23];

— 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

[21] More specifically, as symbols corresponding to what George Lakoff and Rafael Núñez describe as *secondary* conceptual metaphors in [14] (see also [2], §13.F, *Three categories of information*, and [2], §25.F, *The Veridicality of Mathematical Propositions*).

[22] ibid.

[23] 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.*

factually grounded knowledge (in the sense of [2], §5.A) when describing properties of the *actual* universe we inhabit.

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