LOGICAL AND SEMANTIC PURITY

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Many mathematicians have sought 'pure' proofs of theorems. There are different takes on what a 'pure' proof is, though, and it's important to be clear on their differences, because they can easily be conflated. In this paper I want to distinguish between two of them.

I want to begin with a classical formulation of purity, due to Hilbert:

> In modern mathematics one strives to preserve *the purity of the method*, i.e. to use in the proof of a theorem as far as possible only those auxiliary means that are required by the content of the theorem.[1]

A pure proof of a theorem, then, is one that draws only on what is "required by the content of the theorem".

I want to continue by distinguishing two ways of understanding "required by the content of [a] theorem", and hence of understanding what counts as a pure proof of a theorem. I'll then provide three examples that I think show how these two understandings of content-requirement, and thus of purity, diverge.

1. Logical purity

The first way of understanding purity that I want to consider takes what is "required by the content of [a] theorem" to be just what suffices for proving that theorem. The ideal is what Hilbert pursued in his *Grundlagen der Geometrie*: to determine which of the axioms he gave for geometry are sufficient for proving interesting geometric theorems, such that if any of those axioms were left out, the theorem would no longer follow.[2] As an first approximation, then, this ideal can be

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[1]Translation in [24], pp. 393–4. The original ([15], pp. 315–6) reads, "In der modernen Mathematik wird solche Kritik sehr häufig geübt, wobei das Bestreben ist, *die Reinheit der Methode* zu wahren, d.h. beim Beweise eines Satzes wo möglich nur solche Hülfsmittel zu benutzen, die durch den Inhalt des Satzes nahe gelegt sind." Hilbert continues by remarking that "Dieses Bestreben ist oft erfolgreich und für den Fortschritt der Wissenschaft fruchtbar gewesen." Hilbert seems to have had in mind recent work on circle quadrature and the parallel postulate.

[2]For more on Hilbert's interest in purity in his geometric work, see [12].

made more precise by defining a set of axioms S as "logically minimal" for a theorem P just in case S proves P, but no proper subset of S proves P; and a proof of a theorem P as "logically pure" if it is a proof of P from a logically minimal subset S of a set of axioms T. This is only an approximation, because if we allow as a set of axioms, say, the conjunction of Hilbert's axioms of geometry, then every theorem provable from Hilbert's axioms has a logically pure proof from that single conjoined axiom. To avoid this trivialization, we'd need to restrict our attention to the sorts of axiomatic theories that arise in ordinary practice. What I have in mind are ordinary examples like Hilbert's axioms for geometry, or the Peano axioms for arithmetic. We don't at present have a convincing way to characterize completely non-trivial axiomatic theories. So I'll just leave this as an approximation, but one that I take is clear enough.

For a theorem P and a set of axioms T, there may be several different logically pure proofs of P, since there may be several logically minimal subsets of T for P. Furthermore, for a given theorem there may be several good candidates for axiom sets T relative to which we can search for logically pure proofs.[3]

One way to pinpoint what suffices for proving a given theorem is to find a set of axioms that is logically equivalent to that theorem (over a logically weak base theory). This is what is done in "reverse mathematics" as developed by Harvey Friedman and Stephen Simpson: starting with a mathematical theorem and an interesting collection of set-theoretic axiom sets, we try to determine which of these axiom sets is logically equivalent to that theorem (over a base theory that is set-theoretically weaker than the theorem and axiom sets under consideration).[4] When successfully carried out, reverse mathematics locates both necessary and sufficient conditions for a given theorem, and thus locates *the* logically weakest axiom set (among a given set of candidate axiom sets) for proving a given theorem. Let's call a proof from such a set of axioms "strongly logically pure".

In this paper I'm just concerned with logical purity, not strong logical purity. Advocates of logical purity have not always been clear enough about which of the two projects they're pursuing. For instance, Pambuccian writes that "in geometry one would want to know what axioms are needed to prove a particular theorem, an enterprise that might be

[3]Cf. [25], p. 20, for some discussion of this point.

[4]Cf. [9], [29]. The name "reverse" is due to the part of the project in which we prove the axiom set from the theorem.

called *reverse geometry*."[5] Aligning his work with reverse mathematics suggests that he seeks strong logical purity, rather than merely logical purity. This suggestion is supported by his citing the following passage of Hilbert's as articulating his view on purity:

> By the axiomatic study of mathematical truth I understand an investigation which does not aim to discover new or more general theorems with the help of given truths, but rather the position of a theorem within the system of known truths and their logical connections in a way that indicates clearly which conditions are necessary and sufficient for the grounding of that truth.[6]

However, I think the details of Pambuccian's work bears out that he's primarily interested in logical purity, rather than strong logical purity. (I also don't think it's clear that Hilbert was interested in logical purity as opposed to another type of purity, but I'll return to this in the next section.)

2. Semantic purity

In contrast to the logical reading of "required by the content of [a] theorem" considered in the last section, consider the following "semantic" reading: namely, whatever must be understood or accepted in order to understand that theorem.[7] These concepts and truths are the conditions for understanding the theorem, and so are part of its content. A proof of a theorem, then, is "semantically pure", if it draws only on what must be understood or accepted in order to understand that theorem.

It's difficult to say precisely what must be understood and accepted in order to understand a given theorem. It's easier to focus on specific cases in order to see how this is supposed to work. I'll focus here on H.S.M. Coxeter's work on Sylvester's problem, which says:

> Let n given points have the property that the straight line joining any two of them passes through a third of

[5]Cf. [24], p. 393. For more on reverse geometry, see also [25], p. 19.

[6]Cf. [14], p. 50. My translation. The original reads, "Unter der axiomatischen Erforschung einer mathematischen Wahrheit verstehe ich eine Untersuchung, welche nicht dahin zieht, im Zusammenhange mit jener Wahrheit neue oder allgemeinere Sätze zu entdecken, sondern die vielmehr die Stellung jenes Satzes innerhalb des Systems der bekannten Wahrheiten und ihren logischen Zusammenhang inder Weise klarzulegen sucht, daß sich sicher angeben läßt, welche Voraussetzungen zur Begründung jener Wahrheit notwendig und hinreichend sind." Pambuccian cites this passage in [25], p. 19.

[7]For more on this semantic reading, see [1], pp. 4–6.

the given points. Show that the n points lie on a straight line.[8]

What must be understood and accepted in order to understand Sylvester's problem? The surface grammar of the problem concerns points, straight lines, and the incidence of points on straight lines. So we must understand and accept definitions, including axioms, of these concepts. Coxeter thought a line segment is, by definition, the set of points between two points.[9] So to understand Sylvester's problem, Coxeter thought it was necessary to understand and accept incidence and betweenness axioms for geometry; i.e., the theory he called "ordered geometry".[10] Hence a solution to Sylvester's problem in ordered geometry would be semantically pure; and indeed Coxeter found such a solution.[11]

Coxeter's understanding of straight line isn't the only possible one, of course. Differential geometers sometimes define straight line as the shortest line between two given points. Consequently, for them distance, and not betweenness, must be understood to understand Sylvester's problem. I don't know a plausible way of establishing that Coxeter is right and the differential geometers are wrong. We don't need to do this for our purposes here, though. It's enough to note that

[8]This problem was originally posed by J.J. Sylvester in [30], hence the name. I've given Erdős' formulation from [8], p. 65.

[9]Coxeter writes ([3], p. 176), "The essential idea [for problems such as Sylvester's] is *intermediacy* (or 'betweenness'), which Euclid used in his famous definition: *A line (segment) is that which lies evenly between its ends.* This suggests the possibility of regarding intermediacy as a primitive concept and using it to define a line segment as the set of all points between two given points." I want to note that in addition to relying uncharacteristically on the authority of Euclid, Coxeter uses an unusual translation of Euclid's definition I.4. For instance, Heath's translation reads, "A *straight line* is a line which lies evenly with the points on itself."

[10]Coxeter presents the axioms of ordered geometry, adapted from Pasch and Veblen's earlier treatments, in [3], pp. 177–8. In particular, Coxeter's presentation follows Veblen's in [31], with some minor rearrangement. Veblen notes that his axioms "presuppose only the validity of the operations of logic and of counting (ordinal number)" (p. 344). Why must logic and principles governing the finite ordinal arithmetic of counting be understood and accepted in order to understand Sylvester's problem? I think Veblen and Coxeter would say that anyone who failed to understand and accept these principles would not completely understand any mathematical problems. I agree with this point of view—though teasing out exactly what logical principles are involved here would be challenging.

[11]The proof is given at [3], pp. 181–2.

Coxeter's understanding makes explicit a single community's understanding, and to focus on semantic purity relative to that community's understanding.

While Pambuccian gives a logical reading of purity as formulated by Hilbert, I want to make clear that the semantic reading is also consistent with Hilbert's formulation. The semantic purist believes that she seeks proofs of theorems that are restricted to what is "required by the content of the theorem." She agrees with Hilbert that in seeking a pure proof of a given theorem, she is seeking "conditions [that] are necessary and sufficient for the grounding of that truth". She just seeks a semantic grounding relation, rather than a purely logical one.

3. Distinguishing logical from semantic purity

One question I'm not going to address here is why we should value logically pure proofs of theorems over logically impure proofs. Nor will I address here the value of semantic purity. These are important questions, but my goal in this paper is simply to distinguish sharply logical purity from semantic purity. I'll do so demonstrating two theses:

(1) Some results require more concepts and/or proposition to be proved than to be understood.
(2) Some results require more concepts and/or proposition to be understood than to be proved.

I'll demonstrate these by presenting three case studies.

3.1. **The casus irreducibilis.** Consider the problem of finding exact solutions to cubic polynomial equations with rational coefficients that have three real roots, e.g. $x^3 - x = 0$, since $x^3 - x = x(x+1)(x-1)$. What must we understand and accept to understand this problem? Rational polynomials are built up using the six algebraic operations, namely, addition, subtraction, multiplication, division, exponentiation, and extraction of roots. So we must understand those, and accept the usual algebraic laws thought to govern these operations (e.g. commutativity, distributivity) as definitions of those operations. In addition, we must understand how to carry out these operations on rational numbers. But we needn't understand how to carry out these operations on *all* rational numbers. We don't need to understand how to divide by zero, for instance. We also needn't understand how to extract the square root of a negative number: as evidence for this I point out that the early workers on this problem (e.g. Cardano, Bombelli) didn't understand how to do this, or what it would mean to find the square root of a negative number; and yet they understood the problem of exact cubic solution.

I'll next explain what's needed to *solve*, rather than just *understand*, this problem. Firstly, note that we can restrict our attention to cubics of the form $x^3 = qx + r$ (such as $x^3 = 15x + 4$), since every cubic can be put in this form by a change of variables.[12] We can then use Cardano's formula to solve these cubics:

$$x = \sqrt[3]{-\frac{q}{2} + \frac{1}{18}\sqrt{-3D}} + \sqrt[3]{-\frac{q}{2} - \frac{1}{18}\sqrt{-3D}} \tag{1}$$

where D is the *discriminant* of the cubic.[13]

To understand Cardano's formula, we need to understand only what we need to understand in order to understand the problem of solving cubics with rational coefficients and three real roots. It would be usable in a semantically pure solution—except for the following problem. It turns out that $(x_1 - x_2)(x_1 - x_3)(x_2 - x_3) = \sqrt{D}$. So when all three roots x_1, x_2, x_3 are real, it follows that $D > 0$. But then to find a solution using (1), we will have to evaluate $\sqrt{-3D}$, the square root of a negative number and hence imaginary. In applying Cardano's formula to solving cubics with three real roots, we must use imaginary numbers along the way. This case has historically been known as the "casus irreducibilis", or "irreducible case", because of the difficulty in showing that these imaginary terms reduce to real terms. Because of the casus irreducibilis, Cardano's formula isn't usable in a semantically pure solution. Its use in the casus irreducibilis involves imaginary numbers, and these needn't be understood to understand our problem.[14]

After the discovery of the casus irreducibilis in the sixteenth century, mathematicians found other methods for solving cubics. These include the geometric solutions of the seventeenth century[15] and the infinite

[12] That is, all cubics of the general form $z^3 + a_1 z^2 + a_2 z + a_3 = 0$ can be put in the form $x^3 = qx + r$ using the substitution $z = x - \frac{a_1}{3}$.

[13] For cubics in our restricted form, the discriminant D is $4q^3 - 27r^2$. Cardano's formula will locate one of the three real roots; we can use the quadratic formula to find the other two.

[14] One could object that complex numbers are just ordered pairs of real numbers, and so ought to be usable in a semantically pure solution to cubics in the casus irreducibilis. This objection misses the point that to define complex numbers as pairs of reals, one must define the algebraic operations for complex numbers in terms of the algebra of ordered pairs of reals. But these peculiar definitions on ordered pairs of reals exceed what must be understood and accepted in order to understand ordered pairs of reals.

[15] Viète and Descartes were able to avoid the use of complex numbers by constructing the solutions to these cubics as the lengths of sides of triangles determined

series solutions of the eighteenth century.[16] Mathematicians began to wonder if there is a semantically pure solution.[17] In 1892, Otto Hölder answered this question, using Galois theory to show that there can be no solution to cubics in the casus irreducibilis that avoids using complex numbers, while at the same time using just finitely many instances of the six algebraic operations.[18]

Hölder's result demonstrates that to solve cubics in the casus irreducibilis exactly, imaginary numbers must be used. Since complex numbers needn't be understood in order to understand this problem, we have an example where what is needed to solve a problem exceeds what is needed to understand it. This demonstrates Thesis 1, that some results require more concepts and/or proposition to be proved than to be understood.

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by the cubics. Viète's work on the casus irreducibilis is located in two places: his 1593 text *Supplementum Geometriae*, and his 1615 text *De Aequationum Recognitione et Emendatione Tractatus Duo*. Both are available in translation in [32]. Descartes' work on the casus irreducibilis is in Book III of *La Géométrie* ([6], pp. 215–16). This trigonometric construction can be expressed as an equation, by making use of the non-algebraic trigonometric operations cos and arccos; though, interestingly, this formula requires complex numbers in cases where Cardano's solution avoids them. This geometric method isn't semantically pure, as it requires understanding either geometry or non-algebraic operations.

[16]Newton and Leibniz both attempted to use infinite series to avoid complex algebra for solving cubics in the casus irreducibilis, but the idea came to full fruition a little later in work of François Nicole and Alexis Clairaut. For Newton's work on this, see his letter to Collins, dated 6/20/1674, in [28]. For Leibniz' work, see a 1675 (approx.) letter to Wallis, in [21]. Nicole's work may be found in [22] and [23], while Clairaut's extension of Nicole's work may be found in [**?**]. Nicole's papers contain the essential details, applying Newton's binomial theorem to Cardano's equation, except that he missed a few details about the conditions under which binomial series converge. Clairaut corrected those mistakes in his 1746 algebra textbook. This method isn't semantically pure, since it requires understanding infinite series.

[17]For instance, Lagrange wrote that the "irreducible case of equations of the third degree... is constantly giving rise to unprofitable inquiries with a view to reducing the imaginary form to a real form and ... presents in algebra a problem which may be placed upon the same footing with the famous problems of the duplication of the cube and the squaring of the circle in geometry." ([20], p. 62) Proof of the impossibility of doubling the cube and squaring the circle using just straightedge and compass was still thirty years away at the time Lagrange gave his lectures, but their impossibility was generally accepted in his time as fact. The passage suggests that a similar attitude had taken hold concerning the casus irreducibilis.

[18]For Hölder's proof, see [16]; see also Hölder's commentary in [17]. Hölder's impossibility theorem may be stated precisely as follows: if a cubic equation $x^3 + qx + r = 0$ has three real, unequal roots and is irreducible over the field $F = \mathbb{Q}(q, r)$, then it is not solvable by real radicals.

3.2. **The infinitude of primes and fragments of arithmetic.** Though it sounds strange, some results can be proved using fewer resources than are needed to understand it. I want to consider one example of this from arithmetic.

The infinitude of primes (IP) asserts that for all natural numbers a, there exists a natural number $b > a$ such that b is prime. It was proved by Euclid in *Elements* Book IX, Proposition 20. To understand IP, we must understand what a natural number is. It's been argued that to do so, we must understand and accept the second-order induction axiom $\forall X[0 \in X \wedge \forall y(y \in X \rightarrow S(y) \in X) \rightarrow \forall x(x \in X)]$, where S is the successor function and X ranges over all subsets of the natural numbers.[19] This is controversial, but let's temporarily grant the claim and focus on its consequences. IP can be stated formally in the language of first-order arithmetic, and Euclid's proof can be carried out in first-order Peano Arithmetic (PA) with only minor modifications.[20] But PA is weaker than second-order arithmetic: instead of an induction axiom for every subset of the natural numbers, it includes countably many induction axioms of the form $\forall \bar{y}[\varphi(0, \bar{y} \wedge \forall x(\varphi(x, \bar{y}) \rightarrow \varphi(S(x), \bar{y})) \rightarrow \forall x \varphi(x, \bar{y})]$, for each formula $\varphi(x, \bar{y})$ in the language of first-order arithmetic. That is, PA includes induction axioms for each subset of the natural numbers that is definable by a first-order formula, but there are only countably many of these, and there are uncountably many subsets of the natural numbers. So PA requires less to be understood and accepted than IP does. Yet IP can be proved in PA. So less is needed to prove IP than to understand it.

The argument I just gave depended on the claim that the second-order induction axiom must be understood and accepted in order to understand what a natural number is. Let's drop that claim, and suppose instead that understanding and accepting first-order PA is necessary and sufficient for understanding what a natural number is. But IP can be proved in logically weak fragments of PA, specifically by weakening the induction schema. It is straightforward to check that the Euclidean proof, when formalized in PA, uses the induction schema for formulas of complexity at most Σ_1. That is, the proof can be carried out in $\mathrm{I}\Sigma_1$, which is PA with the induction schema restricted to Σ_1 formulas. On the other hand, the Euclidean proof cannot be carried out in $\mathrm{I}\Delta_0$, which is PA with the induction schema restricted to formulas

[19]Cf. [18], pp. 204–206, and [19], p. 42, for example.

[20]The formalized infinitude of primes is $\forall a \exists b[b > a \wedge \forall x[\exists y(x \cdot y = b) \rightarrow (x = 1 \vee x = b)]]$.

with just bounded quantifiers.[21] It is open whether IP can be proved in $I\Delta_0$.[22] However it is known that the Euclidean proof can be carried out using bounded induction provided that we add another axiom asserting the totality of the exponential relation, resulting in a theory called $I\Delta_0(exp)$.[23] So IP can be proved in $I\Delta_0(exp)$, but understanding and accepting $I\Delta_0(exp)$ isn't sufficient for understanding what a natural number is. Once again, we have that less is needed to prove IP than to understand it.

Thus, in either case we have demonstrated Thesis 2, that some results require more concepts and/or proposition to be understood than to be proved.

3.3. **Gödel sentences.** Finally, I want to consider the case of Gödel sentences. As is well-known, there are sentences expressed in the language of PA that are unprovable in PA, but are provable in stronger formal systems (such as ZFC). In virtue of being expressed in the language of PA, Gödel sentences have arithmetical content, and as a result, a grasp of the arithmetical definitions provided by the axioms of PA suffices for understanding these sentences. So Gödel sentences are another example where more is needed for proof than understanding.

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[21]This is because in the Euclidean proof, we suppose that we have a finite enumeration $p_1, \ldots, p_n$ of all the primes, and generate the quantity $Q = (p_1 \cdot p_2 \cdots p_n) + 1$ towards showing that there is another prime not on this list. But to show that Q exists, we need that multiplication is total, and the usual proof of this uses Σ_1 induction. How much induction is *needed* to prove this? That is an open question. However, it is known that in $I\Delta_0$ it is unprovable that every product of primes exists (cf. [4], p. 13). This follows from Parikh's result (in [26]) that every Δ_0-definable function that is provably total in $I\Delta_0$ has polynomial growth. Cf. [5] pp. 164–7 for more on what is known concerning the rate of growth of the function yielding products of primes in $I\Delta_0$.

[22]In his dissertation, A. Woods [33] was able to solve IP in $I\Delta_0$ together with a weak version of the pigeonhole principle. He did not present a modified version of the Euclidean resolution, but instead, a modified version of a solution due to Sylvester. Woods' theory, called $I\Delta_0 + PHP$, is logically weaker than $I\Delta_0(exp)$, in that $I\Delta_0(exp)$ proves $I\Delta_0 + PHP$ but not vice-versa (cf. [27]; also [5], pp. 162–4). Later Paris, Wilkie, and Woods replaced Woods' earlier proof with one using an even weaker version of the pigeonhole principle (cf. [27]; also [5], pp. 162–4).

[23]Cf. [5], p. 153. $I\Delta_0(exp)$ is sometimes studied under the name EFA, for Elementary Function Arithmetic (cf. [10]) or EA, for Elementary Arithmetic (cf. [2]). In [2] Avigad explains Harvey Friedman's conjecture that every result in elementary number theory (for instance, every result in Hardy and Wright [13], a canonical elementary text in number theory) can be proved in $I\Delta_0(exp)$ (cf. [5], p. 149n1). Due to a result of Gödel [11], we know that the exponential relation is definable in $(\mathbb{N}, 1, S, +, \cdot)$. For a reasonably explicit definition of this type, see [7], pp. 276–9.

I want to consider two objections to this. The first is based on a view of Daniel Isaacson. Isaacson writes that "the only way to *see* the arithmetical truth of the Gödel sentence is" to see it as having coded metamathematical content, i.e. to see that it says of itself that it isn't provable in whichever axiom system is being considered. I have my qualms about this point[24], but let's grant it for the sake of argument. Isaacson argues that this shows that the Gödel sentence doesn't have arithmetical content, and so to be understood requires understanding and accepting higher-order truths, such as those of set theory. This is because he believes that the type of content a sentence has depends on what must be grasped to "perceive" that that sentence is true. As he writes:

> [A] truth expressed in the (first-order) language of arithmetic is arithmetical just in case its truth is directly perceivable on the basis of our (higher-order) articulation of our grasp of the structure of the natural numbers *or* directly perceivable from truths in the language of arithmetic which are themselves arithmetical.[25]

Isaacson explains that by the "grasp of the structure of the natural numbers" clause he has in mind axioms, while by the second clause he has in mind theorems.

If Isaacson were correct, it would undermine my claim that more is needed to prove Gödel sentences than to understand them. I don't think he is correct, though, as I want to show by two different replies. Firstly, Isaacson's view renders obviously arithmetic sentences like the Goldbach conjecture un-arithmetical, since there is at present no reason to believe its truth (or the truth of its negation, should it turn out to be false) is like an axiom in being "directly perceivable" just from our grasp of the structure of the natural numbers, and we know of no proof of it from any truths at all at present, arithmetic or not. I don't

[24]The Gödel sentence for PA can be proved in ZFC. It's plausible that a person who knew nothing about metamathematics, but had a command of set theory, would encounter the Gödel sentence for PA, but would not recognize it as such. She could then prove the Gödel sentence without seeing it as having coded metamathematical content.

Isaacson moderates his view later in the paper, saying that Gödel sentences can and must be "shown to be true by an argument in terms of truths concerning some higher-order notions", including "essentially set-theoretical principles" (pp. 220–1). But once again he cites "the relationship of coding" as the "rigid link between the arithmetical and the higher-order truths", and I don't see why we should believe this, as I explained above.

[25]Cf. [18], p. 217.

think we should accept a view that implies that the Goldbach conjecture is un-arithmetical. Secondly, it's true that we can't see that the Gödel sentence is a Gödel sentence without grasping its coded metamathematical content; but we can grasp it as a universally quantified sentence in the language of PA, without seeing that it's a Gödel sentence. It's true that *our* reason for interest in the Gödel sentence may be that it is a Gödel sentence, rather than because we encountered it in ordinary arithmetical work, but we shouldn't conclude from that contingency that Gödel sentences aren't arithmetical. After all, we still *could* encounter sentences independent from PA in the course of future work in mainstream number theory, without knowing beforehand that these sentences are independent of PA. The arithmetic character of a problem is independent of the reasons we have for choosing to solve that problem.

The second objection to my claim that Gödel sentences require more for proof than for understanding, is the following: Gödel sentences expressed in the language of PA are, practically speaking, unintelligible because they contain so many symbols; but when expressed in higher-order terms (such as in set theory), they become intelligible.[26] So, practically speaking, grasp of the axioms of PA is not sufficient for understanding Gödel sentences.

I don't think we should be troubled by this objection either. For this observation holds not just for Gödel sentences, but for many obviously number-theoretic sentences, for instance those involving exponentiation (such as Fermat's last theorem). It is possible to express exponentiation in the language of PA, using the Gödel β-function, but the sentences resulting from the subsequent substitution will be quite long and complex, compared to the simple sentence with which we began. So if the gain in intelligibility resulting from using higher-order terms tells against a sentence expressible in the language of PA being arithmetic, then we will have to conclude that many statements of elementary number theory will also fail to be arithmetic. Since I think this is implausible, I reject this second objection.

I conclude that the case of Gödel sentences demonstrate Thesis 1, that some results require more concepts and/or proposition to be proved than to be understood.

[26]Isaacson makes a related point, using the observation that higher-order notions "can be essential for shortening an otherwise unsurveyable proof." (p. 221)

References

[1] Andrew Arana. On formally measuring and eliminating extraneous notions in proofs. *Philosophia Mathematica*, 2008. Forthcoming.

[2] Jeremy Avigad. Number theory and elementary arithmetic. *Philosophia Mathematica*, 11:257–284, 2003.

[3] H. S. M. Coxeter. *Introduction to Geometry.* Wiley, second edition, 1989.

[4] Paola D'Aquino. Local behaviour of the Chebyshev theorem in models of $I\Delta_0$. *J. Symbolic Logic*, 57(1):12–27, 1992.

[5] Paola D'Aquino. Weak fragments of Peano arithmetic. In *The Notre Dame Lectures*, volume 18 of *Lecture Notes In Logic*, pages 149–185. Association for Symbolic Logic, Urbana, IL, 2005.

[6] René Descartes. *La Géométrie.* Dover Publications, 1954. Translated by D.E. Smith and M.L. Latham, 1925.

[7] Herbert B. Enderton. *A mathematical introduction to logic.* Harcourt/Academic Press, Burlington, MA, second edition, 2001.

[8] Paul Erdős. Problem 4065. *American Mathematical Monthly*, 50(1):65, January 1943.

[9] Harvey M. Friedman. Systems of second order arithmetic with restricted induction. I. *Journal of Symbolic Logic*, 41(2):557–8, June 1976.

[10] Harvey M. Friedman. A Strong Conservative Extension of Peano Arithmetic. In H. J. Keisler J. Barwise and K. Kunen, editors, *Proceedings of the 1978 Kleene Symposium*, pages 113–122. North-Holland, 1980.

[11] Kurt Gödel. Uber formal unentscheidhare Sätze der *Principia Mathematica* und verwandter Systeme I. In Solomon Feferman et. al., editor, *Collected Works*, volume 1, pages 145–195. Oxford Univ. Press, 1986.

[12] Michael Hallett. Reflections on the Purity of Method in Hilbert's *Grundlagen der Geometrie.* In Paolo Mancosu, editor, *The Philosophy of Mathematical Practice.* Oxford University Press, 2008.

[13] G. H. Hardy and E. M. Wright. *An introduction to the theory of numbers.* Oxford University Press, New York, fifth edition, 1979.

[14] David Hilbert. Über den Satz von der Gleichheit der Basiswinkel im gleichschenkligen Dreieck. *Proceedings of the London Mathematical Society*, 35:50–68, 1902/03.

[15] David Hilbert. *Lectures on the foundations of geometry, 1891-1902*, volume 1. Springer, 2004.

[16] Otto Hölder. Über den Casus Irreducibilis bei der Gleichung dritten Grades. *Mathematische Annalen.*, 38:307–312, 1892.

[17] Otto Hölder. *Encyklopädie der mathematischen Wissenschaften. Band I. Algebra und Zahlentheorie. Teil 1. B. Algebra. Heft 3. Teil 27. Casus irreducibilis der kubischen Gleichung.* W. F. Meyer, Leipzig, 1898-1904.

[18] Daniel Isaacson. Arithmetical truth and hidden higher-order concepts. In W.D. Hart, editor, *The Philosophy of Mathematics*, pages 203–224. Oxford University Press, 1996.

[19] Richard Kaye. *Models of Peano Arithmetic.* Oxford University Press, Oxford, 1991.

[20] Joseph-Louis Lagrange. *Lectures on elementary mathematics*. Open Court, Chicago, 1898. Based on lectures given to L'Ecole Normale in 1795, and originally published in Journal de L'Ecole Polytechnique in 1812. Translated by T.J. McCormack.

[21] Gottfried Wilhelm Leibniz. Letter to Wallis, 1675 (approx.). In *Gothofredi Guillelmi Leibnitii Opera Omnia*. Apud Fratres de Tournes, Geneva, 1768.

[22] François Nicole. Sur le cas irréductible du troisiéme degré. *Memoires de L'Academie Royal*, pages 97–102, 1738.

[23] François Nicole. Sur les equations du troisiéme degré. *Memoires de L'Academie Royal*, pages 244–259, 1738.

[24] Victor Pambuccian. Fragments of Euclidean and hyperbolic geometry. *Scientiae mathematicae Japonicae*, 53(2):361–400, March 2001.

[25] Victor Pambuccian. Euclidean geometry problems rephrased in terms of midpoints and point–reflections. *Elemente der Mathematik*, 60:19–24, 2005.

[26] Rohit Parikh. Existence and feasibility in arithmetic. *J. Symbolic Logic*, 36:494–508, 1971.

[27] J. B. Paris, A. J. Wilkie, and A. R. Woods. Provability of the pigeonhole principle and the existence of infinitely many primes. *J. Symbolic Logic*, 53(4):1235–1244, 1988.

[28] Stephen Rigaud, editor. *Correspondence of scientific men of the seventeenth century, including letters of Barrow, Flamstead, Wallis, and Newton*, volume 2. Oxford University Press, Oxford, 1841.

[29] Stephen G. Simpson. *Subsystems of Second Order Arithmetic*. Perspectives in Mathematical Logic. Springer, 1999.

[30] J. J. Sylvester. Mathematical Question 11851. *Educational Times*, 59:98, 1893.

[31] Oswald Veblen. A system of axioms for geometry. *Transactions of the American Mathematical Society*, 5(3):343–384, July 1904.

[32] François Viète. *The Analytic Art*. Kent State University Press, 1983. Translated by T. Witmer.

[33] Alan Woods. *Some problems in logic and number theory and their connections*. PhD thesis, University of Manchester, 1981.

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