Syntactic analogies and impossible extensions

Brendan P. Larvor

Department of Philosophy, University of Hertfordshire, Hatfield, AL10 9AB, United Kingdom

E-mail: B.P.Larvor@herts.ac.uk

Mathematicians study shapes, structures and patterns. However, there are shapes, structures and patterns within the body and practice of mathematics that are not the direct objects of mathematical study. Rather, they are part of the explanation of how mathematical study is possible, and thus demand the attention of epistemologists and phenomenologists as well as mathematicians. Partial philosophical accounts of these enabling structures include heuristic in the senses of Polya and Lakatos; principles in the sense of Cassirer; ideas in the sense of Lautman and notions in the sense of Grattan-Guinness (Polya, 1954; Lakatos, 1976; Cassirer, 1956; Lautman, 2006; Grattan-Guinness, 2008). The study of these structures lies in the intersection of mathematics and philosophy because some of these shapes, structures and patterns may eventually submit to mathematical treatment, but others may have a 'Protean' quality that will always escape formal treatment.

The examples given here are heterogeneous in their origins and functions. Cassirer and Grattan-Guinness find their principles and notions (respectively) in applied mathematics and empirical science.[1] Lautman sought the same ideas (in his Platonic sense of 'idea') in mathematics and physics, though he looked longer and harder in pure mathematics than in physics (this doctrine, that mathematics and physics have a common root, is part of his Platonism). The heuristic patterns in Lakatos and Polya are more closely specific to pure mathematics, though this may be an artefact of their contingent mathematical interests. Regarding function, heuristic patterns are not necessarily the deepest of these shapes, precisely because they are evident before (or at least during) mathematical investigation, whereas Lautman's Platonic ideas typically come into view late in the day, when they are instantiated in several different mathematical theories. Cassirer's principles and Grattan-Guinness' notions seem to be intermediate, having both heuristic and ontological significance (reading 'ontological' here in something like Lautman's Platonic-Heideggerian sense).

The aim of this paper is not to undertake the large task of classifying and comparing these various kinds of enabling structures. Rather it is to

[1] Cassirer also gives an example of a principle in mathematics—the use of groups. I am grateful to David Corfield for this point, among others.

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explore two kinds of pattern that are, I shall suggest, unique to mathematics: syntactic analogy and generalisation by extension. I hope thereby to illustrate some of the contrasts that such a classification would require.

1 Syntactic analogy

Mathematicians prior to Leibniz had already begun to reap the benefits of operating with mathematical expressions without waiting to find out what they mean. As early as 1545, Cardano calculated with complex numbers, though he could not use them in his proofs and had no way of understanding them. The development of symbolic algebra in the early decades of the seventeenth century introduced a distinction between the syntax of mathematical notation and the meanings of mathematical symbols. This allowed mathematicians to work with the syntax first and then, later, make sense of the new expressions it gave them. Of course, this gave rise to a debate among mathematicians about the value and reliability of results obtained this way. It was Leibniz who turned these *ad hoc* devices into something like a programme of research. He experimented with many new symbols in addition to those he introduced that are still in use today (see Cajori, 1925). When the mathematician Tschirnhausen objected to these typographical novelties, Leibniz reminded him that Arabic numerals had once been new, as had letters standing for numbers. Leibniz continued:

> In signs one observes an advantage in discovery which is greatest when they express the exact nature of a thing briefly and, as it were, picture it; then indeed the labour of thought is wonderfully diminished. (Gerhardt, 1899, vol I, p. 375); (Cajori, 1925, p. 416).

This advantage is not unique to mathematics; other discourses have notions that 'picture' the relationships among the elements of their objects. Chemical formulae picture the relations among the elements that constitute molecules. However, in chemistry, the relata matter. We would not suppose that H_2O and Ag_2F (silver subfluoride) must be, on some level, the same stuff just because their formulae picture the same relations between their elements. In contrast, mathematics abstracts from the relata, the better to study the relations. If two mathematical structures are isomorphic, then for many mathematical purposes they are identical. Therefore, if two mathematical formulae (in Leibniz's words) 'express briefly and as it were, picture' the 'exact natures' of their referents, and the formulae are isomorphic, then we should expect the referents to be, at some level of description, identical (or at least, systematically related).

A well-known example from Leibniz illustrates this point. In a letter to Johann Bernoulli of October 1695, Leibniz proposed his general formula for repeated differentiation of a product.[2] He started with Newton's binomial formula:

$$(x+y)^n = x^n y^0 + nx^{n-1}y + \frac{n(n-1)}{1\cdot 2}x^{n-2}y^2 + \ldots + x^0 y^n$$

Notice the inclusion of x^0 and y^0 to perfect the homogeneity, and thus the 'harmony', of the expression. Leibniz rewrote it as follows, with exponentiation treated as an operator, denoted by 'p':

$$p^n(x+y) = p^n x p^0 y + np^{n-1}xp^1 y + \frac{n(n-1)}{1\cdot 2}p^{n-2}xp^2 y + \ldots + p^0 x p^n y$$

Now Leibniz made a pair of inspired substitutions. He replaced the sum on the left hand side with a product and replaced the 'p's (for 'power') by 'd's (for 'differential'). This gave him the general product formula for differentiation:

$$d^n(xy) = d^n x d^0 y + nd^{n-1}xd^1 y + \frac{n(n-1)}{1\cdot 2}d^{n-2}xd^2 y + \ldots + d^0 x d^n y$$

It is not clear what inspired Leibniz to make precisely these substitutions (note the insight necessary to replace sum by product on the left hand side only). Nor can we tell quite how much confidence he placed in this formula before he tested it. We do know that Leibniz must have considered this procedure respectable, because otherwise he would not have shared it with Bernoulli. What is clear is that the simple heuristic of reading $\frac{dy}{dx}$ as a quotient of infinitesimals does not, alone, lead to this result.

As he was unable to prove many of his most important results, Leibniz did have to rely on a combination of corroboration from successful applications and faith that similarities in syntax indicate a common structure. A modern mathematical eye may find irrationality in Leibniz's willingness to rely on syntactic similarities of this sort.[3] After all, the trick works only if the 'signs' do indeed express the 'nature of the thing' perfectly—but if in any given case we know that, then we probably do not need to appeal to features of the notational syntax at all. Leibniz's confidence in his procedure

[2] (Gerhardt, 1863, vol. III, p. 221); (Coe, 1950, p. 459). Cf. also (Serfati, 2005, pp. 389–390).

[3] "The play of symbols can thus seem opposed, not only to usage, but also, in a certain sense, to rationality" ("Le jeu combinatoire peut ainsi sembler s'opposer, non seulement à l'usage, mais aussi, en un certain sens, à la rationalité.") (Serfati, 2005, p. 390). Strictly speaking, the status of 'knowledge' belongs to results that the mathematical community accepts into the common canon, so judgments of epistemic rationality attach properly to the practice of whole communities of mathematicians rather than of individuals.

arises from his rationalism, and specifically from the metaphysical principle that 'harmony' is an indication of reality (because maximal harmony is one of the merits of the best of all possible worlds). This is the mood, familiar from Pythagoras and Plato, in which rationalism becomes mystical. In any case, the mathematical community did not collectively share Leibniz's rationalism, knew that proofs and explanations were lacking and (eventually) supplied them.

Notice the difference between this case and the many structural analogies in empirical science. Take, for example, Rutherford's planetary model of the atom. This captures some gross features of atoms—the massy centre orbited by (relatively) tiny specks, and the emptiness of almost all of the space 'occupied' by an atom. But no-one would suppose that atoms and solar systems are instances of the same general phenomenon. The gross similarities sustain nothing more than a rough analogy. By contrast, the common binomial form of Newton and Leibniz's formulae, as 'pictured' in the notation, suggests that they are both instances of a single mathematical structure. Certainly, it is reasonable (even without sharing Leibniz's metaphysics) to seek a mathematical explanation of their common binomial form (for which cf. Coe, 1950). The reason is obvious: mathematics studies phenomena 'up to isomorphism'—including, especially, mathematical phenomena. If the notation in each of two cases 'expresses the exact nature of the thing briefly and, as it were, pictures it', and if there is an isomorphism between the *notations*, then we should expect to find a level of description where the two *phenomena* appear as instances of a common mathematical structure. In empirical science, a notational isomorphism would not demand an explanation of the same sort. Isomorphisms between formulae in (say) mechanics and economics might suggest a heuristically useful analogy, but they would not prompt a search for a common genus (unless the formulae were abstracted from their empirical origins and treated as purely mathematical objects). This intimacy between mathematical objects and mathematical notation is unique to mathematics and reflects its nature as the science of structures.

Leibniz was probably the first mathematician to work in this way, but by no means the last. In 1861, John Blissard introduced a general method of this sort. Blissard's technique was to take identities involving sequences of polynomials with powers a^n, and create new expressions by changing the polynomials to discrete values and replacing the a^n with the falling factorial $(a)_n \equiv a(a-1)\ldots(a-n+1)$. Many of the resulting identities are both true and interesting. Sylvester named this method the 'umbral calculus' because the newly generated identities are 'shadows' of the generating identities. In the 1970s, Steven Roman and Gian-Carlo Rota developed Blissard's idea rigorously and offered a mathematical explanation of the successes it brought to Blissard, Sylvester and others (Roman, 1984). Beyond pure mathematics, there are mathematical practitioners who routinely exploit syntactic (or 'formal') likenesses. Cartier observes that "For the physicists, [the word 'formal'] is more or less synonymous with 'heuristic' as opposed to 'rigorous"'. Cartier goes on to give a survey of mathematics conducted in this 'formal' spirit (including a detailed discussion of the Leibniz's formula; Cartier, 2000, p. 4).

In terms of our wider enquiry, we have here a pattern with two features. First, it offers a general heuristic rule: where there is a structural similarity between two mathematical expressions, seek a corresponding structural similarity between the mathematical matters they express. Second, we can articulate this pattern almost entirely in mathematical terms. Where Leibniz wrote of 'picturing', we can express our rule in terms of isomorphisms, understood literally in the usual mathematical sense.

For the sake of contrast, I turn now to a pattern that lacks these two features.

2 Impossible extensions

These are cases in which a function is defined for some limited range of values in such a way that an extension beyond that range seems impossible. To take one of the simplest and earliest examples, Descartes popularised the current exponential notation a^x, to indicate x multiples of a (though he was not the first to use this notation).[4] This notation abbreviated the previous practice of writing a^3 (for example) as aaa. Thus defined, exponentiation requires x to be a natural number, but nothing in this syntax inhibits us from replacing x by a negative number, a fraction or a complex number.[5] In these cases, it is relatively straightforward to extend the domain of a function. For example, power series allowed mathematicians to calculate values of e^z and of trigonometric functions of z where z is a complex number. Of course, this required mathematicians to revise their understanding of what these functions mean. It was precisely the ease of calculating the new values that demanded this semantic revision. One of the characteristic features of this period of mathematics is that calculating practice ran ahead of the semantics, so that there were (in Serfati's terms) 'meaningless forms', that is, mathematical expressions in use that no-one could explicate, let alone define.

More interesting are cases where it is not possible to extend a function simply by calculating values for arguments outside the original domain. In these cases, Serfati identifies the following pattern: shift attention from the

[4] Cf. (Stedall, 2007, p. 399).

[5] Serfati (2005, pp. 262–266) traces this development in some detail, recording the introduction of fractional (Newton), irrational (Leibniz) and imaginary (Euler) exponents.

definition of the function to one or more of its trivial consequences. This corollary may then serve as a definition of the new, expanded function, or at least as a 'bridge' from the original restricted function to its new, extended version. The difficult part is then to find a function on the new, expanded domain that (a) satisfies this corollary of the original definition, (b) coincides with the original function on the original domain, and (c) does some useful mathematical work. Serfati offers several examples,[6] the earliest and simplest of which is Euler's extension of the factorial function from natural numbers to positive real numbers (Serfati, 2005, pp. 366–368). As long as we concentrate on the definition of the factorial function, it seems impossible to extend it beyond the natural numbers. However, Euler found a function that satisfies conditions (a), (b) and (c). But how?

In a letter to Goldbach[7] of 1729, Euler observed that for a natural number m,

$$m! = \frac{1 \cdot 2^m}{1+m} \cdot \frac{2^{1-m} \cdot 3^m}{2+m} \cdot \frac{3^{1-m} \cdot 4^m}{3+m} \cdot \ldots \cdot \frac{n^{1-m} \cdot (n+1)^m}{n+m} \cdot \ldots .$$

Euler's guiding heuristic here is the use of infinite products. From this equation, it is a short step to this:

$$x! = \lim_{n\to\infty} \left(\frac{1 \cdot 2 \cdot \ldots \cdot n}{(1+x)(2+x)\ldots(n+x)} (n+1)^x \right).$$

Euler explains in his letter that for natural numbers, this function coincides with the factorial function, but it is also well-defined for fractions. Having satisfied our condition (b), Euler then goes on to argue (c) (that this development is mathematically worthwhile). In fact, in a slightly different guise, this function becomes the Gamma function, so with hindsight we may agree that (c) is satisfied.

In terms of Serfati's pattern, the 'bridge' in this case between the original factorial function and the extended version is the trivial fact that factorial satisfies the functional equation $g(x) = x\,g(x-1)$. It is easy to see that Euler's new function satisfies this equation too. Call this function $f(x)$.

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[6] Factorial of a positive real number; Exponentiation by a complex number or square matrix; Trigonometric functions of complex numbers; Matrix pseudo-inverses; Derivative of a non-differentiable function; Derivative of a function on normed vector-spaces; Union and intersection of r-partitions (Serfati, 2005, pp. 366–376). The second and third examples seem ill-chosen as there is no obvious 'bridging' identity.

[7] Eneström number 00715. Quoted from (Fuss, 1843, pp. 3–7).

Then:

$$f(x) = x \lim_{n\to\infty} \left(\frac{1 \cdot 2 \cdot \ldots \cdot n}{x(1+x)(2+x)\ldots(n+x)} (n+1)^x \right)$$
$$f(x) = x \lim_{n\to\infty} \left(\left(\frac{1 \cdot 2 \cdot \ldots \cdot n}{x(1+x)(2+x)\ldots(n+x-1)} (n+1)^{x-1} \right) \frac{n+1}{n+x} \right)$$
$$f(x) = xf(x-1) \lim_{n\to\infty} \left(\frac{n+1}{n+x} \right) = xf(x-1)$$

Despite Serfati's description of the case, Euler did not bother to make this argument. Euler was certainly trying to extend the domain of the factorial function, but he did not seem to be looking for a function that satisfies the functional equation $g(x) = x\,g(x-1)$. Rather, his procedure was to express the factorial function as an infinite product, and then observe that he could calculate this product for fractional values of m. Certainly, it took a moment of mathematical imagination to wonder what happens if m is a fraction, and in this sense this case is like the extension of exponentiation to irrational and complex values. However, we do not have mathematical syntax operating ahead of the corresponding semantics as we had in the earlier cases. By Euler's time, addition and exponentiation were perfectly well understood for fractional values, which is all that Euler's infinite product requires. Moreover (as Serfati mentions) Euler did not use the ! sign, which Christian Kramp introduced in his work of 1808 (Serfati, 2005, p. 367n72). Nor, in this letter, did Euler use any other symbol in its place. Rather, he refers to this series, 1, 2, 6, 24, 120, etc. Serfati's chapter is entitled 'forms without meaning' but there is no meaningless form here. Moreover, the bridge Serfati identifies (the functional equation $g(x) = x\,g(x-1)$) does not seem to have played any role in Euler's thinking. The case certainly fits Serfati's 'bridging' pattern, but only in his *post hoc* analysis.

This case is almost three centuries old, and depends in its details on a style of mathematics that now seems antique. What about Serfati's more up-to-date cases? I shall consider his fourth example of an extension, namely, matrix pseudo-inverses (Serfati, 2005, pp. 370–372). Serfati considers two kinds of pseudo-inverse: Moore-Penrose pseudo-inverses and pseudo-inverses defined using a norm on the space of matrices.

Serfati introduces this example by asking "Can we give a meaning to the 'form' A^{-1} if A stands for any matrix, that is to say, possibly not square or if square, non-invertible?"[8] Here again, we see Serfati trying to fit the example

[8] "Peut-on fournir une signification à la 'forme' A^{-1}, où A est le signe d'une matrice complexe quelconque, c'est-à-dire qui peut être non carrée, ou bien carrée non inversible?" (Serfati, 2005, p. 370).

into the seventeen-century mould of mathematicians using symbols without really knowing what they mean. In fact, Penrose defines a new 'form', $A^\dagger$, to stand for his new function, just as one would expect (Penrose, 1955, p. 407). By the twentieth century, Leibniz's attitude to mathematical symbols was commonplace and no-one would bother to complain as Tschirnhausen did about the introduction of a new symbol. Indeed, the separation of syntax from semantics was complete in theory as well as in practice by the end of the nineteenth century. Consequently, mathematicians seeking to introduce a new concept have no reason to do so surreptitiously by abusing the symbol for an already existing notion. The 'meaningless form' aspect of Serfati's analysis sits even less happily with the case of Moore-Penrose pseudo-inverses than it does with the Euler factorial.

On the other hand, Penrose's paper does illustrate the pattern that Serfati identifies in these extensions: using a trivial consequence of the original definition as a 'bridge' to extend the function to a wider domain. In this case, the bridge consists of four trivial identities. For any invertible matrix A, where A^* is the conjugate transpose, or adjoint of A:

$$\begin{aligned} AA^{-1}A &= A \\ A^{-1}AA^{-1} &= A^{-1} \\ (AA^{-1})^* &= AA^{-1} \\ (A^{-1}A)^* &= A^{-1}A \end{aligned}$$

These four identities follow immediately from the definition of the inverse, and from the fact that the identity matrix is its own adjoint. At the outset of his paper, Penrose proves by construction that for every matrix A there is a unique matrix X such that:

$$\begin{aligned} AXA &= A \\ XAX &= X \\ (AX)^* &= AX \\ (XA)^* &= XA \end{aligned}$$

That is to say, for every matrix A there is a unique matrix X that plays the role of an inverse in these four identities. Having proved the existence and uniqueness of this X, Penrose labels it $A^\dagger$ and proceeds to prove corollaries and show applications. As this is a formal, published proof rather than a private letter, Penrose gives no account of how he chose just these four identities to serve as a bridge, nor of how he arrived at his construction. Examination of the proof does suggest some plausible guesses; Penrose derives the pseudo-inverse from the fact that A^*A, $(A^*A)^2$, $(A^*A)^3$, etc. cannot be linearly independent (rather than simply introducing it as a *deus ex machina* in the sense of Polya, 1954, volume II, *Patterns of Plausible Inference*, p. 148). These hints aside, the proof does not expose its heuristic background. In its proof at least, this case does follow Serfati's 'bridging' pattern. However, given the complexity of the construction it seems unlikely that Penrose first somehow selected the four trivial identities and then later went in search of something that would satisfy them in the general case.

Serfati further illustrates the 'bridging' pattern with matrix pseudo-inverses of another kind, which use norms defined on the space of matrices. Given such a norm, it follows from definitions that for any invertible matrix A, this trivial identity holds: $\|AA^{-1} - I\| = 0$ This serves as the bridge. However, in this case, we cannot simply replace A^{-1} by X (as in the Moore-Penrose case), because $\|AX - I\| = 0$ has solutions only if A is invertible. However, for any non-zero complex matrix A there is a unique matrix X such that $\|AX - I\|$ is minimal. Thus by loosening the condition a little, we can define X as the right-hand pseudo-inverse of A. As with the Moore-Penrose pseudo-inverse, it seems unlikely that the bridging condition came first. Indeed, whenever we find this pattern, we should expect the choice of bridging condition to be informed by some effective heuristic (the use of infinite products in the case of Euler; linear dependency of the $(A^*A)^n$ in the Moore-Penrose case and perhaps the presence of rounding errors in the case of the second kind of pseudo-inverse).[9]

Serfati's 'bridging' pattern is objectively present in the cases he describes, but (for the reason just given) it cannot function as a heuristic in the sense of Polya or Lakatos. 'Look for a bridge to a useful general function' is hardly a helpful hint. Rather, the pattern that Serfati identifies in these cases is more like a 'dialectical structure' in something like the sense of Lautman. That is to say, it is objectively present in the mathematics but discernible only *post hoc* (Lautman, 2006, especially pp. 228–9). In respect of heuristic usefulness, Serfati's pattern contrasts with the case of syntactic analogy. These two patterns also contrast in respect of mathematical tractability. While it may be possible to articulate the syntactic analogy pattern in mathematical terms, this seems less likely in the case Serfati's pattern.

3 Structures in practice

One of the obvious ways in which mathematics differs from other sciences is the freedom that mathematicians have to invent new mathematical objects. In mainstream philosophy of mathematics, this is often understood as a freedom to investigate whichever axiom-systems seem interesting. That

[9] I am grateful to Thomas Müller for this last suggestion.

is to say, insofar as philosophy of mathematics treats mathematics as a collection of axiomatic systems, the mathematician's freedom seems limited to the moment when the axioms are chosen. The choice of axioms fixes the theorems; thereafter the task is to identify them correctly. Finding and proving the theorems may take ingenuity, but the possibility of creating radically new mathematics has passed.

In practice, not all of mathematics is axiomatised and even where there are axioms, these do not prevent mathematicians from creating new mathematics (Rav, 1999, pp. 15-19).[10] Serfati's examples of functional extensions remind us that the creation of new mathematics is rarely a matter of choosing axioms. Rather, the growth of mathematics (in these cases) depended on shrewd choices of 'bridge'. These choices are constrained by the requirement that the new function should coincide with the old one on the original, restricted domain, and that it should lead to some insight or useful application. Nevertheless, as we saw in the case of the pseudo-inverses, these constraints do not always pick out a unique candidate. On such occasions, the mathematician is free to make a judgment. Freedom of invention distinguishes mathematics from most other sciences, but we get a better understanding of the scope and character of that freedom by attending to examples such as these than by reflecting on a foundationalist conception of mathematics as the investigation of 'interesting' axiom systems. 'Interesting' makes mathematical decisions sound like exercises of taste, when in fact they are more likely to be exercises of shrewdness (which is not to say that taste plays no role in mathematics). At the outset, I gathered heuristic in the senses of Polya and Lakatos; principles in the sense of Cassirer; ideas in the sense of Lautman and notions in the sense of Grattan-Guinness under the umbrella-term 'enabling structures'. What these structures—and patterns such as Serfati's functional extension—enable is mathematical growth that is free but not random. For philosophers, these patterns enable us to explicate for particular cases the meaning of terms like 'tasteful', 'shrewd' and 'interesting'.

Our first example was Leibniz's appeal to notational symmetry in his discovery of the formula for repeated differentiation of a product. Here too, the philosophy of mathematical practice can claim an advantage over philosophical approaches directed at 'foundational' questions. The thought that mathematics is a science of patterns (to take up the title of Resnik's 1997 book) or that mathematical objects are structures (as argued in Shapiro's work also of 1997) is attractive and plausible to anyone with experience of doing mathematics. However, this plausibility depends in part on cases such as this, in which structure or pattern plays a *heuristic* role. That

[10]Cf. also (Corfield, 1997) and (Corfield, 2003, p. 166). This is not to suggest that axiomatisation is never productive or heuristically valuable. Cf. (Schlimm, 2010).

is to say, the study of mathematical practice can account for some of the philosophical intuitions that guide philosophical enquiries that are remote from the practice-oriented approach.[11] Resnik and Shapiro both turn to mathematical practice to explain how a science of patterns is possible, but only as a supplement to their metaphysical arguments. Having argued for structuralism as a metaphysical thesis, they have to supply a corresponding epistemology. Their question is, how is it possible to make discoveries about structures? By contrast, the Leibniz case prompts us to ask, how do structures (in particular, syntactic structures) help us to make discoveries? Structures can play heuristic roles in empirical science, but in the Leibniz case (and in umbral calculus more generally), syntactic structure plays a role unique to mathematics.

Bibliography

Cajori, F. (1925). Leibniz, the master-builder of mathematical notations. *Isis*, 7(3):412–429.

Cartier, P. (2000). Mathemagics. *Séminaire Lotharingien de Combinatoire*, 44. Article B44d.

Cassirer, E. (1956). *Determinism and Indeterminism in Modern Physics.* Yale University Press, New Haven CT. Translated by Otto Benfrey.

Coe, C. J. (1950). The generalized leibniz formula. *The American Mathematical Monthly*, 57(7):459–466.

Corfield, D. (1997). Assaying Lakatos's philosophy of mathematics. *Studies in History and Philosophy of Science*, 28(1):99–121.

Corfield, D. (2003). *Towards a Philosophy of Real Mathematics.* Cambridge University Press, Cambridge.

Fuss, P. H. (1843). *Correspondance mathématique et physique de quelques célèbres géomètres du XVIIIe siècle.* Académie impériale des Sciences de Saint Petersburg, Saint Petersburg. Quoted from the edition (Fuss, 1968).

Fuss, P. H. (1968). *Correspondance mathématique et physique de quelques célèbres géomètres du XVIIIe siècle*, volume 35 of *The Sources of Sciences.* Johnson Reprint Corporation, New York NY.

Gerhardt, K. I., editor (1863). *Leibnizens Mathematische Schriften.* A. Asher, Berlin.

[11] Colin McLarty (2008) argues a similar point with much greater detail and subtlety.

Gerhardt, K. I., editor (1899). *Der Briefwechsel von G. W. Leibniz mit Mathematikern.* Mayer & Muler, Berlin.

Grattan-Guinness, I. (2008). Solving Wigner's mystery: The reasonable (though perhaps limited) effectiveness of mathematics in the natural sciences. *The Mathematical Intelligencer*, 30(3):7–17.

Lakatos, I. (1976). *Proofs and Refutations: The Logic of Mathematical Discovery.* Cambridge University Press, Cambridge. Edited by John Worrall and Elie Zahar.

Lautman, J., editor (2006). *A. Lautman. Les Mathématiques, les idées et le réel physique.* Vrin, Paris.

McLarty, C. (2008). What structuralism achieves. In Mancosu, P., editor, *The Philosophy of Mathematical Practice*, pages 354–369. Oxford University Press, Oxford.

Penrose, R. (1955). A generalized inverse for matrices. *Proceedings of the Cambridge Philosophical Society*, 51:406–413.

Polya, G. (1954). *Mathematics and plausible reasoning.* Princeton University Press, Princeton NJ. Two Volumes.

Rav, Y. (1999). Why do we prove theorems? *Philosophia Mathematica*, 7(1):5–41.

Resnik, M. D. (1997). *Mathematics as a Science of Patterns.* Clarendon Press, Oxford.

Roman, S. (1984). *The Umbral Calculus.* Academic Press, New York NY.

Schlimm, D. (2010). On the creative role of axiomatics. The discovery of lattices by Schröder, Dedekind, Birkhoff, and others. *Synthese.* to appear.

Serfati, M. (2005). *La Révolution Symbolique: la constitution de l'écriture symbolique mathḿatique.* Éditions Petra, Paris. With a preface by Jacques Bouverasse.

Shapiro, S. (1997). *Philosophy of Mathematics: Structure and Ontology.* Oxford University Press, Oxford.

Stedall, J. (2007). Symbolism, combinations, and visual imagery in the mathematics of Thomas Harriot. *Historia Mathematica*, 34(4).