

For a thicker semiotic description of mathematical practice and structure

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1 Thick description

As this volume is concerned with sociological aspects and mathematical practice in the philosophy of mathematics, it seems fitting to open with a quotation from an anthropologist. “Once human behavior,” explains Clifford Geertz,

is seen as [...] symbolic action—action which, like phonation in speech, pigment in painting, line in writing, or sonance in music, signifies—the question as to whether culture is patterned conduct or frame of mind, or even the two mixed together, loses sense. The thing to ask about [social practices such as] a burlesqued wink or a mock sheep raid is not what their ontological status is. It is the same as that of rocks on the one hand and dreams on the other—they are things of this world. The thing to ask is what their import is: what it is, ridicule or challenge, irony or anger, snobbery or pride, that, in their occurrence and through their agency, is getting said. (Geertz, 1973, p. 10)

One reason for opening with this quotation is that for many contemporary philosophers of mathematics this quotation explicates why mathematical practice is incompatible with the research framework promoted by Geertz. It’s true that mathematical practice is symbolic, and that it is about signifying phonation, lines and gestures. But whether mathematics is a frame of mind, patterned conduct or referenced reality (or the three mixed together)—these questions don’t seem to lose their sense, at least not for contemporary philosophers of mathematics.

A key to why questions concerning mathematical ontology retain a sense, which mainstream anthropology has given up with respect to its own objects of study, is provided by Geertz’s examples: “a burlesqued wink or a mock sheep raid”. In mathematical practice, is there burlesque and mockery? And, if mathematical signs do not risk burlesque or mockery, at least not *inside* mathematics, then perhaps, where mathematical practices are concerned, there’s not much point in “asking what is getting said” in the socio-cultural interpretive sense that Geertz promotes (which is not the

same as the kind of interpretation offered in a maths classroom)? After all, mathematical practices, unlike Geertz's objects of concern, are not supposed to be about "ridicule or challenge, irony or anger, snobbery or pride"; such practices are never dreamt, but stand firm as rocks; and the rock-solid status of such practices within our cultural world is something that a philosopher should venture to explain.

For a sociologist of mathematics, however, the idea that mathematics might be intractable to the kind of analysis that Geertz promotes would sound absurd; mathematics is, with all things said and done, a practice that is social. But what I'd like to do here is show that philosophers too can follow Geertz's lead and gain a great deal (my engagement with contemporary philosophy of mathematics in this paper will be mostly through structuralist philosophy of mathematics). We'll try to see what philosophical insight into mathematical practice we gain, when we practice what Geertz calls (following Ryle) a "thick description", that is, when we describe mathematical practice as "a stratified hierarchy of meaningful structures in terms of which [all sorts of signifying gestures] are produced, perceived and interpreted, and without which they would not [...] in fact exist [as signifying gestures], no matter what anyone did or didn't do" (Geertz, 1973, p. 7). The purpose of this paper is to experiment with such a thick description of a mathematical case study.

But even at this early stage philosophers may raise an objection to one of the premises of thick description: that without "a stratified hierarchy of meaningful structure", social or mathematical practices "would not [...] in fact exist". This statement should not be taken lightly. Much has been said by philosophers of mathematics about meaningless mathematical signs and about mathematical realities that exist independently of their human, interpreted expression. But a thick description needn't a-priori exclude either. What would not exist without meaningful articulation is mathematical practice. Even a formalist's uninterpreted mathematical sign is distinguishable in its use by mathematical practitioners from a random line in the sand, and is meaningfully articulated as an uninterpreted mathematical mark. And whatever, if anything, lies mathematically beyond practice—such being may safely rest beyond the scope of this essay as well.

Sociologists of science may raise an objection as well. They may justly claim that there is no need to 'experiment' with thick descriptions of mathematical practice, as such descriptions have already been successfully produced. One may bring up Livingston's ethnomethodological work on Gödel's proof (Livingston, 1986) and other proof practices (Livingston, 1999); Rosental's study on the work of logicians (Rosental, 2008b) and on university logic teaching (Rosental, 2008a); Netz's work on Greek geometry (Netz, 1999) and my reaction to his work in Wagner (2009a); this list is obviously just

a small sample. These and other texts deal with mathematical writing, institutions, scholars, students, classrooms, publications, errors, competition and more. But it is precisely their ethnomethodological, sociological and cognitive settings that take away the specific edge in which I'm interested here. What's (relatively) special about the approach I take here is the specific slice I carve from the vast range opened by thick descriptions. I will concern myself with the semiotic level of a thick description, which I feel is particularly relevant to the interests of most philosophers of mathematics (but which I don't pretend is more important or more fundamental than other levels of thick descriptions).

2 A case study

The mathematical signs that we shall study are 2-by-2 matrices. These are arrays of four numbers ordered in two columns and two rows, such as

$$\begin{pmatrix} 1 & 2 \\ 3 & 4 \end{pmatrix} \text{ or } \begin{pmatrix} -10 & 0.74 \\ 1/2 & 0 \end{pmatrix}.$$

There are many things that a matrix can be interpreted as standing for. One such object is a parallelogram in a Cartesian plain. Another is a linear motion. For example, the matrix

$$A = \begin{pmatrix} 1/\sqrt{2} & -1/\sqrt{2} \\ 1/\sqrt{2} & 1/\sqrt{2} \end{pmatrix}$$

can stand for the square whose vertices are the origin (0,0), the point $(1/\sqrt{2}, 1/\sqrt{2})$, and the point $(-1/\sqrt{2}, 1/\sqrt{2})$ (the fourth vertex is determined by the three given vertices and the postulation that the shape we're describing is a parallelogram). The same matrix A can also stand for a counterclockwise rotation around the origin by an angle of 45 degrees. Another example, the matrix

$$B = \begin{pmatrix} \sqrt{3}/2 & -1/2 \\ 1/2 & \sqrt{3}/2 \end{pmatrix},$$

can stand for the square whose vertices are the origin (0,0), the point $(\sqrt{3}/2, 1/2)$, and the point $(-1/2, \sqrt{3}/2)$, and also for a counterclockwise rotation around the origin by an angle of 30 degrees.

The rule is easy: to interpret a 2-by-2 matrix as a parallelogram, set (0,0) and the two columns of the matrix as vertices. To interpret a 2-by-2 matrix as a linear motion, consider the product of the matrix and vectors in the Cartesian plane (for convenience, I will only use *positive determinant orthonormal matrices* in my examples, so we shall always end up with squares and rotations, rather than general parallelograms and linear motions).

So far we have polysemy—several references for the same sign. This is indeed a prerequisite for a thick description, which is about a “hierarchy of meaningful structures”. But in itself polysemy is not terribly interesting, not enough for constituting a thick description, and not what I am after. After all, we can interpret any sign as standing for any object, and there’s nothing either thick or thin about describing this fact. That a signifier¹ has several interpretations is less than interesting in this context, unless these interpretations interrelate.

In order to get to the point of interrelation let’s consider matrix products. Matrix product is an operation that takes two matrices, and yields another matrix. It’s a little more complicated than multiplying the matrices term by term. The product of matrices

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix} \text{ and } \begin{pmatrix} v & w \\ x & y \end{pmatrix}$$

is defined as

$$\begin{pmatrix} av + bx & aw + by \\ cv + dx & cw + dy \end{pmatrix}.$$

Multiplying our above A and B , for example, we get

$$A \cdot B = \begin{pmatrix} \frac{\sqrt{3}-1}{2\sqrt{2}} & \frac{-\sqrt{3}-1}{2\sqrt{2}} \\ \frac{\sqrt{3}+1}{2\sqrt{2}} & \frac{\sqrt{3}-1}{2\sqrt{2}} \end{pmatrix}.$$

What’s important for us about matrix multiplication is that if X is a matrix that stands for a square, and Y is a matrix that stands for a rotation, then $Y \cdot X$ stands for the square you’d get by applying the rotation represented by Y to the square represented by X . So the product $A \cdot B$ above is a matrix that stands for the square you’d get by applying a 45 degrees counterclockwise rotation to the square represented by B .

At this point our two interpretations (square and rotation) relate to each other and interact. Mathematical practitioners interpret signs in different ways, and compose these interpretations. One can introduce many such compositions. For example, on top of the above rotation-applied-to-square interpretation of products, one can interpret the product of matrices as a composition of the rotations they represent. The product of a matrix that stands for a 45 degrees rotation and a matrix that stands for a 30 degrees rotation will then stand for the combined 75 degrees rotation. Both these interpretation are useful in practice.

One may of course come up with many different competing interpretations, but so far, the term ‘competing’ does not appear to be justified. So

¹I am using here the structuralist terminology of the tradition attributed to de Saussure (1966).

far the interpretations that we presented coexist; each is viable in itself. They do not penetrate each other, do not affect each other, and are mutually neutral. Interpretations here do not have the descriptive thickness suggested by Geertz's term "hierarchy". More describing is needed to get there.

So let's look at one more thing that we can do with matrices: raising them to the second power. The formula is:

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix}^2 = \begin{pmatrix} a^2 + bc & ab + bd \\ ca + dc & cb + d^2 \end{pmatrix}.$$

We can think of this operation as a function that takes a matrix standing for a square, and yields another matrix standing for another square. We can check and verify that the angle between the resulting square and the x -axis is twice the angle between the original square and the x -axis.

So far we're just adding more operations and interpreting them in coherent ways. But what happens when we put things together? For instance, what happens if we note the simple fact that $X^2 = X \cdot X$? Here things start to turn interesting. On the left hand side, our interpretation has only to do with matrices as squares. But when we apply our previous interpretation to the right hand side, we are forced to take the sign X , which on the left hand side stood alone with a single interpretation, and impose upon it another interpretation: that of a rotation.

The single left hand side X splits into two at one and the same time. Here we don't just have polysemy. By setting this equality *and* retaining our previous interpretations we force a *shift of meaning*: a matrix designating a square suddenly designates a rotation, because it happened to be raised to the second power and set in a formula. A formal manipulation, combined with inherited interpretations, forced a shift of meaning: from one sign and one interpretation, we turn to a reiterated² sign and two interpretations. We can no longer think of the left-hand X as *only* standing for a square, as we could before, because the right hand side and the connecting equality force on X our second interpretation. The left hand interpretation was contaminated by the excess meaning in the right hand interpretation. One interpretation was forced on another following a formal identity.

But we have to qualify in what way this shift of meaning is *forced*. Of course, we need't have acknowledged any of the interpretations I have suggested above. One can do matrix algebra with many other interpretations, including an interpretation that views matrices just as arrays of four numbers. However, here we're dealing with mathematical practice, and *in practice* we interpret. Furthermore, mathematics is useful and interesting

²For a deep analysis of reiteration as constitutive of signs see (Derrida, 1988); but my use of this term at this point is much more limited in its scope.

because it is interpreted. And in saying that, I am not only referring to interpretation for the purpose of application, but also to interpretation for the purpose of generating mathematical conjectures and proofs. I know no mathematician, who never interprets her or his symbols, when thinking about mathematical problems. I know no mathematician, who sticks to just one interpretation at a time.

Whenever one has an isomorphism, one has (at least) a double interpretation: one in terms of the domain of the isomorphism, and one in terms of its range. My point is that mathematical interpretations can and do bump against each other to force shifts of meaning as above. Going from the left hand side to the right hand side of an equality, a sign may change its interpretation. And this holds not only for specific signs, but for entire domains of knowledge as well. Analytic geometry, for example, is not simply two independent ways of thinking put together—geometric and algebraic. It is a novel geometrico-algebraic way of thinking, which is historically distinct, and not reducible to a disjoint union between classical geometry and algebra based on their intermittent use to interpret signs.³

But I want to make the plot even thicker. I want to show how interpretations strike limits. For that purpose, we shall introduce one more matrix operation: transposition, denoted by a superscript T. Its definition is

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix}^T = \begin{pmatrix} a & c \\ b & d \end{pmatrix}.$$

This operation is easy to interpret in terms of both squares and rotations. If a matrix stands for a square, its transpose stands for the square obtained by reflecting the sides of the original square across the main axes. If a matrix stands for a rotation, its transpose stands for the same rotation in the opposite direction.

Now there's an easy theorem stating that $(X \cdot Y)^T = Y^T \cdot X^T$. Let's try to read this theorem in the terms of our interpretations above. If we think of Y as a square and of X as a rotation, then the left hand side makes sense (rotate the square and then reflect its sides), but the right hand side involves multiplying a square to the left of a rotation, which is not something we've considered so far (recall that in general matrix multiplication is not commutative, so we can't just switch the matrices around).⁴ If, on the other

³A modern example for this kind of effect (an arbitrary example among unboundedly many, included here just to give a flavour for how to generalise my claims) is the Gelfand representation of elements of commutative C^* -algebras as functions operating on the algebra's maximal ideals. This isomorphism or reinterpretation is much more than two distinct interpretations of the same signs; it is a whole that's greater than the sum of its parts.

⁴Actually, the product of positive determinant orthonormal 2-by-2 matrices is com-

hand, we read X as a square and Y as a rotation, then the left hand side no longer makes sense.

One way to deal with the issue is to offer another interpretation for transposition. For instance, we can stipulate that if X is read as a square, we should read X^T as a rotation, and vice versa. Then, if we decide that X is a rotation and Y is a square, the left-hand side product XY yields a rotated square, while on the right-hand side of the equality, Y^T , a rotation, stands to the left of X^T , a square, and we can maintain the same interpretation to yield, again, a rotated square. Both sides now make sense. But given this interpretation, the equality between the two sides no longer makes sense. Indeed, on the left hand side, we have a transposed rotated square, which according to our interpretation of transposition must stand for a rotation. But the right hand side is simply the application of a rotation to a square, and is, according to our interpretation, a square. We obtain a situation where a rotation on the left equals a square on the right, which is likely to appear more objectionable than the simple claim that a single matrix can represent either.

Now, when confronted with this kind of interpretive dead-end, we can react in various ways. One reaction is to seek other interpretations for transposition and multiplication that work coherently together, and at the same time allow us to retain a sense of rotating squares for application purposes. This would be a reconstruction of interpretations. Another reaction is to keep using our interpretation *locally*, that is to change our interpretations of multiplication and transposition as we go along, even if it means that X or transposition is interpreted in more than one way across a single line of equality. One can refer to this as superposing interpretations. Another strategy is to set interpretations (in terms of squares or rotations) aside for a while, and bring them up only in specific locations, where they are actually useful. This might be called deferring interpretation. All these approaches have productive roles in contemporary mathematical practice. Mathematics is practiced through and across reconstructions, superpositions and deferrals of interpretations. These processes never come to an end, because formal manipulations never come to an end. Things only get more and more involved, and the example above only traces a few initial steps in a rhizomatic⁵ web of semiosis.

There's yet another strategy, of course, more respectably philosophical: to seek an all consuming ontological grounding or logical reconstruction. But this brings us back to Geertz's initial quotation. Mathematics does not

mutative, but if we extend our reading to general parallelograms and linear motions commutativity is lost.

⁵For a discussion of the rhizome as a model of dynamic structural relations with unstable hierarchies, see (Deleuze and Guattari, 1987).

require a global grounding any more than social phenomena do. Society and mathematics work across, against and in conflict with locally reconstructed, superposed and deferred interpretations. That mathematical marks are there is no more questionable than the fact that winks, either burlesque or ‘serious’ are there, no more questionable than the existence of rocks and dreams.

Indeed, I would like to emphasise how thoroughly local a local interpretation can be. Here’s an example. Suppose we want to decompose the matrix product $(I - A)^{-1}(I - 2A)^{-1}$ as $a(I - A)^{-1} + b(I - 2A)^{-1}$, where a and b are numbers, I is the identity matrix, and the superscript $^{-1}$ stands for matrix inversion. Suppose that we already know that the decomposition is possible, and that the values of a and b are independent of the matrix A . One of the most straightforward ways to obtain a and b is to multiply both terms by $(I - A)$ and $(I - 2A)$, and equate them. We obtain

$$a(I - 2A) + b(I - A) = I.$$

Next we substitute I and $I/2$ for A . We then get the equations $-aI = I$ and $bI/2 = I$, namely $a = -1$ and $b = 2$.

The point here is to note that the manoeuvre we used to obtain the decomposition is dodgy. In particular, we substituted for A a value, which renders the initial expression (the product of inverses) undefined. Nevertheless, this manipulation is taught and practiced widely. Experienced practitioners will have noted the problem at one time or another, and can easily come up with a rigorous justification (e.g., substituting $(1 + \varepsilon)I$ for A and letting ε go to zero, or extending the matrix algebra to a consistent framework that sets rules for the acceptability of such a manoeuvre). But this doesn’t mean that they will let go of the dodgy manoeuvre or refrain from reproducing it in class for their students, with or without an explicit acknowledgement of its dodgy bits.

This example demonstrates how a local interpretation can involve a local logic, which is at odds with formal rigour. Reducing such local logics to errors or shorthand for rigorous practices is a gross misunderstanding of mathematical practice. The reproduction of such practices depends on authority and local knowledge, rather than on a reterritorialisation into a global logical framework. It is true that such practices can and do lead to inconsistencies, but avoiding such inconsistencies depends on experience, rules of thumb and gut feelings (I witnessed the use of all these expressions in similar contexts by prominent practicing mathematicians) no less than on rigorous reconstructions. And no philosophy of mathematics should repress these thick descriptive facts. Latour instructed us to follow the scientists wherever they go. Since the approach of this essay is semiotic, I suggest here that we obey the slightly different directive of following signs wherever they

go. And following signs does not require grounding them in an ontology or a logic—but more on that in the next section.

Before we continue with the argument of this paper, we must acknowledge the shortcomings of the above discussion with respect to our promise to experiment with a thick description of a mathematical case study. In my quotations from Geertz above I suppressed under square brackets, ellipsis and a premature quotation mark Geertz's reference to fraud, parody and rehearsal. This suppression was a strategic device. Suppressing forgery, parody and rehearsal made it easier to graft the anthropologist's notion of thick description onto a realm where it might seem foreign. But once the grafting is performed, it becomes easier to force onto the surface of mathematical practice that which we've been suppressing in our strategic formulation. My purpose is not merely to analyse an existing object (mathematical practice) with a given technique (thick description) but to synthesise them into something that can grow in somewhat less traditional directions.

So let's see if a mathematical sign can express its meaning fraudulently, parodically or in way of a rehearsal. This question has an easy positive answer, if we consider the social framework. Teachers openly 'cheat' in classrooms (misusing signs, practicing invalid manipulations, cutting corners), as do professionals, more or less cautiously, in more or less formal communications. Ideally, all these 'frauds' are correctable, and perhaps merit the title 'white lies' rather than frauds, but cases of scholars making unsubstantiated false claims to gain prestige are not unheard of. As for rehearsals—students rehearse mathematical performances when they do exercises, so that they'll get it right when they're demanded to perform mathematics in exams or in 'real life'. And it is also the case that our academic culture is replete with parodic expressions (algebraic abstractions that are perceived as producing no added value are a target for parody, as are tedious variations on well known themes).

But I stated above that I am interested in the semiotic slice of a thick description, and I should therefore look for fraud, parody or rehearsal at the semiotic level of textual interpretation, not at the level of social context. Indeed, suppose I make algebraic manipulations of matrices pretending that they were real numbers, then check to verify the end result, and finally go back to adapt the original derivation to the non-commutative ring of matrices with its zero divisors. Was my first step some sort of rehearsal? When I follow a geometric argument by thinking of a specific example, or verify it by means of tracing a concrete diagram, am I not fraudulently exchanging the specific for the general? And when Russell introduced his antinomy into Frege's notion of set, did he not follow Frege's own rules and way of working, bringing them to an extreme that exposed an unsubstantiated pretense in Frege's conduct—in other words, performed a parodying mimicry of Frege's practice?

One can come up with good objections to the use of the terms ‘fraud’, ‘rehearsal’ and ‘parody’ for these examples. Indeed, a premature wholesale endorsement of such terms for these examples would underplay the unique features of mathematical practice. A finer reading of the above examples, which will describe them in more accurate terms, may indeed be philosophically interesting, but I will not pursue it here. My point here is simply that there’s enough richness to mathematical practice, even at the semiotic level, that justifies grafting the thick description framework onto it. This graft will undoubtedly require some adaptations, but could provide an interesting and challenging analytic framework, the main advantage of which would be its insistence on including aspects that do not fit into prevalent logical or ontological regulative ideas. After all, as I will argue below, a mathematics that’s not re-interpreted is a mathematics that’s more likely to become irrelevant and outdated.

Finally, inclusion and regulative ideas bring us to the last component of thick descriptions that will concern us here. I am referring here to the “hierarchy” in Geertz’s “hierarchy of interpretations”. Interpretations are not free floating products of unconstrained human thought. Interpretations are discursive productions, invented, reformed, taught and reproduced. As such, they depend on what Foucault calls a “principle of rarity”. This principle reminds us that “on the basis of the grammar and of the wealth of vocabulary available at a given period [that allow unboundedly many combinations], there are, in total, relatively few things that are said”, and that the discourse formed by these relatively few actually said statements therefore

appears as an asset—finite, limited, desirable, useful—that has its own rules of appearance, but also its own conditions of appropriation and operation; an asset that consequently, from the moment of its existence (and not only in its “practical applications”), poses the question of power; an asset that is, by nature, the object of a struggle, a political struggle. (Foucault, 1972, pp. 19–20)

In the philosophical and public discourses about mathematics some interpretations are reproduced more than others. The local, unstable, perpetually deferred aspects of mathematical sign interpretations are often suppressed. They are assigned hardly any philosophical importance or consequence. They are banished to the domain of mathematicians’ colloquial practices, which supposedly do not reflect the respectable image of mathematics, Galileo’s “language of mathematics”, the language of “this grand book—the universe” (Galilei, 1623, pp. 123–123). The grand global narratives (not without grand problems themselves) of set theory, category theory, model and modal reconstructions and foundational ontologies are those that gain the upper hand in the discursive struggle (which, according

to Foucault, would be a “political struggle”) over philosophical and public representations of mathematics.

I do acknowledge that most foundational theories are interesting and productive, and can, to an extent, serve as interpretations of mathematical practice. As such they are no less worthy than any other local interpretation, each with its various practical roles. But when the debate focuses, as it often does, on which *one* of these theories is the universally fundamental one, rather than on how they interact with each other and with more local interpretations, one ends up with a problematic image of mathematics that has everything to do with protecting its uniquely authoritative place in contemporary politics of knowledge. This leads to a tension that’s often felt between the promise of foundational interpretations and the disturbingly incongruous and conflicting results of mathematical statistics and economics, or the spin into ‘epicycles upon epicycles’ in natural science models. The fault for these tensions is never perceived to lie with mathematics. And indeed it shouldn’t. Because mathematics is a web of local interpretations, and cannot support the weight of such a “grand book” as “the universe”. What is at fault here are the great expectations projected onto mathematics by holding on to a single grand, and excruciatingly thin, description of mathematics.⁶

3 Structure

In order to critically evaluate the argument above concerning local interpretations and shifts of meaning, let us try to present it again with respect to a more elementary case study. Let’s consider not matrices, squares and rotations, but integers, rows of oranges and repetitions.

A positive integer x can be interpreted in many ways. For example, a positive integer x can stand for a row of oranges in 1-1 correspondence with the elements of $\{1, 2, \dots, x\}$. An integer x can also stand for the application of repetitions in 1-1 correspondence with the elements of $\{1, 2, \dots, x\}$. We

⁶One may object that the thick description framework advocated here is just as foundational and universal as the classical approaches, in that it claims that a hierarchy of interpretations along the lines presented above is universally applicable to mathematics. But a thick description is a methodology, which, when applied to different circumstances, draws very different pictures. I do believe that mathematics is always practiced with reconstructions, superpositions or deferrals of interpretations, but this is not a claim I seek to demonstrate. When I find such phenomena in different mathematical case studies and different historical periods (Gödel’s proof, contemporary graph theory and algebraic combinatorics, classical Greek geometry and my forthcoming work on Renaissance algebra) I find them expressed so differently that even the theoretical resources I use keep changing (Wagner, 2008, 2009b,c,a). Still different expressions of such phenomena are the *multiplexity* of Dynkin diagrams studied in Lefebvre (2002) and the “useful ambiguity” that Grosholz (2007) finds in the work of Leibniz. There is something universalising in the decree to interpret thickly, but this does not impose a single language or system of interpretation.

can then interpret the product $x \cdot y$ as x repetitions (set side by side) of the row of y oranges. The x is interpreted as repetitions, the y as a row of oranges, and the product is an integer that's also interpreted as a row of oranges. Raising to the second power can also be easily interpreted: if x is interpreted as a row of oranges, we can interpret x^2 as what we'd get by completing the given row of oranges to form a square.

Now, when we observe an equality of the form $3^2 = 3 \cdot 3$, we find again two of the phenomena encountered above. First, there's a shift of meaning. The term 3, a single sign interpreted on the left-hand side strictly as a row of oranges, is forced, by way of its formal plugging into an equality, to become two different things on the right-hand side: a row of oranges and a bunch of repetitions. A formal manipulation, coupled with the retention of our earlier interpretation, forced a sign to split into two signs with two different interpretations. Moreover, while on the left hand side we have a square of oranges, on the right hand side we have a row of oranges, which our retained interpretations force around the sign of equality, that is, posit as one and the same.

Now that we're in the philosopher of mathematics' favoured domain—arithmetic and the integers—the philosopher's response springs up much more clearly and emphatically. x should not be interpreted either as oranges or repetitions. x is, and should be interpreted as, number! And if we carry the same logic back to the previous section, it is obvious that our matrices should not be interpreted either as squares or as rotations. Matrices should be interpreted as that mathematical object which matrices are. Of course, we can apply mathematical knowledge to oranges, repetitions, squares and rotations—but that's at the level of application. As long as we're doing mathematics, a number should be a number and a matrix should be a matrix.

Suppose we allow this marginalisation of what practitioners of mathematics actually do. We are then left with the question: what are numbers and matrices as mathematical objects? This question catapults us back into the debate of ontologies and epistemologies of mathematics. In order not to go again over the already infinite and outmoded debate of realism-intuitionism-formalism, I suggest we take a detour through the more recent ontologico-epistemological approaches grouped under the term *structuralism*, where some relatively new interesting remarks can still be made.⁷

The common point for structuralists is that they're interested not in individual mathematical objects, but in their relational aspects. This, how-

⁷Not that this would be too much of a detour. A quick glance at contemporary forms of structuralism shows that, like the formalist-intuitionist-realist divide, many of them carry the traces of mediaeval nominalist-conceptualist-realist positions on the problem of universals (I do not mean this as a reproach, but as the expression of an important positive link with the history of philosophy).

ever, can be done in various different ways. One can hold a *methodological structuralist* position, according to which mathematics is concerned with what follows from axiomatically postulated relations, and set aside the entire question of objects. There's the *set theoretical structuralist* position, according to which mathematics deals with what's common to models of axiomatic theories built inside set theory, but defers the question of what sets precisely are, or solves this question non-structurally. There's the *ante-rem structuralist*, for whom structures are abstractions of mathematical models, whose elements are 'objects' that have only relational properties (or at least only relational essential properties, as one can't avoid accidental properties such as 4 being the number of legs that Lassie—or the dogs portraying her in the film – had). And then there are *in re structuralists*, who read mathematical statements not as referring to any specific object, but as quantified over all, or all possible, models of a given structure.

This division above is borrowed from Reck and Price (2000), and is not meant to be exhaustive or authoritative. Variants of these divisions may be found in (Hellman, 2001) and in (Shapiro, 1997), and one should quote at least Resnik (1997), Chihara (2004) and the nice reconstruction of Dedekind's view in Reck (2003)—if not for completeness, then for their interesting highlights. Of course, there's a lively debate concerning the pros and cons of the various structural approaches. These tend to revolve around issues of ontological commitments, uniqueness, individuation, the range of quantifiers and possibilities and the elimination of 'monsters'. Whether or not these problems are essentially shared by all structural approaches (as suggested by Shapiro) or are differently distributed between them (as claimed by Hellman) need not detain us here. Instead, I'd like to look, again, at our case study, in order to point out something that structural approaches tend to leave behind.

So let's go back to our thick description of matrices. As they share the same signs, one could argue, matrices interpreted as squares or as rotations share the same structure.⁸ Moreover, shifts of meaning and conflicts of interpretations are set aside if we defer interpretation while doing mathematics, and focus on mathematical structures instead. But the interpretations of matrices as rotations and matrices as squares still leave their trace when abstracting to structures. Our interpretation of matrices as rotations portrayed rotation-matrices as operating on squares, so the structure under hand was conceived of as a set of functions operating on another structure. The interpretation of matrices as squares, on the other hand, did not share this structural feature. The two interpretations are not, after all, fully structurally equivalent.

⁸To be precise, one should either restrict to positive determinant orthonormal matrices, or generalise to oriented parallelograms and linear motions, but I'd rather not encumber the argument with this excess terminology.

Now, when considering a given structure, one can always add supplementary relations. For example, compare the integers as a structure satisfying the Peano axioms, and the same integers with the addition of the ‘bigger than’ relation. Whether we should see these structures as identical, embedded or unrelated is debated among structuralists, and might be undecidable, at least in some versions of mathematical structuralism. If we considered matrices-as-rotations to be the same structure as matrices-as-squares with an additional group operation (matrix product), then we’d be in a similar situation. But that’s not the case in the example above.

Our interpretation conceives of rotations as functions over squares. The set-theoretic definition of a function, which involves a set of ordered pairs of domain and range elements, means that when we go from squares to rotations we are not concerned with taking a structure and supplementing it with an extra relation. You can’t take just any set and ‘add’ to its members the ‘property’ of being functions, as you would add the ‘bigger than’ relation to a model of the integers.

There’s also a difficulty in constructing a single model for both rotations and squares, despite their isomorphism. The canonical set theoretical aversion to sets that are members of themselves prevents the construction of functions that take themselves as arguments. So, if we interpret matrices as rotations operating on squares, we can’t simultaneously use the same model both for the rotations and for the squares, even though a single model can model both squares and rotations operating on some other model of squares. What I mean is that we can construct models A , B and C , where A is a model of matrices as squares, B is a model of matrices as rotations operating on the model A (and therefore itself another model of squares), and C a model of matrices as rotations operating on the model B . So the pair (A, B) can model our square-rotations combination, and so can the pair (B, C) . But strangely enough, even though the set B can model both squares and rotations, the pair (B, B) can’t model rotations operating on squares. From a structural point of view, once a structure starts operating on one of its isomorphs, their previous isomorphism may break into a non-reversible hierarchy. And—here is the crucial point—whether a structure will or will not operate on an isomorph—that is something we needn’t decide in advance.

Of course, if we use a set theory that allows functions to operate on themselves, or if our set theory uses ur-elements that can be reconstructed post-hoc as sets of ordered pairs, or if we describe the operation of rotations on squares as a binary operation rather than as a functional relation, the problems above might go away. But I believe that a good philosophy of mathematics should allow us to use (but not confine us to) canonical set theory, and should not deny us thinking of rotations as functions sending

squares to squares. And of course, even if the specific problem described above can be avoided by some formal reconstruction, other cases of signs standing for themselves and an excess are likely to surface elsewhere.

My claim is that even the structural interpretation of mathematical signs may be deferred, reconstructed and superposed. Mathematics does not have to have foundations. One can spend a lifetime using matrix algebra for rotating squares without ever having to make decisions concerning splitting the structure of matrices into two structures, of which one operates on the other as functions. This splitting of a structure into two structures, 'active' and 'passive' clones, and the decision whether they are the same or not, can sometimes be a pathology of formal reconstruction, rather than part and parcel of mathematical practice.

Now, I have no intention of denying that some sorts of structuralism in mathematics have contributed to mathematical development (e.g., Dedekind, Noether, Bourbaki). I also do not disqualify structuralism as a philosophical interpretation of mathematics; indeed, structuralism is an important catalyst of interactions between various mathematical interpretations. Furthermore, there are mathematical practices that require the careful articulation of function and domain structures that an analysis of the above example would demand (set theory is, after all, part of mathematics!). But my point is that such articulations need not be made once and for all or in advance. When interpreting matrices, we can consider matrices-as-rotations and matrices-as-squares as the same, as isomorphic, or as essentially hierarchical. And we may also defer or superpose these decisions, or reconstruct them in hindsight (and the same goes for integers, rows of oranges and repetitions). The self-identity of a mathematical object or model is not all that rigid.⁹ This is a kind of thickness that structuralism, as well as other universalising ontological-epistemological systems, fail to describe. This is not reason enough to flush structuralism; but this is good reason not to let it monopolise the field.

What underlies the discussion above, and what makes structuralism miss that which a thick description grasps, is the way mathematical structuralists diagnose semiotic problems. To make this point, let's go from contemporary mathematical structuralism to 20th century continental linguistic structuralism. I acknowledge the differences in standards of articulation, which might render even the most rigorous formulations of structural linguists offered by de Saussure, Martinet, Hjelmslev, Jakobson, Trubotskoy or Benveniste less than satisfactory for some contemporary philosophers of

⁹The difference and mimesis based philosophies presented in such work as Deleuze (1994) and Derrida (1993), which problematise relations of identity and repetition, enable critical thinking that is not committed to Aristotle's first law of logic ($A = A$). The above argument is, in fact, a restricted and elementary application of Deleuze's interpretation of *eternal return* and/or Derrida's *différance* to mathematical signs.

mathematics. I further suspect that Deleuze's highly philosophical reconstruction of structuralism (Deleuze, 2004), or its post-structural reformulation under the title of "virtual" reality (Deleuze, 1994) would fare even worse. But I think that mathematical structuralists do have something to learn from the above scholars of humanities.¹⁰

A problem posed by mathematical structuralists since Benacerraf (1983) is that of 'too many' models, each with its own annoying contingencies. These should either be replaced by an 'ur-model', or by a logical reconstruction of whatever's common to them all. But this was not the problem of continental linguistic structuralists. Their problem was not that a language such as French had too many models, which they had to unify. Their problem was that no individuated model of an entire language could ever be made present. Linguistic structure

is a fund accumulated by the members of the community through the practice of speech, a grammatical system existing potentially in every brain, or more exactly in the brains of a group of individuals; for the language is never complete in any single individual, but exists perfectly only in the collectivity. (de Saussure, 1966, p. 13)

And it is the structural linguist's task to derive the structure from these local models that are never complete.

But the linguistic structuralists' problem was not simply a problem of patching together relatively consistent partial models. The different partial models may be in conflict. If one considers all the different ways of expressing a given linguistic phoneme, one gets an unbounded and fuzzy realm of vocal manifestations. In strict physicalist terms, no two expressions of a phoneme are ever the same; on the other hand, trying to define a phoneme in terms of limits on the range of relevant physical properties (the physical motions of the mouth and larynx, measurements of sound waves) crashed against grey areas of intersection that were too large to ignore (e.g., de Saussure, 1966, p. 106). But not only the distinctions between what's reconstructed as different phonemes prove problematic; it also made systematic sense to reconstruct phonetic elements that had no vocal expression at all! (e.g., the presentation of such a manoeuvre by de Saussure in Hjelmslev, 1966, p. 36). As de Saussure put it,

speech sounds are first and foremost entities which are contrastive, relative and negative [...] *In the language itself there are only differences* [...] although in general a difference presupposes positive terms

¹⁰ An anonymous reviewer suggested that in the Bourbaki group such learning did take place. Without neglecting the differences between earlier mathematical structuralists and contemporary structuralist philosophers of mathematics, this would make an interesting topic for a historical survey, which would hopefully be valuable to contemporary philosophy of mathematics.

between which the difference holds, in a language there are only differences, *and no positive terms* [...] the language includes neither ideas nor sounds existing prior to the linguistic system, but only conceptual and phonetic differences arising out of that system. (de Saussure, 1966, pp. 117–118)

And nowhere does this force us to hypothesise a consistent and exhaustive model or modal interpretation of the system, however ideal.

This is not Benacerraf's conundrum of canonizing one model from among many candidates. This is a problem of extracting aspects that are common to many incomplete and relatively conflicted forms of expression. My point here is not to advocate linguistic structuralism as a philosophy of mathematics (it should be quite obvious by now that I am a follower of some of the reactions to structuralism and its rearticulations subsumed under the title of post-structuralism). My point is that the problem of mathematical philosophy might not be that of unifying too many models, but that of coping with the lack thereof.

The position I'm putting here is reflected by Wittgenstein. For Wittgenstein mathematical statements are never simply empirical descriptions of states of affairs. Indeed, descriptions of states of affairs depend on more or less accurate measurements. As Wittgenstein puts it,

"what reality corresponds to the proposition that if you turn a match twice through 180° it gets back to its original position?" If this is a geometrical proposition, the reality which corresponds is: if we use a good protractor, then normally it brings us back, or more nearly back the better it is (where "better" is determined by other criteria). (Wittgenstein, *Lectures on the Foundations of Mathematics*: Diamond, 1975, p. 246)

Realities corresponding to mathematical statements are always about "normally", "nearly" and "better". But unlike their corresponding realities or states of affairs, mathematical statements themselves set standards. That the measurement and implementation tools which model the standard (the match and protractor above) are never quite up to the standard—this does not quite undermine the standard as such. Indeed, Wittgenstein asks

If [after rotating a match twice 180°] it didn't point in the same direction, would you say the protractor was wrong, that it had expanded, etc.,—or would you say that in this case twice 180° does not bring you back to the same position? (Diamond, 1975, pp. 245–246)

At least there and then, Wittgenstein opts for the former response. The statement according to which two 180° rotations bring us back to the same position is a standard according to which we calibrate our instruments. No practice of measurement or calculation can *model* this standard without

risk of failure. What's most relevant here in this (obviously very sketchy) allusion to Wittgenstein is that, as with continental linguistic structuralism, we do not deal here with what's common to 'too many' good models; mathematics here is a reconstruction related to partial, substandard and interpretation-laden practices.

The breakthrough of the continental linguistic structuralist approach was to understand that a scientific system of rules or differences can be valuable even if it sets an impossible standard, and even if no object, real or ideal, in fact lives up to standard. This holds for mathematical systems as well. They depend for their practice on bits and pieces of substandard formal manipulations and on local, deferred, superposed and reconstructed semantic interpretations. Philosophy of mathematics should affirm this reality and weigh its consequences, rather than restrict itself to attempts at putting together new ideal ur-models or logical interpretations that can stand as referents or senses for mathematical signs. The latter activity can indeed be valuable, but it should be complemented by thick descriptions of mathematical practice, and should not presume to dominate or suppress such descriptions as philosophically inferior.

4 Deferring interpretations

I'd like to conclude with some clarification concerning the concept of interpretation that I have been discussing, and how it necessarily thickens the description. I have insisted that in mathematical practice interpretations of signs are subject to local practices of deferral, reconstruction and superposition. A quick look into the term 'superposition' will help us clarify what interpretation is like.

I borrow the term superposition from physics. This borrowing is extremely loose, and its point is not so much to force an analogy as to learn from its limitations. When certain aspects of an object cannot be reduced to a single state (say, a specific location, or a specific spin), we say that the object is in a superposition of states. But to establish superposition it is not enough that we simply don't know the object's state. It is required that we have some testimony to its plurality of states through observed phenomena (such as interference), and that we can learn something about the distribution of those states by measurement. Interference testifies to an object's plurality of states by making it hard to explain the interaction of objects under the hypothesis that each has only one (possibly unknown) state. To explain the interaction one needs (or finds it useful) to assume that the plurality of states of each object influences the results of the interaction. The double-slit experiment is the obvious paradigm.

Can we think of a mathematical sign as being in a superposition of various interpretations, and point out moments of interference? Consider

the theorem $(X \cdot Y)^T = Y^T \cdot X^T$ concerning products of matrices, and our interpretation of the product of 2-by-2 matrices as rotations applied to squares. We've already noted that, if the left-hand factor of the product should be interpreted as a rotation and the right hand side as a square, then only one side of the equality can make sense: the left-hand side of the equality if X is the motion, the right hand side if Y is the motion. In order to maintain both the equality and our interpretation, we might want to allow X and Y to be in a superposition of rotation and square interpretations, and consider the equality as an indication of interference.

Of course, one can conclude that this problem rules out the interpretation of matrix product as rotation applied to square. But that would be an overkill, and annoying news for people who use this interpretation in technical applications of geometric linear algebra. But I'm not going to defend this notion of interference of interpretations against such overkill, as my point is not to press the analogy between superposition of physical states and the superposition of interpretations. I am more interested in following the concept of measurement as it goes through the 'superposition of interpretations' metaphor.

In the quantum context, measurement is a process that collapses a superposition of states to a single state. In the context of interpreting signs we may think of committing a sign to a single interpretation as collapse. From a superposition of interpretations we collapse the meaning of the mathematical sign onto a single sense and referent. We may have played with our matrices and their possible meanings, we may have put them through various formal manipulations that effected shifts of meanings, but finally comes the moment of decision. Here, in this application of the bottom line of our reasoning, this or that specific matrix must finally be used in a solid, well-defined particular way.

But is such a collapse necessarily definitive? The obvious objection is our ability to go back to the mathematical text and interpret or use it differently. When a text has been interpreted, it has never been interpreted once and for all; it can always be re-interpreted, and that goes for a mathematical text as well—"a written sign carries with it a force that breaks with its context" (Derrida, 1988, p. 8).

This objection, however, is perhaps too easy. Can't we say at least that at the moment of interpretation or application there is a definitive collapse into a single interpretation? Well, suppose a programmer used a certain matrix to designate a certain square, which will be displayed and rotated on the screen. Has interpretation come to an end? Not quite. The code is to be processed by a certain compiler, then executed on a certain machine, and eventually output onto a certain medium. Anyone experienced with the endless machine specific variations that can ensue is well aware

that interpretations have not yet come to an end. And when we finally observe the square rotate on some LCD screen, have interpretation now finally come to an end? They have not, at least as long as someone is there to observe the rotating square and interpret its motions: experience them aesthetically, derive information from the display, act on whatever the rotating square prompts them to do, etc. And things needn't end there. This experience may be remembered, recalled, evaluated, recounted, recontextualised; it may instruct us, reproduce itself in future experiences, enter chains of interpretive expectations and habits; in short, interpretations, like explanations, need never come to an end. They might factually come to an end, but they never need end at any given present moment of time.

But the above example takes our interpretation outside mathematics. Is it not the case that as long as we stay inside mathematics, interpretations must eventually come to an end? In fact, it is never finally decided when an interpretation carries a sign outside mathematics. When I interpret a matrix as a square, is the square no longer mathematical? It depends. A square may well be an empirical object of observation. But it may just as well be a mathematical object.

Mathematical propositions might quite well be expressed in terms of people, houses, or what not. The word "men" may come in and it may still be mathematics; and the word "lines" may come in and it may not be mathematics. (Diamond, 1975, p. 116)

For Wittgenstein, whether the square (or men, or a line) is inside or outside mathematics depends on how we operate with it. As we observed above, if our dealings with the square set standards (say, if we state that the square's diagonal divides it into two congruent parts, regardless of what empirical measurements suggest), then the square is still mathematical. If our dealings with the square are more empirical or pragmatic, then, according to Wittgenstein, we're no longer inside mathematics. The catch is that "of course the sentence 'The figure I have drawn here [...]' may be used either mathematically or non-mathematically" (Diamond, 1975, p. 117). There's nothing in the square itself that forces us to use it either way, that forces us in or out of mathematics. Our interpretation may oscillate in and out of mathematics in ways that might question the topology of these in/out relations.

Unlike the apparently once-and-for-all quantum collapse, mathematical interpretations do not mark a clear boundary of final interpretation or a way out of mathematics. Furthermore, unlike quantum superpositions, which superpose a well defined space of once-and-for-all given states, mathematical interpretation does not take place in a closed and well articulated domain. Mathematical reconstruction of interpretation is an open ended process.

This open-endedness is not the margin of mathematics. It is its decentred centre—a condition of possibility that is not reducible to a stable core. Even if mathematics is grasped as geared not towards practice, but towards some ideality, a mathematics that cannot be reinterpreted is a mathematics that is bound to become outdated, once travelling—historically, culturally, intersubjectively—across our life worlds renders a given interpretation obsolete (a glimpse at the kind of algebraic geometry that dominated 19th century professional mathematical literature will provide a fine instance of this claim).¹¹

Reducing mathematics from a thick practice of interpretation to a structural ontological or logical core prevents philosophers of mathematics from acknowledging its plurality.¹² By confining mathematics in such manner, scholars actually prevent mathematics from attaining its cross historical and cross cultural ideality—an ideal openness to reinterpretation. Indeed, such confinement makes mathematics inaccessible to the many, who would otherwise access mathematics through the thick of different interpretations. Conceptually unified and confined, mathematics might not survive the historic obsolescence of our fashions and conceptual schemes.

As Cantor stipulated “the *essence* of mathematics lies precisely in its *freedom*” (quoted in Reck, 2003, p. 392). To maintain this freedom is to affirm the thickness of the scientist’s reinterpretation of mathematical signs across life worlds and across moments of her own life. To maintain this freedom, even as a freedom to idealise, is to acknowledge the constitutive role of locally superposed, reconstructed and deferred interpretations in the production, transmission, reformation and sustenance of mathematical signs. Without such reinterpretation mathematics might not survive long enough to become ideal.

¹¹This interpretation relates to Derrida’s interpretation of Husserl’s work in Derrida (1989). Indeed, even for an ideally oriented thinker such as Husserl, for whom mathematics is a “product arising out of an idealising, spiritual act, one of ‘pure’ thinking”, mathematics must derive from “factual humanity and [the] human surrounding world” (Husserl, 1939, p. 179). Since this is a changing factuality and world, not only historically, but culturally and intersubjectively as well, a scientist, who tries to impose an ‘ur-interpretation’ on mathematics (structural or other), detaches mathematics from worldliness in general, and ties it down to “something valid for him as a merely factual tradition”. His subsequent interpretation therefore “would likewise have a merely time-bound [or community bound] ontic meaning: this meaning would be understandable only by those men who shared the same merely factual presuppositions of understanding” (Husserl, 1939, p. 179).

¹²Greiffenhagen and Sharrock (2006) provide a recent typical example of this manoeuvre: the authors hack off all cultural or historic differences between mathematical practices, interpretations and systems as minor and non essential, avoid the task of articulating what it is—if anything—that survives this hacking, and conclude that mathematics is highly non-relative.

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