# Naturalism in the philosophy of mathematics

Mikkel Willum Johansen

Dissertation submitted for review with the purpose of obtaining the degree of Ph.D.

October 31, 2010
The PhD School of Science, Faculty of Science
Center for Philosophy of Nature and Science Studies
University of Copenhagen
Denmark

**Abstract in Danish**
I moderne matematikfilosofi findes en række forskellige naturalistiske forklaringer af matematik. I denne afhandling præsenterer jeg tre forskellige typer af sådanne sådanne forklaringer, og diskuterer deres styrker og svagheder. Disse tre naturalistiske forklaringstyper tager udgangspunkt i hhv. 1) evolutionær biologi, 2) kognitionsvidenskab og 3) videnssociologi. Hver af disse retninger inden for naturalismen hævder, at matematisk viden kan forklares med en bestemt type af kendsgerninger om menneskets natur, men den præcise type af kendsgerning varierer fra retning til retning; den evolutionære tilgang til naturalismen peger på kendsgerninger om evolutionære historie, den kognitionsvidenskabelige tilgang peger på kendsgerninger om menneskets kognitive apparat og den videnssociologiske tilgang peger på sociale kendsgerninger.

Mit mål i denne afhandling er dobbelt. Dels præsenterer og evaluerer jeg de forskellige naturalistiske teorier, nævnt ovenfor, og dels ønsker jeg at give en ny og mere adækvat naturalistisk beskrivelse af matematik. Som jeg ser det, er matematik så komplekst et fænomen, at det ikke er muligt at forstå det ved hjælp af et enkelt forklaringsniveau. Forklaringer, der udelukkende opererer indenfor en enkelt teoretisk ramme, så som kognitive semantik eller evolutionær biologi, fører uundgåeligt til en uproduktiv reduktionisme. Af den grund ønsker jeg at vise, hvordan forklaringer fra både det biologiske, det kognitive og det sociale forklaringsniveau kan sammensættes til at forme en konsistent, ikke-reduktive forståelse af matematik.

**Abstract**
A number of different naturalistic explanations of mathematical knowledge have been given in modern philosophy of mathematics. In this dissertation I presents and discuss the strengths and weaknesses of three such naturalistic approaches. The three approaches takes departure in respectively 1) evolutionary biology, 2) cognitive science, and 3) sociology of science. Each of the approaches claims mathematical knowledge to be explainable by a particular type of facts about human nature. The type of facts varies with the approach; the evolutionary biology approach states facts about humans' evolutionary origin, the cognitive science approach states facts about human cognition and sociology of science states social facts.

My aim in the dissertation is double. Firstly, I want to present and evaluate the different naturalistic approaches described above, and secondly, I want to give a new and more adequate naturalistic explanation of mathematics. As I see it, mathematics is much too rich to be understood on a single level of explanation. Explanations solely operating within a single theoretical framework, such as cognitive semantics or evolutionary biology, are valuable in many ways, but in my view they inevitably lead to an unproductive reductionism. As I see it, mathematics cannot be reduced to a single type of phenomena or facts. For this reason, I wish to show how explanation from both the biological, the cognitive and the social level of explanation can be pieced together to form consistent and non-reductive understanding of mathematics.

# Naturalism in the philosophy of mathematics

Mikkel Willum Johansen

This dissertation is submitted in partial fulfillment of the requirements for obtaining the degree of Ph.D. at the Faculty of Science, University of Copenhagen. The study has been conducted under the Ph.D. School of Science, at the Center for the Philosophy of Nature and Science Studies, Faculty of Science, University of Copenhagen, and was financed by a scholarship from the Faculty of Science, University of Copenhagen, Denmark.

The study has been supervised by professor Jesper Lützen, Department of Mathematical Sciences, University of Copenhagen, and associate professor Claus Emmeche, Center for the Philosophy of Nature and Science Studies, University of Copenhagen.

Copenhagen, October 31, 2010

Mikkel Willum Johansen

# Acknowledgements

I wish to thank my advisors, Jesper Lützen and Claus Emmeche, for all their useful comments, suggestions and ideas. Especially I wish to thank Jesper for housing me in Pasadena and for making the trip with me to Berkeley – that was more than anyone can expect from their advisor. I also wish to thank Paolo Mancosu, Rafael Núñez, David Kirsh, David Bloor, Richard Otte, Abraham Stone and Alison Pease for readily meeting with me in person to discuss various parts of my work in progress. the same goes for the seminar groups 'SiV' and 'Skemakredsen'. Finally, I want to thank my colleagues at the Center for the Philosophy of Nature and Science Studies for keeping spirits high and always being ready to share a thought and a cup of tea.

For Klemens
Who never got to know the joy of mathematics

# Contents

# Chapter 1

# Introduction

## 1.1 Background

The subject for my master thesis was artificial intelligence (AI). The main goal with my thesis though, was not AI in itself, but rather to find out what we can learn about human cognition from the so far failed attempts to simulate it. There are several different AI research paradigms, and all of them are rooted in strong assumptions about what characterizes human intelligence. When it turns out to be impossible to build intelligent machines within a given paradigm, some of the basic assumptions of the paradigm must be wrong. Finding out which can teach us something about human intelligence. It cannot tell us what human intelligence is, but it might tell what it is not, and that is almost as valuable.

One of the most important lessons learned in this way, is the fact that logic and formal reasoning can only explain and simulate a surprisingly small part of human intelligence. Although logic driven AI systems are good at playing chess and performing other tasks traditionally associated with high intelligence, they have turned out to be incapable of simulating many of the core components of human intelligence. Most surprisingly, it was realized that logic driven AI systems cannot plan in a dynamic environment (because of the so-called 'frame problem' (see Pylyshyn, 1987; Ford & Pylyshyn, 1996)). So although a classical logic driven AI system might be able to plan a chess move at grandmaster level, it cannot plan even simple actions such as making a sandwich in a real world environment. This was a clear proof of the shortcomings of formal reasoning and logical deduction. This surprising discovery (amongst others) led parts of the AI community to broaden their view on intelligence. Intelligence was no longer associated exclusively with logic and formal reasoning, but also with more capacities, such as the ability to plan and stay alive in a dynamic environment. “Elephants don't play chess”, as Rodney Brooks famously remarked, but they nevertheless seem to be intelligent — in some ways much more so than classical AI systems playing chess at a human level (Brooks, 1990).

There is a close connection (both in content and history) between the classical AI paradigm, where thinking is conceived as nothing but formal manipulation of signs, and the formalist conception of mathematics. Although few mathematicians actually believe that mathematics is to be practiced in accordance with the formalist ideal (i.e. as a purely formal game), the surprising limitations of formal reasoning evidenced by the development of AI-research, seemed to me to stress the need to give a more adequate picture of mathematical reasoning. Mathematicians do play chess, but do they also do more than that?

## 1.2 Aim and scope

In this dissertation, I decided to see the question of cognition in mathematics as part of the more general questions: What is the origin of mathematical knowledge, and how is it produced?

The general aim of this dissertation is to give at least a partial answer to these questions. In doing so, I will furthermore test and discuss the strength of naturalism as a general approach to answering this kind of epistemological and ontological questions in the philosophy of mathematics. My motivation for choosing naturalism over the more traditional rationalistic approach to the philosophy of mathematics will be explained in section 3.

Although my aim is to give naturalistic explanations, I will not commit myself to explanations within a specific theoretical framework, such as cognitive semantics or evolutionary biology. Some attempts have been made at explaining all of mathematics from such singular theoretical standpoints (e.g. De Cruz from evolutionary biology, Lakoff and Núñez from cognitive semantics, Bloor form sociology). These attempts are valuable in many ways, but they inevitably lead to an unproductive reductionism. As I see it, mathematics is much to rich to be reduced to a single type of phenomenon or process. For this reason, I will work with explanations on three different levels:

1. Evolutionary biology (i.e. how much of mathematics can be explained as evolved behavior and capacities?)

2. Cognitive science (i.e. how much of mathematics can be explained by our specific cognitive style?)
3. Sociology (i.e. to what extend is mathematics dependent on our participation in social institutions?)

I believe that explanations on one level are irreducible to explanations on any of the two others. I will give a theoretical argument for this anti-reductionist stance in section 3.2.1.

## 1.3 Summery

In chapter 2, I begin by addressing the traditional conception that mathematical knowledge is *a priori* knowledge produced by logical deduction from secure first principles. As I see it, this conception is flawed for two reasons: Firstly, there is no consensus on what types of arguments to accept as valid. Secondly, it is not possible to find secure first principles in the form of self-evident axioms. On the contrary, it seems that mathematics takes departure in some set of already accepted theorems, and set out to find the axioms needed in order to prove those theorems.

This relationship between theorems and axioms blocks a traditional answer to the research question outlined above. We cannot simply say that mathematics is produced as logical inferences from axioms given to us by reason. In order to address the question, we must instead investigate why some theorems and some modes of reasoning get accepted in mathematics in the first place. This conclusion is a clear motivation for adopting a naturalistic method in answering where our mathematical knowledge comes from.

In the chapter 3, I present my own conception of naturalism, and address some of the common objection raised against naturalistic explanations of mathematics. This includes the charge of psychologism proposed by Edmund Husserl and Gottlob Frege.

After these preparatory chapters, I will begin the naturalistic account of mathematics in chapter 4. Here, the claim that our mathematical knowledge and abilities are a direct product of our evolutionary history is discussed. The claim has been defended in a large body of empirical work examining the mathematical skills of non-human animals and the apparently inborn mathematical skills of human infants. As I see it, the available empirical work in the area is only able to account for a very limited part of the modern human's mathematical knowledge and abilities. For this reason, it is necessary to move beyond explanations given at the level of evolutionary biology in order to understand and explain mathematics.

In chapter 5 I present a particular understanding of human cognition, where cognition is treated not as a purely internal phenomenon, but as a phenomenon involving the body and the physical surroundings to some extend. More specifically, humans seem to use two powerful cognitive strategies. The first consists in a process of externalization, where problems, which could be solved using mental calculations, are instead externalized and solved using bodily and physical resources. The second strategy consists in the use of conceptual mapping. Such mapping is used to guide our treatment of unknown or abstract domains by transferring structure from well-known domains (such as physical experience) onto the unknown domains. In chapter 6 I discuss, how and in which ways our use of these cognitive strategies have influenced our conception of what we take mathematics to be.

Neither the biological nor the cognitive level of explanation however, seems to be able to account for the apparent normativity of mathematics. For this reason, I move to the social level in chapter 7 in search for a naturalistic account of the normativity of mathematics. I here take departure in a number of theories giving a collectivist account of rule following.

Finally, in chapter 8 I recapitulate my findings and show how the naturalistic approach can answer two of the major problems facing any philosophy of mathematics: why do we feel that we are working with something objectively existing, when we do mathematics? And: why can mathematics be applied with empirical success in descriptions of the physical world?

Chapter 8 also contains my final conclusion. In brief, the material presented below shows, in my view, that mathematics cannot be accounted for as strictly objective knowledge produced as logical derivations from secure first-principles. Our mathematical knowledge is exactly that – *our* mathematical knowledge. It is knowledge produced by a particular kind of biological being, and it is shaped by our biology, our particular way of existing and interacting with our environment, the kind of cognitive strategies we prefer, and even by particular cultural ideas and practices. Mathematics as we know it is created by us. It is a construction, but as I see it, not an *arbitrary* construct. Choices are made for reasons – mostly good reasons – and mathematics is to a large extend constrained by our interest in and need for handling particular aspects of the world, we inhabit.

## 1.4 Other activities

Obtaining a ph.d.-degree requires more than writing a dissertation. A ph.d. should be training to become a university researcher, and the job description of a researcher includes much more than just doing research (sadly, some would say). Apart from doing the research reflected in this dissertation, I have had the main responsibility for, and have been the main teacher at the University of Copenhagen's annual course in the philosophy of science for mathematics students. During my time as a ph.d.-student, more than 200 students passed this 7.5 ECTS-course. In 2009 I was nominated for the 'Harald' (the University of Copenhagen's teacher of the year award) by the students (but did not win).

Furthermore, if research in an area such as philosophy of science is to be beneficial to the general public, it is imperative not only to communicate with fellow philosophers, but also to communicate research results to the relevant scientific society (in this case mathematics), to students, and to the general public. To reach this goal, I have been giving ten lectures (to hi-school students, hi-school teachers, ph.d.-students and the general public), and I have written numerous newspaper articles, popular science articles, blog-entries (on the national science outreach webpage Videnskab.dk), and a paper to a popular publication discussion the relationship between God and mathematics. An overview over these activities is included in the following lists:

#### List of scientific papers

- "Embodied strategies in mathematical cognition", pages 179–196 of Löve, B. and Müller, T. (eds.): *PhiMSAMP. Philosophy of Mathematics: Sociological Aspects and Mathematical Practice*, College Publications, 2010

#### List of outreach and other papers published without peer-review

- "Does the usefulness of mathematics prove the existence of God?", pp. 71-93 of Kragh, H. and Nielsen, M.V. (eds.): *God - a Mathematician?*, Volume 5. of *Proceedings of the Danish Science-Theology Forum*, University of Aarhus, 2010.
- "Når matematikken slår rødder", *Mona*, nr. 3, 2010
- "Er Gud matematiker?", *Videnskab.dk*, 31. March 2010

- “Private universiteter i Danmark?”, *Videnskab.dk*, 15. December 2009
- “Hvad skal vi med videnskaben?”, *Videnskab.dk*, 25. November 2009
- “Markedsføring af videnskaben kan overdrives”, *Videnskab.dk*, 5. October 2009
- “Miljøministerium undergraver forskeres autoritet”, *Videnskab.dk*, 28. September 2009
- “Haarder og Galilei i frit fald”, *Videnskab.dk*, 11. September 2009
- “Humanistisk videnskabsteori er noget særligt”, *Videnskab.dk*, 28. August 2009
- “Videnskabens varedeklaration er vigtig”, *Videnskab.dk*, 6. August 2009
- “Sponseret forskning påvirker resultater”, *Videnskab.dk*, 14. July 2009
- “Om videnskabelig sikkerhed og usikkerhed”, *Videnskab.dk*, 25. June 2009
- “Lær statistik og lev længere”, *Videnskab.dk*, 4. June 2009
- “Kan man stole på en computer?”, *Videnskab.dk*, 28. May 2009
- “Regnedyr”’, *Weekendavisen* 30. April, 2008
- “Den sidste skilpadde”, *Aktuel Naturvidenskab*, nr. 1, 2008
- “Post-akademisk videnskab”, *Aktuel Naturvidenskab*, nr. 2, 2007
- “Computere i matematikken”, *DR Viden Om*, spring 2007
- “Preparing Ph.D. students for the post-normal age”, (with Tom Børsen Hansen and Claus Emmeche), *INES Newsletter*, nr. 55, 2007
- “Fra faktura til forskning”, *Weekendvisen*, 2. marts 2007

As a note on scientific ethics, the reader should be aware that some of the ideas and findings presented in this dissertation have been presented (mostly in popular form) in these various papers and articles. This is particularly the case for the paper “Embodied strategies in mathematical cognition”. Several passages of this paper are reproduced with slight or no change in chapter 5 and 6 of this dissertation.

---

Finally, the ph.d-education at the University of Copenhagen includes a ‘change of environment’ stay at a foreign university, normally for a semester. For personal reasons, a stay of this length was not possible in my case. Instead, I visited several relevant university departments for shorter periods of time. This included short visits at UC Berkeley and UC Santa Cruz, a longer stay at the Department of Cognitive Science at UC San Diego, and finally a visit at the University of Edinburgh. I wish to thank everybody involved in these stays for welcoming me.

# Chapter 2

# If not in mathematics?

## 2.1 The Euclid Myth

In traditional rationalistic accounts of mathematics, mathematical knowledge is believed to be produced by deductive proofs collected in an axiomatic-deductive system. The system as a whole is supposed to rests on a small number of self-evidently true axioms. As deductive inferences are truth preserving, truth will be transported from the axioms throughout the system as a whole. Consequently, mathematical knowledge produced by valid proofs in this way can be known to be true beyond any reasonable doubt.

This conception of mathematics is for instance expressed in the following passage of the textbook *AT-Håndbogen*, aimed at the philosophy of science teaching in Danish hi-schools.

> Mathematics is constructed as a deductive network consisting of axioms, definitions and theorems.
>
> Mathematical theorems are usually general and independent of culture and time. It only happens very rarely that a theorem, once accepted as valid, ends up getting disproved. Pythagoras' theorem applies to all right-angled triangles, no matter how large they are, and it has been known and accepted for thousands of years.
>
> Mathematics has these two properties, universality and validity, because mathematical theorems are proved, i.e. justified by logically valid deductions from a firm foundation that is regarded as being true.
>
> (Dideriksen *et al.* , 2009, p. 52, my translation)

We do not know the exact genealogy of this axiomatic-deductive method. It probably has its origin in Greek philosophy, and was used with great perfection by Euclid, who in his *Elements* from around 300 B.C.E. managed to compile most of the knowledge of geometry of the time in a single axiomatic-deductive system, using only ten unproven axioms[1].

For the sake of the discussion below, I have illustrated the axiomatic-deductive method by reproducing Euclid's proof of the fact that the interior angles of a triangle equals two right angles (see figure 2.1). The proof appears as theorem nr. 32 of book I of Euclid's *Elements*. Apart from the axioms presupposed as true by Euclid, the theorem is proved only by appealing to three other theorems (theorems nr. 13, 29, and 31). All of these theorems are

[1]The *Elements* also deals with number theory in the three books VII, VIII, and IX. However Euclid wanted to separate numbers and magnitudes (probably following Aristotle), and consequently these three books form an entirely independent unit (Katz, 1998, p. 84).

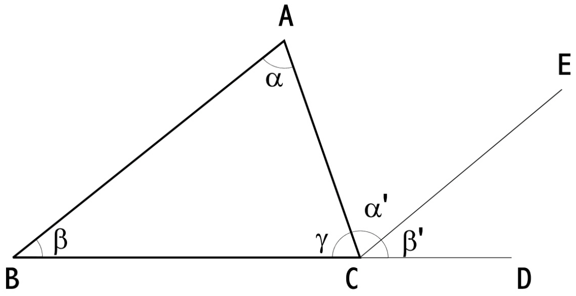
in turn proved by appeal to other theorems closer to the axioms (theorems nr. 13 for instance is proved by appealing to theorem 11 plus axioms, and theorem 11 is proved by appeal to theorem 1, 8, and 3 plus axioms, and so on). Tracing the proof further back, we will find that in the end it relies on nothing but the ten indubitable axioms. So in conclusion, the proof of theorem 32 along with its position in the deductive system as a whole allows us to state beyond any doubt, that the interior angles of the triangle are equal to two right angles. This, in short, is how the axiomatic-deductive method is supposed to work.

For centuries the *Elements* was considered an ideal of mathematics, and the truth of its theorems were largely acknowledged. The certainty and clarity of the mathematical method even had a tremendous impact outside of mathematics. Rationalist philosophers beginning with Plato saw mathematical thinking as an epistemic paradigm to be followed, and renaissance philosophers such as Spinoza and Descartes tried to apply the axiomatic-deductive method to other areas of knowledge; Spinoza tried to axiomatize ethics (the name of his major work on ethics is *Ethica Ordine Geometrico Demonstrata*), and Descartes of course used his *cogito* as the fundamental axiom on which he could rest and justify all other knowledge. More surprisingly – and even somewhat contradictory to their general views - -the empiricists (save John Stuart Mill) accepted mathematics as *a priori*, necessarily true knowledge.

In the words of Imre Lakatos:

> Classical epistemology has for two thousand years modelled its ideal of a theory, whether scientific or mathematical, on its conception of Euclidean geometry. The ideal theory is a deductive system with an indubitable truth-injection at the top (a finite conjuction of axioms) – so that truth, flowing down from the top through the safe truth-preserving channels of valid inferences, inundates the whole system.
>
> (Lakatos, 1976a, p. 205)

This idea of mathematics as eternally true knowledge produced by deductive proof from self-evident axioms was however met with stark criticism, especially during the last half of the 20th century (not least by Lakatos). As part of this criticism, Philip Davis and Reuben Hersh famously dubbed it 'the Euclid Myth' (1992, pp. 322). And it is, as I see it, a myth. A myth, not so much about Euclid's work, but rather about the role played by axiomatic thinking in mathematics, and about the idea that the axiomatic-deductive method can warrant the truth and certainty of mathematical knowledge.




*In any triangle, if one of the sides be produced, the exterior angle is equal to the two interior and opposite angles, and the three interior angles of the triangle are equal to two right angles.*

Let $ABC$ be a triangle, and let one side of it $BC$ be produced to $D$; I say that the exterior angle $ACD$ is equal to the two interior and opposite angles $CAD$, $ABC$, and the three interior angles of the triangle $ABC$, $BCA$, $CAB$ are equal to two right angles.

For let $CE$ be drawn through the point $C$ parallel to the straight line $AB$. Then, since $AB$ is parallel to $CE$, and $AC$ has fallen upon them, the alternate angles $BAC$, $ACE$ are equal to one another. Again, since $AB$ is parallel to $CE$, and the straight line $BD$ has fallen upon them, the exterior angle $ECD$ is equal to the interior and opposite angle $ABC$. But the angle $ACE$ was also proved equal to the angle $BAC$; therefore the whole angle $ACD$ is equal to the two interior and opposite angles $BAC$, $ABC$. Let the angle $ACB$ to each; therefore the angles $ACD$, $ACB$ are equal to the three angles $ABC$, $BCA$, and $CAB$. But the angles $ACD$, $ACB$ are equal to two right angles; therefore the angles $ABC$, $BCA$, and $CAB$ are also equal to two right angles.

(Euclid's *Elements*, theorem I.32. Text from Heath, 2006, pp. 164-5)

**Figure 2.1:** Proof that the interior angles of a triangle equals two right angles.

Consequently, I prefer to call it *the myth of axiomatic truth and certainty.* If the myth had been right, there would be no place for naturalized explanations of mathematics; mathematics would simply be the produce of pure reason, and nothing else. Consequently, any naturalized accounts of mathematics must start with showing the myth of axiomatic truth and certainty to be false, and that is what I will do in the rest of this chapter. The discussion of this myth of axiomatic truth and certainty will force us to ask some new and very interesting questions regarding the status and nature of mathematical knowledge.

## 2.2 End of the myth

First of all, it should be noted that the truth and certainty of the Euclidean proofs have not always been accepted. Today it is well known that there are gaps in many of the proofs of the *Elements*, and that Euclid presupposes several hypotheses not explicitly stated as axioms. The very first proposition for instance, is a demonstration showing that an equilateral triangle can be constructed on any finite straight line AB (see figure 2.2). In the proof, two circles with radius $AB$ is produced, one with center $A$ and the other with center $B$, and the point $C$ of intersection is used as the third vertex in a triangle $ABC$, which is then shown to be equilateral. The trouble is that Euclid does not demonstrate that the point $C$ of intersection exists. It is intuitively obvious from the figure, but from a modern point of view the existence of the point nevertheless presupposes an axiom of continuity.

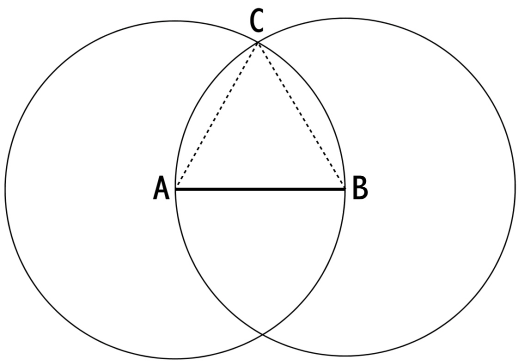



**Figure 2.2:** The construction of an equilateral triangle

The skepticism towards the *Elements* is not exclusively a modern phenomenon. The proof given for theorem I.32 above was, for instance, hotly debated in the Renaissance and in the 17th century. The proof takes advantage of external angles $\alpha$', $\beta$', and auxiliary line segments $CD$ and $CE$, but these external lines and angles cannot be said to be the cause of the fact proven (that the sum of the interior angles equals two right). Hence, it was argued, the proof does not produce proper scientific knowledge according to the Aristotelian criteria set forth in the *Posteria Analytics*[2].

So the proof techniques and standard of rigor found in the *Elements* has not universally been accepted. The conception of precisely which techniques to accept vary over time and between different philosophical schools. As René Thom put it: "There is no rigorous definition of rigor" (Thom, 1971, p. 697).

Secondly, with the *Elements* only geometry was axiomatized. Large parts of mathematics such as arithmetic, probability theory and analysis was developed without Euclidean axiomatic – and during some periods with much looser demands on rigor. In the case of analysis, the lack of rigor and questionable use of infinitely small quantities was famously pointed out by Bishop George Berkeley (1685–1753) (Berkeley, 1754), but with little effect. The mathematicians continued to develop the today celebrated bulk of analysis (including the impressive work of Leonhard Euler (1707-1783) and Carl Friedrich Gauss (1777–1855)) without axioms or foundations, and, one might add, at times with a somewhat hazardous interpretation of central concepts[3].

In the case of arithmetic negative numbers, rational numbers and even complex numbers were introduced and used for centuries without axioms and without deep and rigorous understanding of their nature (Kline, 1980, pp. 113). In other words: Through the history of mathematics the rigor and axiomatic-deductive structure of the *Elements* was an ideal only met occasionally.

Thirdly, the Euclidean system was not as perfect and indubitable as the Euclid myth suggests. Euclid's axioms consist of two groups: Five 'common

[2]This line of criticism actually goes back to Proclus' commentary on the *Elements* (see Proclus 1970, pp. 161). A thorough discussion of this example is found in Mancosu (1996, pp. 8). Note, that the same critique can be (and was) raised against indirect proofs including the celebrated exhaustion technique used by the Greeks to determine areas of certain plane figures.

[3]Euler's conception of the derivative $dy/dx$ can serve as an example. The expression $dy/dx$ was originally (by Leibniz) conceived as a ratio of infinitesimals, that is, infinitely small quantities. Euler, on the other hand, saw the infinitesimals as actually zero, and so $dy/dx$ to him equaled $0/0$. This is normally considered an undefined term. Euler however, argued that since $n \cdot 0 = 0$ we get by dividing with 0 that $n = 0/0$ for any number $n$. Consequently, $0/0$ can have many values – including the desired one. (Kline, 1980, p. 147).

notions' describing general rules of inference applicable outside of mathematics, and five 'postulates' concerning permitted geometrical constructions. For instance the first common notion states that "Things which are equal to the same thing are also equal to one another", and the first postulate states that it is possible "To draw a straight line from any point to any point" (both from Heath, 2006, p. 2-3). All of this seems rather innocent, but the fifth so-called 'parallel postulate' seems to be something different. It states: "If a straight line falling on two straight lines make the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles" (Heath, 2006, p. 3; see figure 2.3).

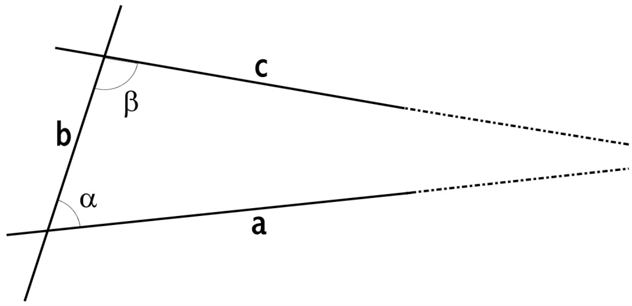



**Figure 2.3:** Euclid's fifth postulate: If the sum of $\alpha$ and $\beta$ is less that two right angles, the straight lines $a$ and $c$ will meet on the side of $\alpha$ and $\beta$ if prolonged indefinitely.

The postulate is crucial, because it allows parallels to transport equal angles. In the proof for the theorem that the sum of the interior angles of a triangle equals two right angles, Euclid used this property in form of theorem I.29, which states that a straight line falling on two parallel lines makes the alternate angles equal. But this theorem cannot be proven without the parallel postulate, and hence, without the parallel postulate it is not possible to prove that the angle sum of a triangle equals two right angles. Indeed, it turns out that the parallel postulate and theorem I.32 are equivalent, that is: if any one of them is assumed, the other can be demonstrated.

The trouble with the parallel postulate is the word 'indefinitely'; the lines must be produced indefinitely. But what does that precisely mean? Do we have any clear conceptions of what happens when lines are produced indefinitely? What if for instance the sum of the interior angles is just a little bit smaller than two right angles? Can we be sure that the straight lines will meet? And what if they do not meet? Is it because the parallel postulate is false, or is it just because, we have not prolonged the lines enough? Anyway, the postulate can hardly be said to be self-evident, and from remarks in Aristotle we know, that the Greek mathematicians even before Euclid discussed the problem of parallels and the angle sum of the triangle (for references see Tóth 1969, pp. 90)[4].

After Euclid, many mathematicians considered the parallel postulate to be a blemish to the *Elements*, and consequently tried to do something about it in order to provide secure foundations for the Euclidean geometry. Three different strategies can be identified: Some mathematicians tried to substitute the parallel postulate with truly self-evident axioms, others tried to prove the postulate directly by deducing it from the remaining nine Euclidean axioms, and finally yet others tried to prove it indirectly by showing that the negation of postulate led to a contradiction. The list of mathematicians who thought they had succeeded in proving the postulate is long, beginning with Claudius Ptolomy (about A.D 150), through a number of Islamic mathematicians (for instance Omar Khayyam (b. 1048)), to John Wallis (1616–1703), Girolamo Saccheri (1667–1733) and Adrien Marie Legendre (1752–1833). However, all the proofs of the postulate were faulty, and all the candidates for more self-evident axioms turned out to be just as questionable as the disputed postulate itself.

During the first half of the 19th century it was realized (first by Gauss, who did not publish, and during the 1820's by Nikolai Lobachewski (1792–1856 ) and János Bolyai (1802–1860), who did publish) that without the parallel postulate, one could actually reach a fruitful and interesting geometry, although it in a number of ways seemed quite strange. In this new so-called (hyperbolic) non-Euclidean geometry, the angle sum of the triangle would for instance depend on the size of the triangle and always be less than 180°. Finally, in 1868 it was realized[5], due to a model for the hyperbolic geometry constructed by Eugenio Beltrami, that the hyperbolic geometry was relatively consistent to the Euclidean geometry, that is: If the Euclidean geometry is without contradictions (but we do not know that), then non-Euclidean geometry will also be without contradictions (Gray 1989, pp. 147). So, from a purely rational or logical point of view, both of the geometries are equally good. They are both equally thinkable, and hence pure thinking cannot determine which geometry to count as the correct and objectively

[4] According to Imre Tóth, it seems that Aristotle and his contemporaries were aware, that the postulate could not be proven by logic, but that you somehow had to make a choice between different geometries. One, where the sum of the angles of a triangle equals two rights, and others where it does not. It should be noted, tough, that this theory is highly controversial, and one might rightly accuse Tóth of going beyond what can safely be concluded from shattered remarks in Aristotle. The remarks leaves, however, little doubt that the problem of the parallels was discussed.

[5] Exactly by whom is a little unclear, see Grey 1989, p.149 for a discussion.

true description of the Universe.

The discovery of the relative consistency of hyperbolic geometry was certainly a massive blow to the traditional conception of Euclidean geometry. The theorems of the *Elements* could still be counted as true, but only true within the system of Euclidean geometry. Euclidean geometry as a whole was given a different status. It could no longer be counted as a body of necessary truths about the Universe, but only as a mathematical system that might (or might not) be useful in describing the real world. This is what Caroline Dunmore (1992) calls a 'meta-revolution' in mathematics: The truth of the Euclidean geometry was not overthrown; rather the very meaning of mathematical truth was about to change from a classical correspondence theory of truth to something more similar to a coherence theory of truth (a theorem is true, if it can be proved from a set of consistent axioms). This step, however, was only to be taken fully by Hilbert years later.

The discovery of non-Euclidean geometry was only the beginning. In the last half of the 19th century several other surprising – and equally disturbing – discoveries were made. The most striking was probably the 'space-filling curves', i.e. curves with an area strictly larger than zero (discovered by Giuseppe Peano in 1890) and everywhere continuous, but nowhere differentiable functions (first announced by Karl Weierstrass in 1872, but already discovered by others in the 1830's (Kline, 1980, p. 177))[6]. The everywhere continuous, nowhere differentiable functions were especially shocking to the mathematical society, since until then, most mathematicians had simply assumed, that continuity implied differentiability.

Discoveries such as these made two things clear to the 19th-century mathematicians: 1) our geometric intuition is not to be trusted and 2) more rigor is needed. The rigorization of analysis had already begun with Augustin-Louis Cauchy's (1789–1857) *Cours d'analyse algébrique* from 1821. This rigorization program was continued primarily in the last half of the 19th century

[6]Intuitively speaking a function $f(x)$ is differentiable at the point $a$, if it is possible to draw a definite tangent to its graph in the point $(a; f(a))$. A function is continuous, if small changes in the argument (input) only result in small changes in the values (output), or, as the high-school teacher tells you: If you can draw its graph without lifting the pencil from the paper. So an everywhere continuous, but nowhere differentiable function is a function, which can be drawn without lifting the pencil from the paper, but in such a way that it does not have a well-defined tangent at any point. Try imagining such a curve – it seems alt least to me to be very, very hard. Weierstrass' everywhere continuous, nowhere differentiable function was given in a strictly analytical way as the equation: $f(x) = \sum_{n=0}^{\infty} b^n \cos(a^n x)\pi$, with $a$ odd, $b \in [0; 1]$ and $ab > 1 + 3\pi/2$. Helge von Koch later gave his 'snowflake' as an example of a tangentless curve, which could be visualized geometrically (see Mancosu, 2005, p. 16-17).

by men like Karl Weierstrass (1815–1897), Richard Dedekind (1831–1916), Georg Cantor (1845–1918), Gottlob Frege (1848–1925) and Giuseppe Peano (1858–1932). One of the primary strategies of this program was to redefine central mathematical concepts in terms of set theory. The concept 'continuity', which is intuitively very closely connected to movement in space, was for instance reduced to properties of sets of real numbers with the $\epsilon$-$\delta$-definition[7] . Real numbers in turn were defined using infinite sets of rational numbers. Rationals were easily defined using integers, and these were axiomatized in 1889 by Peano and given a set-theoretical definition by Frege in his *Die Grundlagen der Arithmetik* from 1884[8]. In this way, the traitorous intuitions of space and motion were eliminated and replaced with the assumingly more clear and simple ideas of arithmetic and sets. Finally mathematics was on safe grounds again, and at the International Congress of Mathematicians at Paris in 1900, Henri Poincaré (1854–1912) famously uttered: "One may say today that absolute rigor has been attained" (quoted in Kline 1980, p. 195).

One feature of the rigorization program should however be noted. With the ideal of the axiomatic-deductive method in mind, one would have expected the rigorization to proceed from secure and self-evident foundation and upwards. But it did not. It proceeded in the exact opposite direction, starting with the top-level concepts of analysis and working its way down to the basic concept of natural numbers. This top-down approach has inspired the following comment by Morris Kline:

> The newly founded rigorous structure presumably guaranteed the soundness of mathematics but the guarantee was almost gratuitous. Not a theorem of arithmetic, algebra, or Euclidean geometry was changed as a consequence, and the theorems of analysis had only to be more carefully formulated [...] Indeed, the axioms had to yield the existing theorems rather than different ones because the theorems were on the whole correct. All of which means that mathematics rests not on logic but on sound intuition.
>
> (Kline, 1980, pp. 194)

[7]It goes like this: A function $f(x)$ is continuous in a point $a$, if $f(x)$ is defined in an open interval containing $a$, the function has a limiting value for $x$ approaching $a$, and the limiting value equals $f(a)$, that is: $\lim_{x\to a} f(x) = f(a)$. The limit of the function at $a$ is in turn defined using the $\epsilon$-$\delta$-definition: The statement $\lim_{x\to a} f(x) = f(a)$ means that for every $\epsilon > 0$ there exists a $\delta > 0$, such that if $0 < |x - a| < \delta$ then $|f(x) - f(a)| < \epsilon$ .

[8]The definition goes: "Die Anzahl, welche dem Begriffe F zukommt, ist der Umfang des Begriffes 'gleichzahlig dem Begriffe F' " (Frege, 1974, p. 79-80).

## 2.3 The foundational crisis

Not all mathematicians were as confident about the rigorization of mathematics as Poincaré. At the 1900 Congress David Hilbert presented a list of 23 problems that remained to be settled. As the second of these problems, he stressed the need for a proof of the consistency of the real number system:

> ... above all I wish to designate the following as the most important among the numerous questions which can be asked with regard to the axioms: *To prove that they are not contradictory, that is, that a definite number of logical steps based upon them can never lead to contradictory results.*
>
> (Hilbert quoted from Feferman, 1998, p. 23)

As sketched out above, most of mathematics was reduced to arithmetic and set theory during the rigorization process of the 19th century. Furthermore, it was well known at the time (and Hilbert also pointed this out in his talk) that the axioms of geometry could be modeled in the real number system using the method of analytic geometry. This meant that geometry was consistent relative to the arithmetic of the real number system. In other words, everything seemed to depend on the consistency of arithmetic. But can we be sure that arithmetic is without contradiction? Hilbert anyway called for a proof, and he even raised the stakes by tying mathematical existence very closely to consistency: If a concept is consistent, it can be said to exist. So, if the real number system could be shown to be consistent, the infinite set of real numbers might be said to have actual existence: "Indeed, when the proof for the compatibility of the axioms [of the real number system] shall be fully accomplished, the doubts which have been expressed occasionally as to the existence of the complete system of real numbers will be totally groundless" (Hilbert in Feferman, 1998, p. 24).

In this, Hilbert touched upon another problem that had become apparent in the development of set theory during the last decades of the 19th century, namely the use of actually infinite sets. The actual infinite had been banned in mathematics since Aristotle, who only allowed mathematicians to use potential infinite[9]. But during the 1870's George Cantor began developing a

[9] To Aristotle, infinity only meant that a certain process of either prolonging or dividing could always continue one step further. Hence "It turns out that the infinite is the opposite of what people say it is: it is not that of which no part is outside, but that of which some part is always outside. [...] Nothing is complete unless it has an end, and an end is a limit" (Aristotle, *Physics*, 206b33-207a14, quoted from Hussey 1983, pp. 15-16). The mathematicians however do not need to change their practice: "This reasoning does

theory of infinite sets as actually existing wholes. Cantor soon realized the need to distinguish between different orders of infinitude. He defined two sets to be of the same size, if a one-to-one mapping between the members of the two sets existed. This is a rather trivial and straightforward definition when finite sets are examined, but it is more questionable when applied to infinite sets. Does it really give any meaning to compare the size of two infinite collections? Nevertheless Cantor did so, and by using this definition, he easily proved that the set of natural numbers and the set of rational numbers are of the same size, or 'equipollent'. Both sets are what mathematicians call denumerable.

To some surprise, Cantor also showed that the set of real numbers have a larger cardinality than that of the natural numbers. In other words, the set of reals is larger than the set of naturals, even though both sets are of infinite size. Even more surprising Cantor showed that the real numbers can be well-ordered, that is: ordered in such a way that every subset of the reals have a first element. Note that in the standard ordering this is not the case. What is for instance the first positive real number? However, not everybody was convinced by Cantor's proof, and Hilbert as part of the first of his 23 problems called for a direct proof of this so-called well-ordering theorem (Moore, 1982, pp. 55).

Cantor's set theory was met with a lot of opposition. Firstly, the conception of sets as existing wholes seemed to imply some sort of realism or Platonism (and indeed Cantor had a Platonist conceptual framework (Moore, 1982, pp. 54)); the infinite sets were supposed to exist and have definite properties independent of and before mathematicians discovered them.

Secondly there seemed to be some logical problems in connection to Cantor's set theory. Most prominently, Cantor used the set $\Omega$ of all ordinal numbers in his proof of the well-ordering theorem. This concept requires a little explanation. Numbers can be used for two different purposes: They can be used as cardinal numbers to describe the size of a collection or they can be used as ordinal numbers to describe the position of an element in a sequence. The sequence 0,1,2,3,4,5 for example has 6 elements (6 as cardinal) and 5 is the $6^{th}$ element in the sequence (6 as ordinal). Cantor considered transfinite ordinals, so to him the sequence of ordinals, begun above, went on to the first infinite ordinal $\omega$ and beyond: $0, 1, 2, 3, \ldots, \omega, \omega + 1, \ldots, 2 \cdot \omega, \ldots, \omega^{\omega}$ and so forth. In other words, Cantor introduced the possibility of counting

---

not deprive the mathematicians of their study, either, in refuting the existence in actual operation of an untraversable infinite in extent. Even as it is they do not need the infinite, for they make no use of it; they need only that there should a finite line of any size they wish" (Aristotle, *Physics*, 207b27-207b31, quoted from Hussey 1983, pp. 17-18).

a transfinite number of elements, that is the $\omega^{th}$ element, the $\omega + 1^{th}$ and so on.

The set $\Omega$ of all ordinal numbers is itself well-ordered, and hence it must have an ordinal number $\delta$. As hinted in the example above (where 5 has the ordinal number 6), $\delta$ must be greater than - and consequently different from - any ordinal in $\Omega$. But, since $\Omega$ is the set of *all* ordinals, $\delta$ must also be a member of $\Omega$. This is a contradiction (commonly known as Bureli-Forti's paradox). The paradox was discovered by Cantor and communicated in a letter to Dedekind (Cantor, 1899). Cantor however did not see the contradiction as a problem to his general theory. Instead, he distinguished between consistent and inconsistent sets, and saw the paradox as a proof for the fact that $\Omega$ was a set of the latter type.

The Bureli-Forti paradox involves complicated concepts like well-ordering and ordinal numbers, but in 1901 Bertrand Russell (1872–1970) found another paradox involving only the simple and basic concepts of set and membership. The paradox was famously communicated to Frege in a letter in June 1902 (reproduced and translated as (Russell, 1902)). The idea of the paradox is this: In naïve set theory, a set can be formed freely as the extension of a determinate property, or, to put it more formally: If $P(x)$ is a propositional function containing $x$ as a free variable, there will exist a set whose members are exactly those things $x$ having the property $P$. The trouble is that this definition makes it possible for a set to be a member of itself. As an easy example, we can let $P$ be the property 'being a set', then $P(x)$ is the set of all sets. Since the set of all sets is itself a set, it must be a member of itself.

Russell used another, similar, set in his paradox, namely the set of all sets that are not members of them selves: $M = \{x | x \notin x\}$. The question is of course whether $M$ is a member of itself or not. Testing the different possibilities, we will find that $M \in M$ implies $M \notin M$ and the other way around. In other words: if $M$ is a member of itself, it cannot be a member of itself, and if $M$ is not a member of itself, it must be a member of itself. This is a very clear and undisputable paradox in set theory, and it had as Hilbert puts it "a downright catastrophic affect in the world of mathematics" (Hilbert, 1925a, p. 375).

The paradoxes of naïve set theory, the lack of a proof of the consistency of arithmetic and the (to some) rather dubious metaphysical status of Cantor's transfinite numbers and actual infinite sets were the main ingredients in a cocktail of problems that one way or the other inspired the so-called three foundational schools: logicism, formalism (in form of the Hilbert program), and intuitionism. We shall not go into the details of the three schools here[10]. The three schools all failed for different reasons, and a few remarks on why they failed is all that is of interests to us here.

### 2.3.1 Logicism

Logicism was the attempt to reduce mathematics to logic. The basic logicist idea can (to some extend) be found in the work of Frege, but unfortunately Frege's program suffered a fatal blow by the discovery of Russell's paradox. After Frege, the logicist program was primarily carried out by Bertrand Russell who, in collaboration with Alfred North Whitehead, presented his ideas in the monumental three-volume work *Principia Mathematica* published from 1910-13. The main idea of logicism was to reformulate all mathematical concepts, i.e. concepts such as 'number', 'addition' and so forth, in logical (set-theoretical) terms, and to derive all mathematical theorems from a few axioms, which were all logical tautologies, using nothing but logical deduction. In other words, Russell and Whitehead wanted to demonstrate mathematics to be purely analytic *a priori* knowledge.

The paradoxes of set theory were blocked by type theory. With type theory an elaborate hierarchy of different types of sets is introduced. Type 1 sets were sets containing only individuals as their elements. Type 2 sets were sets allowed to contain either individuals or sets of type 1 as elements. Type 3 sets were sets of either individuals, type 1 sets or type 2 sets, and so forth. The basic idea was that no sets were allowed to contain sets of its own type as elements, but only sets of lower types (and individuals). In this way, self-membership was ruled out, and neither Russell's paradox nor the Bureli-Forti paradox could be formulated.

As it turned out, it was not possible to carry out the logicist program. Russell and Whitehead had to introduce two non-tautological axioms: the Axiom of Infinity and the Axiom of Reduceability. The Axiom of Infinity states that at least one actual infinite set exists. The existence of an infinity is certainly not a logical tautology; living in a finite world it is not even common sense plausible[11].

[10]I will refer the reader to Stewart Shapiro (2000) for a thorough textbook introduction and to Jean van Heijenoort (1967) and Benacerraf & Putnam (1983) for selections of the primary texts.

[11]It is sometimes argued that we actually do know infinite sets. For instance, a line can be divided in halves an infinite number of times, and thus the endpoints of the line segments will form an infinite set. It should be noted though that actual lines like this: ____ can only be divided in halves a few times before subatomic length is reached. What

The Axiom of Reduceability was needed in order to fix a problem caused by the introduction of type theory. Numbers were defined as sets, but because of the type theory, we would have different types of sets, and consequently different types of the same numbers – so the number 3 for example, would exist both as a type 2 number, as a type 3 number and so on. This made any kind of mathematics very cumbersome, and in order to fix the problem, Russell and Whitehead introduced an axiom stating that every set of a higher type is coextensive with sets of the lower levels – the Axiom of Reduceability. The axiom is not a logical tautology, but was clearly introduced as an *ad hoc* move in order to fix a specific problem in the theory developed (indeed, one might even say that the very theory of types was an *ad hoc* move taken to block the known paradoxes. At least, the existence of an elaborate hierarchy of types does not in itself seem to be an a priory necessary truth).

The lesson learned form the logicist program is this: Mathematics (as we know it) cannot be reduced to logic – or at least we do not yet know how.

### 2.3.2 Intuitionism

Intuitionism is the best example of a genuine attempt to give a bottom-up justify of mathematics. The theory was primarily worked out by the Dutch mathematician Luitzen Egbertus Jan Brouwer (1881–1966) (starting with his doctoral dissertation from 1907), his student Arend Heyting (1898–1980), and Herman Weyl (1885–1955) (who was 'converted' to intuitionism round 1920). Following the constructivist stance advanced by Leopold Kronecker (1823–1891), Brouwer wanted to rebuild mathematics from the bottom and up. To Brouwer, mathematical objects did not have any real and independent existence. Mathematics was a mental activity, and mathematical objects were mental construction rooted in a basic Kantian intuition of time.

Roughly speaking, the intuitionist took the following path in the attempt to rebuild mathematics: The natural numbers could be constructed from the basic intuition of time. The rational numbers could be constructed from the naturals, and from these constructions the real number system could be

---

is cut in halves after that point cannot be the real line, but only the mathematical line, that is: the mathematical abstraction of the real line. So this proof of the existence of an infinite collection only works, if the real existence and reality of the mathematical abstraction (that is: a Platonist ontology) is assumed in advance.

On a more pragmatic note it might be added that according to Salomon Feferman (1998), a mathematics suitable for the needs of natural science can be developed without the use of infinite sets. Hence, even the so-called indispensability argument, which from the outset is much weaker than logical necessity, is blocked constructed. Finally, geometry could be constructed from the reals using analytical geometry (Heyting 1931, pp. 52; Shapiro 2000, p. 177).

As it turned out, the mathematics actually constructed by the intuitionists seemed somewhat unfamiliar to many of Brouwer's contemporaries. Theorems – even fundamental theorems – that seemed intuitively correct to most, did not hold good to the intuitionists. For instance, Brouwer disproved the following two theorems (Brouwer, 1923, pp.337):

1. Every number is either smaller than, equal to or greater than zero
2. Every continuous curve defined on a closed interval has a maximum

Brouwer even called for revisions of the basic logical inferences allowed. According to Brouwer, the principle *tertium non datur* ($P \vee \neg P$) could only be used in finite cases, but not in infinite (ibid. p. 336). This robbed the intuitionists of the strong tool of indirect proof in the infinite case.

However, the intuitionists did not only restrict ordinary mathematics, they also claimed the truth of theorems that seemed obviously wrong or even bizarre from a traditional point of view. It was for instance a theorem of intuitionistic mathematics, that every real function is continuous (Feferman, 1998, p.47).

It can be – and indeed has been (for instance in Brown, 1999, p.115) – discussed, whether private intuitions can be used as a safe foundation for mathematics. However, the main thrust of the criticism raised against the intuitionist school was not directed against its foundational program, but towards the results it produced — or rather: the results it did *not* produce. The constructive stance of intuitionism seemed to impose heavy and unnecessary restrictions on mathematics. As Hilbert put it:

> They [Weyl and Brouwer] seek to ground mathematics by throwing overboard all phenomena that make them uneasy and establishing a dictatorship of prohibitions à la Kronecker. But this means to dismember and mutilate our science, and if we follow such reformers, we run the danger of losing a large part of our most valued treasures. Weyl and Brouwer calumniate the general notion of irrational number, of function, even of number-theoretic function, the Cantorian numbers of the higher number-classes [transfinite ordinal numbers], etc. [...] and even the logical *tertium non datur*.
>
> (Hilbert, 1922, p. 200)

On the last he later commented:

> Taking the principle of the excluded middle from the mathematician would be the same, say, as proscribing the telescope to the astronomer or to the boxer the use of his fists.
>
> (Hilbert, 1925b, p. 476)

So intuitionism failed, not (or at least not only) because it failed to provide safe foundations for mathematics, but because it failed to produce foundations for the body of knowledge already accepted to be the true mathematics.

### 2.3.3 Formalism

Formalism understood as a foundational school, is primarily represented by the Hilbert-program, set forth by David Hilbert in 1917 in his address *Axiomatisches Denken* (Hilbert, 1918) and in several other addresses in the following years. During the 1920's, Hilbert's program was taken up by a number of mathematicians including John von Neumann (1903-1957) and Paul Bernays (1888–1977).

The motivation for the program was the discovery of the paradoxes in set theory. As Hilbert put it:

> Let us admit that the situation in which we presently find ourselves with respect to the paradoxes is in the long run intolerable. Just think: in mathematics, this paragon of reliability and truth, the very notions and inferences, as everyone learns, teaches, and uses them, leads to absurdities. And where else would reliability and truth be found if even mathematical thinking fails?
>
> (Hilbert, 1925a, p. 375)

The main goal of Hilbert's program was to deliver a proof of the consistency of mathematics – the proof Hilbert had already asked for as the second of the 23 problems set forth at the 1900-congress. First of all, it should be noted, that Hilbert considered finite arithmetic to be safe. A formula like $2 + 3 = 3 + 2$ can simply be checked in our intuition. 2, 3 and 5 can be represented as $||$, $|||$ and $|||||$ respectively, and the verification of the truth of $2 + 3 = 3 + 2$ amounts to checking that the concatenation of $||$ and $|||$ and the concatenation of $|||$ and $||$ both amounts to $|||||$. In other words: As long as only finite quantities are involved, there is no problem. The problem only sets in when infinite quantities are introduced.

The basic idea (as presented in Hilbert, 1925a) is first of all to formalize and axiomatize all of mathematics – including the infinite parts such as Cantor's theory of infinite sets. This formalization makes it possible to treat the mathematical formulas as strings of meaningless logical and mathematical signs, following each other according to definite rules. As Hilbert puts is: "hence contentual inference is replaced by manipulation of signs according to rules, and in this way the full transition from a naïve to a formal treatment is now accomplished" (Hilbert, 1925a, p. 381).

With the formalization completed, it should secondly be made sure, that the system is without contradictions. Given that $1 = 1$ is clearly a true theorem of finite arithmetic, the inspection for consistency amounts to demonstrating that the formula $1 \neq 1$ cannot be proven from the axioms chosen[12]. Luckily this task can proceed as an inspection of the finite formulas and proofs of the axiomatic system. As Hilbert puts it: "... a formalized proof, like a numeral, is a concrete and surveyable object. It can be communicated from beginning to end", and hence the task of showing that the proof of $1 \neq 1$ does not exist is a task, that "fundamentally lies within the province of intuition" (*ibid.*, p. 383). Actually, mathematics itself can be used in order to perform this inspection or analysis of our system, but since all the proofs and theorems of the axiomatic system are finite objects, we will only need the safe, finite part of mathematics in order to inspect them. Proceeding in this way it should, Hilbert believed, be possible to prove the consistency of all of mathematics using only the safe, finite parts.

As it is well known, the dream of such a proof was shattered with the advent in 1931 of Kurt Gödel's incompleteness theorems. From these theorems it follows, that no consistent axiomatic system powerful enough to reproduce arithmetic can prove its own consistency (a very precise statement of this is found in the note added in 1963 to (Gödel, 1931, p.616)). So the Hilbert program, where the consistency of all of mathematics was to be proven by a subsystem of mathematics, was doomed to failure.

---

As noted in the beginning, the axiomatic-deductive method presupposes a foundation in the form of axioms, the truth of which is transported through the system as a whole by valid logical inferences. Consequently, the truth and certainty of the theorems of the system is always relative to the axiomatic foundation. Due to the discovery of non-Euclidean geometry and the paradoxes of set theory, neither geometrical intuition nor naïve set theory could

[12]For those not familiar with logic it should be noted that (logically) anything follows from a contradiction. Consequently, if a system contains one contradiction, it will contain all other possible contradictions, including $1 \neq 1$.

give the type of foundation needed in order to realize the ideal of mathematics as a body of eternal and objective truth. The three foundational schools tried out different strategies for solving this problem, but all of them failed; as it seems, mathematics cannot be reduced to logic, mathematics cannot be shown to be consistent (by safe means), and mathematics as we know it cannot be constructed from a basic intuition of time.

There were – and there still are – attempts of carrying the programs of the foundational schools through. The basic tenants of the logicist program can, for instance, be found in Bob Hale and Crispin Wright's neologicism, and the intuitionist school is, to a certain extent, carried on in Michael Dummet's philosophy (see for instance Hale & Wright (2001)). The validity and scope of Gödel's incompleteness theorem has also been attacked (see Shapiro, 2000, pp. 167 for an overview). However, most mathematicians and the mathematical society in general have settled for something less, or, should I say, something else than absolute, objective certainty. The nature of this 'less' or 'else' is the subject of the next section.

## 2.4 Modern foundations

The generally accepted foundation for modern mathematics is set-theoretic axiom system called ZFC. The system is named so after to of its creators – Ernst Zermelo (1871-1953) and Abraham Fraenkel (1891-1965) – and one of the axioms, the Axiom of Choice. ZFC is a set-theoretic axiomatic-deductive system so powerful that all ordinary mathematics can be developed within the system. At first sight, it looks as if ZFC is the realization of the dream of axiomatic-deductive certainty. However, on closer inspection it turns out that ZFC, from an epistemic point of view, is very far from the ideal of axiomatic certainty and truth.

The goal of Zermeleo, who published the first version of ZFC in 1908, was not to solve philosophical and epistemic problems. It was, as Jean van Heijenoort puts is, to give "an immediate answer to the pressing needs of the working mathematician" (Van Heijenoort, 1967, p. 199). At the time, what the working mathematicians needed was simply to get Cantor's set theory without the paradoxes. And that was exactly what Zermelo set out to provide:

> Under these circumstances [i.e. the existence of the paradoxes] there is at this point nothing left for us to do but to proceed in the opposite direction and, starting from set theory as it is historically given,

> to seek out the principles required for establishing the foundations of this mathematical discipline. In solving the problem we must, on the one hand, restrict these principles sufficiently to exclude all contradictions and, on the other, take them sufficiently wide to retain all that is valuable in this theory.
>
> (Zermelo, 1908a, p. 200)

In this quote Zermelo clearly states that he is not trying to provide safe foundations the ‘bottom-up’-style. He starts with the historically given theory and will then try to find the axioms needed.

In justifying the axioms, self-evidence is, Zermelo writes, “surely a necessary source of mathematical principles” (Zermelo, 1908a, p. 187). But, he goes on, self-evidence is merely a subjective fact, whereas what can be objectively decided is whether a principle (i.e. an axiom) is necessary for the mathematical science or not. “Actually, principles [axioms] must be judged from the point of view of science [mathematics], and not science from the point of view of principles fixed once and for all. Geometry existed before Euclid’s *Elements*, just as arithmetic and set theory did before Peano’s *Formulaire*, and both of them will no doubt survive all further attempts to systematize them in such a textbook manner” (*ibid.* p. 189).

Following Penelope Maddy (1997, p. 37), we can distinguish between intrinsic and extrinsic justifications for an axiom. Intrinsic justification regards how self-evident and reasonably the axiom seem in itself, whereas extrinsic justifications are judgments based on the consequences of the axiom. From the above, it seems as if Zermelo values extrinsic justification above intrinsic (even though he does not disregard intrinsic justifications all together).

Such a view might seem very surprising, even slightly disturbing. If the axioms are justified by their consequences, i.e. the theorems it is possible to derive from them, the theorems in turn cannot be justified by appealing to the axioms. That would be a perfectly circular way of reasoning. So if axioms are to have epistemic value as a secure foundation for a body of mathematical theorems, the axioms must be justified exclusively by intrinsic means. But apparently, that was not what Zermelo intended to deliver.

Interestingly, Zermelo was far from alone in this view on the relationship between axioms and theorems. In a discussion on the justification of the Axiom of Reducibility, Russell and Whitehead write:

> That the axiom of reducibility is self-evident is a proposition which can hardly be maintained. But in fact self-evidence is never more than a part of the reason for accepting an axiom, and is never

> indispensable. The reason for accepting an axiom, as for accepting any other proposition, is always largely inductive, namely that many propositions which are nearly indubitable can be deduced from it, and that no equally plausible way is known by which these propositions could be true if the axiom were false, and nothing which is probably false can be deduced from it.
>
> (Whitehead & Russell, 1910, p.62)

Here, Russell and Whitehead seem to abandon the fundamental ideas of logicism. Instead of a bottom-up approach, where mathematics is deduced from logically secure foundations, they seem to endorse a top-down approach where axioms (or at least some of them) are judged in the light of theorems that somehow seem 'nearly indubitable'.

### 2.4.1 The axioms of ZFC

Now, let us spend a little time evaluating the justification given for some of the actual axioms of ZFC[13]. First of all, we find the Axiom of Infinity. The axiom claims the existence of "at least one set $Z$ that contains the null set as an element and is so constituted that to each of its elements $a$ there corresponds a further element of the form $\{a\}$" (Zermelo, 1908b, p. 204). This gives us a sequence of elements $a, \{a\}, \{\{a\}\}, \ldots$ or in other word a denumerable infinite set like the natural numbers. Now, the actual existence of anything infinite does, as noted above, not seem self-evident. The standard justification for the axiom is, as Maddy puts it "purely extrinsic" (1997, p. 52), being that analysis and the theory of rational numbers cannot be developed in set theoretical terms unless infinite sets can be treated as existing wholes.

In the formulation of the Axiom of Infinity Zermelo used the null, or 'empty' set. The existence of this set is also ensured by axiom: "There exists a (fictitious) set, the *null set*, 0, that contains no elements at all" (Zermelo, 1908b, p. 202). The existence of such a set also seems very hard to justify – note that even Zermelo strangely calls it 'fictitious'. A set is a collection of objects, and a collection of objects without any objects seems like a contradictory entity. Maddy (1997, p. 39–40) sums up the justification for the empty set given by Fraenkel *et al.* (1973) in two points: 1) We need some kind of starting point in order to get the construction of sets started,

[13]A number of arguments pro and con the axioms are examined and categorized as intrinsic or extrinsic in (Maddy, 1997, p. 36)). In (Feferman, 1998, p. 44–45) six themes or features of current concern regarding ZFC and Cantor's set theory is collected and discussed. Here the intrinsic-extrinsic-distinction is not observed.

and 2) we want the intersection of two sets to be always a set, even when the two sets have no elements in common. These are, from a practical point of view, very good reasons for including the empty set, but the reasons are, of course, purely extrinsic.

However, the most debated axiom of ZFC is, without doubt, the Axiom of Choice. In its intuitive form, it states that if you have a collection of non-empty, mutually disjoint sets, you can always form a new set by choosing exactly one element from each of the sets in your collection. In Zermelo's 1908-version, the axiom states that if $T$ is a set whose elements are non-empty, mutually disjoint sets, then there exists at least one choice-set having one and only one element in common with each element of $T$ (Zermelo, 1908b, p. 204). In Zermelo's version the emphasis is on existence rather than choice, but the result is the same.

In the finite case, the axiom is uncontroversial. It just states that you can form a new collection by picking one element from a set of already existing collections – you can make a collection of candy, by picking one piece of candy from each non-empty jars in the candy store. But what if the collection of sets is infinite? The axiom just states the existence of the choice-set, but it does not define the set or in any way tell us how to construct it, and that is a problem. How can a mathematical object exist without being constructed or at least defined in a specific way by us? It seems as if the axiom presupposes realism concerning infinite sets: Infinite sets exists and have definite properties, including choice-sets, independently of the human mind. We might come to know these properties or we might not, but nonetheless, they are there.

Zermelo's defense of the axiom includes both intrinsic and extrinsic elements. Even before the axiom was stated explicitly, it had been used heavily, but implicitly, by Cantor and a number of other mathematicians (Moore, 1982, pp. 30). Zermelo notes this (Zermelo, 1908a, p. 187), and see it as a sign of the axiom's self-evidence, that is, as an intrinsic justification for the axiom. Zermelo then goes on to give his extrinsic justification. He lists seven theorems and problems that cannot be dealt with without the Axiom of Choice, and states that "as long as [...] the principle of choice cannot be definitely refuted, no one has the right to prevent the representatives of productive science [i.e. mathematics] from continuing to use this 'hypothesis' " (Zermelo, 1908a, p. 189). In other words: The Axiom of Choice makes it possible to prove a number of important results, so, as long as the axiom has not led us into any trouble, it should be used. According to Moore (1982, p. 285), these two arguments have remained as the primary arguments used in defense of the axiom.

We do not need to follow the debate to a conclusion here. From what we have seen it is, I believe, safe to say, that the Axiom of Choice is not self-evident, unless you share a specific metaphysical view on mathematics (i.e. realism). "Yet", as Moore concludes his thorough investigation of the axiom, "as the years passed, the Axiom [of Choice] continued to be used fruitfully in those branches of mathematics undergoing rapid development. Later mathematicians, who had not been involved in the controversy, were increasingly likely to apply the Axiom with no qualms of conscience" (Moore, 1982, p. 290).

---

We have now examined the justification given for three of the axioms of ZFC. I have fixed the scales a bit by picking some of the most dubious axioms for inspection, but somewhat similar stories could be told for several of the remaining axioms. We do not need to go through that here – I believe the general point has come through. The axioms of ZFC are not self-evident and indubitable principles. Just like the rigorization program of the 19th century, Zermelo had a top-down approach. He started from the theorems he believed to be true – i.e. the historically given Cantorian set theory – and then he found the axioms he needed in order to deduce these theorems. So, contrary to the myth of axiomatic certainty and truth, mathematics does not proceed by logical deduction from a number of self-evident axioms. Instead, mathematics starts with something. We know some propositions to be true, and then we might (or we might not) set out to find the more or less self-evident axioms needed in order to deduce those propositions. But what is given, what is the starting point, is not the axioms, it is some set of propositions.

The fate of intuitionism is a very telling illustration of this point. Brouwer set out to provide the kind of safe 'bottom-up' foundation, the myth of axiomatic truth and certainty would need for its realization. But as it turned out, that was not at all what the mathematical society wanted. They wanted the already accepted theory to be justified. The goal was set in advance, and when Brouwer's intuitionistic mathematics missed that goal, it was discarded. Logicism and the Hilbert-program set out to justify the already accepted body of mathematical knowledge, but even when that turned out to be impossible, the accepted mathematics was not modified. Instead, the quasi-justifications provided by ZFC were adopted as the foundation of mathematics.

Finally, we might also briefly return to Euclid. Why did Euclid accept the fifth postulate as an axiom? Note, that he postponed the use of the postulate as long as possible – it is only used for the first time in the proof of the $29^{th}$ postulate. This suggests that he did not see the postulate as self-evident, or at least he was suspicious towards it. So, why did he include a principle, he was suspicious towards, in the very foundation of his system? Obviously, we will never know Euclid's actual motives, but in the light of the above one very straightforward answer is this: He simply knew that he needed the postulate in order to prove an important theorem, he believed to be true, i.e. that the sum of the angles of a triangle equals two right.

## 2.5 Axioms and the essence of mathematics

In the empirical sciences it is sometimes possible to explain a phenomenon by reducing it to a more basic phenomenon. Standard examples of successful reductions of this type are the reduction of chemical properties (such as valence) to physical properties and the kinetic theory of heat. The reduction makes it possible to understand and predict important aspects of the reduced phenomenon (such as transfer of heat), and one might even say, that the reduction reveals the true nature of the reduced phenomenon. Heat is not presence of the element of fire or a mysterious weightless fluid, as supposed by the caloric theory. Heat is average speed of the molecules of a substance. Nothing more, nothing less.

Now, one might wonder if something similar can be done in mathematics. Can the true nature or essence of a part of mathematics be revealed by a reduction to a more basic theory? Such an idea would go very well – and can sometimes be seen as a part of – the myth of axiomatic truth and certainty: By axiomatizing mathematics we find the fundamental principles (i.e. the axioms) hidden beneath the immediate phenomena, and these principles reveal the true nature of the reduced theory.

As it turns out, such reductions are not unproblematic, neither in empirical science (which I will not comment further on here) nor in mathematics. Firstly, there is what one might call a problem of underdetermination; it is well known that the same parts of mathematics can be axiomatized by several different systems of axioms. As a competitor to ZFC, we have for instance the von Neumann axioms, which are actually stronger than ZFC, i.e. every theorem deducible in ZFC is also deducible in the von Neumann axioms (Wang, 1949, p. 152). Also, entirely different types of axioms can be used. The synthetic Euclidean axioms, which were perfected in Hilbert's

*Grundlagen der Geometrie*, offer one way of axiomatizing geometry, but as it is the case with most parts of mathematics, geometry can also be axiomatized in ZFC using analytic methods. In comparison to Euclid and Hilbert's axioms, ZFC has the advantage of unifying most of mathematics, but does it follow from this fact, that geometry is really set theory, i.e. that all this time, when we have been talking about circles and triangles and other geometrical shapes, we have *really* been talking about sets? No. ZFC and the axioms of Euclid and Hilbert are different ways of describing geometrical properties – they are different models (where the word 'model' is in a way similar to the way it is used in physics). But the geometric properties, we wish to describe, do not fully determine which axiom system we should use. The choice of axiom system is underdetermined by geometry, so there is no 'right' or 'true' axiom system.

Secondly, the idea that axioms might somehow reveal the true nature or essence of mathematics is challenged by the existence of so-called 'non-standard' models[14]. A non-standard model is a model that is not isomorphic to the intended interpretation of a given set of axioms. The existence of such models was discovered by Thoralf Skolem (1887–1963) in a number of papers published from 1922 to 1934.

As an example, (first-order) Peano arithmetic is usually axiomatized with the following axioms (Manzano, 1999, p. 173):

1. $\forall x\ c \neq fx$
2. $\forall xy(fx = fy \rightarrow x = y)$
3. $\forall x\ x + c = x$
4. $\forall xy\ x + fy = f(x + y)$
5. $\forall x\ x \cdot c = c$
6. $\forall xy\ x \cdot fy = (x \cdot y) + x,$

and the first-order induction axiom $\varphi(c) \wedge \forall x(\varphi(x) \rightarrow \varphi(f(x)) \rightarrow \forall x \varphi(x)$.

Normal arithmetic on the usual set of natural numbers $\{0, 1, 2, 3, 4, \ldots\}$ is a model of these axioms (with $c$ interpreted as 0 and $f$ as the successor-relation). The natural numbers (and isomorphic structures) are considered the standard model for the axioms as they probably constitute what Peano

[14] And here the word 'model' is to be understood in the exact reverse sense in comparison to its use in physics (and natural language). In mathematical logic, a model is, loosely put, a structure that constitutes an interpretation of a given set of axioms

and others intended to axiomatize. However, several non-standard models can also be constructed, for instance by adding a so-called infinite collection of $\mathbb{Z}$-chains after the standard natural numbers. In non-technical terms, each of these chains will have the same structure as the integers, so we might have the non-standard number, say $1^*$, which is the successor of the non-standard $0^*$, which is the successor of the non-standard $-1^*$ etc. Each non-standard number is the successor of another non-standard number, but a non-standard number will never be the successor of a standard number. Consequently, you can never count to the $1^*$ by starting with 0 (Manzano, 1999, p. 177), and all in all we get a structure of the following type:

$$0, 1, 2, 3, \dots, -2^*, -1^*, 0^*, 1^*, 2^*, \dots, -2^{**}, -1^{**}, -0^{**}, 1^{**}, 2^{**} \dots$$

This model verifies all the Peano axioms (in first order logic), and consequently is a valid interpretation of the axioms.

How should we react to this? Should we embrace the non-standard models as new knowledge given to us by the axioms, or should we rather reject the non-standard models as a chimera?

As it seems, the underdetermination goes both ways; given a set of accepted theorems, there is always more than one way to axiomatize them, and given a set of axioms, they can always be verified by other models than the intended one (in first order logic and due to the Löwenheim-Skolem theorem)[15]. At the very least, this should serve as a reminder of the epistemic footing we are on, when we are axiomatizing. The axioms are partly justified by a fit between some of their consequences and some of the mathematical facts we – one way or another – believe to be true. Does this mean that the axioms capture the essence of mathematics? That they explain the true nature of the things we are dealing with? That the reformulation of mathematics in set theory thought us that mathematics *really* is and has always been properties of sets, in the same way physics taught us that heat is really just kinetic properties of molecules? Absolutely not! The axiomatic-deductive method is an important tool in the mathematician's toolbox, but no set of axioms can capture the essence of mathematics, and the mathematical method should not be identified with the axiomatic-deductive method.

[15]On a technical note it should be remarked that the problem of non-standard interpretations can be solved by using second-order logic. Second-order logic, however, has other problems, which I will not comment on further at this place (see e.g. Jané, 2005).

# Chapter 3

# Remarks on naturalism as scientific method

## 3.1 The motivation for naturalism

In the previous chapter, I explained why the myth of axiomatic certainty and truth is but a myth. Unfortunately, this did solve much in terms of the research question pursued here. On the contrary, if the starting point of mathematics is not self-evidently true axioms, but propositions we somehow intuitively know or take to be true, we need to ask: why? Why do we believe certain propositions to be true? How do we acquire this knowledge? How certain is it? Does it develop, and if so: how and due to which mechanisms? Is mathematical knowledge objective, and if so: in what sense? Is it strongly or only weakly objective, i.e. is it the kind of knowledge any thinking being would agree to, or only the kind of knowledge any human being would agree to?

In order to answer these questions a change of focus is necessary. Instead of examining the logical foundations and justifications of mathematics, we should study how mathematics is practiced and develops. In recent decades a still growing number of philosophers of mathematics have changed their focus from the logical foundations to the practice and history of mathematics (see e.g. Domínguez & Gray, 2006; Mancosu, 2008). This have resulted in a number of fruitful studies. In the present study, I will however take this development a step further. I will view the mathematical practice from the outside in order to put it into a cognitive, biological and sociological context. In other words, I will take a naturalist stance and view mathematics as an inherently human activity, i.e. an activity carried out by particular biological creatures in possession of certain cognitive and bodily resources, placed in certain physical and cultural contexts and pursuing certain interests.

Before starting these investigations, a short presentation and discussion of naturalism as a scientific method is in place.

## 3.2 What is naturalism?

In a modern setting, naturalism is connected to the essay "Epistemology Naturalized" by Willard Van Orman Quine (1969). Here, Quine states that "Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, *viz.*, a physical human subject" (1969, p. 82-83). Naturalism in broad follow Quine in this general idea. Consequently, the task of understanding why we form the type of beliefs about the physical world we do, and the task of determine the conditions of possibility governing our epistemic relation with the physical world, are empirical questions that should be solved using scientific knowledge about the world and about human beings. To Quine, the relevant type of scientific knowledge about human subjects was psychology. Later definitions of naturalism seems to favor cognitive science (e.g. Kornblith, 1994, p. 43), but naturalistic epistemology can also include sociological, anthropological and other types of empirical knowledge explaining the epistemic situation of human agents.

Turning to mathematics, a naturalistic account simply amounts to a scientific study of why we form certain mathematical beliefs. This is to be seen in contrast with other, more traditional accounts such as Platonism and formalism. To spell out the contrast, we can take a look at the following often-quoted Platonistic account given by Gödel:

> The objects of transfinite set theory [...] clearly do not belong to the physical world and even their indirect connection with physical experience is very loose [...]. But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have any less confidence in this kind of perception, i.e. in mathematical intuition, than in sense perception.
>
> (Gödel, 1947, p. 483-4)

Perhaps Gödel is right – perhaps transfinite sets do exist in an immaterial Platonic world, and perhaps we do have a special faculty that makes it possible for us to perceive them. The problem is, that we can never know. It is a simply metaphysical assumption that can never be justified empirically. A Platonistic account, such as the one given by Gödel, does answer many of the questions surrounding mathematics. But sometimes answers are to easily had.

The great attraction of naturalism, as I see it, is that it does not need to invoke mysterious entities, such as a platonic realm of eternal entities with no causal properties and no location in space and time, or mysterious faculties, such as the Gödelian intuition, that can bring us into contact with these entities. It tries to explain mathematics using generally acknowledged empirical methods and scientific knowledge that fits into our overall web of beliefs about the state of the world. It does not simply accept the feeling Gödel – and many other mathematicians – have that mathematical entities exist, at face value. On the contrary, the feeling that mathematical entities exists in a platonic world and the feeling that we somehow are capable of intuiting them are part of the phenomenology of mathematics that needs explaining on the naturalistic account; we must understand *why* mathematicians feel this way, and not necessarily honor those feelings as revealing the truth (as also explained in Bloor, 1991, p. 86).

A lot of work that could be categorized under the general heading of naturalism in the philosophy of mathematics has been done during the last few decades. This work includes Lakoff & Núñez (2000), where explanations from cognitive science are used, de Cruz (2007), where a Darwinian approach to mathematics is taken, and Bloor (2002, 1983), where sociology is used to give naturalistic explanations of mathematical beliefs.

In the chapters below, I will present and discuss several of these approaches in order to give a nuanced naturalized picture of our mathematical beliefs. This picture includes scientific knowledge from biology and neuroscience about the evolutionary shaped biological hardware of the human brain. It includes knowledge from cognitive science about the specific cognitive strategies generally used by human beings. And finally, it includes sociological knowledge explaining how mathematics depends on our social surroundings and relations with social groups.

This approach, where knowledge and models of explanation from more levels are used, allows me to break up my overarching research question. Instead of asking: where does mathematics come from? I can more indirectly ask: How much (if any) of mathematics can be explained by evolutionary biology?, by cognitive science? and by sociology of science? – or, as I like to put it: do our biology, cognitive style and social behavior have an *impact* on our mathematical beliefs? So instead of starting at the bottom and explain the origin of our mathematical beliefs, I start with the already given naturalistic theories, and discuss whether they are able to contribute with a piece of the answer to the general question. As historian of science Ryan Tweney has strikingly put it "there is not likely to be a single theory that accounts for everything. You must assemble bits and pieces of this and that, hoping that your account is going to get closer and closer to the truth" (Callebaut & Bechtel, 1993, p. 339). And that is exactly how I propose to proceed. The challenge in this way of proceeding is of course to make the pieces fit together in a consistent way.

### 3.2.1 Levels of explanations

Before moving on, I will discuss some possible objections to my particular conception of naturalism, and some of the standard objections raised against the use of naturalistic methods as such in the philosophy of mathematics.

Beginning with the first, the naturalistic description I wish to give is *inclusive*; it involves knowledge from several branches of science, and operates with several levels of causes and explanations. This type of approach is not universally endorsed amongst naturalists. Helen de Cruz (2007) for instance, does not accept this kind of non-reductive approach. Naturalism is supposed to use scientific knowledge in the study of human beliefs, but according to de Cruz, science – and hence naturalism – is committed to reductive physicalism:

> Science has an implicit ontological commitment to physicalism. In order to engage in science, we assume that the real world is material, and that there are no autonomous subject-free ideas, and that every event in the world is the outcome of prior causal events or laws; there are no uncaused causes [...]. In other words, scientific investigation explains events or properties of the world by looking for causal mechanisms, which can be traced to material, tangible objects in the world. The fundamental thesis of physicalism is that the only existents are material things. [...]
>
> This means that we simply cannot evoke culture as an ultimate causal mechanism to explain the complexities of human mathematical thought. The complex body of beliefs and behaviors that constitutes each culture is caused by lower-level processes. Like all other events in the world, human culture must be explained as the outcome of specific causal mechanisms, which can be described in purely materialistic terms. Mathematical constructions, according to this physicalist perspective, are products of individual human mathematicians, equipped with a human brain.
>
> (de Cruz, 2007, pp. 32-33)

First of all, even granted materialistic reductionism to be true, it might from a pragmatic point of view be vise to operate with more levels of explanation than the purely material. If we want to describe and explain, say, the phenomenon of normativity in mathematics, the correct level of explanation is, as I see it, clearly the social level, and not the purely material level, where the neurophysiological processes of the individual actors are described.

This being said, I do not agree with the doctrine that science is necessarily committed to this kind of materialistic (ontological) reductionism. This type of all-encompassing reductionism is highly questionable. To mention only one problem, there is a seemingly insurmountable explanatory gap between the material, neurophysiological processes of the brain and our conscious, first-person experiences (see e.g. Zahavi, 2004). Consequently, no one can explain how human culture is supposed to be explained in "purely materialistic terms", as de Cruz puts it. Perhaps, it simply cannot be done.

Furthermore, there are alternatives to materialistic reductionism, and some of them do seem better suited to bridge the explanatory gap mentioned above. A promising candidate is the theory of emergentism, claiming that reality ultimately consists of a hierarchy of levels. The properties of each level emerges from the interactions of the relatively simpler elements of the level below, but they do so in a non-reducible way (the idea was originally suggested in Anderson (1972). For further developments, see Emmeche *et al.* (1997, 2000). For criticism, see Kim (2006)). Although theories of emergence are heavily debated, they do constitute a possible alternative to materialistic reductionism, and consequently science is not *committed* to materialism as claimed by de Cruz. At least it seems safe to say that the jury is still out on this question. On a stronger note, I believe that the account of mathematics given in this dissertation will furthermore show the advantages of operating with more explanatory levels in a non-reductionist way.

### 3.2.2 Internalism vs. externalism

In the area of mathematics, a distinction between internal naturalism and external naturalism is furthermore relevant (Van Kerkhove, 2006). The heading 'external naturalism' is used to characterize approaches, such as the one outlined above, where knowledge relatively external to mathematics is applied in order to explain how we arrive at our mathematical beliefs. Internal naturalism on the other hand, takes an entirely different approach. Here, mathematics and mathematical methodology is only to be evaluated by its own standards and ideas of fruitfulness, and not by anything outside of mathematics itself. Mathematics is in other words only to respond to purely mathematical considerations.

The most prominent proponent of the internalistic school is no doubt Penelope Maddy (Maddy, 1997, 1998). The internalistic (and Maddy's own) position is clearly presented in the following passage, where Maddy comments on the acceptance of the axiom of choice and the use of impredicative definitions (*viz.* a particular kind of self-referring definitions such as 'least upper bound'):

> Impredicative definitions and the Axiom of Choice are now respected tools in the practice of contemporary mathematics, while the philosophical issues remain subjects of ongoing controversy. The methodological decision seems to have been motivated, not by philosophical argumentation, but by consideration of what might be called, for want of a better expression, mathematical fruitfulness [...].

> What are we to make of this? One response would be to insist that the mathematical community has been too hasty in its embrace of these disputed methods, that they are being used, as it were, without justification, without their essential philosophical underpinnings. The response I propose turns this position on its head: Given that the methods are justified, that justification must not, after all, depend on the philosophy. Mathematical naturalism, as I understand it, is just a generalization of this conclusion, namely, that mathematical methodology is properly assessed and evaluated, defended or criticized, on mathematical, not philosophical (or any other extra-mathematical) grounds. The particular instances of mathematical fruitfulness that played the decisive roles in the impredicativity and choice controversies stand as ready examples of the type of 'mathematical grounds' I have in mind.
>
> (Maddy, 1998, p. 164)

This type of naturalism is supposed to be purely descriptive. Its main aim is to propose candidates for new axioms and to help settle the debates over whether to accept new axioms candidates or not. This is done by creating descriptive models of the modes of reasoning and methods of justification already accepted as part of the (fruitful) mathematical practice, and use these model as a way to evaluate and ultimately justify the rationality of arguments involved in current discussions (Maddy, 1997, p. 199).

It has however been questioned, whether the resources available to internalistic naturalism are adequate for reaching this goal of justification (Roland, 2007). The main problem lies in the fact, that the modes of reasoning that are accepted as delivering rightful justification for particular axioms, are themselves only justified by the fact that they have previously lead to mathematically fruitful results. But what could mathematical fruitfulness be? As explained by Roland (2007), Maddy can neither appeal to an understanding of fruitfulness as 'correctness' understood as correspondence with an independent platonic, mathematical realm, or (because of the proclaimed independence from anything extra-mathematical) to usefulness in applications in other sciences. The claim that a mathematical result is fruitful is not accountable to anything except the collective endorsement of the practitioners.

In contrast, naturalism in broad and externalistic naturalism in mathematics use empirical knowledge to evaluate and explain a given epistemic practice. Empirical knowledge is always accountable to something outside of itself. It can be criticized or even falsified if it does not correspond to the part of reality is supposed to describe in the way intended. Furthermore, science is not monolithic. As pointed out by Roland (2007), *disciplinary holism* ensures that different branches of science might criticize each other. So all in all, science has meaningful and robust means of criticizing itself. This might not add up to the kind of ultimate justifications, sometimes wanted for mathematics, but it does at least render a particular scientific discipline accountable to something other than itself.

Because of the declaration of independence, these means of external criticism are not available to the internalistic mathematical naturalist. Consequently, the internalistic justification of an axiom comes down to the fact that the axiom can be justified by the kind of reasoning that have previously led to the kind of results, we as a collective happen to perceive as fruitful. It is hard to see why the same maxim of reasoning could not just as well be used to justify astrology, numerology or 'guru-ology' (as suggested by Paseau, 2008), as long as these disciplines are perceived as being fruitful by their participants, for instance by being able to answer the guiding questions of the various practices. Consequently, the justifications delivered by internalistic naturalism are very week; mathematics is as justified as astrology or guru-ology – but that in itself is, I believe, not very reassuring, and probably not what Maddy set out to achieve. It simply seems that you cannot both justify mathematics in any real or strong sense of the word and have independence from anything extra-mathematical at the same time.

As explained above, the axioms of set theory are at least partially justified by the fact that they capture results, we somehow already accepts. So if one wants to justify the axioms, one should examine why we have come to accept some results as correct (or fruitful), and what part this type of mathematical beliefs play in our general epistemic practice. One should in other words apply an externalist naturalistic approach, and examine mathematics from the outside. This being said, the project of creating naturalistic models of the actual reasoning used in mathematical practice is an interesting and valuable project – there is much to be learned from the analysis of Maddy. However, such models cannot by them selves provide justification for the validity of the reasoning maxims they describe.

To clarify expectations, I should add that my primary goal is not to justify mathematical practice or particular mathematical beliefs. I simply wish to explain and understand why we form these beliefs – although the two things are not unrelated, as I will explain in the next subsection.

### 3.2.3 Discovery and justification

As an interesting objection to Quine's initial exposition of naturalism, Jaegwon Kim (1988) argues that epistemology and the naturalized investigation suggested by Quine are in fact two very different things; naturalism investigates how and why we form certain beliefs, i.e. it investigates a causal relation between us and the world, whereas epistemology is concerned with how beliefs can be justified, and in other words investigates a justificatory relation between us and the world. For that reason, naturalized epistemology cannot replace classical epistemology.

As I see it, this objection hinges on the differentiation between the context of discovery and context of justification. In Kim's view, naturalism only concerns the discovery process – how we come to hold certain beliefs –, and that is something else than justifying these beliefs. The division between these two contexts has its origin in the positivist movement, where it was meant as a mean to separate the (ideally) wholly rational and logical process of justification from the irrational process of discovery. The latter could be influenced by sociological and psychological factors, religious feelings or even dreams (as in the famous story of Friedrich Kekulé's discovery of the structure of benzene), but as long as the former was kept clean as a strictly rational and logical process, the idea of science as a cumulative and objective body of knowledge could be upheld. In other words, with the divide, the positivists acknowledged what they could not deny; that external and irrational factors inevitably play some kind of role in science, but at the same time claimed it to be possible to isolate the irrational element, and keep the crucial process of justification clean and rational.

Good as this might sound, the possibility of observing an ultimate division between the two contexts has been questioned, not the least in the influential work by Thomas Kuhn (1922-1996). Briefly put, Kuhn pointed out that justification is always set within the context of something else, which cannot itself be ultimately justified – the measuring stick must be chosen before any measuring can be done. This point is in fact very well exemplified in the history of mathematics. As described in chapter 2 above, a specific theory, i.e. Cantorian set theory, was chosen by the mathematicians (or rather a subset of them) as the right one. There were good reasons for this choice (and also good reasons against it), but no ultimate justification. Rather, once the choice was made, it determined what was to count as justification. Today, a theorem is justified if it can be derived from the axioms of ZFC, which were in turn tailored to match the Cantorian theory. In other words, the discovery of the Cantorian theory was prior to the context of justification offered by ZFC.

But other more fundamental choices went into the manufacturing of ZFC as well. The rules of inference of ZFC might seem self-evident and objectively true, but they should be seen in contrast with other options, such as the intuitionistic logic and the Aristotelian ideal of scientific logic. Clearly, a choice was made here. Furthermore ZFC is, as we have seen, the product of a long process of rigorization and axiomatization, but this process was not something objectively necessary. It was a contingent event in the history of mathematics, fueled by discoveries within mathematics (such as the advent of non-Euclidean geometry), the choice of specific values as principal (rigor and certainty) and other factors. Most parts of mathematics had for centuries done well without modern standards of justifications. Also, the choice of a strict axiomatic system as justification can be debated. Why can diagrams or computer experiments not count as justification?

All of this demonstrates that the distinction between context of justification and context of discovery should be used with the outmost caution, if at all. There is no ultimate, neutral and objective context of justification – not even in mathematics. There is only the justification we ourselves have manufactured. Consequently, the relevance of naturalistic methods both in explanations of how we acquire mathematical knowledge and in judging how justified it is, is ensured.

### 3.2.4 Psychologism

The last criticism against naturalism, I will treat at this point, is the charge of psychologism. This criticism was primarily developed by Edmund Husserl (1859-1938) and Gottlob Frege. Husserl and Frege directed their attack against several naturalistic theories proposed in the 19th century. The today most well known of these is probably the empiricistic theory of John Stuart Mill. Mill and the other naturalists saw mathematics and logic as psychological phenomena, and consequently all theories adhering to this basic principle were given the name 'psychologism'. Neither Husserl nor Frege did not agree with the psychologistic idea of conceiving logic and mathematics as psychological phenomena, and consequently positioned themselves as anti-psychologists. Husserl and Frege more or the less agreed in their criticism of the naturalistic theories, although Husserl's positive account of mathematics and logic is embedded in a more elaborate and thought-through epistemic theory, than the epistemic theory provided by Frege.

Frege's anti-psychologistic arguments were published in *Grundlagen der*

*Arithmetik* from 1884 and in the foreword of *Grundgesetze der Arithmetik* from 1893. Here, I will only focus on the arguments proposed in *Grundlagen*. What I see as the main argument of the text can be summed up in the following way: Frege rejects both the idea that numbers can be properties of physical objects and the idea that numbers are the products of subjective psychological factors. To Frege, numbers must be something objective: "Und wir kommen zu dem Schlusse, dass die Zahl weder räumlich und physikalisch ist, wie Mills Haufen von Kieselsteinen und Pfeffernüssen, noch auch subjectiv wie die Vorstellungen, sondern unsinnlich und objectiv" (Frege, 1974, p. 38). One can wonder what objectivity more precisely means in this connection. Frege tells us that being objective means being independent of our subjective ideas and psychological processes. "Während jeder nur seinen Schmerz, seine Lust, seinen Hunger fühlen, seine Ton- und Farbenempfindungen haben kann, können die Zahlen gemeinsame Gegenstände für Viele sien, und zwar sind sie für Alle genau dieselben, nicht nur mehr oder minder ähnliche innere Zustände von Verschiedenen (Frege, 1974, p. 105). Furthermore, Frege notices the normativity involved in mathematics: "Schaffen wir auch Zahlen, welche divergirende Reihen zu summiren gestatten! Nein! auch der Mathematiker kann nicht beliebig etwas schaffen, so wenig wie der Geograph; auch er kann nur entdecken, was da ist, und es benennen" (Frege, 1974, p. 107-8). In this way, numbers are in some sense similar to physical objects: "So ist auch die Zahl etwas Objectives. Wenn man sagt 'die Nordsee ist 10,000 Quadratmeilen gross,' so deutet man weder durch 'Nordsee' noch durch '10,000' auf einen Zustand oder Vorgang in seinem Innern hin, sondern man behauptet etwas ganz Objectives, was von unsern Vorstellungen und dgl. unabhängig ist"(Frege, 1974, p. 34). By 'North Sea' we do designate an external, physical object, which is clearly independent of us and our subjective psychology, but what can we designate by the word '10,000'? Having excluded both internal, psychological states and external physical objects, ultimately Frege chooses a form of realistic conception of numbers – "wir haben schon festgestellt, dass unter den Zahlwörtern selbständige Gegenstände zu verstehen sind" (Frege, 1974, p. 73), and those objects are, in Frege's view (and very briefly put) extensions of concepts.

Husserl presents the bulk of his anti-psychologistic argument in the prolegomena to *Logische Untersuchungen* (originally published in 1900). I will not go through all of the many details of his argument here. As I see it, the main point of the argument is the claim that we must distinguish between the mental *acts* involved in mathematical judgments, and the objects of those acts. The mental acts are themselves psychological events that can well be described by experimental psychology, but the objects are not. Husserl here agrees with Frege's Platonism; the mathematical theorems (and the laws of logical) are *ideal* objects that are independent of anything factually given. The logical laws and mathematical truths are timelessly and objectively true. They transcend the individual subjects psychological states of approval. Consequently, it is (to use Husserl's expression) a μετάβασις εὶς ἄλλο γένος, *viz* a fallacy similar to what we, following Gilbert Ryle, would call a *category mistake*, to explain mathematics and logic using the tools of an empirical sciences such as psychology.

Husserl's attack is mainly aimed at psychologistic explanations of logic, but as he sees mathematics as a branch of general logic, the same general attack applies to both naturalistic theories of logic and mathematics (Husserl, 1993, § 45). Here, however, I will restrict myself to describing his position concerning mathematics. This is very well summed up in the following two passages. In the first, Husserl attacks the naturalistic idea (called *anthropologism*) that the truths of mathematics are relative to us as a particular species:

> Die Konstitution der Spezies ist eine Tatsache; aus Tatsachen lassen sich immer wieder nur Tatsachen ableiten. Die Wahrheit relativistisch auf die Konstitution der Spezies gründen, das heißt also ihr den Charakter der Tatsache geben. Dies ist aber widersinnig. [...] Wollte man sich darauf stützen, daß doch wie jedes Urteil, auch das wahre aus der Konstitution des urteilenden Wesens auf Grund der zugehörigen Naturgesetze erwachse, so würden wir entgegen: Man vermenge nicht das Urteil als Urteilsinhalt, d. i. als die ideale Einheit, mit dem einzelnen realen Urteilsakt. Die erstere ist gemeint, wo wir von dem Urteil "$2 \times 2$ ist 4" sprechen, welches dasselbe ist, wer immer es fällt. Man vermenge auch nicht das wahre Urteil, als den richtigen, wahrheitsgemäßen Urteilsakt, mit der *Wahrheit* dieses Urteils oder mit dem wahren Urteilsinhalt. Mein Urteilen, daß $2 \times 2 = 4$ ist, ist sicherlich kausal bestimmt, nicht aber die Wahrheit: $2 \times 2 = 4$.
>
> (Husserl, 1993, p. 119, emphasis in the original)

In the second, Husserl explains why the objects of mathematics must be ideal:

> Niemand faßt die *rein mathematischen* Theorien und speziell z. B. die reine Anzahlenlehre als "Teile oder Zweige der Psychologie", *obgleich* wir ohne Zählen keine Zahlen, ohne Summieren keine Summen, ohne Multiplizieren keine Produkte hätten usw. [...] Und trotz dieses "psychologischen Ursprungs" der arithmetischen

> Begriffe erkennt es jeder als eine fehlerhafte μετάβασις an, daß die mathematischen Gesetze psychologische sein sollen. Wie ist das zu erklären? Hier gibt es nur *eine* Antwort. Mit dem Zählen und dem arithmetischen Operieren als *Tatsachen*, als zeitlich verlaufenden psychologischen Akten, hat es natürlich die Psychologie zu tun. Sie ist ja die empirische Wissenshaft von den psychischen Tatsachen überhaput. Ganz anders die Arithmetik. Ihr Forschungsgebiet ist bekannt, es ist vollständig und unüberschreitbar bestimmt durch die uns wohlvertraute Reihe idealer Spezies $1, 2, 3 \ldots$ [...] Die Zahl Fünf ist nicht meine oder irgend jemandes anderen Zählung der Fünf, sie ist auch nicht meine oder eines anderen Vorstellung der Fünf.
> (Husserl, 1993, p. 170, emphasis in the original)

So to summarize the arguments given by Frege and Husserl, mathematics is concerned with ideal objects that are untouched by the contingency of our psychological states. So although we apprehend mathematics through mental acts, it would be a category mistake to explain the content of such acts by the use of psychology (or any other empirical sciences).

At the outset, the argument does not seem to exclude naturalistic investigations altogether, only to delimit their scope. The naturalistic project, as I have described it above, is concerned with the origin of our mathematical beliefs, not with mathematical truth in itself. Such an investigation could be of value, even if the mathematical objects exist as ideal, platonic elements, as described by Frege and Husserl. The main conflict with naturalism lies with Frege and Husserl's conviction that mathematical knowledge is apodictic; that our mathematical beliefs are absolutely clear and certain *knowledge* about the ideal, platonic world of mathematics. As Husserl describes it above, our belief that $2 \times 2 = 4$ is not just a contingent psychological event. It is an absolute truth that we somehow have come into contact with. This kind of absolutism does, however, seem hard to justify. As we saw in chapter 2, it does seem impossible give the feeling of certitude any real substance in the form of secure foundations for mathematics. We simply do not have the means to justify our mathematical beliefs independently from the content of those beliefs (as argued in section 3.2.3 above). In fact, as pointed out by Kusch (2007) something similar can be said about logic: "In the light of the development of over the past 70 years, it is no longer evident that the laws of Frege's and Husserl's classical logic are necessary and unique" (Kusch, 2007, §7). In modern mathematics, there is a *choice* of logical framework, and this choice in itself is seen as part of the theory construction (see e.g. Goodman, 1990, p. 185).

All of this does not rub Frege and Husserl of their distinction between our contingent and psychologically caused mathematical beliefs, and the absolute mathematical truth, although the need for justifying the existence of such an absolute truth is perhaps somewhat strengthened. Concerning that, Frege and Husserl seem to be using an inference to best explanation; they take departure in the apparent objectivity of mathematics, and from that they infer the existence of a platonic realm of ideal, mathematical elements as the only explanation. Husserl and Frege do seem to have a very good point here. Even though mathematics might not be completely certain, the content of our mathematical beliefs does seem to lie beyond my individual psychological dispositions. They have an objectivity, or rather *normativity*, my individual, contingent psychological dispositions cannot account for. As Husserl describes it, the number 5 does not seem to belong to me or any other individual persons, and my belief that $2 \times 2$ is 4 does seem to be something completely different from my disposition, say, to pick a particular brand of milk when I am in the supermarket. If I picked a different brand of milk, I would simply be differently disposed, but if I believed $2 \times 2$ to be anything but 4, I would be *wrong*. The fact that $2 \times 2$ is 4 is a truth that transcends my individual psychology. It appears to be true no matter what I believe of it, and that appearance is something any naturalized account of mathematics should be able to account for.

As I see it, there are other and more plausible way to account for the normativity and perceived objectivity of mathematics than proposing the existence of a platonic world of ideal mathematical objects. A satisfactory answer does however presupposes the introduction of a great deal of relevant theory, so I will not give it at this place, but return to the challenge and give an answer in chapter 7.

# Chapter 4

# The biological level

## 4.1 The biological level

In December 2007 the astonishing result of a research project on monkeys' mathematical skills made news around the world. As it turns out, the mathematical skills of monkeys rivals that of college students – or at least, that was the surprising conclusion of the study (Cantlon & Brannon, 2007).

In the study a simple, non-verbal test of numerical and arithmetical skills was used. The test-subject would at first see a set of dots on a computer screen. Then a blank screen was presented for a short time, followed by a screen showing another set of dots. Finally, the subject was presented with two sets of dots shown side by side; a sum set in which the number of dots corresponded to the arithmetic sum of the number of dots in two first sets, and a distractor set, in which it did not. The subject had to choose between the two sets by touching the screen (see figure 4.1).

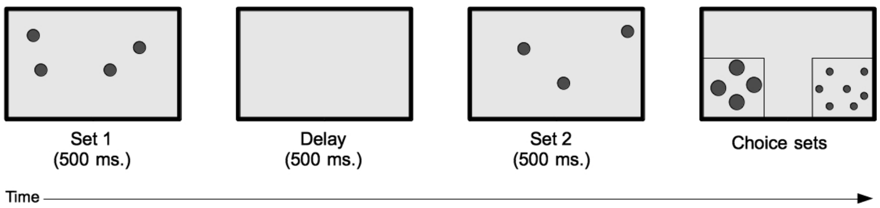



**Figure 4.1:** Experimental paradigm for a non-verbal addition task.

Two rhesus macaque monkeys were trained to choose the sum set using a limited number of addition problems and distractor sets ($1 + 1 = 2, 4$, or $8, 2 + 2 = 2, 4$, or 8 etc.). After 500 trials, the range of result sums were expanded to 2, 4, 8, and 16, and finally the monkeys were tested for approximately 600 trials using the novel sums 3, 7, 11, and 17. In order to prevent simple reinforced learning, the monkeys were rewarded regardless of whether they picked the right result or not during these last tests.

On both the novel and the familiar problems the monkeys performed significantly better than chance (giving respectively 70% and 75% correct answers for the novel problems). Furthermore, the monkeys' performance seemed to depend on the ratio of the numerical value of the sum and the distractor set; the performance declined as the ratio between the two sets approached one. This dependence of the ratio between the correct and the distractor choice, known as Weber's Law, is widespread and has been observed in the numerical performance on many tests of both human and non-human animals. In addition to the ratio of the sum and the distractor, the numerical size of the sum also seemed to have some impact on the performance of the monkeys. As the sum increased, the precision of the monkeys decreased (Cantlon & Brannon, 2007, p. 2916).

In order to make sure that the monkeys were actually basing their choice on the number of dots and not on their cumulative area, the size of the dots were varied to create trials, in which the total area of the dots of the distracter set was closer to the total surface area of the dots in the two addends than that of the sum set. The monkeys still performed significantly better than chance on this subset of trials, indicating that they actually relied on the number of dots, not on their surface area. Furthermore, a number of other false strategies (such as: 'always choose the larges of the two sets on the result screen') were ruled out through analysis of the data.

---

The study by Cantlon and Brannon is only one amongst a vast array of studies investigating the evolutionary roots of mathematics. The question seems to be simple: How much of mathematics is there to begin with? How much is inborn, developed through evolution and given to us from birth? Or differently put: Does our evolutionary history have an impact on our mathematical beliefs? Any naturalized account of mathematics must give some sort of answer to those questions.

The empirical investigations of those questions draw on data from four different sources. First of all there is the study of the mathematical skills of non-human animals, secondly there is the comparison between the performance of humans and animals, thirdly there is the investigation of the mathematical performance of human infants and finally there is the study of the adult human brain and the attempt to locate the neurological basis for the observed mathematical performance. In the following, I will go through the main points of the first three types of this evidence, starting with an overview and discussion of the studies of the mathematical skills of non-human animals. In this, I will only focus on the numerical and arithmetic skills, not on any of the geometrical skills animals might possess. The last type of evidence, *viz.* the study of the adult human brain will be covered in chapter 6. I should be understood that it is surprisingly difficult to do empirical work in this area (as the well-known story of the mathematical skills of Clever Hans reminds us). There are many possible methodological errors, and it is easy to draw too strong conclusions from the available data. For that reason I will describe the different empirical studies in some detail.

This is, I believe, necessary in order to be able to discus and evaluate the studies properly.

## 4.2 Mathematics in the wild

### 4.2.1 Relative size

The most basic way to exploit numerical aspects of sensory input is to judge the relative size of two given sets, i.e. to judge which set contains most elements. Several species of animal has been tested positive on this ability, including rhesus macaque monkey (Hauser *et al.* , 2000), pigeon (Emmerton & Renner, 2009), horse (Uller & Lewis, 2009) capuchin monkey (Beran, 2008), chimpanzee (Wilson *et al.* , 2001, 2002), lion (McComb *et al.* , 1994), rat (Church & Meck, 1984), pigeon (Roberts & Mitchell, 1994) and meadow vole (Ferkin *et al.* , 2005).

Various test-paradigms have been used in these experiments. In several of the tests, a spontaneous free choice paradigm, where a test-subject is allowed to choose freely between two different sized sets, was used (see figure 4.2). As an example, a free choice test was used on semi-free ranging rhesus macaque monkeys by Hauser *et al.* (2000). In each test-run, a monkey was allowed to see different number of food items (slices of apple) being placed in two opaque boxes by two researchers, and subsequently given the opportunity to approach the boxes. As it turned out, the monkeys significantly more often chose to approach the box containing most slices on 1 versus 2 slices, 2 versus 3, 3 versus 4, and 3 versus 5 slices, but they were not able to go for more on 4 versus 5, 6, or 8 and on 3 versus 8 slices.

Controls were established in order to make sure that the monkeys were not basing their choice on either volume or the time spend placing the apple slices in the boxes. The time factor was ruled out by the conduction of a series of experiments, where the same number of items was placed in both containers, only some of the items in one container were pieces of rock instead of apple. So for instance, three pieces of apple would be placed in one container, and two pieces of apple plus one piece of rock would be placed in the other. In order to rule out – or at least make it harder for the monkeys to base their choice on a direct visual comparison of the volume of food, the slices of apple were placed in the container one by one. Furthermore, a test situation was constructed, where three slices of one-sixth of an apple were placed in one container, while half an apple was placed in the other. The vast majority of monkeys chose the box containing three slices and not the box containing the

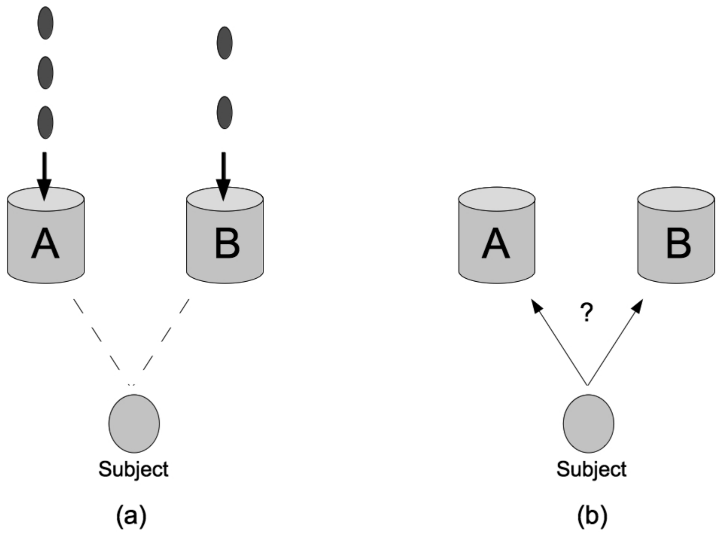



**Figure 4.2:** The spontaneous choice paradigm. Here, in (a) the subject sees a number of items being placed one at the time in two opaque boxes. In (b), the subject is subsequently allowed to choose one of the boxes. The paradigm is used in slightly different versions.

full half apple, indication that the monkeys were not – or at least not directly – judging the volume of the food items, but their numerosity. Finally, the subjects were not trained, and all subjects were only allowed to perform the test once in order to make sure that the observed behavior was spontaneous, and not acquired.

In analyzing these results, it is important to note that the monkeys' performance violated Weber's law: Their performance seemed to depend more on the total sum of items than on the ratio between the size of the two sets – surprisingly, the monkeys performed worse on 3 vs. 8 than on 3 vs. 4!

Spontaneous choice tests have been applied to a number of other species, including mosquitofish (Agrillo *et al.* , 2007), domestic dog (Ward & Smuts, 2007), salamander (Uller *et al.* , 2003) and capuchin monkey (Addessi *et al.* , 2008). Unfortunately, in all of these studies, the subjects had both choice quantities in plain view at the time they were choosing (or in the case of domestic dogs, just before). Since no controls were made for non-numerical aspects of stimuli we cannot be sure whether the subjects based their choice on numerosity or on surface area or other factors co-varying with the number of items in the choice sets. The seriousness of this problem was underlined by a follow-up study on mosquito fish (Agrillo *et al.* , 2008). In the first study, fish faced with the choice of two schools had consistently chosen to join the most numerous. However, when controls were made for total surface area of fish in the two choice sets (by having a large quantity of small fish in one school and a small quantity of large fish in the other), a negative result was obtained. This indicates that mosquito fish' choice of school depends on the total surface area of the fish in the school, and not on the number of fish.

A few other tests-paradigms have been applied. As a particularly interesting example, a 'many' versus 'few' discrimination test has been used on wild female lions in Serengeti National Park (McComb *et al.* , 1994). In the test, recordings of roars of unfamiliar lions were played back to packs of wild female lions. The recordings were of either a single female lion or of three female lions roaring in a chorus. The lions interpreted the recorded roars as the roars of intruders, and they would either approach the loudspeaker (in order to attack the simulated intruders) or not. As it turned out, the lions carefully adjusted their decision of approaching the intruders or not to both the size of their own pack and the size of the intruding pack. Similar behavior has been observed in wild chimpanzees (Wilson *et al.* , 2001, 2002).

Finally, a few tests using training by reinforcement have been applied to rats (Church & Meck, 1984), pigeons (Roberts & Mitchell, 1994), capuchin monkeys (Beran, 2008) and rhesus macaque monkeys (Brannon & Terrace, 1998). In the last mentioned test (on macaque monkeys), the subjects were simultaneously shown four sets with one to four elements on a touch-screen. After 35 training sessions, the subjects proved able to choose the sets in ascending order by touching the screen. The size of the elements was varied in different ways in order to provide controls for total surface area of the elements of the sets.

Subsequently, the monkeys were given the choice between only two sets containing 1 through 9 elements. Without further training, the monkeys proved able to choose the smallest of any of the possible pairs (i.e. 1 vs. 2, 1 vs. 3 *etc.*). However, their performance depended directly on the numerical distance between the two sets. So in this condition, rhesus monkeys clearly performed differently than in the spontaneous choice test described above (in Hauser *et al.* , 2000), where no dependence on numerical distance between choice sets, but a clear limit to performance (when more than 4 elements in either choice set was present) was observed. A similar performance profile was obtained with capuchin monkeys, although in this case no controls were made for surface area (Addessi *et al.* , 2008). I will discuss the apparent context-dependency of the performance profile of the subjects at length in section 4.2.6.

### 4.2.2 Cardinality

A vast number of species of animal also seem to have some understanding of cardinality, i.e. of the absolute number of elements in a set. In a series of experiments conducted in the 1930's and 40's by Otto Koehler, several species' ability to discriminate the cardinality of sets was tested. Koehler distinguished between two different types of abilities: 1) the ability to discriminate simultaneously presented quantities and 2) the ability to estimate the number of repetitions in a sequence of action (such as picking a grain).

The first ability was tested using a 'matching to sample' protocol, where a subject was shown a sample with a certain number of dots, and was required to pick the one out of two or more test samples that bore the same number of dots. The shape, size and position of the dots were changed in an irregular way from experiment to experiment. This insured that the birds could not use non-numerical information to solve the task. Positive results were obtained using op to six dots on jackdaws (Koehler, 1941) and ravens (Koehler, 1951).

In another set-up, birds had to chose the group containing a specific given number of units from a choice of two groups. Koehler (1941) reports positive results for doves in a set-up, where the birds had to chose between two groups containing different numbers of grains. However, as no controls were made for the total surface area of the grains, the result must be regarded as inconclusive.

Koehler tested the sequential understanding of numerosity on doves by training the birds to eat a specific number of grains either out of a larger heap or from a feeding mechanism, where peas were released one at the time. Positive results were obtained training the birds to pick five grains (51,7% correct out of 2166 trials with the feeding machine), but the animals could not manage to pick six grains consistently. In another set-up, subjects were trained to open boxes containing small amounts of baits until they had found a specific number of baits. The number of baits in each box was changed from trial to trial. This ensured that subject could only rely on the number of baits eaten, and not on the number of boxes opened. Koehler (1941) reports positive results for six bates with doves and five with jackdaws (however, the tasks posed to the jackdaws were considerably more demanding, as the birds had to secure a different number of baits depending on the color of the lids).

This experiment was later repeated by Koehler's student M. Hassmann using squirrels (Hassmann, 1952). Hassmann reports positive results for up to six bates. The squirrels were also able to pick a lid with a specific number of up to six dots amongst four other dotted lids. The dots were of different shape and size. Interestingly, the squirrels' performance apparently depended on the numerical distance between the number of dots on the correct and the distracter lids, and not on the ratio. For instance, the success rate in locating a lid with four dots hidden amongst four lids with six dots was approximately 50%, whereas the success rate was only approximately 15% when they had to locate a lid with two dots hidden amongst four lids with three dots.

The basic numerical skills of rats have also been thoroughly tested since the 1950's using an 'match to anchor value' experimental paradigm invented by the animal psychologist Francis Mechner. In the original experiment by Mechner (1958a), a rat was placed in a small chamber equipped with two levers, lever A and lever B. During the experiment, the rat was deprived of water, but could receive a reward in the form of a few drops of water by responding on lever B after having completed at least a preset anchor number $n$ of consecutive responses of lever A. If the rat switched to lever B before completing the sequence of $n$ presses on lever A, it would as a punishment have to start over with the entire procedure, pressing lever A $n$ times.

After a training period, where the rats were given easy tasks in order to learn how to operate the levers, each rat was trained under four different values of $n$ (4, 8, 12, and 16). The training of each value of $n$ lasted for nine consecutive days, but only the behavior during the last five days were included in the experimental data. The result of the experiment (see figure 4.3) quite clearly indicates that the number of times a rat would press lever A before pressing lever B changed as a function of the value of $n$, with the mean number of presses a little above $n$. In other words: If a rat was required to press lever A four times before switching to lever B, it would press lever A about four times before switching to lever B, and it would more often press lever A five times than three times. Furthermore, the accuracy of the rats seemed to depend on the magnitude of $n$; the rats were much more precise for low values of $n$ than for high values. For $n = 16$ the rats would press lever A anywhere between 11 and 26 times before switching.

In order to test whether the rats were in fact estimating the number of presses and not only the time elapsed during the pressing sequence, Mechner and Guevrekian conducted a similar test in 1962 (Mechner & Guevrekian, 1962). In this test, the rats were deprived of water to different degrees (ranging from 4 to 56 hours) on some of the trials. After a long period of deprivation, the rats completed the pressing sequence much quicker than normal, but no significant change in the number of presses were observed. In other words: The rats seemed to rely on the number of presses, and not on the duration of the pressing sequence. In conclusion, the results obtained by Mechner shows that rats are capable of estimating the number of times they press a lever, although they do not have the digital precision of human

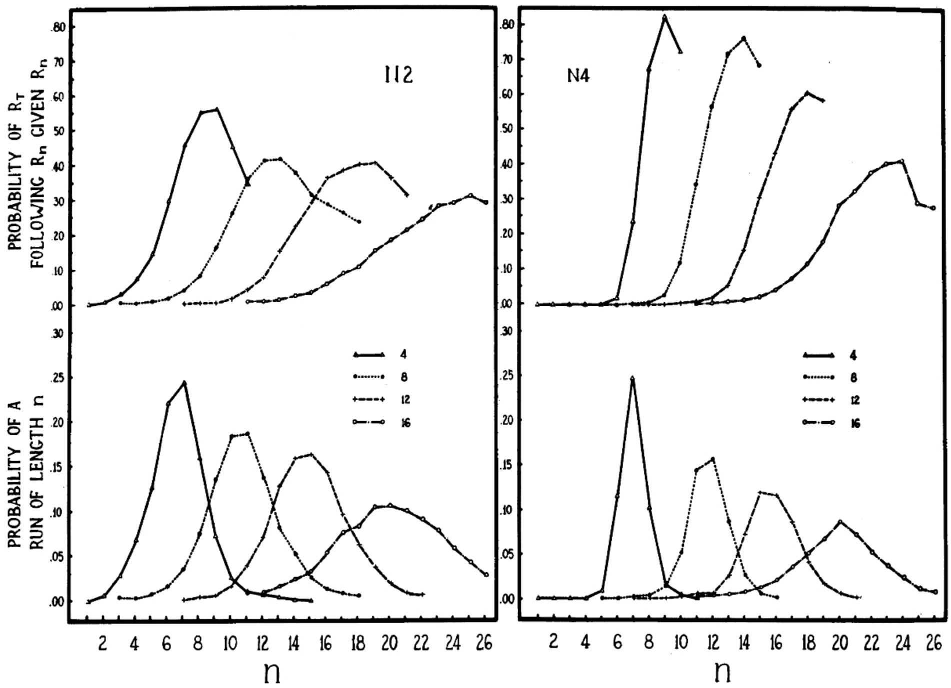



**Figure 4.3:** Data from two of the subjects in Mechner's experiment (adapted from Mechner, 1958a, p. 113).

counting. In this setup, the rats seem only to have an approximate sense of numbers.

I a similar work on pigeons by Li Xia, Martina Siemann and Juan D. Delius, pigeons were trained to match visual symbols with the numbers 1 through 6. When a key with the symbol for the number $n$ was lit, the pigeon was supposed to peck the key $n$ consecutive times before finishing with one peck on a special 'enter' key. Only a correct combination of pecks resulted in a food rewarded, all other combinations of pecks were punished with a time out. The birds were trained on the numbers 1-4 before learning 5 and 6, but in the final session all of the six symbols were in use. As seen on the distribution of pecks (figure 4.4), the spread of errors increased with the number of pecks supposed to be given.

Subsequent work by Russell Church and Warren Meck suggests that the rats' representation of numbers is rather abstract. In their experiment, a rat was placed in a cage, much like the cage of Mechner's original experiment. This time however, the rats were not supposed to perform a specific number of actions themselves, instead they were conditioned to associate the two

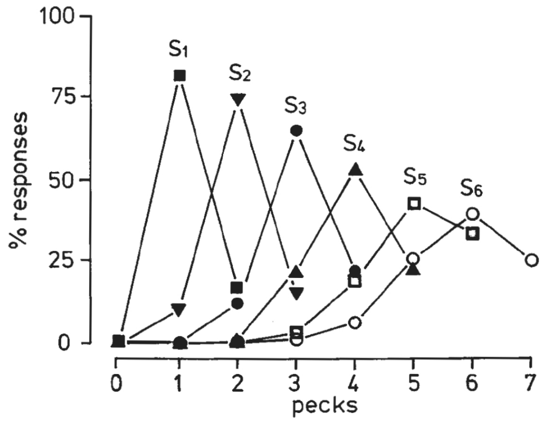



**Figure 4.4:** Distribution of responses with stimuli for the numbers 1 through 6 (where "S1" is the symbol associated with 1 etc.) (adapted from Xia *et al.* , 2000, p. 40).

levers with different numbers of sequential stimuli; if the rat heard two tones or saw two flashes of light, it was supposed to press the right lever, and if it heard four tones or saw four flashes of light, it was supposed to press the left lever. After this conditioning, the rat was presented with compound (i.e. cross-modality) stimuli in the form of either one tone followed by one flash of light or two tones followed by two flashes of light. Interestingly, the rats pressed the right lever when presented with a total of two stimuli, and they answered with the left lever when presented with a total of four stimuli. Apparently, the rat was capable of estimating the total number of stimuli, independent of the modality (Church & Meck, 1984). This indicates that the rats' representation of numbers is abstract and not closely related to a specific kind of sensory input. However, the task posed by Church and Meck was a simple dichotomy task, and other attempts to observe a transfer between different modalities on more complicated tasks have failed (for a review see Davis & Perusse, 1988, pp.576).

Apart from birds and mammals some species of social insects such as bees and ants have also been tested for the ability to use numerical aspects of their environment as a way to adaptively improve their behavior. In a series of experiments, scouts of the European red forest ant proved able to successfully communicate the location of a food source located in a branched maze to their fellow ants (see figure 4.5) (Reznikova & Ryabko, 1996, 2000, 2001). Effective controls were made for odor trails and other alternative cues the ants might use to find the food. This indicates, that the forester ants were relying on the information received from the scout. However, the actual nature of this information is unknown. It might (as speculated by the authors) be numerical (i.e. a branch number), but as no controls were made for co-varying continuous factors such as duration or length, the experiments are in my view inconclusive.

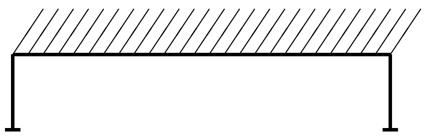


**Figure 4.5:** One of the mazes used in (Reznikova & Ryabko, 1996). A source of food was hidden at the end of one of the branches of the maze. A scout ant would be placed near the food and allowed to return to its nest and convey information about the location of the food to a team of forester ants. Apart from this horizontal trunk shaped maze, vertical trunk shaped, circular, spheric and grid shaped mazes were used in the experiment.

Honeybees rely on both distance information and sequences of landmarks when they navigate between their hive and a known food location (Menzel *et al.* , 1996). Such a sequence of landmarks could in theory include numerical cues, but it is to the best of my knowledge not well understood whether numerical information forms part of the landmark sequences of bees, or whether their landmark sequences are purely featural (i.e. it is not known whether their sequences are of the form "landmark A, B, C" or of the form "first landmark, second landmark, third landmark).

### 4.2.3 Basic arithmetic

Several species of animal also seem to be able to perform basic arithmetic operations such as addition and subtraction on small sets of objects. Such competences have for instance been observed in a population of semi-wild ranging rhesus monkeys in several experiments conducted by Marc Hauser and various colleagues (Hauser *et al.* , 1996, 2000; Hauser & Carey, 2003). The experiments were conducted using a violation of expectancy paradigm, where the monkeys' arithmetic expectations were tested by observing their reaction to arithmetically possible and impossible events (see figure 4.6). At first, a box containing a small number of eggplants was shown to a monkey.

Then a screen temporarily occluded the monkey's view to the box while one or more eggplants were either added to or removed from the box. Finally, the occluding screen was removed, so the monkey could see the eggplants left in the box.

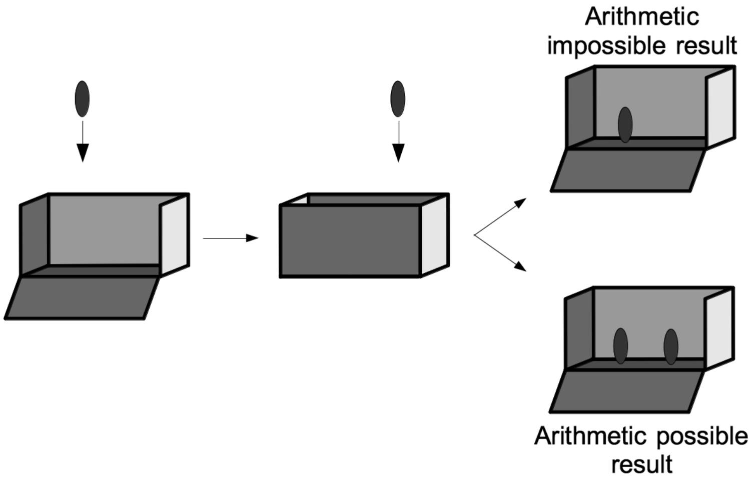



**Figure 4.6:** The violation of expectancy paradigm, here used to test $1 + 1 = 1$ or 2. An object is located in a box. Subsequently the subjects view to the box is occluded by a screen and one more object is placed in the box. The occluding screen is removed, and the subject's response is observed.

On some trials, the monkeys could clearly see all of the eggplants being added to or removed from the box, but on others further eggplants were either added or removed using a hidden trap door. As a result, the number of eggplants in the box would either seem arithmetically possible or impossible from the monkey's point of view once the occluding screen was removed. So for instance, if one eggplant was visible in the beginning and one eggplant was added while the screen was up, a result of two eggplants would seem possible while a result of three eggplants would seem impossible. When the occluding screen was removed from the box, it was recorded how long time the animal looked at the eggplants. If the monkey looked at an impossible result for a longer time than at the corresponding possible result, it was taken as a sign of surprise and evidence, that the subject had an expectation of the outcome of the arithmetic operation being performed.

Using this set-up, Hauser *et. al.* got positive results for the problems $1 + 1 = 1$, 2 or 3 and $2 + 1 = 2$, 3 or 4. However, the results obtained on $2 - 1 = 1$ or 2 were not conclusive, and the monkeys failed tests such as $2 + 2 = 3$, 4 or 5 where the total number of items in the expected outcome exceeded three. Furthermore, it should be noted that each monkey was only tested once, and that no reinforcement was used. This indicates that the observed behavior was spontaneous.

#### 4.2.3.1 Discussion of the violation of expectancy

The violation of expectancy paradigm has been heavily discussed, and at least three interpretations other than ascription of precise arithmetical expectations to the subjects have been suggested (Cohen & Marks, 2002). First of all, there is the *direction hypothesis*. This hypothesis suggests that the subjects do expect more objects to be present after an addition event and less objects after a subtraction event, but that the subjects do not have any expectations to the precise number of objects in the final result. In the present study however, the monkeys seemed to be equally surprised whether presented with impossibly many or impossibly few eggplants following an addition event. As this is not consistent with the direction hypothesis, it seems to be ruled out at least as an interpretation of this particular study.

Another interpretation is the *continuous quantity hypothesis*. This hypothesis states that the subjects are not encoding and forming expectations concerning the number of objects, but are instead encoding a continuous factor such as total surface area, total contour length, total volume or other factors co-varying with the number of objects. In order to rule out this hypothesis, Hauser & Carey (2003) conducted a set of trials with 1 small + 1 small = 2 small or 1 big eggplant. The monkeys looked significantly longer at the numerically impossible outcome of one big, even though the expected surface area of eggplant was roughly present. This result makes interpretations in terms of continuous factors unlikely.

Finally it has been suggested (in Cohen & Marks, 2002) that the subjects in violation of expectancy tests are merely responding to a simple habituation. So for instance, when subjects look longer at the impossible 1-result of a 1+1 test, it is merely because they started out seeing one object, and are now looking longer at this familiar stimulus. In order to rule out this hypothesis, Hauser & Carey (2003) used a very careful familiarization procedure. Prior to the actual test, the monkey was familiarized to the experimental set up of box, screen, and eggplants. No impossible outcomes were used during the familiarization runs, but the outcome of the familiarization runs was always equal to the outcome of the actual test, the particular monkey was going to be subjected to. So monkeys tested on $1 + 1 = 2$ were familiarized with outcomes of 2, while monkeys tested on $1 + 1 = 3$ were familiarized with outcomes of 3. This procedure makes an interpretation of the result in terms of simple habituation unlikely.

In conclusion, the study by Hauser and his group seems to provide very strong evidence to the hypothesis that rhesus macaque monkeys do have some expectations to the outcome of arithmetic behavior of small sets of objects.

---

In subsequent tests, the violation of expectancy paradigm has also been used on four species of lemur (Santos *et al.* , 2005). The animals tested positive for the conditions $1 + 1 = 1, 2$ or 3 and for the surface area control 1 small + 1 small = 2 small or 1 big. The violation of expectancy paradigm has also been used to test domesticated dogs (West & Young, 2002) and cotton-top tamarins (Uller *et al.* , 2001) with positive results. However, these studies did not include controls for all of the alternative hypotheses discussed above, so the results cannot unambiguously be interpreted as sign of arithmetical competences in the two species.

The results obtained using the violation of expectancy paradigm have been backed up by a variant of the spontaneous choice test with semi-free ranging rhesus macaque monkeys. As usual in spontaneous choice tests, the monkeys had to choose between two quantities of food items, but in this experiment, the animals were at first allowed to see both quantities of food clearly. Then the food was occluded and the monkeys were allowed to see one or more items being removed from either one or both of the quantities. Finally, the monkeys were allowed to approach one of the quantities of food. The monkeys reliably chose the set containing the largest quantity of food items on tests such as $3-1$ vs. 1, $3-1$ vs. $2-1$, and $2-1$ vs. $1+1$ (Sulkowski & Hauser, 2001). This indicates that the animals had some expectations to the outcome of the simple manipulations.

### 4.2.4 Advanced skills

Finally, very few species are known to master advanced arithmetic such as symbolic calculations and calculations involving fractions. In a study by Guy Woodruff and David Premack, a chimpanzee was taught to match a fraction of an object with the similar fraction of a dissimilar object. So for instance, the animal would be presented with a half-full glass of blue liquid, and was to point to half an apple when presented with the choice of half an apple and three-quarter of an apple. It is not clear precisely how the animal completed this task, but it seems as if it must at least have had some primitive understanding of part-whole relationships and proportionality (Woodruff & Premack, 1981).

We also have a few examples of abstract, symbolic competences in animals. One such example is the chimpanzee Ai. Ai has been taught to use the Arabic symbols for the numbers 1 through 9 as both cardinal and ordinal numbers, that is: Given two numbers, Ai can pick the largest (ordinality), given a set of dots, she can pick the numeral corresponding to the size of the set (cardinality), and given a numeral, she can point to a set with the corresponding number of dots (cardinality) (Biro & Matsuzawa, 2001).

Another impressive example of abstract symbol based mathematical reasoning is found in the female chimpanzee Sheba, who was trained by Sarah Boysen and colleagues. At first, the chimpanzee was taught to pick the correct Arabic number symbols from five plastic cards depicting the numerals 0 through 4 when presented with a number of food items. Secondly, the experimenter would hide a number of oranges at fixed locations in the cage. The chimpanzee was instructed to inspect the hiding places one by one, return to the starting place and pick the numeral corresponding to the total sum of oranges. The oranges at one hiding place were not visible form other hiding places or from the place, where the chimpanzee was supposed to give its answer. The chimpanzee performed well above chance (75% correct answers on 267 trials), indicating, that she were somehow able to mentally keep count of the number of oranges seen.

Finally, instead of oranges, the instructor would hide two plastic placards, each imprinted with one of the Arabic numerals known to the chimpanzee, at two separate hiding places. The chimpanzee was instructed to inspect the hiding places as before, and to answer with the numeral corresponding to the arithmetic sum of the hidden placards. The trials included tasks such as $n + 0$ and $2 + 2$. Once more, the chimpanzee performed well above chance (74% correct out of 38 double blind tests, where the possibility of getting subconscious cues from the instructors were eliminated) (Boysen & Berntson, 1989). Sheba was subsequently taught to use the numerals 5 to 9, but the summation experiment described above has, to the best of my knowledge, not been repeated with the new numerals (Boysen, 1993).

### 4.2.5 Possible mechanisms

The data presented above has led to speculations on the specific mechanism or mechanisms with which animals handle numerical aspects of stimuli. Several suggestions have been made.

In several of the studies, especially studies involving larger numbers, the performance of the animals seems to vary with the size of the numbers involved. Furthermore, if the subjects are required to choose between two quantities, their performance seems to depend on the arithmetical ratio of the quantities following the characteristic Weber Law. The observation of these two effects has led to the suggestion, that the animals must represent numbers using a continuous magnitude that is an analog of number. As an example of this, Meck & Church (1983) suggests an accumulator model, where an approximate unit quantity of energy is let into a mental container or accumulator for each entity in the set the animal is counting. In this way, the final state of the accumulator will be an analog representation of the total quantity of the set. So for instance, two will be represented by an analog magnitude perhaps something like this: —— and three with a magnitude like this: ———.

Yet, because of small variations in the amount of energy led into the accumulator for each entity, the representation will only be an approximation, the precision of which will decrease as the number of items enumerated increase. In this way, the accumulator model can account for the observed variation of the precision with size of the collections handled, and as a side effect of this fuzziness, for The Weber Law signature. Several theorists have elaborated on this idea (e.g. Gelman & Gallistel, 1992; Dehaene, 1997; Dehaene & Changeux, 1993, p. 46).

The analog magnitude model however, does not seem able to explain the observation of primates' arithmetical competences. The animals' performance did not decrease continuously as the number of objects was increased. Instead, they seemed to reach an absolute limit, beyond which the subjects could not perform at all. These finding indicate that primates have a special system for handling small quantities complimenting the approximate system for handling larger quantities (Hauser & Spelke, 2004).

Several suggestions have been made to the nature of this special system for handling small quantities. One suggestion is *subitizing*. The term was coined by E. L. Kaufman as a way to describe adult human subjects ability to recognize the number of up to six simultaneously presented objects rapidly and precise (Kaufman *et al.* , 1949)[1]. Following von Glasersfeld (1982), subitizing is usually taken to be a perceptual mechanism, probably based on pattern

[1]The term is not accidental, but derived from the Latin adjective *subitus* (meaning 'sudden') and the verb *subitare* (meaning to 'arrive suddenly') (Kaufman *et al.* , 1949, p. 520).

recognition, and not a cognitive or rational mechanism. Subitizing is supposed to make it possible for the animal to perceive or intuitively apprehend the numerosity of a given stimuli; the subject is simply supposed to directly see 'threeness' or 'twoness' when three or two objects are presented, just as the subject can see color or shape. In the original suggestion, subitizing was only intended to work for small quantities of simultaneously observed objects, but in the context of animal cognition, subitizing has been suggested to apply to sequential stimuli as well as simultaneous stimuli, and to stimuli of any of the six modalities (Davis & Perusse, 1988, p. 570).

Another suggestion is the object-file model. As subitizing, this model was also originally developed in the context of human cognition (Kahneman *et al.* , 1992), but has since been adapted to animal cognition (for instance by Sulkowski & Hauser, 2001). Unlike subitizing, the object-file model does not rely on pattern recognition. In the object-file model, it is instead assumed that an index of attention or 'pointer' can be attached to objects encountered in the world. The indices are 'sticky' and follow the individual objects through space and time. When an index is assigned, a file in short-term memory is opened keeping track of the number of indices currently under use. So basically, objects are represented in a one-to-one way by a mental model consisting of tokens in short term memory.

According to the object file model, arithmetical tasks such as those involved in violation of expectancy tests can be accomplished by a simple one-to-one comparison of object files. In the $1+1=2$ or 3 situation for instance, the two opened object files modeling the initial situation are compared to the two or three objects perceived in the result situation (or perhaps the two or three opened object files in the model of the result situation), and a violation of expectancy can be detected. This process does not necessarily involve any concepts of numbers or abilities to perform symbolic calculations, but only the ability to form mental models that capture the numerical aspect of a situation, and the ability to perform simple one-to-one comparison between two such models. Limitations to the number of indices it is possible to handle in parallel by short-term memory, result in the observed limitation to the number of objects, subjects are able to handle by the precise small system. For human beings this number is supposed to be somewhere between three and five (Hauser & Carey, 1998, p. 73).

Finally, the animals might simply be counting using a list of either innate or acquired symbols. This idea origins in Gelman and Gallistel's suggestion, that human infants possess a list of innate mental symbols called 'numerons' (Gelman & Gallistel, 1978). A similar hypothesis is sometimes discussed in the context of animal cognition (e.g. in Hauser & Carey, 1998).

The counting hypothesis is, however, almost universally rejected in the literature. There are several reasons for this. Counting is a quite demanding process. According to Davis and Memmott (1982), the counting agent should firstly be able to tag each item to be counted once. The tags do not need to be similar to the conventional number words and symbols used by humans. They could be non-verbal or other types of entities, but secondly they should be applied in a repeatable, stable order. Thirdly, the agent should understand that the final tag has special significance of stating the cardinality of the set counted. As all of this is very complicated, simpler models such as subitizing and the accumulator- or object-file model is preferred if possible.

Furthermore, counting or numeron-list models is neither consistent with the approximate and fuzzy nature of the animals handling of larger numerosities nor with the absolute limit to the small and precise system of approximately four objects. If the animals were counting larger sets, they should exhibit digital precision not fuzziness, and if the mechanism of the small precise system was counting, the limit of approximately four objects seems strange. There is no reason why the list of numerons should be limited to only four items, but there are good reasons why short-term memory should be limited in this way.

In other words: If animals were counting, they should be able to handle sets larger than four with digital precision. As this is not in general the case, it seems safe to conclude, that the animals in the experiments I have described so far do not count.

On the other hand, there are a few (very few!) examples of animals that are able to handle sets larger than four with digital precision. One of those is the gray African parrot named Alex. Alex was trained to apply the number words 'one' through 'six'. Presented with a tray with objects of various types, sizes and color, Alex could answer questions of both the form 'how many $x$' (where $x$ was a type of object), and 'what object/color $n$?' (where $n$ denotes a number 1-6). As Alex' performance did not seem to depend on the size of numbers involved, he probably was not using an analogue representation type of mechanism. And because he dealt with as many as six objects, neither the object file model nor subitizing seems to be an option. This leaves a cognitive process perhaps similar to digital human counting as a possible explanation of his performance.

However, the perceptual system of birds is supposed to be very different from that of mammals, so Alex might be using simpler non-counting mechanisms such as subitizing or the object file mechanism even though mammals supposedly cannot use such mechanisms for sets with more than four elements (see Pepperberg & Gordon, 2005, for a review and discussion).

The chimpanzee Ai, briefly mentioned above, exhibits another very interesting example of counting-like behavior. When Ai had learned to enumerate arrays of one to five dots, her reaction time was measured. As it turned out, her reaction time was essentially the same when one to three dots were displayed, then it increased for four dots and surprisingly decreased again when five dots were displayed. This pattern repeated itself when the maximum number of dots presented to her was increased first to six and finally to seven: Same reaction time for one to three dots, then a steady increase and then a decrease with the largest number of dots (Murofushi, 1997). This clearly indicates that Ai used two different mechanisms for the enumeration of arrays with many (4-7) and few (1-3) dots. The flat reaction time for one to three dots speaks for a quick, parallel process, such as subitizing, whereas several models might explain the linear increase in reaction time for larger collections.

Some commentators, including Kiyoko Murofushi (Murofushi, 1997) and later Sarah Boysen and Karen Hallberg (Boysen & Hallberg, 2000), conclude that Ai must have been estimating the numerosity of the larger sets, perhaps using the analog magnitude model discussed above. This is a tempting possibility, but on the other hand, had Ai been estimating, one would expect the precision to decrease as the number of dots increased, but no such effect was observed – the percentage of correct responses stayed well above 80% for all arrays. So for sets larger than three, the reaction time seemed to increase linearly with the number of dots, and the precision was constant. In other words: exactly what one would expect, if the chimpanzee had indeed been counting. The drop in reaction time for the largest number known to the animal is hard to explain. Perhaps the animal simply associated the largest number $n$ with 'more than $n-1$' and estimated when this was the case. Anyway, we do not know the exact mechanisms used by either Alex or Ai, but at least we cannot rule out actual counting based on the given data. Yet, it should be noted, that Alex and Ai are exceptions, and that their behavior is the result of intensive training. So even though the two individuals might be counting, it cannot be concluded that any species of animal in general count. Indeed, all the observations of the spontaneous numerical behavior of animals seem to support the very opposite conclusion: animals do not count spontaneously. But perhaps some animals can learn to count.

Turning to the debate on subitizing vs. object file mechanism a more open conclusion must be advocated. Ai's flat reaction time might have spoken in favor of subitizing, but on the other hand it seems improbable that a purely perceptual mechanism, such as subitizing, can account for the arithmetic skills of primates. As the animals apparently formed expectations to the outcome of operations of groups of objects they could not directly perceive, it seems as if some kind of mental modeling of the situation must have taken place – and this of course is just what the object file model suggests. So perhaps primates use both mechanisms in different situations, or perhaps they are subitizing the number of object files (as suggested by Sara Cordes and Rochel Gelman, (2005, p. 138)). Or they might use neither subitizing nor object files, but something entirely different. Given the current data it seems very hard to reach any final and definite conclusions. The only thing that seems fairly probable is the suggestion that two different systems are at play; an approximate for the handling of larger sets, and a more precise for smaller sets.

### 4.2.6 Is mathematics evolved?

If the observed mathematics skills are to be counted as a direct consequence of evolution, they must be adaptive, i.e. somehow increase the fitness of their bearer. This is clearly the case for some of the observed capabilities. The ability observed in lions to judge the relative size of groups clearly seems to increase the chances of success when deciding whether to engage in intergroup aggressions or not. A precise judgment of small numbers might also be a benefit when animals form coalitions and when mothers track the number of offspring present (as suggested by Hauser & Spelke, 2004, p. 862). Also, the ability to estimate the relative number and type of scent marks can play an adaptive role in sexual selection of some species of animal. This type of behavior has been reported by Ferkin *et al.* (2005) in a study on female meadow voles acting in simulated natural conditions. The behavior is adaptive, as it enables the females to choose the male producing the highest quantity of high quality scent marks.

There have been made other suggestions of adaptive use of arithmetic and numerical skills, but they are all supported by unclear evidence. Bruce Lyon for instance suggests that the American coot might use estimations of the number of eggs in the nest as a means to avoid parasitism (Lyon, 2003). It is however not possible to test whether the birds rely on numerosity or on volume or other factors covarying with the number of eggs in the nest, so the suggestion must be seen as rather hypothetical. Similarly, male mealworm beetles are able to discriminate between odor signals made by different numbers of female beetles; the male beetles spend relatively more time investigating signals from many females than from few (Carazo *et al.* , 2009). This behavior is adaptive, as the beetles are promiscuous, but the behavior does not show that the beetles are directly sensitive to the actual number of females, only that they are sensitive to the greater chemical complexity in signals from more females. Finally, insects might use numerical features of a landscape as a way to remember the location of a food source (Menzel *et al.* , 1996) and direct other individuals of its group to a food source by transmitting such numerical information (Reznikova & Ryabko, 1996). Such skills are adaptive, but it is not clear whether the insects rely on numerical or other types of information (such as featural information about landmarks or duration of travel).

It has also been suggested that animals can benefit from numerical judgment in foraging situations. As Stanislas Dehaene straightforwardly describes the supposed mechanism: "The squirrel that notices that a branch bears two nuts, and neglects it for another one that bears three, will have more chances of making it safely through the winter" (Dehaene, 1997, p. 23). This suggestion however, is more dubious. In foraging situations the principal interest of an animal should be the total amount and quality of the food, not the number of individual food items collected. As food items vary in size we would expect selective pressure against foraging strategies focusing solely on the number of food items (such as the one suggested by Dehaene).

In fact, in a recent study, cotton-top-tamarins have been shown to favor the total amount of the food over the total number of food items (Stevens *et al.* , 2007). The study consisted of a spontaneous free choice test where the monkeys had to chose between two trays with food items of varying size and number. Both trays were in view of the subjects when they made their choice. Given a choice between a few large food items vs. many small food items, the subjects consistently chose the largest amount of food (measured in weight) and not the largest number of items. Also, when the same amount of food was distributed in either many small or a few large foot items, the monkeys chose the few, large items. This last result gives evolutionary sense, as it is faster to collect and easier to handle and monopolize a few large than many small food items.

This result is not necessary inconsistent with the studies reporting positive results for numerical capacity (including studies specifically on cotton-top tamarins, such as (Uller *et al.* , 2001; Hauser *et al.* , 2003)), as a different experimental setup was used. The study does however clearly show that judgment of numerosity is not the only foraging strategy used by the subjects. In fact, when a direct judgment of food amounts is possible, cotton-top-tamarins prefer this strategy to numerosity judgment. So apparently judgment of numerosity is at most a secondary strategy to this species of monkey.

Something similar can be concluded from the experimental data concerning rhesus macaque monkeys. As explained above, semi-free ranging macaques fail to distinguish between sets containing respectively of 3 and 8 elements in a spontaneous free-choice test (Hauser *et al.* , 2000). One would expect selective pressure against a foraging strategy that leaves an animal unable to distinguish between 3 and 8 similar sized food items. Furthermore, macaques succeed in discriminating between all pars with 1 through 9 elements in another experimental setup, including reinforced training (Brannon & Terrace, 1998). So apparently, rhesus macaque can learn how to discriminate between set with 3 and 8 elements, but they do not do so spontaneously when they are in their natural environment. All in all this points to the conclusion that macaques do not or only to a very limited extend rely on numerical discrimination as part of their natural foraging strategy.

---

The results on cotton-top tamarins and rhesus macaques quoted above points to another and more fundamental problem for the evolutionary claim. For both species different experimental setups lead to diverging results on the same type of task. This shows that the numerical and arithmetic behavior of cotton-top tamarins and rhesus macaques is context dependent; the animals' numerical and arithmetic skills only display themselves in carefully designed experimental setups. This forces us to ask whether the skills are part of the animals' natural behavior or laboratory artifacts produced by experimental setups forcing the animals to behave in specific ways.

The generality of the problem is illustrated by table 4.1 and table 4.2. Here, I have collected all of the papers reporting positive results for numerical or arithmetic skills of animals quoted in this chapter (table 4.1) and all the papers concerning the numerical or arithmetic skills of animals published after year 2000 in one of the leading journals of the area, *Animal Cognition* (table 4.2)[2]. For each study, the species and type of mathematical skills are indicated in order to give an idea of just how widespread which mathematical skills are. The mathematical skills are divided into the subgroups used above, that is: Judgment of the relative size of sets, estimation of cardinality, basic arithmetic, advanced arithmetic and symbol use. Furthermore, the tasks posed to the animals' are divided into three categories: Artificial, partly

[2]All papers containing at least one of the word 'numerical', 'numerosity' and 'arithmetic' were considered. The search gave a total of 60 papers. Of these, 34 papers were discarded as they only accidentally contained the search word and did not address numerical or arithmetic competence of animals. The table contains the remaining 26 papers.

natural and natural. The category 'artificial' cover tasks completely foreign to the animals' natural environment, such as pressing a lever. 'Partly natural' cover tasks that in essence are similar to tasks confronting the animals in their natural habitat, although the test-tasks do not directly simulate the corresponding natural task. This could be tasks such as choosing between two buckets after watching different amounts of food items being hidden in them. Finally, the category 'natural' covers tasks that are either completely natural or at least directly simulate problems confronting the animals in their natural environment. This could be tasks such as deciding whether to attack a pack of intruders after hearing their roars. There are of course borderline cases. Most prominently, I have categorized all experiments using violation of expectancy paradigm as artificial. As this choice is debatable, I have furthermore labeled these cases "VOE". Finally, it is indicated whether the skills are acquired by training or spontaneously arise in the individual animal (either instinctively or by observing fellow members of the species).

All in all, 61 studies are included in this overview. Of these, only five is based on observation of animals' spontaneous behavior under natural conditions. Furthermore, two of these five studies are inconclusive due to lack of control for co-varying continuous factors. This leaves us with only three studies where the use of numerical aspects of reality conclusively can be said to form part of a species' natural behavior. All of these three studies concern inter-group conflict (of either lions (McComb *et al.* , 1994) or chimpanzees (Wilson *et al.* , 2001, 2002)). The remaining 56 studies included in the tables involve either training or carefully designed experimental setups, where the animals are forced to rely on numerical strategies because all non-numerical cues have been removed.

From an experimenters' point of view, the trouble is that the abstract, numerical aspect of most natural environments is fully integrated with concrete physical features (such as area, mass, density, brightness, length, intensity, variety) or even other abstract aspects (such as duration). This is in itself thought provoking. It shows that although human adults (with a modern education) readily abstract and to a large extend rely on the numerical aspect of reality, this aspect is not independent, but is almost always interwoven with other aspects of reality; it takes a lot of ingenuity to design even simulated natural tasks where the subjects are forced to rely solely on numerical judgment. Consequently, if an experiment is conducted in a natural environment, we cannot tell whether the subjects are exploiting the numerical or one or more of non-numerical aspects of their sensory input. This forces the experimenters to invent settings where all (or at least most) non-numerical as-

**Table 4.1:** Studies of the numerical and arithmetic skills of animals used in current chapter (except studies included in table 4.2)

| Species | Type of skill | Type of task | Source of behavior | Reference and notes |
|---|---|---|---|---|
| Doves | Estimation of cardinality | Artificial | Training | (Koehler, 1937). Note: Anecdotal. |
| jackdaws, pigeons, parakeet | Estimation of cardinality | Artificial | Training | (Koehler, 1941) Note: Anecdotal. |
| Ravens, gray parrots, budgerigars, jackdaws | Estimation of cardinality | Artificial | Training | (Koehler, 1951) Note: Anecdotal. |
| Squirrel | Estimation of cardinality | Artificial | Training | (Hassmann, 1952). Note: Anecdotal. |
| Rat | Estimation of cardinality | Artificial | Training | (Mechner, 1958a,b) |
| Rat | Estimation of cardinality | Artificial | Training | (Mechner & Guevrekian, 1962) |
| Chimpanzee | Advanced arithmetic (Fractions) | Artificial | Training | (Woodruff & Premack, 1981) |
| Raccoon | Estimation of cardinality | Artificial | Training | (Davis, 1984) |
| Rats | Judgment of relative size | Partly natural | Training | (Church & Meck, 1984) |
| Chimpanzee | Advanced arithmetic (number symbols) | Artificial | Training | (Matsuzawa, 1985) |
| Rat | Estimation of cardinality | Artificial | Training | (Davis & Bradford, 1986) |
| Chimpanzee | Advanced arithmetic | Artificial | Training | (Boysen & Berntson, 1989) |
| Chimpanzees | Relative size and Basic arithmetic | Artificial | Training | (Pérusse & Rumbaugh, 1990) |
| Chimpanzee | Advanced arithmetic | Artificial | Training | (Boysen, 1993) |
| Lions (wild) | Relative size or estimation of cardinality | Natural | Spontaneous | (McComb *et al.* , 1994) |
| Pigeon | Estimation of cardinality | Artificial | Training | (Roberts & Mitchell, 1994) |
| Red forest ant | Estimation of cardinality | Natural | Spontaneous | (Reznikova & Ryabko, 1996). Note: No control for co-varying continuous factors. |
| Honey bee | Estimation of cardinality | Natural | Spontaneous | (Menzel *et al.* , 1996). Note: Inconclusive due to lack of control for alternative hypotheses. |
| Rhesus monkeys (wild) | Basic arithmetic | Artificial (VOE) | Spontaneous | (Hauser *et al.* , 1996) |
| Chimpanzee | Advanced arithmetic (symbol use) | Artificial | Training | (Murofushi, 1997) |
| Rhesus monkeys | Estimation of cardinality and relative size | Artificial | Training | (Brannon & Terrace, 1998) |
| Rhesus macaque (wild) | Judgment of relative size | Partly natural | Spontaneous | (Hauser *et al.* , 2000) |
| Red wood ant | Estimation of cardinality | Partly natural | Spontaneous | Reznikova & Ryabko (2000, 2001) |
| Chimpanzee | Advanced arithmetic (use of number symbols) | Artificial | Training | (Biro & Matsuzawa, 2001) |
| Chimpanzees (wild) | Judgment of relative size or perhaps estimation of cardinality | Natural | Spontaneous | (Wilson *et al.* , 2001) |
| Pigeons | Estimations of cardinality | Artificial | Training | (Xia & Emmerton, 2001) |
| Rhesus macaque (wild) | Basic arithmetic | Artificial (VOE) | Spontaneous | (Sulkowski & Hauser, 2001) |
| Cotton top tamarin | Basic arithmetic | Artificial (VOE) | Spontaneous | (Uller *et al.* , 2001). Note: Inconclusive due to lack of control for alternative hypotheses. |
| Domesticated dog | Basic arithmetic | Artificial (VOE) | Spontaneous | (West & Young, 2002). Note: Inconclusive due to lack of control for alternative hypotheses. |
| Chimpanzees | Relative size | Natural | Spontaneous | Wilson *et al.* (2002) |
| Rhesus monkeys (wild) | Basic arithmetic | Artificial (VOE) | Spontaneous | (Hauser & Carey, 2003) |
| African grey parrot | Estimation of cardinality, perhaps counting. Symbol use | Artificial | Training | (Pepperberg, 1994; Pepperberg & Gordon, 2005) |
| Chimpanzees | Judgment of relative size | Partly natural | Spontaneous | (Beran & Beran, 2004) |
| Rhesus monkeys (wild) | Basic arithmetic (including 4+4) | Artificial (VOE) | Spontaneous | (Flombaum *et al.* , 2005) |
| Rhesus monkeys | Estimation of cardinality and basic arithmetic | Artificial | Training | (Cantlon & Brannon, 2007) |

**Table 4.2:** Studies of the numerical and arithmetic skills of animal published in *Animal Cognition* January 2000 to May 2010

| **Species** | **Type of skill** | **Type of task** | **Source of behavior** | **Reference/notes** |
|---|---|---|---|---|
| Pigeons | Estimations of cardinality | Artificial | Training | (Xia *et al.* , 2000) |
| Chimpanzees | Estimation of cardinality and symbol use | Artificial | Training | Beran & Rumbaugh (2001) |
| Domestic dog | Judgment of relative size | Partly natural | Spontaneous | West & Young (2002). Note: No control for co-varying factors. |
| Salamanders | Relative size | Partly natural | Spontaneous | Uller *et al.* (2003). Note: No controls for co-varying factors. |
| Chimpanzees | Advanced arithmetic (symbol use) | Artificial | Training | Beran (2004) |
| Rhesus macaques | Estimation of cardinality and symbol use | Artificial | Training | Harris & Washburn (2005) |
| Mongoose lemurs | Estimation of cardinality | Partly natural | Spontaneous | Lewis *et al.* (2005) |
| Meadow voles | Relative size | Partly natural | Spontaneous | Ferkin *et al.* (2005) |
| Lemurs (several species) | Basic arithmetic | Artificial (VOE) | Spontaneous | Santos *et al.* (2005) |
| Rhesus macaques | Estimation of cardinality (match to sample) | Artificial | Training | Jordan & Brannon (2006) |
| African grey parrot | Estimation of cardinality, perhaps counting. Symbol use | Artificial | Training | Pepperberg (2006) |
| Columbian ground squirrels | Negative result on estimation of cardinality | Partly natural | Spontaneous | Vlasak (2006) |
| Cotton-top tamarins | Judgment of relative size. Negative result | Partly natural | Spontaneous | Stevens *et al.* (2007) |
| Mangabeys | Relative size | Artificial | Training | Albiach-Serrano *et al.* (2007). Note: No control for co-varying factors. |
| Mosquitofish | Relative size | Partly natural | Spontaneous | Agrillo *et al.* (2007) Note: No control for co-varying factors. |
| Mosquitofish | Relative size | Partly natural | Spontaneous | Agrillo *et al.* (2008) Note: Negative result when the total surface areas of the choice sets were controlled. |
| Capuchin monkeys | Relative size | Artificial | Training | Beran (2008) |
| Chimpanzees | Relative size | Artificial | Training | Tomonaga (2008) |
| Capuchin monkeys | Relative size | Partly natural | Spontaneous | Addessi *et al.* (2008). Note: No control for co-varying factors. |
| Several species of monkeys | Relative size | Artificial | Training | Shifferman (2009). Note: No control for co-varying factors. |
| Mealworm beetle | Relative size | Partly natural | Spontaneous | Carazo *et al.* (2009). Note: Result could be explained as sensibility to chemical diversity, and not numerical discrimination. |
| Pigeons | Relative size | Artificial | Training | Emmerton & Renner (2009) |
| Capuchin monkeys | Relative size | Partly natural | Spontaneous | Evans *et al.* (2009) |
| Horses | Relative size | Partly natural | Spontaneous | Uller & Lewis (2009) |
| African grey parrots | Relative size | Partly natural | Spontaneous | Aïn *et al.* (2009). Note: No control for co-varying factors. |
| Rhesus macaque | Estimation of cardinality | Artificial | Training | Livingstone *et al.* (2010) |

pects of sensory-input are artificial removed or blurred. Unfortunately, when such artificial settings are used, we cannot tell whether the subjects' performance mimics its natural behavior or whether the performance is merely a laboratory artifact produced only under the artificial settings used in the experiment (or, as in 27 of the 61 studies: as a result of intensive training).

The integration of numerical and other aspects of reality makes it less likely that animals have developed special purpose mechanisms for tracking number. At least a good reason should be given: Why track number, if other co-varying factors can just as well be exploited?

In this connection the use of language in the papers is particularly interesting. Animals are reported as "succeeding" a test, if they behave as the experimenter intended, and "failing" if they do not – although failing a test might well be the adaptively most sensible behavior (such as choosing the largest amount of food over the largest number of food items)[3]. This choice of words betrays the fact that the experimenters clearly know what they are looking for. The experiments are not designed in order to investigate how the animals solve problems or what kind of information they rely on in choice making. The studies are designed specifically in order to test the mathematical skills of the subjects by forcing them into acting in an unnatural way.

This does not detract from the studies, but it does call for a much more careful interpretation, especially concerning the evolutionary value of the observed skills. Most of the studies only give reliable information about the subjects' cognitive potential, not about their actual cognitive behavior. They tell us something about what the subjects can accomplice when pushed or trained. The observed skills might well be laboratory artifacts (as also suggested in Davis & Memmott, 1982). They might never have formed part

[3]This use of language is ubiquitous in the literature, but I will give a few examples of the kind of language use I have in mind. In Hauser & Carey (2003, p 369-70), we find the following passage (my emphasis): "Participants *succeed* (pick the larger number) if both sets are small (upper limit 4 for rhesus adults and upper limit 3 for babies) but *fail* if one of the sets exceeds that limit (e.g., rhesus *fail* at 3 vs. 8; infants *fail* at 2 vs. 4, 3 vs. 6, and even 1 vs. 4)". In Hauser *et al.* (2000, p. 829, my emphasis): "They [the subjects] *failed* at four versus five, four versus six, four versus eight and three versus eight slices". And finally in Boysen & Hallberg (2000, p. 428, my emphasis): "However, further training for discrimination between 7 dots versus 8 dots was not *successful.* The stimuli were modified so that the dots sequence on each card were equidistant from one another on the cards. With this change, the subject's performance fell below chance, and the authors concluded that his *previously successful responses* had likely been based upon density, rather than absolute number. [...] Presumably, given the animal's *failure* with these tasks, Douglas & Whitty modified their procedures ..." Similar examples can be found in almost all papers discussed in this chapter.

of the animals natural behavior, and their development is most likely the side effect of a non-numeric adaptation or the result of the selection of general-purpose devices (which are general enough to support mathematical cognition under the pressure of training or absence of other cues, as suggested by (Church & Meck, 1984).

Unfortunately, this observation is not always noticed in the literature. To take a few examples of typical over-interpretations, the comparative study of rhesus monkeys and college students cited at the beginning of this chapter does not show that monkeys can perform mental addition (as claimed by the authors Cantlon & Brannon, 2007, p. 2916). It only shows that rhesus monkeys can be trained to do so, or in other words that rhesus monkeys have the cognitive capacity needed in order to learn how to perform above chance on tasks involving the estimation of the sum of small addends.

Similarly, the fact that rats can be trained to walk into the $n$'th tunnel in an array of similar looking and smelling tunnels, does not show that rats rely on numerical discrimination in real world setting and that this skill is adaptive (as suggested by Dehaene, 1997, p. 23). In the experiment, the rats were trained and all other than numerical cues had painstakingly been removed from the maze. In the real world however, there are other cues present, and rats and other mammals are known to use both olfactory information and visual cues (in the form of special features or 'landmarks') when they navigate (Maaswinkel & Whishaw, 1999; Vlasak, 2006). A rat might well be better served relying on such non-numerical cues than on its somewhat approximate sense of numbers when it navigates its tunnels. The numerical judgment of the laboratory rats might in other words only have been a last resort strategy.

As I see it, when the lack of observation of numerical and arithmetic skills in real life situation is taken into account, the following conclusion concerning the evolution of animals' numerical and arithmetic skills is reached: The ability to judge the relative size of groups have a direct adaptive significance and has been observed as part of several species' natural behavior. The ability to estimate cardinality and to perform basic arithmetic seems to be highly context dependent, i.e. they can appear spontaneously at least in some species, but only in carefully structured experimental setups where all but numerical cues have been removed. This indicates that the skills are at most last-resort strategies used for instance when a direct judgment of the amount of food hidden in two locations is not possible. Last-resort strategies can be adaptive, however we lack evidence that the animals in fact use such strategies as part of their natural behavior. So no firm conclusions can be drawn concerning the evolutionary origin of these abilities. Finally, advanced arithmetic skills and the use of abstract symbols take intensive training. Such skills are clearly laboratory artifacts and not part of any species of animals' natural and spontaneous behavior. As such, they cannot be products of direct selection, but is most likely the result of the selection of general-purpose cognitive mechanisms.

### 4.2.7 Partial conclusion

Based on the studies discussed above the following conclusions can be drawn: The basic capacity to exploit numerical aspects of the environment is found in several species of both bird and mammal. Many species of primates furthermore seems to be able to perform basic arithmetic operations, i.e. addition and to some extent subtraction involving sets with less than five elements. Chimpanzees finally seem to be able to learn more advanced mathematical skills such as calculation with fractions and the use of small number symbols in simple addition tasks.

Primates seem to poses two different systems; a small and precise system for handling small numerosities, and a larger and imprecise system governed by Weber's Law for the handling of larger numerosities. The mechanism behind the mathematical performance of birds' and other species of mammals is not well understood.

Only basic mathematical skills, i.e. the ability to judge the relative size of two sets, have been observed in a reliable way as part of a species natural behavior. All other skills only express them selves in partly artificial settings or after intensive training. For this reason, only little can be concluded about the evolutionary origin of the observed mathematical abilities. They might be the direct result of adaptive selections, but they might just as well be the result of the selection of perceptual or general-purpose cognitive mechanisms.

### 4.2.8 Mathematics in the kindergarten

The study of the mathematical capabilities of animals is of interest to us here mainly because the existence of such mathematical abilities in animals might reveal something about the evolutionary origin of similar abilities in humans. As it turns out, humans seem spontaneously to possess roughly the same basic mathematical capacities as primates, i.e. a precise system for the handling of small quantities and an approximate system for the numerical judgment of larger sets. This is primarily tested in two ways: By testing human infants, who have not yet learned advanced human mathematics, and by testing human adults in ways that make it impossible for them to use advanced techniques such as digital counting.

From the 1980's and on a number of habituation experiments provided evidence that very young infants are able to discriminate between different small numbers of objects (see figure 4.7).

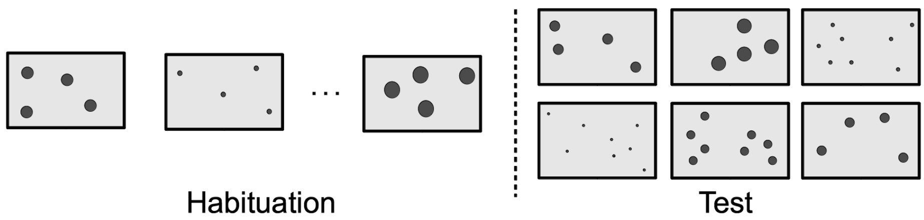



**Figure 4.7:** The habituation paradigm. In a typical habituation tests the subject is repeatedly shown different displays of a fixed, given number of objects. The objects' size, spatial arrangement and even type is varied from display to display, but the number of objects is kept constant. After a while, the subject typically habituates; it looses interest and only takes a brief look when a new display is introduced. When a preset number of habituation displays have been shown, the subject is presented with one or more test display containing either the same or a different number of objects as the habituation displays. During the test, it is observed whether the subject looks longer at displays containing a novel number of objects (dishabituates). If it does, it is taken as an indication of sensibility to the change in the number of objects. Apart from bounded surfaces (dots, pictures of objects etc.), other types of visual stimuli (such as jumps of a doll) have been used i habituation tests. Other sense modalities, such as such tones or syllables have also been used with the test paradigm.

Using this paradigm, 22 weeks old infants have been tested able to distinguish between two and three dots varying in arrangement, but not between four and six dots (Starkey & Cooper, 1980). 6-8 month olds have been tested able to distinguish between photographs of two and three household objects varying in type, size, and arrangement, and they in addition seemed able to associate two and three sounds with the same respectable number of objects (Starkey *et al.* , 1990). Six-month old infants were able to discriminate two from three jumps of a doll (Wynn, 1996).

Infants were also early on subjected to violation of expectancy experiment similar to those used on primates. The method was in fact introduced in a landmark experiment on infants conducted by Karen Wynn in 1992. The result of the experiment suggested that five-month-old infants had expectations to the outcome of the arithmetic operations $1 + 1$ and $2 - 1$. Wynn saw this as evidence that infants are not only capable of discriminating and representing numbers, but are also capable of performing actual calculations (Wynn, 1992a).

The optimistic conclusions by Wynn and others have been replicated and used in parts of the philosophical literature (such as Dehaene, 1997; Lakoff & Núñez, 2000). Unfortunately, very few of the early studies using the violation of expectancy paradigm made systematic controls for non-numerical factors such as surface area, total contour length and other factors co-varying with the number of objects. Even though Starkey, Spelke and Gelman (1990) for instance randomized the size of the objects, this does not constitute a systematic control, and on average two objects of random size will have twice the area of one object of random size (provided all objects are taken from the same collection).

The seriousness of the problem of co-varying non-numerical factors became apparent after two studies, one with systematic control for contour length (Clearfield & Mix, 1999) and the other with systematic control for surface area (Feigenson *et al.* , 2002a). As it turned out, the infants very clearly responded to a change in surface area and contour length, but they did not respond to changes in numerosity if the surface area or contour length was held constant. So for instance, in one of the set-ups used by Feigenson *et al.* , infants were habituated to one large figure, and then presented with either two small figures (same surface area, different number) or one small figure (same number, different area). No significant change in looking time was observed when the two small figures were introduces, whereas a significant response was observed when the surface area was changed with the introduction of one small figure. Similar results were obtained in all of the other set-ups of both studies.

In a subsequent study by Feigenson, Carey and Hauser a similar conclusion was obtained using free choice test on 10 and 12-month-old infants. The infants were given the choice of two quantities of crackers placed one by one in two opaque buckets. Both groups of infants reliably chose the largest quantity on 1 vs. 2 and 2 vs. 3 crackers, but not on 3 vs. 4, 2 vs. 4 and 3 vs. 6. The results were stable on controls for complexity and duration, but not for controls for area. Given the choice of one big (78 $cm^2$) vs. two small crackers (total area of 39 $cm^2$) the children went for the big cracker, and when given the choice between the same amount distributed in either one big or two small crackers, the children chose by chance (Feigenson *et al.* , 2002b). This clearly shows that the children's choice is based on quantity, nor numerosity.

These results clearly refute the original interpretation of Karen Wynn's experiments. The subjects in the study probably formed expectations about the total surface area of the objects used in the tests, and not about their number. The same explanation applies to several other early experiments where no systematic control for surface area was performed, including the study by Starkey & Cooper (1980).

However, sensitivity to surface area cannot explain all of the results. In some instances, number does seem to play a role for the infants' performance. The was the case in the study by Starkey & Cooper mentioned above. Here, no controls were made for surface area, and (as expected) infants were able to distinguish between two and three dots (of the same size). This simply shows that infants are able to judge surface area at a 2:3-ration. However, the infants were not able to distinguish between four and six dots (of the same size). As the ratio of surface area was exactly the same in the two conditions, surface area cannot explain this difference in performance. Is simply seems as if numerosity indeed somehow is important for the infants' performance in some situations.

This conclusion is backed up by a number of experiments where infants indeed do seem to base their judgment on numerosity, and not on continuous factors such as surface area. More specifically, children seem to base their judgment on number in two different conditions.

Firstly, when many ($\geq$ 4 or 5) objects are present, infants do seems primarily to pay attention to numerosity, and not to surface area. Several habituation tests with careful control for non-numerical factors such as surface area and density have been conducted. In these tests, six-month-old infants are able to discriminate between 8 and 16 dots, and between 16 and 32 dots, but do not reliably discriminate 2 from 4 dots, as expected in a set-up with effective controls for size. Also, the infants cannot discriminate 8 from 12 and 16 from 24 dots, suggesting that six-month-olds are capable of discriminating large numerosities with a 1:2 ratio, but not with a 2:3 ratio (Xu & Spelke (2000); Xu (2003); Xu *et al.* (2005)). In a similar subsequent study 10-month-olds were able to discriminate 8 from 12 elements, that is in a 2:3 ratio, but not 8 from 10 elements (4:5-ratio) (Xu & Arriaga, 2007). These results show that infants clearly base their judgment on numerosity when more objects are involved, and on area when less objects are involved. Furthermore, they show that the judgment of large sets is less precise than judgment of small sets.

Secondly, under the right circumstances infants base their judgment on numerosity, even when only a few ($<$ 4) entities are present. The 'right circumstances' might be stimulus consisting of events instead of objects, or situations where the number of objects, not their size, is of principal concern to the infants. As an example of the event-type of stimuli, six-month-old infants have been shown able to discriminate two from three jumps of a doll in a habituation study by Karen Wynn (Wynn, 1996) (although Wynn did not control for the height of the jumps the study this and thus left the possibility of discrimination in terms of total trajectory length open to the infants). And neonates seems to be able to discriminate between two and three syllable utterances, but not between four and six syllable utterances (controls were made for duration) (Bijeljac-Babic *et al.* , 1993).

Turning to situations where numbers are of more importance than size, a search-box study by Feigenson and Carey might serve as example. In the experiment 12- to 14-month-old infants were able to base their decision to reach in a search-box on the number of objects known to be in the box. That is, if $n$ toys were placed in the box, the infants would use considerably more time reaching in the box when $n-1$ or less toys were retrieved than when $n$ toys were retrieved for all $n < 4$. This clearly shows that the infants were able to represent exactly one, exactly two and exactly three, but not exactly four. Controls were made for size using a set-up, where two small toys were placed in the box. On some tests, one of the small toys were surreptitiously swapped with one big toy, but the infants used the same amount of time reaching in the box again whether they retrieved a small or a big toy in their first search. This proofs that the infants based their decision to reach in the box on the number of toys known to be in the box, and not on the total amount of toy. As the toys furthermore were placed in the box one by one on this last trial, the result in addition indicates that infants had correct expectations to the outcome of the operation $1+1$ (Feigenson & Carey, 2003).

### 4.2.9 Innate arithmetic

Next, we might return to the claim made by Karen Wynn (1992a), that infants not only represent numbers, but are also capable of reasoning about numerical information. Wynn's claim was originally based on the violation of expectations experiments indicating that infants have expectations to the outcome of simple operations such as $1+1$ and $2-1$. As we have seen, subsequent work questions Wynn's interpretation of the experiments. The infants' performance might not have been the result of numerical computations, but is most likely the result of simpler object-tracking mechanisms. Even though infants correctly expect two toys to be present in a box when they are placed there one by one, this can easily be explained with simpler mechanisms such as the opening of object-file tokens in short term memory and one-to-one comparison between object files and the physical situation.

So infants might no after all be able to perform true computations involving small numbers. However, two resent studies by Karen Wynn and Koleen McCrink indicate that infants have some numerical competences beyond mere representation when it comes to larger sets. In the first experiment, Wynn and McCrink used a violation of expectancy paradigm with careful control for surface area and contour length to demonstrate that 9-month-old infants look significantly longer (indicating surprise) at the unexpected outcome of the operations $5 + 5 = 5$ or $10$ and $10 - 5 = 5$ or $10$ (McCrink & Wynn, 2004). The second test was a habituation test, where 6-month-olds were presented with pictures containing different numbers of large yellow Pac-Men and small, blue pellets mixed in different rations. The result of the test indicate that the infants were able to discriminate between ratios that differed by a factor 2, but not between ratios that only differ by a factor 1,5 (McCrink & Wynn, 2007). So infants might after all have some expectations to the approximate (very approximate) outcome of operations on large sets of elements, although there is no direct proof that they are capable of performing numerical computations with digital precision.

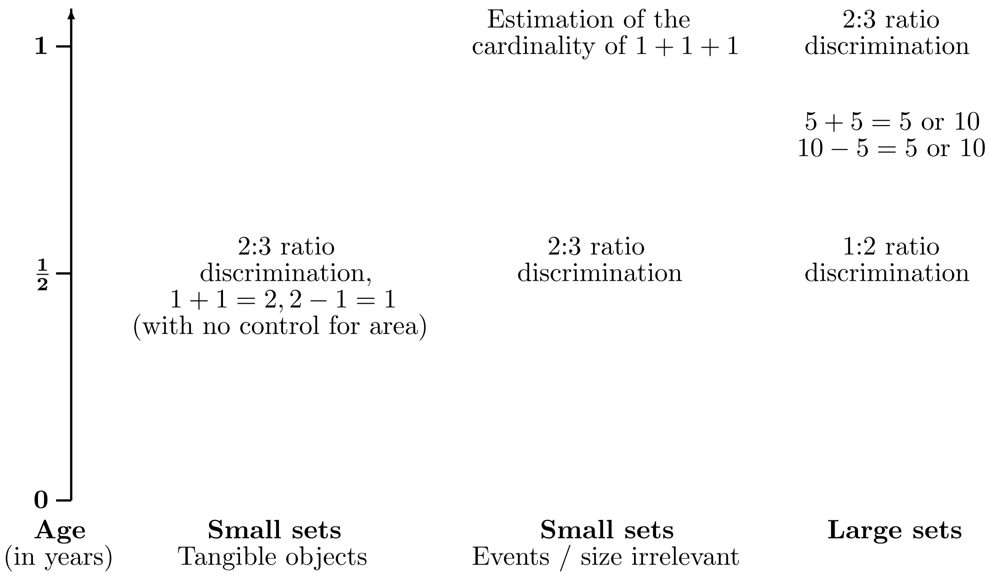



**Figure 4.8:** Overview over infants' mathematical skills. Here, small sets are counted as sets containing less than 4 or 5 elements.

### 4.2.10 Mechanisms for encoding

So in sum, infants seem to be able to estimate the numerosity of large numerosities, and they seem to rely on precise numerical information about small collections of entities when either no information about size is available or size is of secondary importance to them (see table 4.8 for an overview). This data has been taken as proof of the existence of two different systems, just as we saw in the case of primates; one system for the handling of large collection and one for the handling of sets with less than four elements (as suggested by Feigenson *et al.* , 2002a). As the infants' performance on large collections is governed by the characteristic Weber Law, it seems reasonable to hypothesize that numerical information is encoded in analog magnitude representation, such as the accumulator model or similar.

Turning to the smaller system, several mechanisms such as subitizing, counting using innate 'numerons', and the object-file mechanism has been suggested. As pointed out by Marc Hauser and Susan Carey (1998) numeron counting is not consistent either with the existence of an absolute upper limit to the system or with the learning curve of children learning to count using conventional numerals. It also seems very difficult to explain the given data with a pattern-recognizing mechanism such as subitizing. Why do the infants react differently to the same pattern of say 'three elements' in different situations; when three crackers are presented, infants react to area, not number, but when three toys are hidden in a search box, they react to number.

Much of the data on the other hand can be explained by a modified version of the object file model (as suggested by Feigenson *et al.* , 2002a, p. 63). In the object file model, objects are encoded in short memory. Numerical aspects of the situation is supposed to be represented by the number of files opened, but Feigenson and colleagues suggest that other aspects of the situation, such as the size or type of the individual objects might also be encoded in the individual files as well. Various computations could be performed using such multi-dimensional files. Infants might form numerical expectations, but they might also exploit information about non-numerical aspects of the situation, such as surface area or size of the represented entities. So in both subitizing, the numeron-counting model and analog magnitude model only quantitative information is represented, but the modified object file model is supposed to be a richer form of representations, where both quantitative and qualitative aspects of the situation is encoded. This of course fits very well with the fact that infants, when dealing with stimuli containing less than four elements, apparently can base their decision on either total size (as in the case of the crackers) or numbers (as in the case of the toys in the search box).

In sum, Lisa Feigenson and colleagues imagines a two-system model consisting of modified object file representations for situations where few elements are present, and the usual analog magnitude representations for situations with more elements. In the object files, both quantitative and qualitative information is represented where only quantitative information is represented in the analog magnitude system.

This combination of the modified object file model and analog magnitude representations is able to explanations some of the more puzzling data. In Cooper's experiment, infants were able to distinguish two from three dots, but not four from six. This is explained by the limit to the number of object files. Since the two and three dots could be represented using object files and all dots were of the same size, the infants could rely on either one-to-one numerical comparison or judgment of total surface area to distinguish between habituation stimuli and test stimuli. In the four vs. six dots situation on the other hand the number of elements was above the upper limit of the object file system, and the situation could only be handled by the less precise analog magnitude representations. So even though the ratio between dots were the same in both test situations, the infants performed differently because they handled two situations with two different representational systems.

A similar explanation can be given for Fei Xu's observation that six-month-olds are able to distinguish between 4 and 8 dots, but not between 2 and 4 (Xu, 2003). In this case, the 1:2 ratio was big enough for the infants to discriminate between the different sets of dots using analog magnitude representations. Furthermore, controls were made for area in this experiment, so there would be no difference between mean area of dots in test and habituation situations. This explains why the infants were more likely to notice a difference in the case where they only represented quantity (4 vs. 8) using analog magnitude representations, than in the case where they represented both area and quantity using object files (the 2 vs. 4 case).

Finally, when confronted with non-tangible events such as sounds or jumps of a doll, infants also seem sensible to the numerical aspects of the situation. Feigenson *et. al.* suggest that infants might use analog magnitude representations in these situations, where no objects are present (Feigenson *et al.* , 2002a). This however is not consistent with six-month-old infants being able to discriminate jumps of a doll in a 2:3 rate (Wynn, 1996). So it seems as if infants do use object file representations also in the case where events and not objects are observed. The explanation of their sensitivity to number in these cases might simply be that the number of something is a much more salient feature in the case of events than in the case of objects, where particularly area for foraging reasons might be very interesting to the infants.

In conclusion, infants do seem able to represent numerical aspect of stimuli, either approximately by use of analog magnitudes or by creating simple mental models in the form of object files. Infants can habituate to numerical aspects of a situation, and they can base a decision (to reach in a search-box or not) on numerical information.

### 4.2.11 Innate arithmetic in adults

Finally, let us turn to human adults. Many adults of course have learned to perform verbal counting and perhaps also symbolic computations to some extend. But apart form these advanced techniques, human adults also seem to have something similar to the large, approximate system detected in both human infants and several species of animal. This has been shown by a number of experiments, where adults were prevented from using verbal counting, typically by forcing them to respond too quickly to be able perform a digital counting process. Under such circumstances, adults' performance is governed by the characteristic Weber Law, indicating the use of analog magnitude representations. This was for instance the case in the experiment by Cantlon and Brannon cited at the beginning of this chapter, where college students were asked to estimate the sum of two sets of dots (Cantlon & Brannon, 2007).

Similarly, Hilary Barth, Nancy Kanwisher and Elizabeth Spelke conducted a number of experiments, where adults were asked to pick the more numerous of two sets of stimuli. The stimuli could be sequences of tones, sequences of flashes, arrays of dots or combinations, where the subject for instance had to compare the number of flashes of light with the number of dots in an array. The results indicated a clear Weber Law signature: The accuracy decreased as the ratio between the two sets got closer to one, but the accuracy did not depend on the absolute size of the sets. Furthermore, the accuracy for both modes (visual and auditory) and both formats (sequential and simultaneous presentation) were approximately the same, and the accuracy only decreased slightly when cross modal and cross format stimuli was used (Barth *et al.* , 2003). This result suggests that the internal representation is abstract, i.e. amodal and independent of the format of the stimulus.

The experimental paradigm used by Mechner (1958a; 1958b) to test the numerical abilities of rats has also been applied to human adults. In a study by Cordes, Gelman and Gallistel, test-subjects were required to press a key a specific number of times under two different conditions; one in which they were required to count the key presses out loud and one in which they repeated the word “the” on each key press in order to prevent any form of counting. The adults were able to estimate the number of key presses even when counting was suppressed. In this condition furthermore, the variability of the number of presses was proportional to the mean number of presses for the given target number (i.e. scalar variability) as the rats of Mechner’s experiment, which is in agreement with Weber’s Law. In contrast, when subjects were allowed to count out loud, the variability was proportional to the square root of the mean number of presses (binomial variability). This change of variability strongly indicates that two different representational systems were used in the two different conditions (Cordes *et al.* , 2001).

Interestingly, when human adults are asked to decide which of two Arabic numbers are the largest, a characteristic distance effect is observed: The reaction time decreases as the distance between the two numbers increases (Moyer & Landauer, 1967; Dehaene *et al.* , 1990; Temple & Posner, 1998). This suggests, that even symbolically presented numbers are encoded using analog magnitude representations.

We can also ask, whether adults use something similar to the special mechanism primates and infants use for the handling of small numerosities. There is strong evidence, that human adults have some sort of object-based mechanism that allows them to track and encoding properties (such as color and orientation) of up to four objects in parallel (Pylyshyn & Storm, 1988; Luck & Vogel, 1997; Kahneman *et al.* , 1992). It has also been suggested that this object file mechanism (perhaps in combination with subitizing or other rapid pattern recognizing mechanisms (Cordes & Gelman, 2005)) is used when adults enumerate up to four elements. The reason why it is assumed that a special mechanism is used in the enumeration of small quantities is a characteristic ‘elbow’ in the reaction time curve; When adults are asked to enumerate small quantities of elements, both reaction time and error rate is flat for the first four elements, with fast reactions and very few errors. But after four, both reaction time and error rate increase with the number of elements (Mandler & Shebo, 1982; Trick & Pylyshyn, 1994).

However, recently this claim has been the subject of some discussion. No special signature for small numbers was found in the key-pressing experiment by Cordes, Gelman & Gallistel (2001). The subjects were apparently using the same analog magnitude representations for all sets regardless of their size. This might be explained by the sequential nature of the stimuli, but the result at least indicates that object files cannot be the only non-verbal mode of representation of small numbers. The existence of the ‘elbow’ in reaction time even for simultaneously presented visual objects has also been contested by Balakrishnan and Ashby (1992), who were not able to reproduce the proposed discontinuity in a test where adults were asked to rapidly enumerate arrays of simultaneously presented blocks. These results cast some doubt on the original studies of reaction time, and it remains controversial whether adults use a special system for estimation of size etc. of small sets of elements or whether they use the same analog magnitude representations for both small and large sets (see Cordes & Gelman, 2005, p. 138 for a discussion).

In sum, we have identified two core systems of numerical competence; a large approximate system for the handling of large quantities and a small, precise system for the handling of small ($< 4$ or 5) quantities. Both systems have been identified in human infants enabling them to estimate the size of large collections of items and form expectations about the outcome of simple arithmetic operations (such as $1 + 1 + 1$, $5 + 5$ and $10 - 5$). Human adults also seem able to access at least the large, approximate system when digital counting is not possible. Tests involving the comparison of two number symbols even suggest that number symbols and words are mapped onto and internally represented using the large system of analog magnitudes.

## 4.3 Limitations of the innate skills

The limitations of inborn mathematics are well illustrated by a recent discussion about the innateness of the number line. The number line is a basic mathematical concept. It is typically introduced early in mathematical education, and most people seem able to learn and understand how to use it. The concept is, on the other hand, not too basic; it is far from trivial to learn how to map numbers onto a unidirectional line in the right order and using the right metric. This makes the number line concept perfect for a discussion about the limitations of innateness.

As noted above, humans – and several species of mammals – seem to posses a large, approximate system where numbers are encoded as some kind of analog magnitudes. Several authors have suggested that this unspecified analog magnitude may be likened to an innate, mental number line (see for instance Dehaene *et al.* , 1993; Dehaene, 1997; Zorzi *et al.* , 2002). This idea is primarily supported by two different sources of evidence.

Firstly, a puzzling effect was observed in an experiment conducted by Dehaene and colleagues 1993. In the experiment, adult, western subjects were requested to judge whether a target number (represented with Hindu-

Arabic numerals) was larger or smaller than a given anchor. The subjects were given two buttons, one to indicate that the target was smaller than the anchor, and the other to indicate that the target was larger than the anchor. The subjects were furthermore required to operate one of the buttons with one hand and the other with the other hand. As it turned out, it took the subjects significantly longer time to give a 'larger than'-response when the 'larger than'-button was operated with the left that with the right hand. And conversely, it took significantly longer time to give a 'smaller than'-response when the 'smaller then'-button was operated with the right hand. This asymmetry in response time was called the SNARC effect (Spatial Numerical Association of Response Codes effect), and according to Dehaene *et al.* , the effect shows the existence of an quasi-spatial, mental number line bearing "a natural and seemingly irrepressible correspondence with the left-right coordinates of external space" (Dehaene *et al.* , 1993, p. 394).

Secondly, a number of experiments probing the ability to map numbers onto a line segment have been conducted on both Western children (Booth & Siegler, 2006) and members of an indigenous tribe (the Mundurukú), known only to possess very limited mathematical knowledge (Dehaene *et al.* , 2008). In the experiments, subjects were presented with a line segment and were told the numbers located at the endpoints (0 or 1 at the left endpoint and 10, 100 or 1000 at the right endpoint, depending on the experiment, see figure 4.9). After this instruction, subjects were required to indicate the location on the line of a test set of the numbers in the interval between the endpoint-numbers. In the experiments on Western children, numbers were always represented symbolically (using Hindu-Arabic digits), in the experiment on members of the Mundurukú people, numbers were presented either symbolically (as Mundurukú or Portuguese number words) or as sets of tones or dots.

According to the two studies, all subjects were able to map numbers onto a line segment while consistently observing the basic principle of order (i.e. in this setting: greater numbers are always located to the right of relatively smaller numbers). However, Western kindergarteners and Mundurukú people used a logarithmic mapping, allocating relatively more space to smaller numbers. For Western children the logarithmic component gradually disappears during the first few grade, and they start mapping numbers onto space linearly, allocating the same amount of space to all numbers. According to Dehaene *et al.* (2008), these results reveal the existence of both a universal and a cultural-dependent facet of the number line. Numbers are innately represented in a logarithmic format, but with the effect of education, this logarithmic representation can be transformed into a linear representation.

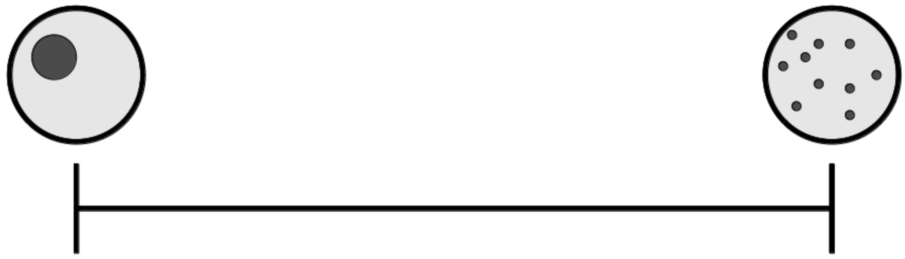


**Figure 4.9:** In (Dehaene *et al.* , 2008) members of the Mundurukú tribe were presented with a horizontal line segment, labeled with a set of 1 dot on the left endpoint and a set of 10 dots on the right endpoint, similar to the above. Subjects were told that 1 was located at the left endpoint and 10 on the right endpoint. Subsequently, subjects were required to locate numbers presented either symbolically as number words or as sets of dots or tones onto the line segment. Both line segment and labels were present during the test.

Several points of criticisms can, however, can be raised against the data supporting the idea of an innate number line. Concerning the SNARC effect, several experiments have shown the effect to be a laboratory artifact. It is a product of a strategic choice used in a particular experimental setup, rather than the product of a universal mode of mental representation (Fischer, 2006). Most strikingly, the SNARC effect was reversed when subjects were presented with a usual clock face and asked, whether selected numbers in the interval 1 to 11 was greater or smaller than 6. In this experimental setup, small numbers (1-5) were apparently associated with right and large numbers (7-11) with left (Bächtold *et al.* , 1998). Also, in an experimental setup where subjects were required to classify numbers as larger or smaller than a given anchor by pressing either a key close to or a key far away from a given center, the SNARC effect completely disappeared (Santens & Gevers, 2008). Instead of the left-right association of the SNARC effect, an association between the close response-key and numbers smaller than the anchor, and the far response-key and numbers larger than the anchor was observed.

Concerning the ability to map numbers onto a line segment, two lines of criticism have been followed. Firstly, it should be remembered that the number line was brought into the experiment by the experimenter, not by the test-subjects; due to the experimental paradigm, subjects were forced to report their numerical experiences using a number line (as also noted in Núñez, (accepted for publication)). For this reason, the experiments can only be used to determine to what extend subjects are able to master this mode of reporting. A positive answer to this question does not allow us to conclude that the subjects' internal representations of numbers are also linear.

This point was underlined in a number of recent experiments, where subjects were required to use other modalities, such as squeezing a ball, clanging a bell or singing with varying intensity, to report the relative size of numbers (Núñez (accepted for publication), and personal communication). As it turned out, subjects were able to use these alternative forms of reporting while maintaining the crucial property of order. Such experiments, however, clearly does not show that the subjects internally represent numbers as squeezing or vocalizing force, only that they are able to report numerical experiences using these modes of reporting.

Secondly, a careful study of the answers given by the Mundurukú reveals a much more profound cultural component than that accepted by Dehaene *et al.* . Such an analysis is carried out in (Núñez, (accepted for publication)). It should be noted, that the Mundurukú population tested in (Dehaene *et al.* , 2008) is not uniform; it includes both adults, children and subjects with or without mathematical training. The only part of this population suitable for testing the innateness of a mental number line is the uneducated adults. A closer study of this part of the population reveals that the subjects 1) failed to associate the lowest numerosities with the left endpoint of the number line, and 2) in the case of tonal input even failed to observe the fundamental principle of order – 1 tone was on average located 40% down the response line, while 2 tones were located only 30% down the response line (Núñez, (accepted for publication)). These results imply that the uneducated Mundurukú in fact were not able to map numerosities onto space in a consistent way. So if anything, the experiments by Dehaene *et al.* (2008) show that a mental number line (log or linear) is not even partly universal, but a complete product of cultural specific education.

This conclusion is backed up by historical evidence. Although numbers have been used for geometrical measurements, the actual number line was only introduced relatively late (in the 15th or 16th century). One of the first well-known introductions to the number line concept is found in Wallis (1685). As noted by Núñez ((accepted for publication)), Wallis take great care in describing the details of the number line concept, and explains it using analogies to well-known physical activities (walking so-many steps forwards or backwards). This clearly marks the introduction of something new and foreign to the mathematical community, and not simply the use of a well-known or intuitive idea.

All in all, the evidence does not prove the existence of an innate, mental number line. Numerosity might be internally represented somehow as an analog magnitude, but the representation is not inherently spatial. The ability to map numbers onto space in a consistent way (either log or linear) is culture-dependent – it must be learned.

---

The controversy over the innateness of the number line is interesting for two reasons. Firstly, it reveals a clear limit to our innate, mathematical skills; the number line is not innate, but culturally mediated. Secondly, the case serves as a warning against over-interpretation of experiments. The observed abilities turned out to be laboratory artifacts produced by a particular experimental setup (in the case of the SNARC effect) and by a particular choice of reporting mode (in the case of the ability to map numbers onto space). However, the reporting mode is part of the experiment, not part of the test subjects' cognitive apparatus, and that should not be forgotten.

## 4.4 Mathematics as a product of biological evolution

Finally, we can return to the overall research question guiding this work: What is mathematics and how was it developed? In the first section, we saw that it is not possible to give mathematics a foundation in the form of either logical truisms or otherwise self-evident axioms. On the contrary, the ZFC-axioms currently accepted as a foundation for most mathematics was not accepted because of their self-evidence, but because they made it possible to derive the mathematics already accepted. That is, the axioms were accepted for extrinsic reasons, not for intrinsic. This left us with the question: Where does the accepted mathematics come from and why is it accepted as true?

In this chapter, we are investigating extend to which mathematics is created by evolution. After having reviewed the available empirical evidence, we can conclude that humans have cognitive systems that allow them to estimate the approximate, relative size of large collections and the precise size of small collections. Furthermore, we seem able to form expectations to the outcome of basic, arithmetic operations. These abilities are innate and shared with several species of animals, both evolutionary close to us (such as primates) and evolutionary distant (such as birds).

From this, it might seem that the foundation of mathematics is formed by evolution, and not by logic or abstract reasoning. This claim must, however, be modified for several reasons. As we saw in section 4.2.6, it is questionable whether the abilities used for numerical and arithmetic judgment are selected for these purposes. In most real-world settings, numerical and non-numerical factors are interwoven, and the cognitive mechanisms used to form arithmetic expectations in one setting are in others used to choose the largest amount of food. For that reason, it is much too strong to claim these abilities to be rudimentary mathematics or to say that they form a number sense. The cognitive abilities allowing animals and uneducated humans to perform arithmetic or numerical judgments are most likely general-purpose mechanisms that express themselves as mathematical only when subjects are situated in highly artificial experimental setups. Evolution has not give us mathematical abilities; it has given us general-purpose abilities that happen to enable us to succeed on tasks involving numerical and arithmetic judgment.

Furthermore, the fact that the behavior of animals and human children is in some cases consistent with mathematical reasoning does not prove that it is the result of mathematical reasoning. The tasks posed to the subjects can be solved by counting, adding, subtracting and so forth, but – as far as we know – this is not how the subjects actually proceed. The cognitive mechanisms supporting the mathematical skills are most likely pattern recognition, simple mental models (in the form of object-files) and some kind of analog magnitude representations. None of these mechanisms are inherently mathematical, and using them to solve problems involving numerical features do not constitute mathematical reasoning, let alone conceptual, mathematical knowledge.

What is worse, the innate mechanisms used by infants and animals seem to have qualitative shortcomings; simply widening the range of the object file mechanism or making the analog magnitude encoding more precise will not in itself be enough to constitute mathematical reasoning and knowledge, such as the concept of natural numbers, understanding of the property of order, the ability to add or subtract. Cognitive mechanisms qualitatively different from the innate mechanisms displayed in the experiments reviewed above are needed. For this reason, evolutionary theory can only explain a relatively small part of mathematics. The biological evolution has provided us with the cognitive prerequisites needed in order to develop mathematics, not with primitive mathematics. In order to understand and explain why and how mathematics proper was developed from these prerequisites, theories other that evolutionary biology are needed. We must move to another level of explanation.

## 4.5 Mathematics as a product of cultural evolution

There is one possible objection to this conclusion. Although mathematics is only to a very little extend the product of biological evolution, it might still be a product of evolutionary process. The mechanism of variation, selection and inheritance is in principle content free, and it might be used to model development in areas other then the world of living beings. In the 1970s, several authors proposed human culture to be developing through such evolutionary processes (e.g. Richard, 1976; Cloak, 1975). Cultural ideas and practices – sometimes called *memes* in analogy with biological genes – spread themselves, develop and compete for attention in ways, similar to the fight for life and proliferation seen in the biological world.

This theory of universal Darwinism (or universal selection theory) has been applied to the development of mathematics by several theorists, including Stanislas Dehaene (1997) and more thoroughly Helen de Cruz (2007). The theory takes departure in the facts that 1) mathematical ideas can 'replicate' by being communicated (in speech or writing), and 2) there is an over-production of ideas, so consequently ideas will have to fight for mathematicians' attention and for space in journals and university curricula *etc.* This sets the stage for a Darwinian process, where mathematical ideas are developed through mechanisms of random variation and non-random selection. In the case of mathematics, both internal and external selection forces are at play: Sometimes ideas a selected according to their internal mathematical value, and at others in accordance with their value to the surrounding society. The last type of selection forces can, as speculated by Dehaene, perhaps even explain why mathematics is useful in the description of nature:

> The evolution of mathematics is a fact. Science historians have recorded its slow rise, through trial and error, to greater efficiency. It may not be necessary, then, to postulate that the universe was designed to conform to mathematical laws. Isn't it rather our mathematical laws, and the organizing principles of our brain before them, that were selected according to how closely they fit the structure of the universe? The miracle of effectiveness of mathematics, dear to Eugene Wigner, could then be accounted for by selective evolution, just like the miracle of the adaptation of the eye to sight. If today's mathematics is efficient, it is perhaps because yesterday's inefficient mathematics has been ruthlessly eliminated and replaced.
>
> [...] Mathematicians generate an enormous amount of pure mathematics. Only a small part of it will ever be useful in physics. There

> is thus an overproduction of mathematical solutions from which physicists select those that seem best adapted to their discipline – a process not unlike the Darwinian model of random mutations followed by selection. Perhaps this argument makes it seem somewhat less miraculous that, among the wide variety of available models, some wind up fitting the physical world tightly.
> (Dehaene, 1997, pp. 250-51)

As I see it, the main problem facing such a theory is the question whether the development of new mathematical ideas can be characterized as *random*. In the case of biological evolution, the random variation is produced by mutations of individual genomes and (mainly) by the combination of genomes in sexual reproduction. It is hard to find exact analogies to these sources of variation in the development of mathematical ideas. In the case of cultural ideas, it could be argued that at least some of the variation is a product by transmission errors; mistakes are made, and sometimes they turn out to be adaptive, i.e. they are reproduced by other practitioners. But in mathematics, even this type of random variation is almost completely ruled out. As noted by Azzouni (2006), mathematics is unique as a social practice exactly because it resists change introduced by mistake. Although mistakes are ubiquitous in mathematics, they are easily recognized as mistakes – even by the practitioners who made them. Mistakes are seldom reproduced and never lead to deviant practices. So transmission errors cannot be a source of random variation in mathematics.

Furthermore, evolution is blind. It does not proceed towards a particular purpose or goal. Although expressions such as ‘natural selection’ or ‘the hand of nature’ does imply intentionality and goal-orientedness, such expressions should not be taken at face value. They are clearly metaphorical. Nature is not a sentient being selecting individuals with a particular goal in mind.

This feature of evolution also contrasts the development seen in mathematics. Mathematics is evidently not static. Variation and change do occur, but not randomly. Variations can at most be described as part of an trial-and-error process (Lakatos, 1976b), but trial-and-error processes are not necessarily evolutionary. Rather, as I see it, evolution is a type of trial-and-error, but there are other types, which not centered on the element of random variation. In the case of mathematics, new practices, definitions and techniques are frequently tried out, but they are done so intentionally and with a particular purpose in mind. They are never introduced as random variations of existing practices, and never without a particular goal in mind.

de Cruz seems to acknowledge parts of this line of critique, and she counters it with the observation that often mathematical ideas or techniques developed for one purpose, ends up being used for something completely different, not intended by the original creators (de Cruz, 2007, pp. 276). This, however, does not constitute randomness and purposelessness. Although a mathematical idea might end up being used for purposed not intended by its creator, it was still not created at random. And although a mathematician might use an existing idea for something new, she does so intentionally and with a purpose in mind. The evolutionary process of blind progress through random variation simply does not seem to apply to the development of mathematics.

As concluded above, in order to understand what mathematics is and where it comes from, something qualitatively different from evolutionary theories most be invoked. Mathematics cannot be explained either by evolutionary biology or the theory of cultural evolution. In the next two chapters, I will move to a different theoretical level and investigate the extend to which mathematics is a product of our particular cognitive style.

# Chapter 5

# Theories of human cognition

## 5.1 Computational theory of mind

The main goal of the next chapter is to discuss if and how the particular cognitive style and strategies used by humans have influenced our mathematical beliefs. Before doing so, it is however necessary to address what a sufficiently adequate theory of general human cognition should look like. And that is the goal of this chapter.

Cognitive science emerged as an independent discipline during the 1950's. By then, the discipline was dominated by the computational theory of mind, where cognition is believed to be nothing but information processing; The human brain was assumed to be a special kind of computer, equivalent in computational powers to a universal Turing machine, and all cognitive processes were taken to be purely syntactic, rule governed manipulations of internal symbolic representations.

Jerry Fodor's 1976 *The Language of Thought* is a very clear formulation of this theory. Fodor holds that thinking takes place in a special mental language, often for convenience referred to as *Mentalese*. The relation between Mentalese and natural languages, such as English or Danish, is the same as that between the machine language and the programming language of a computer (Fodor, 1976, p. 67). Although we communicate using natural languages, the actual processing only takes place, when the information has been translated into Mentalese.

Furthermore, sentences of Mentalese are represented as physical structures in the brain. The operations on such representations are purely causal, as the brain, like any physical object, is governed by the laws of physics (Fodor, 1976, p. 74). As a consequence, the operations on a sentence are only sensitive to the syntax, i.e. the physical form of the sentence, whereas the semantics or meaning of the sentence is causally irrelevant. Fodor furthermore assumes that the physical machinery of the brain (conveniently) is constructed in such a way, that a physical brain state $S_n$ only follows form a sequence of physical states $S_1 \dots S_{n-1}$, if it is the case that the corresponding sentences $F_1 \cdots F_{n-1}$ of natural language constitute a logical proof of $F_n$ (Fodor, 1976, p. 73). So to Fodor, thinking is nothing but formal operations on symbols physically instantiated in the brain.

This general outlook was also shared by and found its strongest expression in the classical paradigm of artificial intelligence research, sometimes referred to as GOFAI (Good Old Fashioned Artificial Intelligence). Herbert Simon and Allen Newell for instance famously claimed that a "physical symbol system [i.e. a formal system that is somehow physically instantiated] has the necessary and sufficient means for general intelligent action" (Newell & Simon, 1976, p. 87). Simon and Newell emphasize that they with the use of the word "necessary" want to claims, that *any* intelligent system – including biological organisms such as human beings – are in fact physical instantiations of formal systems. The strength of this claim is only enhanced by the fact that the symbols of the systems imagined by Newell and Simon, apparently do not designate anything external to the system they are a part of, but only refer to relationships and processes occurring within the system (Newell & Simon, 1976, p. 86). The symbols are in other words very similar to the implicitly defined symbols of Hilbertian formalism, and the thinking of symbol systems is to an extreme degree isolated from the external world.

During the 1980s, the computational theory of mind was met with considerably opposition from many different fronts (such as studies in animal vision, robot engineering, philosophy of consciousness, neuroscience *etc.*). I will not review all of the arguments here, but instead focus on a single line of criticism. This line of criticism simply points out that the theory is inadequate, because it fundamentally conceives cognition in a limiting way (for more points of criticism, see Johansen, 2003).

In the computational theory of mind, the brain is metaphorically conceived as a container. All cognitive content (memory, feelings, reasoning, *etc.*) is located inside the container in isolation from the physical world on the outside. Sensing, planning, and acting are supposed to be three clearly distinct activities, and they are supposed to be performed in the mentioned order: First you sense, then you use your internal cognitive resources to form a plan, and finally you enact your plan.

This container metaphor of cognition can be found in many traditional theories of cognition, such as the Cartesian model. It has also been applied in theories of mathematical cognition, for instance the so called 'abstract code model', where a tripartition between comprehension (of the mathematical problem), calculation, and response (for instance in the form of a written number) is hypothesized (Campbell & Epp, 2005).

Much of human cognition can indeed be described as contemplation taking place inside the head, such as the container metaphor suggests. But not all of it. Parts of human cognition seems to be interactive processes involving both the brain, the body, and the surrounding environment. Human cognition in short seems to be distributed and embodied. These interactive aspects of cognition are not captured at all by the container metaphor. On the contrary, the metaphor seems to rule them out as impossible. Consequently, the computational theory of mind (and other theories of cognition that exploits the container metaphor) cannot account for the interactive aspects of cognition. They are simply inadequate.

## 5.2 Embodied Cognition

As an alternative, I will introduce an embodied theory of cognition. This however, also takes a little clarification. Strictly speaking, there is no single 'embodied theory of cognition'. The term 'embodied cognition' has been used to characterize a number of quite different approaches to cognition. All of the approaches reject the basic ideas of the container metaphor, and they focus on examples of cognition that clearly do not fit into the tripartition structure, dictated by the container metaphor. They view cognition as a basically interactive process, somehow involving both the brain, the body and the surrounding world. When these basics intuitions are coined out in actual theory, profound differences, however, starts to show.

Margaret Wilson (2002) has identified six different embodied approaches, each making fundamentally different claims about the nature of cognition, and each taking cognition to be embodied for very different reasons. These six claims are:

1. Cognition is situated, i.e. cognition takes place in real world context and rely heavily on a continuous flow of input and output. This is apparent when, for instance, we try to locate a misplaced item by moving to the location where the item was last in use. Here, the process of remembering is not purely internal or mental, but is rather an interactive process, where perception, movement and the physical environment play vital parts.

2. Cognition takes place under time pressure. As biological creatures living in an dynamic environment we must be able to find workable (but not necessarily perfect) solutions to pressing problems. So in other words, cognition is constrained by – and consequently shaped by – our specific embodied nature.

3. Cognition is off–loaded onto the environment, or rather, we do not necessarily internalize all problems. Instead, sometimes we solve problems by manipulating the external world directly, as when we turn two pieces of a puzzle to *see* if they fit or not, or we reduce the problem space and demands on memory by sorting the pieces of the puzzle into piles of distinguishable parts of the motive.

4. The environment is part of the cognitive system, i.e. the most suitable analytic unit in cognitive science is not the individual mind, but functional relationships of elements participating in a specific task. Cognition might be distributed across the different objects or persons participating in any particular cognitive task.

5. Cognition is for action; our individual representations, percepts and concepts are directly connected to patterns of physical action.

6. And finally, off-line cognition is body based. Our off-line thinking exploits or (stronger) is structured by direct physical experiences and mental structures connected to motor action and perception.

It should be noted that these six different claims are not necessarily in conflict with each other. Rather, as I see it, they highlight different aspects of how our cognition is embodied and how it is performed as an interactive process involving both brain, body and the external environment.

As our main goal at this place is mathematical cognition, it might however be wise to cut the cake slightly differently. Firstly, claim 1), 2) and 5) do not apply directly to mathematical reasoning: Regarding 1), mathematical reasoning is not situated or at least only in ways covered by the remaining three claims. Regarding 2), the solution to a mathematical problem might be needed in a hurry, but in general, mathematics is not under the kind of time pressure aimed at in the second claim. The real world is dynamic, and consequently the optimal solution to a given problem is constantly changing. In contrast, the world of mathematics is static, and the solution to a given problem does not change over time. We might of course want the find the solution as fast as possible, and we might want different problems solved at different times, but still, the kind of time pressure found in mathematics is in principle different from the time pressure constraining cognition in general. Regarding 5), mathematical reasoning might ultimately be action guiding, but apart from basic intuitive mathematical skills such as subitizing, mathematical reasoning is not action guiding in the direct sense aimed at in the claim. For these reasons, I will not treat these three claims further.

Secondly, claim 3) and 4) both express the more general claim that cognition involve our physical surroundings, either because we refrain from mental modeling and use the world as its own best model (claim 3), or because we actively use and exploit the environment as a way to off-load mental content (claim 4). I will treat these two claims under a single heading and investigate if and how mathematical reasoning involves this kind of externalization.

I will treat the sixth claim it more or the less as it is stated, only I will differentiate between two different types of experiences: a) basic life-world experiences and b) culturally created experiences.

## 5.3 Externalization: Tools of thought

The distributed nature of human cognition is witnessed by our use of environmental resources as cognitive tools. As it turns out, our cognitive life seems to involve the physical and social surroundings to a surprisingly large extend. I for once constantly writes notes to my self, and I could not work, if my office was not structured in a very particular way: Books are alphabetized, papers are in organized by theme, I have a shelf for things, I really ought to read, and a shelf for books borrowed at the library. On my desk, I keep pen, stabler, post-it-notes – bills I have to pay – and of course the most important tool of them all; my lap-top computer. Although computers are commonly anthropomorphized and regarded as persons, they are in fact physical devices, build out of mainly plastic, copper and a few grams of silicon. But without a laptop or other means of writing, I am quite sure this dissertation would be a lot shorter, and probably also very different in style and content. So in other words, as a cognizing agent, I rely heavily on physical resources in my environment.

The distributed cognition movement is motivated by observations such as the above. Our cognitive life simply seems to involve the external environment in non-trivial ways. For this reason, the classical definition of cognitive processes as something going on inside an isolated persons isolated brain is simply inadequate. The unit of analysis in cognitive science should not be determined by the location of the process, but by "the functional relationships among the elements that participate in it" (Holland *et al.* , 2000, p. 175). Consequently, the distributed cognition approach is not so much interested in what is going on inside individual brains or minds, but mainly wants to explore how cognition is distributed "across internal human minds, external cognitive artifacts, and groups of people, and across space and time" (Zhang, 1997, p. 182).

### 5.3.1 Epistemic actions

An important concept in the distributed theory of cognition is *epistemic action*. Such actions were identified and defined by David Kirsh and Paul Maglio (1994). Where pragmatic actions are actions performed with a pragmatic goal, such as peeling potatoes, an epistemic action is defined as:

> [A] physical action whose primary function is to improve cognition by:
>
> 1. Reducing the memory involved in mental computation, i.e. space complexity;
> 2. Reducing the number of steps involved in mental computation, i.e. time complexity;
> 3. Reducing the probability of error of mental computation, i.e. unreliability.
>
> (Kirsh & Maglio, 1994, p. 514)

Kirsh and Maglio give several examples of such epistemic actions. If you have a tendency to forget your key, you might for instance leave it in your shoe. Then you will be sure to be reminded of your key, when you put on your shoes before leaving your apartment. By putting the key in the shoe, the key becomes a cognitive device that reduces both the probability of error and the demands on internal memory. Hence, the act of putting your key in your shoe is an epistemic action.

Apart from reference to recognizable every day situations such as leaving a key in a shoe, Kirsh and Maglio back their claims about epistemic actions and externalization of computations up by a thorough empirical investigation of people playing the now classical computer game *Tetris*. According to traditional theories of cognition, one would expect the gamers to build an internal model of the game situation, figure out what to do by manipulating the internal model, and finally enact that plan. So, when a new brick (or 'zoid' as they are more technically called) arrives at the top of the screen, the gamers supposedly should perceive the zoid and use their internal model of the the game situation to plan how to rotate and translate the new zoid in order to fit it in with the other zooids. Finally, the gamers should perform this plan by pressing the buttons used to control the game. Kirsh and Maglio discovered that this is not at all how actual gamers behave. As gamers get more and more skilled, they adopt a strategy, where the step of internal modeling and computation is avoided as much as possible. When a new zoid arrives, the gamers simply rotate the zoid using the rotation button in order to directly *see* where the zoid fits. In other words, they substitute internal, mental computations with a tight loop of action and perception.

It should also be noted that epistemic actions are only one of a number of different ways, humans use space intelligently. Epistemic actions simplify internal computations, but space, or rather the physical arrangement of objects in space, might also be used to simplify choice and perception (as pointed out in Kirsh, 1995).

The first (simplification of choice) can be achieved, for instance, by the use of informational cues or even physical constraints that reduce the perceived or actual degrees of freedom in a given situation. As an example, a door jam can function both as an informational cue, telling you not to close the door, and (if overlooked or ignored) as a physical constraint, blocking a particular action (Kirsh, 1995, p. 44).

The second (simplification of perception) can for instance be achieved, by sorting or segregating. Alphabetizing or thematically sorting the books on a shelf might greatly simplify the perceptual process of finding a particular volume. Or, to use Kirsh's own example, the task of telling the washed from the unwashed tomatoes can be simplified by placing each tomato in an other physical location on the kitchen table, as it is washed (Kirsh, 1995, p. 56).

### 5.3.2 Cognitive artifacts

In the examples give above, various cognitive tasks were performed in a cognitively more economic way by exploiting already existing environmental resources and structures in an opportunistic way. This opportunistic use of already existing objects, however, does not exhaust our use of external cognitive tools. As it turns out, our cognitive lives also include specially created tools or *cognitive artifacts*.

In the most narrow definition, cognitive artifacts are simply defined as "physical objects made by humans for the purpose of aiding, enhancing, or improving cognition" (Hutchins, 2001, p. 126). This definition of cognitive artifacts includes numerous everyday objects, such as shopping lists, calendars and computers. It also includes highly specialized artifacts, such as the so-called 'midwife's wheel' (figure 5.1). This artifact is used by midwifes to calculate the due day of a child. The wheel consists of two discs, one marked with the months of the year and the other with the weeks 1 to 42 of a normal pregnancy. Given the date of conception, the midwife aligns the discs in certain ways, and that allows her to read off both the current duration of the pregnancy and when the child is due to be delivered. In other words, the artifact allows the midwife to substitute complicated mental calculations with manipulative and perceptual (i.e. epistemic) actions; she simply slides the disks and read off the result. In this way, the artifact enhances and improves cognition, not by enhancing the mental powers of the cognizing agent, but by reducing the demands on the internal, mental resources. This way of functioning, by reducing the demands on internal resources, is typical for cognitive artifact.

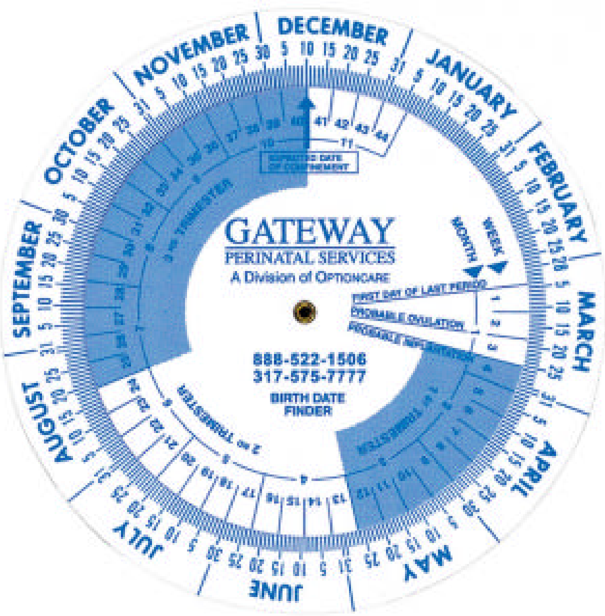



**Figure 5.1:** Midwife's wheel
(from www.imprintitems.com/images/products/healthcare/PregWheel.jpg)

Not everybody accepts the narrow definition of cognitive artifacts as physical objects, but argues for a more inclusive definition including non-physical artifacts such as concepts, rules and procedures:

> Reading, arithmetic, logic, and language are mental artifacts, for their power lies in the rules and structures that they propose, in information structures rather that physical properties. Mental artifacts also include procedures and routines, such as mnemonics for remembering or methods for performing tasks. But whether physical or mental, both types of artifacts are equally artificial: They would not exist without human invention.
>
> (Norman, 1993, p. 4)

As pointed out by Andy Clark Clark (1998a), language itself can be seen as the ultimate mental artifact. Language serves several purposes besides merely being a means of communication. The proper conceptual setting can drastically improve our learning curve, but most importantly, language is a way to turn our thoughts into external object:

> [. . .] as soon as we formulate a thought in words (or on paper), it becomes an object for both ourselves and for others. As an object,

> it is the kind of thing we can have thoughts about. In creating the object, we need have no thoughts about thoughts – but once it is there, the opportunity immediately exists to attend to it as an object in its own right. The process of linguistic formulation thus creates the stable structure to which subsequent thinkings attach.
>
> The key claim is that linguistic formulation makes complex thoughts available to processes of mental attention, and that this, in turn open them up to a range of further mental operations. It enables us, for example, to pick out different elements of complex thoughts and to scrutinize each in turn. It enables us to "stabilize" very abstract ideas in working memory. And it enables us to inspect and criticize our own reasoning in ways that no other representational modality allows.
>
> (Clark, 1998a, p. 177)

Or in other words, language makes it possible or us to turn our thoughts into external objects, inspectable from a third person perspective, and that is a prerequisite for the formation of high level cognitive processes. Something similar might be said about diagrams, drawings and other means to turn mental imagery into external objects. A point, I will discuss further below.

Andy Clark is not the only or even the first to air such views. The idea that language offers a way to turn our thoughts into objects is for instance present in Karl Poppers theory of the three worlds:

> By formulating a thought in some language, we make it a world 3 object; and thereby we make it a possible object of criticism. As long as the thought is merely a world 2 process, it is merely a part of ourselves, and it cannot easily become an object of criticism for us. But criticism of world 3 objects is of the greatest importance, both in art and especially in science.
>
> (Popper, 1980, p. 159)

Also, the idea that language is a powerful tool that both can help us organizing our thoughts and facilitate learning is present in the so-called 'activity theory', taking its departure in the work of Lev Vygotsky. What distributed cognition offer and adds to these theories, is mainly the ability to see the tool of language as part of a more general cognitive strategy, where cognition is enhanced or facilitated by the use of external objects, be they physical 'world 1' objects or culturally shared, mental 'world 3' objects.

Apart from producing objects of inspection and criticism, the representation of thought in external objects also offer conceptual stability. As noticed by Edwin Hutchins (2005), our ability to perform reasoning involving complex conceptual structures depends on our ability to represent these structures in an appropriate way. When we reason with a complex conceptual structure, we manipulate parts of the structure while the rest is kept stable. Our ability to simultaneously represent and manipulate such structures mentally is limited, but it can be facilitated by the use of external representation, which allow us to anchor some of the elements of the structure in stable, physical objects. A stable, external representation of the conceptual structure allows us to focus on the part of the structure manipulated on, and consequently, we might be able to perform much more complex manipulations. There is in other words a direct correlation between the stability of the external representation and the complexity of the manipulations we are able to perform on the conceptual structure, it represents.

Here, Hutchins do not only consider representations in the form of language. He allows for all kinds of physical representations, as we see them for instance in sliding rules, compass roses *etc.*

---

Finally, we might touch an interesting discussion concerning the precise impact of the cognitive artifacts. Do the artifacts merely enhance or strengthen cognition, or do they in fact do more than that? Might they be constitutive of certain cognitive processes? Or do they influence or even determine cognitive content? The answer to such questions will of course depend on which particular artifact we are looking at. The human memory is not unlimited, so even though we can make it to the grossers and buy the things needed without an external shopping list, at one point we will have to rely on external representation in order to extend our mnemonic capacity. Similarly our onboard computational powers are limited, and although a midwife might be able to calculate the due-day without external artifacts such as the midwife's wheel, at one point the naked brain cannot satisfy our calculational needs and we will have to rely on external calculating machines.

So external artifacts are constitutive of certain types of cognitive behavior, but this however might be in a very trivial sense; due to our brains limitations, external artifact must at one point take over and do the brains job, so to speak. This is merely a matter of quantity, but what is more interesting is the matter of quality, i.e. whether our use of external artifacts influence the content of the cognitive processes, and whether the artifacts make entirely new processes possible. An example of a qualitative gain might be our use of language. If Andy Clark and others are right, our ability to externalize thoughts in the form of words is constitutive of an entirely new type of cognition, i.e. abstract and second-order thinking. Whether the more specific properties of language, i.e. the specific concepts available, might have cognitive influence is a more debated question.

At this point, I will not go deeper into the debate over the impact of cognitive artifacts, but we will have to go much deeper into this discussion when we debate the use of cognitive artifacts in mathematics.

Finally, cognitive artifacts are of course not the only type of artifacts with importance for our ability to generate knowledge. Science, for instance, also depend on other artifacts, such as telescopes, microscopes, bubble chambers *etc.* Such instruments, however, cannot in general be classified as cognitive artifacts, since they do not aid cognition, but rather enhance our perceptive powers. A telescope, if anything, could be called an epistemic or perceptual artifact. Similar to the discussion above, it can be discussed whether such epistemic artifacts actually in some instances constitute what is perceived. Unfortunately, such a discussion falls outside the scope of this dissertation.

## 5.4 The use of sensory-motor experience in off-line thinking

### 5.4.1 Conceptual metaphor

The use of epistemic actions and physical cognitive artifacts implies that human cognition is embodied in a very concrete sense. A disembodied mind cannot put a key in her shoe or operate a tool such as the midwife's wheel. Our physical body offers possibilities and has limitations for interacting with the surrounding world, and these constrains on our bodily interactions condition which artifacts we can and cannot use.

But that is not all. Our body and basic bodily experiences also influence our cognitive life in a much more profound way. As it turns out, we seem to use basic life-world experiences as a way to structure abstract thinking. This structuring is revealed by our heavy use of metaphors taking basic life-world experiences as their source-domain.

Examples of such metaphors are easily found in everyday language. Take for instance the expressions: "I couldn't quite grasp what he was saying", "Everything he said just flew over my head", and "Did you get it?" In these examples, ideas are described as physical objects, understanding as grasping or holding such objects, and an exchange of ideas is described as an exchange of objects. As ideas are not physical objects, and cannot literally be thrown or grasped, these descriptions are clearly metaphorical; the abstract domain of knowledge is conceptualized using the concrete domain of physical objects.

This cross-domain mapping is not only used as a clever way to describe the target domain. What makes this kind of mapping interesting and cognitively powerful, is the fact that it can be used to transfer the inferential structure of the source domain to the target domain. If a person A is throwing an object to another person B, we know that B might not be able to catch the object thrown. We also know that it is easier for B to catch the object, if A is able to throw within the grasping range of B. Once the analogy between objects and ideas has been established, such inferences are easily and unconsciously transferred to the domain of ideas: If you are giving a lecture, you should be careful to aim what you say within the grasping range of the audience, or they might not be able to understand you.

Ideas can be conceptualized using a wide range of other metaphors. Ideas can be seen as living organisms; they can be born, mature, get old and die, and they can come to fruition or be planted in someone's mind. Ideas can be understood as food; they can be hard to digest, half-baked, rotten, fresh, or hard to swallow. Or ideas can be seen as cutting instruments or weapons: They can be sharp, dull or cut right to the heart of matters (see Lakoff & Johnson (1980, pp. 46) for further examples).

All of these metaphors help us understanding and structuring the abstract phenomena of ideas using well-known and concrete everyday experiences. The various metaphors highlight different aspects of the target-domain and offer guidance in different situations; when giving a lecture, you should be careful to aim what you are saying at the audience in order for them to catch your ideas, and when going to a debate (which is commonly conceptualized in terms of warfare!) it is wise to bring ideas at least as sharp as those of your opponent.

This type of metaphors, where a cross-domain mapping is used to transfer the inferential structure of one domain to another, is usually called 'conceptual metaphor'.

---

The cognitive approach to metaphor was introduced in the late 1970's, most influentially by George Lakoff and Mark Johnson (1980). According to this approach, the structuring of a concept in terms of concrete experience is not something exceptional or rare. In fact: "[...] metaphor is pervasive in everyday life, not just in language but in thought and action. Our ordinary conceptual system, in terms of which we both think and act, is fundamentally metaphorical in nature" (Lakoff & Johnson, 1980, p. 3).

Especially the abstract is typically conceptualized using concrete, physical terms. Thus, abstract thinking is grounded in basic life-world experiences and our sensory-motor system (Lakoff & Johnson (1980, 59); Lakoff & Núñez (2000, pp. 5)). Our basic, bodily experiences, and the metaphors we infer from them are, in a sense, "the hidden hand, that shapes conscious thought" (Lakoff & Johnson, 1999, p. 12).

A point of debate is the exact cognitive significance of such metaphors. When we talk about ideas as objects using the metaphorical expression "I couldn't quite grasp what he was saying", do we also *think* of ideas as objects, or is the metaphor merely a linguistic phenomenon? It is not very hard to find examples of *dead metaphors*, i.e. metaphors that once might have had cognitive significance, but clearly do not have so any more. Take for instance the expression: "I have examined 14 students today". The word 'examine' originates in the Latin 'examen', which literally means 'tongue of a balance'. So, examining student is – or was – originally a metaphor, where the process of judging the knowledge of a student was described by comparing it to the process of weighing goods at the marketplace. Today however, most English speakers do not know the original meaning of the word 'examine', and they do not think of balances or processes of weighing when they use it. The word has simply obtained a new meaning, and consequently, the original metaphor is dead and has ceased doing any cognitive work.

So, how do we know that not all of the metaphors discussed above are dead? This is a very good question that needs to be answered before the cognitive approach to metaphor and language can carry any philosophical weight. Part of the evidence put forth by the cognitive approach is linguistic. Most people would probably not accept expressions such as: "That idea was hard to swallow, I couldn't grasp it at all". The two parts of the sentence express the same phenomena of not being able to understand, but still, the sentence seems to be somehow inconsistent. The reason for this inconsistency is the fact that the sentence contains two metaphors exploiting two different source-domains; edibles and objects respectively. And although ideas can be understood as both edibles and objects, they cannot be done so at the same time. The metaphors used to conceptualize ideas must in other words be applied in a coherent way. This suggest that the analogies to basic experiences expressed in the metaphors still have cognitive significance and structure, not only the way we talk about ideas, but also the way we think about ideas.

Similarly, a sentence such as “that idea is full of vitamins” will probably be understood immediately and effortlessly by most people familiar with English, even if it is the very first time they hear the expression. This suggests that the analogy exploited in the ideas-are-food metaphor is still active and allows us to use knowledge of nutritional facts to understand new aspects of the abstract domain of ideas.

Apart from such linguistic evidence, the basic claim of the cognitive approach is backed up by empirical evidence from neural science. I will however restrict myself to a more thorough review of this type of evidence specifically put forth in connection to the conceptual metaphors used in mathematics (below, section 6.12.2). The reader is referred to (Lakoff & Johnson, 1999, pp.36) for a comprehensive list of the different types of evidence jused to justify the more general claim.

### 5.4.2 Metaphors of science

George Lakoff and Mark Johnson are particularly interested in metaphors taking life-world experiences as their source-domain. The widespread, almost ubiquitous use of such metaphors in abstract thinking underlines the embodied character of human cognition and the inadequacy of the container metaphor of classical theories of cognition. Although such life-world metaphors are especially important, the source domains of metaphors are not necessarily restricted in this way. The basic cognitive tool of understanding one thing in terms of another works as long as the source domain is something well-know or at least better-known than that, which we try to conceptualize via the metaphor. In science, new and unknown phenomena are frequently conceptualized using metaphors with source-domains in technology or other more well-established parts of science.

The Saturnian system, for instance, was used by early researchers as a source domain for metaphors describing the atom (the source domain was only later and mainly for philosophical reasons changed to the Solar system metaphor known today (Knudsen, 1999, pp. 106)). Similarly, geneticists of the 1950’s used the at the time highly popular field of cybernetics and information theory as a source-domain for metaphors describing hereditability and genetics. Such generative or theory-constructive metaphors help the researcher to structure observations, form expectations, and ask questions about the unknown field, and, as pointed out by Susanne Knudsen (1999, p. 151), they also serve as an effective tool to form a vocabulary for describing new phenomena; take as an example the ‘orbitals’ of electrons (derived from the orbits of planets), or the vast amount of information-related words, such as ‘transcription’, ‘sense & nonsense’, ‘genetic code’, and ‘messenger-RNA’, used by molecular biologists and geneticists to describe the processes involved in protein synthesis.

The cognitive status of such metaphors is somewhat less clear. They clearly had cognitive significance to part of the research community at some point in history, but the question is: when and to whom? The Solar system metaphor for the atom still serves as a didactic tool in explaining basic ideas of the atom to young students, but it is very unlikely that working physicists still use their knowledge about the Solar system to structure and form expectations about the atom. The status of the information metaphor in genetics is, on the other hand, heavily debated. Some commentators, such as Knudsen (1999, p. 152), claim that the information metaphor does not function as a metaphor any more, and question whether molecular biologists and geneticists understand words such as ‘messenger-RNA’, ‘transcription’ and ‘genetic code’ as metaphorical. Other commentators hold the metaphor to be very much alive and still structuring the researchers conception of the processes they describe (see for instance Oyama (2000)).

I will not try to settle the debate here. My point is only to note that similarly to the life-world metaphors we saw above, the cognitive status of the metaphors of science is debated as well. Metaphors are mainly of interest if they reveal something about how we think and understand a phenomenon, but how can we distinguish between cases, where metaphors are mere linguistic forms, i.e. dead metaphors, and cases where the metaphors have actual cognitive significance?

## 5.5 Conceptual blends

Conceptual metaphor is an example of cross-domain conceptual mapping, as one conceptual domain is mapped onto another. We do however, also use other types of conceptual mapping. Apart from cross-domain mappings there seem to be good and important examples of conceptual integration or ‘blending’, where elements from two conceptual domains are integrated and used to form a new conceptual domain instead of simply mapping one domain onto the other. The new blended domain will have emergent properties not in any of the original domains, and knowledge about the original domains can be gained by exploiting the emergent structure of the new domain.

This theory of conceptual blends was primarely developed by Gilles Faucounnier and Mark Turner (Fauconnier & Turner, 1998, 2003; Fauconnier, 1997). According to Faucounnier and Turner, the construction and use of the blended spaces usually involves three different, but interrelated, operations.

- Composition: Selected elements form the two input sets are composed in the blended space. This composition of the different spaces creates relations, which are not available in any of the separate input spaces.
- Completion: The blending is usually created on a background of conceptual and cultural knowledge. This allows the blend to be seen as part of a larger pattern, which can be used to complete the blend.
- Elaboration: When the blend is developed, new inferences can be drawn by elaborating the emergent structure of the blend in accordance with its own internal logic. This is called called 'running the blend' (Fauconnier 1997, p. 150-151; Fauconnier & Turner 1998, pp.142).

The riddle of the buddhist monk is a good example of the use (and power) of blends as conceptual tools (Fauconnier & Turner, 1998, pp. 136). The riddle takes departure in a story of a buddhist monk, who travels up a mountain, spends the night at the top, and travels down again the following day. Both the travel up and the travel down starts at dawn and ends at sunset, and the monk follows the same path on both journeys. The riddle consists in showing that there exists a point on the path that the monk occupies at the same hour of the day on both the travel up and the travel down. The riddle can be solved by creating a blended space taking the two separate journeys as input spaces. In this blended space, the two journeys are imagined as taking place simultaneously. It is intuitively clear that two travelers, who travel the same path in separate directions, will meet each other at some point on the path, or in other words, that the two travelers will be on the same point of the path at the same hour of the day. When this inference is projected back onto the input spaces, it is clear that the monk will be on the same point of the path at the same hour of the day on his two separate journeys.

Here, elements from the two input spaces are selectively chosen (the monk, and the path are chosen, but not the day of the journeys) and projected to a blended space, where the monk, who is identical in the input spaces, can be imagined as two travelers traveling simultaneously. This is the *composition* of the blend. The blend is *completed* by activating a larger pattern of people traveling towards each other, and finally, *running the blend* allows us to infer the encounter of the two travelers, which consequently give us the solution of the riddle.

---

The riddle of the traveling monk is a very simple and clear example of a conceptual blend. The mapping taking place here is clearly non-metaphorical; we do not simply describe one domain by mapping structure from another domain onto it. We clearly integrate elements and structure from the two separate domains into a new, imagined domain. In many other examples however, the division between the different types of mapping (conceptual metaphors and blends) is less clear, as the conceptual mapping taking place involves larger networks of spaces, and involves both cross-domain mapping (i.e. conceptual metaphor) and conceptual integration (i.e. blends) between these spaces.

### 5.5.1 Material anchors for conceptual blends

In the case of the traveling monk, the blended space is purely imaginative. As noticed by Edwin Hutchins, this is not always the case (Hutchins, 2005). In some cases, one of the input spaces has physical form, and here it can be used to create an external representation or 'material anchor' for the blend. One of the examples given by Hutchins is people queuing for theater tickets. According to Hutchins (2005, p. 1559), a line of people is not by it self a queue. The line only becomes a queue, if it is blended with an imagined, directed path or trajector. So here, a physical space – a line of people – is blended with an imagined space – a trajector – to form a blended space, the queue. The blend has emergent structure, most prominently the sequential ordering of the people, which is not to be found in any of the input spaces. It draws on a larger, culturally dependent narrative of 'serving people after a come-first, served-first principle', and by running the blend we can draw inferences such as 'how many people is in front of me'. So far this case resembles the case of the traveling monk, but in contrast to the monk, the queue is physically instantiated by the people actually standing i line. They form a material anchor for the blend.

The use of material anchors for conceptual blends connects the two cognitive tools *externalization* and *cognitive mapping*, described in this chapter. A material anchor for a conceptual blend is in effect an external, physical representation of a cognitive mapping, and it has the advantages and combined power of these two different cognitive tools.

## 5.6 The embodied mind

In conclusion, it seems very clear to me that the computational theory of mind – and the more general container metaphor for cognition, it is modeled upon – are inadequate to describe human cognition. As we have seen, human cognition is embodied in a non-trivial sense. Our physical body and brain is more than just a random piece of hardware necessary in order to run our cognizing program (as it is implied in the computational theory of mind). We actively off-load cognitive tasks onto our environment (both physical and social), and we use our bodily experiences as vital resource in our off-line thinking. These findings should have some impact on how we view ourselves as cognizing beings. As Andy Clark puts it:

> We must abandon the image of ourselves as essentially disembodied reasoning engines. And we must do so not simply by insisting that the mental is fully determined by the physical, but by accepting that we are beings whose neural profiles are profoundly geared so as to press maximal benefit from the opportunities afforded by bodily structure, action, and environmental surroundings. Biological brains, are, at root, controllers of embodied action. Our cognitive profile is essentially the profile of an embodied and situated organism.
>
> (Clark, 1998b, p. 273)

In the following chapter, I will turn to mathematics and discuss whether the changed conception of general human cognition should lead to a changed view of mathematical cognition, and if so, discuss the implications of this changed view.

# Chapter 6

# The cognitive level: Mathematical cognition

## 6.1 The naturalistic account

The mode of mathematical reasoning is commonly believed to be *a priori* deductions. Mathematical reasoning has been taken to be untouched by human nature, i.e. by the fact that we have physical bodies of a particular type and are situated in a specific physical, social and cultural environment. The assumption that mathematical knowledge seems to be eternal and absolutely objective rests on this very fact; if mathematical reasoning is in some way influenced by our biology and conditions of life, we cannot claim our mathematical knowledge to be strictly objective.

This idea fits well with the computational theory of mind. Here, cognition is taken to be hardware independent; cognition is simply a program that happens to run on the carbon based hardware of the human brain, but it would in principle run equally well on other types of hardware, such as silicon based electronic computers or purely mechanic machines such as Charles Babbage's 'difference engine'.

The computational theory of mind is however, as we saw in the previous chapter, inadequate as a theory of human cognition. Vital parts of human cognition are embodied, and consequently, human cognition in general is highly dependent both on the hardware of the human body and on the environment we happen to occupy. This observation does not allow us to conclude that mathematical cognition is embodied as well; if anything, mathematical cognitions seems to be the most likely candidate for an area where cognition could be performed in a way consistent with the computational theory of mind, i.e. as purely *a priori* deductions performed completely inside the cognitive container. On the other hand, if mathematical reasoning proves to be embodied, it will to some extend be dependent on contingent factors, and thus mathematical knowledge might not be the eternal and completely objective knowledge it has traditionally been taken to be.

As I see it, a naturalistic investigation of mathematical cognition should examine the extend to which mathematical cognition deviates from the traditional picture of context free, *a prori* reasoning. In other words, we should investigate whether mathematical cognition is influenced by anything specifically human, such as our hardware or particular cognitive style. Several types of human influence could be investigated, but I will here limit myself to investigate whether mathematical cognition is embodied, and if so, whether this embodied character of the cognition has affected the contend of our mathematical knowledge. Or in other words: Does the embodied nature of human cognition have an impact on our mathematical beliefs? An answer to this question will clearly add to the answer of the general research question motivating this dissertation: What is the origin of mathematical knowledge and how is it produced?

---

As we shall see in the following, it is not difficult to show the use of embodied cognitive strategies in mathematics. The hard part is to find out whether the use of such strategies has had an impact on our mathematical beliefs. In order to discuss this matter, I will introduce four levels of impact, captured in the following four hypotheses:

1. The neutral tool hypothesis: Our use of embodied cognitive strategies only make it easier for us to do the things, we would have done without the aid of such strategies. The embodied cognitive tools are merely heuristic tools.

2. The telescope hypothesis: Embodied strategies make it possible for us to explore parts of the mathematical universe that would not be accessible to the naked brain. Embodied cognition can in other words be seen in analogy with the telescope, which makes it possible for the astronomer to see more clearly and penetrate deeper into the physical Universe.

3. The constructivist hypothesis: Embodied cognitive tools influence the content of mathematics in a non-trivial way, i.e., they influence what we hold mathematics to be and which mathematical theorems we hold to be true. To stay in the analogy to empirical science, the thesis implies that at least some mathematical objects are in fact laboratory artifacts, i.e. effects produced by the instruments used in the investigation.

4. The identification hypothesis. This hypothesis claims, that mathematics can be identified with the use and exploration of one or more cognitive artifacts.

An important part of the discussion of embodiment of mathematical cognition will be centered on an assessment of these four hypothesis; we must know to what extend the embodiment of mathematics has had an impact on our mathematical beliefs. That is the central question.

It should furthermore be noted that this these four hypothesis are analytic tools. The individual hypotheses do not necessarily reflect the position of working mathematicians or philosophers of mathematics. This being said, identification hypothesis is quite clearly the thesis hold by the more extreme formalist schools, such as Haskell Curry's formalist definition of mathematics as "the science of formal systems" (Curry, 1954, p. 204). It could be an interesting historical study to categorize the conception of mathematical cognition hold by key figures in the philosophy of mathematics, within these four different hypotheses. I will however refrain from such a study and instead concentrate on the discussion about which of the four hypotheses – if any – that characterize the actual mathematical practice the best.

On more remark might be in place before moving on to the actual analysis of mathematical cognition. The discussion about the impact of embodied cognitive strategies seems to be a purely epistemic discussion. However, the outcome of this discussion could also have ontological consequences. An ontological realist concerning mathematical objects can easily accept the first two of the four hypotheses, but the third and fourth should be somewhat worrying to the traditional realist. It is very hard to maintain that mathematical objects are real, in the sense of having mind independent existence, if they are in fact either side effects of a specific cognitive strategy (as the construction hypothesis holds) or if mathematics is nothing but the investigation and use of an artifact (as the formalist holds). The key word her is the word 'artifact'; we have created the cognitive artifact, and consequently, if some or all mathematical objects merely reflect the properties of the artifact, we have created the mathematical objects.

I will return to this matter with a more fulfilling treatment at a later point. The reader should keep in mind, that the discussion of the four hypotheses is not a trivial matter or a matter only of interest to cognitive science. The discussion of the cognitive strategies used in mathematics has deep implications for our philosophical understanding of mathematics. This is why I have chosen to investigate this point rather thoroughly.

## 6.2 Cognitive artifacts and the development of arithmetic

### 6.2.1 Counting tools

Cognitive artifacts play a vital and virtually overlooked part in almost all aspects of the mathematical practice. Even an activity as simple and fundamental as counting seems to be crucially dependent on external artifacts of various sorts.

As we saw above, humans – and a range of non-human animals – seem to have an innate ability to handle the numerical aspects of small collections with digital precision, and larger collections with approximation. If large collections are to be handled with the same digital precision as small collections, we must use some sort of external artifacts.

At least two different strategies can be used to handle the numerical aspect of larger collections (and both have been in actual use). The first is the use of reference collections: If you wish to grasp the number of a collection of objects, say the number of sheep in a herd, you form a reference collection by admitting a token, say a small pebble, to your reference collection for each element in the set you wish to count. In this way, you will end up accumulating a collection containing exactly the same number of elements as the collection you need to handle; the heap of pebbles will contain exactly the same number of elements as the heard of sheep, and you can at any time check whether a sheep is missing simply by making a one-to-one mapping between the two collections. The pebbles in other words make it possible for you to grasp – literally – the number of sheep, even if you are not able to count in the conventional sense.

We have some evidence that this strategy has been in actual use. Karl Menninger for instance reports, how the Wedda tribe, who had no number words, used sticks and similar tokens to keep track of things: "If a Wedda wishes to count nuts, for example, he collects a heap of sticks. To each coconut he assigns not a number word but a stick: one nut – one stick; and each time he does so, he says, 'that is one'. So many coconuts, so many sticks; for he has no number-words" (Menninger, 1992, p. 33).

Reference collections can support surprisingly advanced numerical practices. Archaeologists have for instance discovered clay tokens contained in sealed, hollow clay balls in the ruins of several Sumerian cities. These artifacts are still somewhat mysterious, but it is generally believed that the Sumerians in the 4th millennium BCE used the clay tokens as reference collections, and stored them in clay balls in order to keep them safe and unspoiled (Nissen *et al.* 1993; Schmandt-Besserat 1979). Such a practice would allow the users to keep track of dents and property, even if they did not know how to count in the conventional sense or did not have a shared and stable system of numeration.

The use of reference collections is a clear example of embodied cognition in the form of externalization. The cognitive task of handling quantitative aspects of nature is accomplished by bodily action and the use of physical cognitive artifacts. Almost all steps in the process are externalized; you only need to be able to make the bijection between the two collections, and then the external artifacts will take care of the rest. Note however, that the externalization in this case is not – or at least not only – a way to reduce the demands on internal cognitive workload. As the naked brain can only handle the numerosity of very small collections with digital precision, the use of this cognitive artifact greatly expands our inborn cognitive abilities.

This way of handling numerosity, on the other hand, also has obvious limitations. To mention only a few, your ability to handle collections depends on having access to a stock of reference tokens, but what if no pebbles are around – or if you run out of pebbles when faced with a large collection? Also, the reference collection must be kept safe and unspoiled. The practical problems this might pose is perhaps nicely illustrated by the hollow clay balls used by the Sumerian.

The second strategy used to handle numerosity is counting. From a cognitive point of view, counting is much more demanding than the use of reference collections. First of all, you must remember a sequence of signs. Secondly, you must be able to bring the elements of the sequence in a one-to-one connection with the collection you want to count, and thirdly, you must understand a principle of cardinality: the member of the sequence applied to the last member of the collection signifies the size of the collection. All of this makes counting much more cognitively demanding than the use of reference collections, but apparently the flexibility of using a counting sequence outweighs the increased demands on internal mental workloads. This simple observation teaches us a small, but important lesson about mathematics: It is not only determined by the need of cognition, other ends must also be met.

The central artifact used in counting is of course the sequence of numbers. From anthropology, we know that the apparently indefinite sequence of numbers, such as one used by us today, does not simply arise by itself. Other cultures have much lesser developed number sequences. Furthermore, the etymology of some of our number words is related to body parts: The words "five" and "fingers" share a common root in Indo-European, and the word "digit" origins in the Latin word for finger or toe. Studies of the language of the various tribes in Papua New Guinea and on the Torres Strait Islands give us some idea of how number sequences might have been developed (Lancy 1983, pp. 102–4; Butterworth 1999, pp. 55; Ray 1971). When they count, some of the tribes points to a sequence of concrete locations on their body and use the corresponding names for those body parts as counting words. In the western islands of the Torres Strait for instance, counting typically starts with the little finger of the left hand (and the word used for 'one' is identical to the word for 'left little finger'), and goes on following an elaborate pattern of places on the body including: left hand wrist (6), left arm elbow

(7), left nipple (8), left breast (9), sternum (10), right breast (11) *etc.* until it ends at the right little finger (19). Furthermore, in some cases the size of a collection of items is recovered by remembering the body part corresponding to its number (Ray, 1971, p. 47). In other words, in such system the body itself is used as a physical cognitive artifact that supports counting.

According to (Butterworth, 1999, pp. 53), body-counting systems of this type is the first stage in the development of number sequences. From such systems, number-words are gradually replaced by words that are un- or only slightly related to body parts, such as the sequence of words used in modern English. And still, even users of modern English frequently touch or bend their fingers when they count, in order to keep track of the abstract number sequence. So although the sequence of English number words is a conceptual artifact, counting is not unrelated to our embodied nature, and in particular to our use of our own body as a physical cognitive artifact.

---

Developmental psychology also tells us something about just how hard it is to learn verbal counting. In a study by Karen Wynn (Wynn, 1990) 2,5 year old children were able to apply the counting sequence to diverse phenomena, such as objects, sounds and actions. This practice, I might add, included mastery of the bijection principle needed in order to use reference collections, as described above. However, only a few of the children were able to apply the cardinality principle, i.e. respond with the last word in the counting sequence when asked how many objects, sounds or actions they had observed. Furthermore, the children performed very poorly when asked to give a specific number (between 1 and 6) of toy animals to a large puppet. The children were able to succeed when asked to give one toy, but when asked to give more than one, they would simply grasp a random number of toys and give them to the puppy without counting them. Three year olds adopted the same grapping-strategy; only they tended to succeed to give two as well as one toy, but still without counting. Only 3,5 year old children were consistently able to solve the problem by counting and applying the cardinality principle. These study suggest that children start by having performative, but not conceptual knowledge about numbers. At first, children simply learn the counting routine. They learn number words as a sequence of more or the less meaningless signs, and only later do they start to understand that counting determines numerosity. This order of learning is quite interesting from a philosophical point of view, and we will return to it in later discussions.

Based on a longitudinal study, where children between the age of 2 and 3 were followed for seven months, Wynn suggests that the proper understanding of counting is acquired in the following steps: At first, the children know which numerosity the word "one" picks out, then they learn the meaning of "two", then "three". After this slow and stepwise progression, the children seem to realize how the counting sequence represents numbers, and they more or the less simultaneously learn the meaning of all the higher numbers within their counting range (Wynn, 1992b, 1997).

The reason why it is so hard to learn how the counting system works, might be, as hypothesized by Wynn (1997), that the system represents number in a radically different way than we naturally do. As we saw in chapter 4, humans and a number of other species spontaneously represents numerical aspect of reality as magnitude (either continuous for large numbers or as discrete 'object files' for small numbers). The number system, however, represents numerosity by ordinality, i.e. by the position a sign in a sequence. Acquiring conceptual understanding of the number system includes understanding how to map these two different representational systems onto each other.

Counting by reference system is on the other hand quite easy. It only takes the bijection principle that even two year olds master, and numerosity is represented by magnitude in parallel to our innate representations.

All of this show us that the natural numbers are perhaps not so natural after all. The sequence of natural numbers is a culturally created external cognitive artifact. We are not born with innate knowledge about the natural numbers, but we might be born into a culture that possesses them. In learning the numbers, we start by acquiring the artifact, that is the sequence of signs, and only then do we gradually achieve conceptual understanding of the number system. In this case, it is clear that one of our embodied cognitive strategies – i.e. our use of external cognitive artifacts (conceptual or physical) – is constitutive for the observed practice; we simply would not be able to handle large collections with digital precision, let alone construct the natural number system, without some kinds of external artifacts. The artifacts are not merely neutral tools. It seems that at least the telescope hypothesis or possibly even the constructivist hypothesis is more adequate in this case.

Exposure to the artifact of the number sequence is, I might note, not in itself enough to acquire full understanding of the natural numbers. As we saw in chapter 4, chimpanzees who are taught to count are not able to perform the inductive step, human children perform after having understood the meaning of the first few number words; it is equally hard for a chimpanzee to learn the meaning of "four" or "five" as it is for it to learn the meaning for "two" or "three". Consequently, none of the chimpanzees trained in counting have learned more than the first nine or ten number words – despite intensive training.

As a final note on animals and the artifact of counting, it is, I believe, interesting to observe that the only animals known to handle collections of more than four elements with digital precision, is exactly those who were taught to use an external sequence of number signs. This is to a clear demonstration of the power of external cognitive artifacts.

### 6.2.2 Calculating tools

So, counting, or more generally, the handling of numerical aspects of experience with digital precision, seems to be intimately connected with cognitive artifacts, either physical as the pebbles in a reference collection, or mental as a sequence of counting words or signs. Something very similar seems to be the case regarding calculations.

We are actually capable of doing some calculations using nothing but the naked brain, so to speak. The test subjects in the study by Cantlon & Brannon (2007), cited at the beginning of chapter 4, were for instance capable of computing the number of dots without counting. However, their results were less than perfect, and certainly below what one would expect had the subjects been allowed to count or to use other cognitive artifacts.

Most people can (or can learn how to) perform at least basic calculations using only mental and conceptual artifacts, such as the number system, algorithms and rules of thumb. However, purely internal, mental calculation takes practice, skill and effort, and therefore it is no surprise that a large variety of physical cognitive artifacts have been devised throughout history in order to substitute mental calculations with physical and epistemic actions.

The most commonly used calculation-artifacts are probably written tables and different forms of abacai (or counting boards). Written tables expressing the result of arithmetic operations (such as multiplication) or important functions (such as sine and logarithms), are an easy and low-tech cognitive artifact. Such tables have been in use at least since the beginning of the second millennium BCE, where the Babylonians used tables of multiplication, reciprocals, square- and cube roots (Kline, 1990, pp. 5).

Counting boards and abacai are similar low-tech artifacts. In their most basic form, they consist of nothing but some kind of tokens, say a handful of pebbles, and lines drawn on a board or in the sand. The abacus allows mental calculation to be substituted with manipulation of the physical tokens. Basic calculations, such as addition and multiplication, are quite easily performed, but the abacus also supports surprisingly complicated calculations, such as extraction of square and cube roots and the solution of systems of linear equations. Textual evidence suggests that the Sumerians used primitive tallying boards already in the forth millennium BCE (Nissen *et al.* , 1993, p. 134), and abacai and counting boards of different shapes and designs are known to have been in use in large parts of Europe and Asia for several millennia.

Although written tables and abacai are probably the best-known types of cognitive artifacts used for calculations, they are not the only ones. All sorts of calculation instruments have been in use throughout history: Specialized instruments such as astrolabes and sliding rules, modern digital calculating machines and computers. Even the fingers of the human hand have been used as a simple device allowing multiplication to be performed following an easy algorithm (Dantzig, 2005, p. 11).

These devices are very diverse in design, but they all work in a similar fashion: They allow internal, mental calculations to be externalized and substituted by epistemic actions. Using the abacus for instance, calculations are performed by manipulating patterns of physical tokens. These manipulations are purely formal, and can be performed without any knowledge or understanding of what the patterns represent. Once the numbers are encoded, you only need to know the right algorithm for manipulating the tokens – i.e. you only need to know which physical actions to take to turn the given pattern into the wanted pattern –, and then you can read off the correct result from the device when the manipulations are completed.

Such devices are the result of a cognitive strategy, where physical action and environmental recourses are involved in the cognitive process of doing mathematical calculations. They are in other words clear examples of embodied cognition in the form of externalization, and make use of both physical cognitive artifacts (the actual physical devices) and mental cognitive artifacts (the algorithms).

It is hard to determine the precise impact of this strategy. As noted above, we can in fact learn how to do computations mentally, so at least in principle the artifacts do not allow us to do anything, we could not have done without them. They might in other words merely be neutral tools or at most tools that expands the abilities of the naked brain in a neutral way (i.e. the neutral tool or telescope hypothesis might apply to our use of such calculating devices).

This, on the other hand, does not exclude that our *de facto* use of embodied calculation strategies might have influenced the content matter of mathematics. Some historians for instance hypothesize that the development of the Hindu-Arabic numeral system was highly dependent upon the use of particular types of counting boards (see Katz 1998, pp. 230; Barrow 2000, p. 36; Lam 1988). The hypothesis remains highly controversial, but if it is true, it seems to be an example where the use of a cognitive artifact has influenced mathematics in a more profound way.

As a more recent example, the use of digital computers in experimental mathematics and in computer assisted proofs seems to mark a profound change in both epistemic standards and mathematical practices, although we have only begun to see this change recently (see e.g. Tymoczko, 1979; Sørensen, 2010).

## 6.3 Thinking with symbols

Another common way to do calculations is to use written symbols. The reader can for instance consider multiplying five-hundred-twenty-two with four-hundred-seventy-six. Doing it by purely mental means would, I believe, pose a considerable challenge to most people. If on the other hand, pen and paper (or other means of writing) is allowed, the calculation can quite easily be performed by representing the numbers in the familiar Hindu-Arabic numerals and following a simple algorithm, as exemplified in figure 6.1.

**I)**

$$\begin{array}{r} 524 \cdot 476 \\ \hline 00 \\ 0 \end{array}$$

**II)**

$$\begin{array}{r} 524 \cdot 476 \\ \hline 238000 \\ 9520 \\ 1904 \\ \hline 249424 \\ \hline\hline \end{array}$$

**Figure 6.1:** Multiplication in columns using Hindu-Arabic numerals. I) is the initial representation of the problem. II) is the final state.

This is of course a very simple and familiar example of how symbols are used in everyday calculations. The mathematics involved is not very advanced, but the general method of using symbols for calculations is at the heart of modern mathematics. For this reason, I will give in-depth analysis of both the roles played by symbols and of the impact symbol use might have on modern mathematics. I will start by analyzing why Hindu-Arabic numerals support calculations so well, as we saw in the example above. Then I will go on and analyze more advanced uses of symbols.

### 6.3.1 The Hindu-Arabic numerals

In an interesting paper, Jiajie Zhang and Donald Norman (1995) compare the performance of Hindu-Arabic, Greek alphabetic, and Egyptian hieroglyphic numerals on multiplication tasks. Zhang and Norman conclude that the superiority of the Hindu-Arabic numerals can (at least in part) be contributed to the fact that they, compared to the other systems, allow more of the steps of the multiplication algorithm to be externalized; i.e. performed as epistemic actions using pen, paper and the numerals of the system in question.

Unfortunately, this analysis suffers from several weaknesses. Most importantly, all three systems of numerals are compared on the same polynomial algorithm for multiplication[1]. This is highly problematic. Although the polynomial algorithm is the algorithm commonly used today with the Hindu-Arabic numerals, it is very unlikely that the either the Greek or the Egyptians used the algorithm on their respective numeral systems.

We do not know much about how the Greeks did their calculations, but we do know that they at least occasionally used the abacus and counting boards. If this is so, the proper unit of analysis concerning the Greek system is not the alphabetic numbers used in combination with the polynomial algorithm, but the alphabetic numbers taken in combination the with the abacus; that is: the calculations are done using the abacus and the alphabetic numbers are only used to record the result. In this way, multiplication is in fact to a very large degree externalized and performed as epistemic actions on the abacus.

Something similar can be said concerning the Egyptian system. The Egyptians are known to have used a binary, and not a polynomial algorithm

[1] In a numeral system with base $x$, a number $a$ can be represented in polynomial form as $\sum a_i x^i$. In this representation, the algebraic structure of polynomial multiplication of two numbers $a$ and $b$ is: $a \cdot b = \sum a_i x^i \cdot \sum b_j x^j = \sum \sum a_i x^i b_j x^j = \sum \sum a_i b_j x^{i+j}$.

for multiplication[2]. Using the binary algorithm with Egyptian hieroglyphic numerals, multiplication can quite easily and to a large extend externally be performed as a series of doublings and reductions of the written signs.

So in conclusion, when the proper historical context is taken into account, it is questionable whether the Hindu-Arabic numerals in fact allow for a greater amount of externalizations than the two other systems. Thus, we cannot tell why or if it is easier to use the Hindu-Arabic numerals than the Egyptian hieroglyphic only by looking at the ratio between internal and external workload. Other factors, such as the total number of operations performed, must be considered as well.

In order to get a better understanding of the unique qualities of the Hindu-Arabic numerals, we might instead use the typology of numeral systems developed by Stephen Chrisomalis (2004) (see table 6.1).

In this typology, the Hindu-Arabic numerals are characterized as *positional* and *ciphered*. We will analyze these two characteristics one by one starting with ciphering.

In comparison with cumulative systems, the advantage of ciphering is the possibility of a much more compact way of writing numbers. The number eight for instance can be written with a single symbol in the Hindu-Arabic system: '8' (and in other base 10 ciphered systems, such as Greek alphabetic or Egyptian hieratical), whereas it takes eight symbols to represent the same number in the Egyptian hieroglyphic system: 'IIIIIIII', and four symbols using Roman numerals: 'VIII'.

Due to the compactness of the script, one would expect calculations in general to take fewer operations in a ciphered than in a cumulative system. Unfortunately very little empirical work has been done in this area, but the hypotheses is backed up by at least one study (Schlimm & Neth, 2008), where the ciphered Hindu-Arabic system is compared with the cumulative Roman system. Using virtual agents to perform a large number of addition and multiplication tasks in ways similar to human agents, Schlimm and Neth found that the number of basic operations, such as perceptual steps, attention shifts and motor actions, was considerably more numerous when using Roman numerals than when using Hindu-Arabic numerals.

The compactness of ciphered numerals however, comes at a cost. Schlimm

[2]In the binary algorithm, the multiplier $a$ is decomposed into its binary representation $\sum a_i 2^i$ and the multiplicand $b$ is multiplied with each term, thus:

$$a \cdot b = \left(\sum a_i 2^i\right) \cdot b = \sum a_i 2^i b$$

.

| | **Additive**<br>The sum of the value of all the numerals gives the total value of the whole number | **Positional**<br>The position of each numeral decides which power of the base, the numeral is to be multiplied with. |
|---|---|---|
| **Cumulative**<br>Many signs per power of the base. These are added to obtain the total value of that power. | Egyptian hieroglyphic | Babylonian sexagesimal cuneiform |
| **Ciphered**<br>Only one sign per power of the base. This sign alone represents the total value of that power. | Greek alphabetic | Hindu-Arabic |
| **Multiplicative**<br>Two components per power, unit-sign(s) and power-signs, multiplied together, give that power's total value. | Chinese traditional | LOGICALLY EXCLUDED |

**Table 6.1:** Typology of numerical notation systems.
(Redrawn with small adjustments from Chrisomalis, 2004, p. 42)

and Neth note, that using the Hindu-Arabic numerals has higher demands on memory than using the Roman numerals. This is partly due to the fact that in a cumulative system, addition can be performed largely externally by counting and simplifying the written numerals, whereas in a ciphered system, you will have to remember an addition table.

Adding to this, cipherization also has another cost of more philosophical significance. In a cumulative system, the value of each power of the base is represented by a repetition of a specific symbol. So for instance, in Egyptian hieroglyphic system, eight tokens of the symbol 'I' means eight, and eight tokens of the symbol '$\bigcap$' means eight tens (i.e. eighty) and so forth. In other words, in a cumulative system there is an iconic likeness between the value of a power and the number of signs used to represent this value. This is not so in ciphered systems. The sign '8' gives no clue to the fact that its value is eight. In a ciphered system the numerals are conventional symbols. They are meaningless until they are interpreted. This introduces a divide between the symbols as semantic objects i.e. carriers of meaning, and the symbols as syntactic objects, i.e. objects for purely syntactic operations.

In sum, from a cognitive point of view, the choice of a ciphered over a cumulative system is in fact a trade-off, where a reduction in the number of operations is obtained by increasing the demands on internal cognitive work.

This leads us to the positional character of the Hindu-Arabic system. I will here restrict myself to a discussion of ciphered additive systems versus ciphered positional systems.

In ciphered, additive systems such as the Greek alphabetic or the Egyptian hieratical, both base and power values are represented by the shape of the individual numeral. In the Greek system for instance, eighty is represented with the single symbol $\pi$, and the reader will have to infer both the base value – eight – and the power value – tens – from the shape of the sign. Calculating using such a sign, you will either mentally have to separate the base- and the power dimensions in order to reduce the calculation to simpler facts, or you will simply have to memorize the necessary tables for all the numerals used in the system. As the separation of the two dimensions must be done internally, the former option greatly increases the demands on internal, mental resources (see Zhang & Norman (1995) for a detailed analysis). However, as there are 27 different numerals in the Greek alphabetic system, the last option pose a considerable challenge to long-term memory. Choosing either option, the written numerals of the Greek system do not seem to offer much support for calculations. More empirical work need to be done in this area, but it seems as if any ciphered, additive system will pose you with a similar choice of either memorizing very large tables or separating base and power dimensions mentally – or using other artifacts such as the abacus, as the Greeks probably did.

Positional systems on the other hand, allows for an easy separation of the power and base dimensions, as Zhang & Norman (1995) rightly points out. The power of each numeral of a number is represented by its position and the base value by its shape. Due to this fact, calculations can easily be broken down to two simpler tasks; calculations involving only the numerals 0 through 9 and writing the result of such simple calculations in the right positions on the paper. Unfortunately, Zhang and Norman do not give many details of just how the right position on the paper is located, so let us take a closer look at what exactly was going on during the calculation presented above (see figure 6.1).

During the calculations, the working area of the paper is implicitly divided into columns and rows. The columns represent different powers of the base, starting with the power 0 in the rightmost column, and increasing by one each time we move a column to the left. The power dimension of each digit in both the partial sums and the final result are determined by its location in this system of columns. For this reason, it is crucial to place the partial results in the right positions on the paper. This is taken care of by following a simple procedure.

Firstly, the working area is shaped by filling in a number of zeroes. In the first row, the $n$ first columns are blocked with zeroes, in the second row, the $n-1$ first columns are blocked etc., where $n$ is the number of digits of the multiplier.

After this shaping of the working area, the partial sums are produced one digit at a time. In short, the product of the leftmost digit in the multiplier and the rightmost digit in the multiplicand is written in the first vacant position of the first row, and the rest of the digits of the sums are produced by moving one position to the left as each digit in the multiplicand is treated (from right to left), and by moving one row down and begin in the first vacant position to the right as each digit in the multiplier is treated (from left to right). If the result of a partial product is two-digit, only the digit indicating the 1's is written in the designated position, whereas the digit indication the 10's is stored and added to the digit written in the next vacant position. The storing of such carry-overs is typically taken care of by writing and scratching out numbers in the empty area above the multiplier and multiplicand. This area of the paper serves as a temporal working memory.

When all of the partial sums are produced, the final result is obtained, by adding the digits in each column. This might also produce carry-overs.

All of this is a complicated procedure, and it takes a lot of time and practice to learn how to perform it properly. During the procedure, the concrete physical presence and distribution of the numerals written on the paper is heavily exploited. We simply fill in the results of calculations digit by digit following a simple pattern. We do not need to think about the power dimension of any of the partial products. For instance, we do not need to know, that the power of the first partial product of $5 \cdot 6$ is hundreds. As long as we follow the procedure and write the result in the first vacant position of the first line, we automatically get the power dimension right. Also, the procedure neatly lines up the partial sums for the final addition.

As it can be seen from this example, the numerals do not only act as semantic- and syntactic objects during the calculation process. Their presence and physical location on the paper is used as external scaffolding that guides our performance. The symbols are in other words used as purely physical objects with a physical presence and location.

Interestingly, the formal algorithm for polynomial multiplication does not address the distribution of the numerals as physical tokens. The algorithm simply tells us to calculate the partial products and add them. So if anything, it seems to suggest a procedure of this type:

$$\begin{aligned} 524 \cdot 476 &= 24 + 280 + 1600 + 120 + 1400 + 8000 + 3000 + 35000 + 200000 \\ &= 249424 \end{aligned}$$

This procedure serves as an instructive contrast to the multiplication by columns-procedure described above. In this procedure, the physical distribution of the digits is not exploited at all. We have for instance to get the power dimension of the partial products right our selves (and that takes a lot of effort!). Furthermore, the partial products are distributed in a way that does not aid the final addition at all. Calculating in the way suggested by the algorithm simply takes a lot more effort compared to multiplication using the columns-procedure. So the spatial arrangement of the symbols on the paper is indeed very important part of the calculating procedure.

Multiplication can of course be performed using many other physical distributions of the symbols. The numbers to be multiplied can be written in other positions on the paper, and the working area can be shaped in other ways (see figure 6.2)

| | | | | | |
|---|---|---|---|---|---|
| | | | **5** | **2** | **4** |
| | | | **4** | **7** | **6** |
| 2 | 3 | 8 | 0 | 0 | 0 |
| | | 9 | 5 | 2 | 0 |
| | | 1 | 9 | 0 | 4 |
| 2 | 4 | 9 | 4 | 2 | 4 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | **5** | **2** | **4** | | | |
| | | 1 | 9 | 0 | 4 | | | **6** |
| | | 9 | 5 | 2 | | | **7** | |
| 2 | 3 | 8 | 0 | | | **4** | | |
| 2 | 4 | 9 | 4 | 2 | 4 | | | |

**Figure 6.2:** Two alternative layouts for multiplication using columns.

A particular interesting alternative procedure is the so-called *multiplication by jalousie* (or by *diagonal lattice*). Here, the multiplier and multiplicand is written on the rim of a lattice drawn so the number of columns corresponds to the number of digits in the multiplier, and the number of rows to the number of digits in the multiplicand. The diagonals of each square in the lattice is drawn, and the partial products of the multiplication are written in the squares, with the 1's on the right side of the diagonal, and the 10's on the left side. So for instance, the product of 5 and 6 is written as 3/0 in the square made up by the column below 5 and the row to the left of 6 (see figure 6.3). In this physical line up, the power of the partial results are represented by their position in the diagonal columns, and the final result i produced simply by adding the digits in each diagonal column (starting in the bottom right corner and carrying the 10's to the following column).

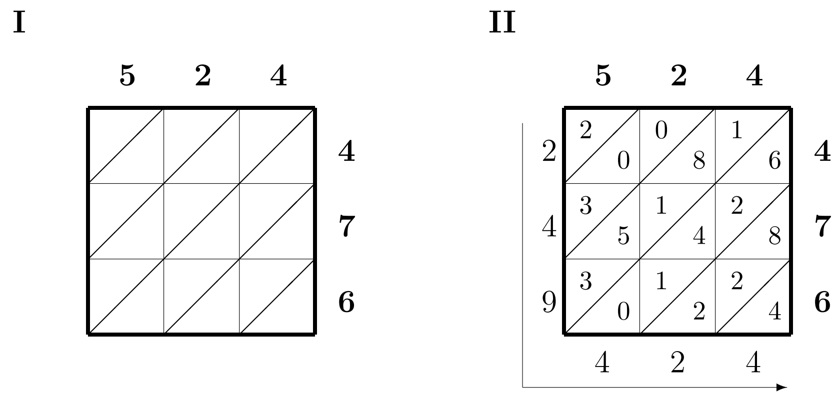



**Figure 6.3:** Multiplication by jalousie. I) is the initially structured working space. II) is the filled-in working space. The result can be read along the edge of the square beginning at the top left corner.

According to Karl Menninger, this method has been in actual use in Arabic and early Italian textbooks on calculations (Menninger, 1992, p. 442). The method is particularly interesting, because it actually allows for a greater degree of externalization than the column-method favored today. The columns and rows are explicitly drawn, and most importantly, we do not need to worry about carry-overs when the partial products are produced. As Menninger writes, it "requires *wenig kopffs*" (Menninger, 1992, p. 442). Today however, the column-method is preferred. The reason for this choice is not known, but I hypothesize that the extra work it takes to explicitly draw the elaborate lattice structure makes the jalousie-method too slow in actual use. This serves once more as a reminder of the fact that we do not only go for the largest degree of externalization possible. Other factors, such as speed and the amount of physical work involved in the procedure, also influence our choice.

The example also draws attention to another aspect of the multiplication by columns-procedure. We do not actually (or only seldom) draw the columns and rows. Instead, we use sophisticated spatio-visual capacities for arranging physical objects in rows and columns. This underlines the fact, that the performance of the calculation using Hindu-Arabic numerals and the column-procedure is indeed an excellent example of embodied cognition. The numerals are treated as physical tokens, distributed in an elaborate pattern on the paper. Such a way of calculation is only available to embodied creatures with sophisticated abilities to arrange physical objects in space.

In conclusion, the Hindu-Arabic numerals are a very special kind of symbols. Unlike ciphered, multiplicative systems, they allow calculations to be performed largely externally as series of epistemic actions, and unlike the numerals of cumulative systems, they are conventional, i.e. abstract symbols that have no iconic likeness with that, which they represent. The symbols can both be treated as semantic, syntactic and physical objects. For this reason, they can easily be manipulated and used to perform epistemic actions in ways similar to the beads of an abacus. To borrow a phrase from Zhang & Norman (1995) (who use it slightly differently), the Hindu-Arabic numerals can be characterized as *object symbols*.

Before leaving the Hindu-Arabic system altogether, it should be noted that the cognitive properties of the numerals cannot in itself explain why the system was adopted, first in the Arabic world and later on in Europe as well. A full explanation should consider other factors as well. The availability and cost of paper does for instance play an important part in the story. As embodied creatures, humans adapt to changes in the environmental resources available. The change from the use of the abacus to pen, paper and Hindu-Arabic numerals is an excellent example of such an adaptation in cognitive style. Other factors of course also played a part, such as the interest in making permanent records of partial calculations and perhaps even the cultural fact that the numerals originally came to Europe from the Muslim Arabic world.

### 6.3.2 The symbol revolution

The Hindu-Arabic numerals are not the only abstract symbols used in modern mathematics. Abstract symbols, however, are a fairly new invention. Before the 16th century, all mathematical text were written in rhetoric style without any other mathematical symbols than (perhaps) numerals.

The first move toward a symbolic style was the so-called syncopated style, where a few of the most frequently used words of the mathematical texts were abbreviated, typically using the first letters of the words. This style was already visible in the Diophantus' *Arithmetica*, where a semi-symbolic notation for the various terms involved in equations, was introduced. The unknown was designated by the sign $\varsigma$ with an accent ‘. Perhaps this sign was a contraction of the first two letters in the word *arithmos*, i.e. “number”, or perhaps the sign was chosen because it was the only one not used in the Greek alphabetic numeral system (for the first hypothesis see (Katz, 1998, p. 174), for the second see (Cajori, 2007, pp. 71)).

Numbers not involving the unknown were indicated by the symbol $\overset{\circ}{\mathrm{M}}$, derived from *monas* (meaning: “unit”), and subtraction by a specially created symbol, which is probably an abbreviation for the word *lepsis* (meaning: “wanting” or “negation”). The square and the cube of the unknown were designated respectively by $\Delta^Y$ and $K^Y$, where $\Delta$ was derived from the word *dynamis* (“power”) and $K$ from *kubos* (“cube”). So, in Diophantus’ formalism, a polynomial expression such as $x^4 + 5x^3 - 3x + 7$ would be written $\Delta^Y\Delta\bar{\epsilon}K^Y\bar{\zeta}\overset{\circ}{\mathrm{M}}\bar{\gamma}\varsigma$, (where $\bar{\epsilon}, \bar{\zeta}$ and $\bar{\gamma}$ are the Greek alphabetic numerals for five, seven and three respectively).

Quite remarkably, Diophantus used his symbolism to designate powers higher than three, so for instance he used $\Delta^Y\Delta$ to designate the forth power of the unknown, $\Delta K^Y$ the fifth and so on. This was a clear break with traditional Greek practice, where powers higher than three were not considered, because they had no geometrical interpretation (Katz 1998, pp. 173; Kline 1990, pp. 138). I will discuss the significance of this result further below.

From Brahmagupta (598-668) and onwards the Hindu algebraists used several abbreviations including (in our alphabet) *ru* for *rupa*, the absolute number, *ya* for *yávat-távat*, the unknown, *c* for *caraní*, the surd or square root and *v* for *varga*, square (Cajori, 2007, p. 75).

In Europe, abbreviations such as *co* for *cosa* (Italian: “thing” viz. the unknown), *aeq.* for *aequales* (“equality”), *R* for *radix* (“root”), *p* for plus and *m* for minus were in use by several authors in the 15th century. The last two were introduced by Nicholas Chuquet, who also developed the notation for roots to include higher order of roots by writing $R^2$ for square root, $R^3$ for cube root and so on. Interestingly, Chuquet developed a typographically similar way to signify the power of the unknown. What we would write as $12x^4$ he would simply write as $12^4$, leaving the base of the exponent out. This praxis even included the negative exponent -1 in expressions such as $12^{1\bar{m}}$ (viz. $12x^{-1}$ in modern notation) (Cajori, 2007, pp. 100).

With the work of François Viète (1540-1603), the use of letters to denote quantities of arithmetic became popular. Although Euclid and other antique writers had used letters in this way, they had only done so sporadically, and it was Viète who introduced the use of letters in a systematic way; the vowels A, E, I, O, U, Y were used for unknown quantities, and the consonants for known quantities. This move to a greater level of abstraction allowed Viète to state the basic laws of arithmetic, such as '$A - B$ times $A + B$ equals $A^2 - B^2$', and solve algebraic problems in a partly symbolic way. As an example of the last, Viète solved the classical problem of finding two numbers given their difference and their sum in the following way: Let $B$ (a consonant) signify the difference, $D$ signify the sum, and $A$ (a vowel) signify the smallest of the two unknown. Then the larger of the two unknown can be expressed as $A + B$. Consequently, $D$ equals $2A + B$, and by transposition, $2A$ equals $D - B$ leading to the result that $A$ equals $(\frac{1}{2})D - (\frac{1}{2})B$ (Viète, 1983, p. 83).

Viète's contribution was not to solve the problem for the first time – it had been solved in full generality by purely rhetoric means before –, but to solve it in a symbolic way. The new use of symbols however, did add to Viète's theory of equations, as it allowed him to treat them in their general form (or at least a more general form) instead of considering several special cases.

In the 16th and late 15th century not only abbreviations, but also true abstract symbols (other than Hindu-Arabic numerals) began to appear. These new symbols included the well-known signs + and - for plus and minus (introduced late in the 15th century by German authors (Cajori, 2007, p.230), a horizontal dash - to signify equality (introduced by Regiomontanus late in the 15th century) and the sign $\surd$ to indicate square root (first used by Robert Recorde (Cajori, 2007, pp.164)). However, abstract symbols were only introduced and used on a larger scale in the 17th century, where authors such as René Descartes, William Oughtred, and John Wallis introduced a wealth of symbols – Oughtred alone is known to have used at least 150 different symbols and abbreviations, most of which are forgotten today (Cajori, 1916, p.28).

Although the symbolic style was met with some opposition, symbols gradually came to play a larger and larger role in mathematics.

## 6.4 What symbols do

Mathematical symbols in general are used very similar to the way the Hindu-Arabic numerals are used. Mathematical symbols are object symbols, and have a tripartite nature. Mathematical symbols are:

- semantic objects, i.e. carriers of mathematical content or meaning,
- syntactic objects, i.e. objects of syntactic transformation following purely formal rules, and

- physical objects, i.e. tangible objects, which can be moved, manipulated and arranged in ways that support our work with certain problems.

The rhetoric style, where mathematical content is represented by written words, of course also use abstract symbols; letters are concatenated to form words, whose physical appearance has no likeness with the objects, the words represent. The word-picture "point", say, does not look like a point, and the word-picture "ten" does not have any more likeness with ten units than the abstract number-symbol "10".

Both written words and abstract mathematical symbols can carry semantic content, i.e. they can be carriers of meaning. The important difference between written words and abstract symbols is the two last points on the list above, i.e., that mathematical symbols, besides their role as bearers of contend, can also be treated as syntactic and as physical objects. With a few rare exceptions (such as avant-garde poetry), written words are never used as more than semantic objects; they cannot be used for purely syntactic transformations or as purely physical objects. So although both the rhetoric and the symbolic style of representing mathematics can be said to use abstract symbols, there is a qualitative difference between the affordances of written words and written mathematical symbols. In the following, I will only be referring to abstract mathematical symbols, when I use expressions such as 'symbol' or 'abstract symbols', although I acknowledge that the written words used in the rhetoric style are equally abstract symbols – but of a completely different kind.

### 6.4.1 Mathematical symbols as syntactic objects

When symbols are treated as syntactic objects, the semantics, i.e. the meaning of the symbols, can be disregarded or suspended, and mathematical problem can be solved (in some instances) by manipulation of the symbols following purely formal rules. In terms of the embodied theory of cognition, the symbols are used as cognitive artifacts allowing computations to be externalized and performed as epistemic actions (as also noted by De Cruz (2005)), similar to the turning of a midwifes disc or the manipulations of the counters on an abacus.

Let me present two examples as illustration. Firstly, we can try to compare one of Euclid's proofs with the algebraic explanation given by the translator, T.L. Heath (Heath, 2006, p. 441-442)[3]. The proof is the proof for

[3]Such algebraic versions of Euclid's proofs are clearly anachronisms. From a history of

proposition 24 in book V of *The Elements*. In Euclid's words, the theorem states:

> **Proposition 24.**
> *If a first magnitude have to a second the same ratio as a third has to a fourth, and also a fifth have to the second the same ratio as a sixth to the fourth, the first and fifth added together will have to the second the same ratio as the third and sixth have to the fourth.*
> (Heath, 2006, p. 441)

In order to prove this proposition, Euclid needs two previously proven results, proposition 18 and 22. They state:

> **Proposition 18.**
> *If magnitudes be proportional* separando, *they will also be proportional* componendo.
> (Heath, 2006, p. 427)

The concepts *separando* and *componendo* are explained in the specification of the proof as: "Let $AE$, $EB$, $CF$, $FD$ be magnitudes proportional *separando*, so that, as $AE$ is to $EB$, so is $CF$ to $FD$; I say that they will also be proportional *componendo*, that is, as $AB$ is to $BE$, so is $CD$ to $FD$" (Heath, 2006, p.427). We are here to imagine the magnitudes $AE$ and $EB$ as parts of a larger magnitude $AB$, and similarly $CF$ and $FD$ as part of a magnitude $CD$.

> **Proposition 22.**
> *If there be any number of magnitudes whatever, and others equal to them in multitude, which taken two and two together are in the same ratio, they will also be in the same ratio* ex aequali.
> (Heath, 2006, p. 337)

Given these propositions to be true, the proof of proposition 24 goes:

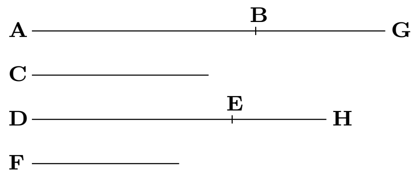
> Let a first magnitude $AB$ have to a second $C$ the same ratio as a third $DE$ has to a fourth $F$; and let also a fifth $BG$ have to the second $C$ the same ratio as a sixth $EH$ has to the fourth $F$;
>
> I say that the first and fifth added together, $AG$, will have to the second $C$ the same ratio as the third and sixth, $DH$, has to the fourth $F$.

mathematics point of view, the value and validity of such anachronisms can be contested. Here, however, I am not primarily interested in uncovering the ideas of Euclid. Rather, my goal is to understand the value of the symbolic method by contrasting it with an entirely other way of reasoning

For since, as $BG$ is to $C$, so is $EH$ to $F$, inversely, as $C$ is to $BG$, so is $F$ is to $EH$. Since, then, as $AB$ is to $C$, so is $DE$ to $F$, and, as $C$ is to $BG$, so is $F$ is to $EH$, therefore, *ex aequali*, as $AB$ is to $BG$, so is $DE$ to $EH$.

And, since the magnitudes are proportional *separando*, they will also proportional *componende*; therefore, as $AG$ is to $GB$, so is $DH$ to $HE$. But also, as $BG$ is to $C$, so is $EH$ to $F$; therefore, *ex aequali*, as $AG$ is to $C$, so is $DH$ is to $F$.

Therefore, [the proposition stated holds true.]




(Text and illustration (redrawn) from Heath, 2006, p. 441-42)

The proof can be followed somewhat easier, by anchoring the conceptual complexity in a drawing representing the magnitudes in question. Heath includes such a figure along with his translation of Euclid's original proof (redrawn and inserted in the proof above).

Heath, however, also gives an algebraic version of the proof. Firstly, the content of the two auxiliary propositions 18 and 22, can be stated algebraically as the rules:

Proposition 18: If $a : b = c : d$, then $(a + b) : b = (c + d) : d$.
Proposition 22: If $a : b = d : e$ and $b : c = e : f$, then $a : c = d : f$.
(Heath 2006, p. 428; Heath 1921, p.390)

Then, proposition 24 can be stated and proven algebraically thus:

Algebraically [proposition 24 states that], if

$$a : c = d : f, \text{ and } b : c = e : f,$$

then

$$(a + b) : c = (d + e) : f.$$

[...] Inverting the second proposition to $c : b = f : e$, it follows, by [proposition 22], that $a : b = d : e$, whence, by [proposition 18], $(a + b) : b = (d + e) : e$, and from this and the second of the two given proportions we obtain, by a fresh application of [proposition 22], $(a + b) : c = (d + e) : f$.

(Heath, 2006, p. 442)

The proof given by Heath is obviously easier to survey, simply because it is much shorter than the original proof give by Euclid. Apart from this somewhat superficial difference, there is a more significant difference in cognitive style worth noticing. The proof given by Euclid is completely contentual; the proof takes place within the domain of magnitudes, and in order to follow the proof, we must understand the content of each expression in terms of its reference to magnitudes, for instance by drawing a figure (such as the illustration accompanying the proof above), illustrating the content of the proposition. In contrast, the algebraic proof given by Heath, is partially formal. Once the theorem is stated, the content is bracketed (in the Husserlian sense). The proof exclusively states and discusses how one symbolic form can be transformed into another by use of previously proved rules of transformation. There is no reference to the meaning or content of the symbols during this process, only references to rules of transformation and the symbolic forms themselves, e.g. in expressions such as: "inverting the second proposition", "from this and the second of the two given proportions we obtain" *etc.* This use of abstract symbols allows Heath to suspend the meaning of the symbols and to treat them solely as syntactic objects. From a cognitive point of view, this is a very economic way of proceeding; we do not need to interpret and follow the meaning of all the steps in the proof. The proof is externalized and reduced to the lawful manipulation of external objects.

---

As an example of the symbol-use in modern mathematics, we can take a closer look at problem and its paradigmatic solution taken from a textbook on calculus (Adams, 1995):

> Show that: $2\sin^{-1} x = \cos^{-1}(1-2x^2)$ holds for $0 \leq x \leq 1$.
> Solution: Let $f(x) = 2\sin^{-1} x - \cos^{-1}(1-2x^2)$. Then
>
> $$\begin{aligned} f'(x) &= \frac{2}{\sqrt{1-x^2}} - \left(-\frac{1}{\sqrt{1-(1-2x^2)^2}}(-4x)\right) \\ &= \frac{2}{\sqrt{1-x^2}} - \frac{4x}{\sqrt{1-(1-4x^2+4x^4)}} \\ &= \frac{2}{\sqrt{1-x^2}} - \frac{4x}{\sqrt{4x^2(1-x^2)}} \\ &= \frac{2}{\sqrt{1-x^2}} - \frac{2}{\sqrt{1-x^2}} = 0, \end{aligned}$$

provided $x \geq 0$, since $\sqrt{4x^2} = 2x$ in this case. Now Theorem 2 shows that $f(x)$ is a constant on $[0, 1]$, and the constant must be 0 since $f(0) = 0$. This provides the given identity.

(Adams, 1995, p. 256)

Firstly, it should be noted, that the problem is at heart a geometric problem – sine and cosine were originally defined as properties of angles – and the identity can quite straightforwardly be given a purely geometrical proof (see figure 6.4[4]).

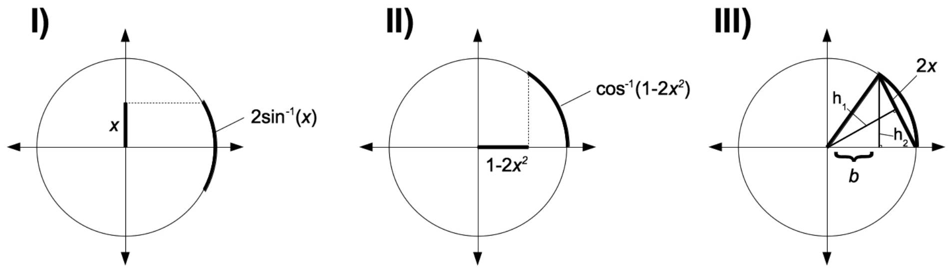



**Figure 6.4:** The arc lengths marked out in I) and II) represent $2 \cdot \sin^{-1}(x)$ and $\cos^{-1}(1 - 2x^2)$ respectively. What is wanted is a proof that lengths of these two arcs are identical. This can be done by considering the isosceles triangle marked out in III). Two legs of the triangle have length 1, and the third has the length $2x$. Consequently, the triangle will span an arc of length $2 \cdot \sin^{-1}(x)$, when it is placed in the unit circle as indicated. The identity can be proven by showing $b$ to be equal to $1 - 2x^2$. For brevity, I will not go through the proof in full detail. In short, by Pythagoras $h_1$ must be equal to $\sqrt{1 - x^2}$. Consequently, the area of the triangle must be $x \cdot \sqrt{1 - x^2}$. From this, we can see that $h_2 = 2x \cdot \sqrt{1 - x^2}$, and by Pythagoras we have: $b = \sqrt{1 - (2x \cdot \sqrt{1 - x^2})^2} = \sqrt{(1 - 2x^2)^2} = 1 - 2x^2$, as wanted. It should be noted that although this proof is closer to the geometrical content, it is not completely without purely syntactic elements.

In the solution given by Adams, the problem is solved by using a typical mixture of conceptual knowledge and syntactic transformations. It has to be known that sine and cosine can be conceived as functions, and it has to be known that a function is constant, if its derivative is zero (this is the content

[4] I am indebted to my advisor Jesper Lützen for pointing this out to me, and for showing me a sketch of the proof.

of Theorem 2 referred to in the solution). Following this idea, the identity can be expressed as a function, and its derivative can be found. The step of determining the derivative is omitted in the solution, but it can be taken simply by consulting a list of elementary derivatives (stated in symbolic form) and (purely formal) differentiation rules. A list of elementary derivatives is conveniently given in the beginning of the textbook in question.

As soon as the derivative is stated in symbolic form, the rest of the problem can be solved as a series of syntactic transformations. I this presentation, two or more transformations are typically performed from one line to the next. From the first to the second line for instant, the following rules are applied:

- $\frac{a}{b} \cdot k = \frac{a \cdot k}{b}$. Applied on the last fraction and $(-4x)$.
- $1 \cdot a = a$. Used to simplify the multiplication $1 \cdot (-4x)$ to $(-4x)$.
- $(-a) \cdot (-b) = a \cdot b$. Used to cancel the minuses in front of the fraction and $-4x$.
- $(a-b)^2 = a^2 - 2ab + a^2$. Applied on $(1-2x^2)^2$ in the denominator giving $1 - 4x^2 + 4x^4$

During these transformations, no reference whatsoever is made to the original geometric content of the identity. Although the steps are not explained in words, in the way Heath explained the moves in his algebraic proof of proposition 24 above, a typical explanation of, say, how to get from the first to the second line could go something like this: "The factor $-4x$ is moved onto the fraction line, and the two minuses cancel each other. In the denominator, the term $(1-2x^2)^2$ below the square root sign is expanded. Finally, the parentheses are eliminated. As they contain only one positive term, this can be done without any further changes."[5]

What we are concerned with here, is the concrete symbolic forms written on the paper, and not whatever the symbols might signify. The original meaning of the symbols is suspended, and the symbols are treated as purely physical and syntactic objects. New strings of symbols are produced only by considering the physical shape and pattern of the already given symbols. This allows mental computations and contentual considerations to be replaced by rule governed physical operation on externally given objects. This is – once more – economic from a cognitive point of view; we do not have to bother

[5] Explanations of this type can be found in more elementary text books, see for instance (Antonius *et al.* , 2009, p 84ff) or other hi-school level textbooks.

with meaning and understand the significance of all the steps in the proof, as long as we follow the rules of syntactic transformation correctly, we will arrive at the right result.

In contrast to the use of Hindu-Arabic numerals in multiplication problems, there is no strict, overall algorithm to follow in this case. We start with one symbolic form ($f'(x) = \frac{2}{\sqrt{1-x^2}} - \left(-\frac{1}{\sqrt{1-(1-2x^2)^2}}(-4x)\right)$) and we know which form we want to obtain ($f'(x) = 0$), but there is no strict algorithm taking us from the given to the wanted state. We must ourselves find out which syntactic transformations to make when in order to get to the goal.

Quite interestingly, a common strategy in solving such problems is simply to try something out on the paper, so the mathematician can see – literally *see* – whether she moves in the right direction or not. The symbols are not merely (or at least not always) used to record a solution already thought out, they are often used actively throughout the solution process. Mathematical symbols are not always used like chess pieces, which are merely moved in order to represent an already thought through plan. They are frequently used more like puzzle pieces that are moved around and manipulated, piled and compared as part of the solution process. This is clearly part of an embodied strategy of externalizing and using the world as its own best model (see section 5.2).

### 6.4.2 Mathematical symbols as physical objects

In my explanation of the calculation given above, the symbols were not only treated as syntactic objects, i.e. as objects of syntactic transformation. They were also treated as palpable physical objects; Heath for instance 'inverted' a proposition, and in my explanation of Adam's calculations, the factor $-4x$ was 'moved'. The idea, that symbols can be moved around like concretely given physical objects, is widespread in modern mathematics. Note for instance the use of the expression *symbol manipulation*. Literally speaking, the symbols are not manipulated (i.e. handled') during solution processes, such as the above. Instead, entirely new strings of symbols are produced in accordance with the given rules of transformation and the shape and pattern of the already given symbols.

In some cases, the mathematician might imagine or visualize some of the reported motions. This idea is for instance exploited in (Giaquinto, 2007, pp. 191), although the empirical evidence is lacking (as admitted by Giaquinto); his main source is a study by John Hayes (Hayes, 1973). Here, it is suggested that we are capable of solving simple algebraic problems (such as "$6 + x = 12$") in imagination, and that at least some subjects do this by visualizing motions of the involved symbols. The study however, is somewhat anecdotal and suffer from the methodological weakness, that the subjects were asked to solve the problems using visual imagination. So the study at most demonstrates that it is possible to solve simple problems using visual imagination, not that visual imagination is involved in actual, everyday problem solving.

A more solid line of evidence for our treatment of mathematical symbols as imaginary objects comes from the study of gestures. Gestures, produced while a person is speaking, are generally thought to be a reliable source to the person's unconscious conception of the topic, she talks about. In a study by Laurie Edwards, the gestures of elementary school teacher students were observed while the students solved problems with and explained the basic properties of fractions (Edwards, 2009). Many of the gestures produced referred to the manipulation of physical objects (such as dividing rods or cutting pies), but a full 10% of the gestures depicted manipulations of written mathematical symbols. The written symbols were imagined as objects located in space, and procedures such as multiplying and adding fractions were discussed by the gestural manipulation of these imagined objects in space.

The use of such visualizations constitutes one way in which we use symbols as physical objects: We imagine that we can move the symbols similar to the ways we move physical objects. This, furthermore, is a clear example of embodied cognition. Our knowledge of how to move physical objects is applied in the process of symbol driven problem solving.

There is however several other ways in which symbols are used as physical objects. In analogy to the example involving multiplication using Hindu-Arabic numerals, the physical properties and distribution of the symbols are exploited in a number of calculation tasks. Matrix multiplication might serve as a typical example[6,7]. At its core, matrix multiplication consists of simple

[6] Giaquinto (2007, p. 242) explains how visual imagination might also be used in matrix multiplication.

[7] For those not aquatinted with matrices, they are in short sets of elements ordered in two dimensions. The elements are typically numbers (real or irrational), but they can also be certain other mathematical objects, such as derivatives of functions (in matrix calculus).

The elements of a matrix are usually presented as an array:

$$A = \begin{bmatrix} a & b & c \\ d & e & f \end{bmatrix}$$

multiplication and addition operations. The hard part of the process is not multiplying and adding, but finding out which elements to perform which operation on when – and where to put the results (i.e. where to locate the result in the two dimensional structure of the product matrix). As anyone who has ever done matrix multiplication will know, the process of picking out the right elements and putting the result in the right place is heavily guided by the physical layout of the matrices; it is simply reduced to a matter of going through the rows of one matrix and the columns of the other in a specific pattern.

The importance of the guidance offered by the physical layout of the matrices is apparent even in textbook definitions of matrix multiplication. In Robert Messer's textbook *Linear Algebra: Gateway to Mathematics* (used to teach undergraduate algebra at University of Copenhagen in the 1990's), matrix multiplication is defined as:

> The matrix product of an $m \times n$ matrix $A = [a_{ij}]$ with an $n \times p$ matrix $B = [b_{jk}]$ is the $m \times p$ matrix, denoted $AB$, whose $ik$-entry is the dot product
>
> $$\sum_{j=1}^{n} a_{ij} b_{jk}$$
>
> of the $i$th row of $A$ with the $k$th column of $B$.
>
> (Messer, 1994, p. 178)

The definition is seemingly formal; the matrices are formally represented by their general elements ($a_{ij}$ and $b_{jk}$ respectively), and in the definition of the central multiplying and summing operation, the elements are identified only using indices. So far, the definition only reveals the algebraic structure of the problem. However, in the final line of the definition, the indices are explained as referring to the 'rows' and 'columns' of the two factor matrices. So the reader is not to think of matrices as sets of elements ordered abstractly in two dimensions by indices (as the representation the algebraic structure could suggest). The reader is clearly to imagine the matrices physically represented as arrays.

---

Here, the ordering of the elements is represented by their physical location in the array, i.e. which row and column, each element is located in. The ordering could be (and sometimes is) represented by other means, such as subscripts or indices. We could for instance represent the matrix above as a list of elements with subscripts representing each element's location in the internal order of the matrix:

$$A = \{a_{11}, b_{12}, c_{13}, d_{21}, e_{22}, f_{23}\}$$

After stating the definition, Messer assures us, that "[t]he pattern of adding the products of corresponding entries across the rows of the first factor and down the columns of the second factor soon becomes automatic" (Messer, 1994, p. 178). Here, Messer explicitly explains how to exploit the physical layout of the problem.

In some definitions of matrix multiplication, the use of the physical layout of the matrices is underlined even stronger. In the Wikipedia-definition, for instance, the following illustration is offered: Here, the parts of the multi-

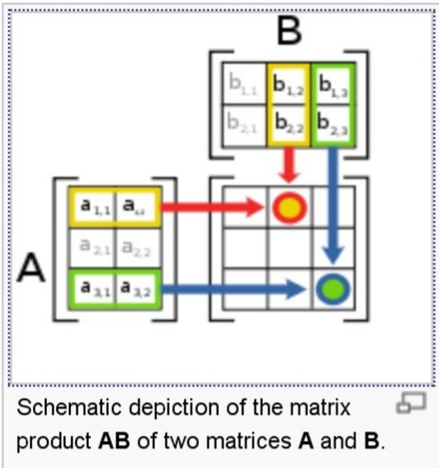



**Figure 6.5:** A description of matrix multiplication.
(From http://en.wikipedia.org/wiki/Matrix_(mathematics))

plication process guided by the physical structure of the matrices are clearly indicated; the problem of finding the right elements and the problem of putting the results in the right places is reduced to a problem of locating and counting rows and columns of physical objects (here mathematical symbols) arranged in arrays. In contrast, imagine multiplying the matrices from figure 6.5 represented as lists of indexed elements, thus:

$$A = \{a_{11}, a_{12}, a_{21}, a_{22}, a_{31}, a_{32}\}$$

$$B = \{b_{11}, b_{12}, b_{13}, b_{21}, b_{22}, b_{23}\}$$

Or even worse, as unsorted lists, for instance thus:

$$A = \{a_{12}, a_{31}, a_{21}, a_{11}, a_{32}, a_{22}\}$$

$$B = \{b_{12}, b_{22}, b_{13}, b_{11}, b_{21}, b_{23}\}$$

If one looks only at the algebraic structure of the problem and the syntactic transformations involved, it does not matter, how the elements of the matrices are presented to the user of the symbols. But if a human agent is actually to multiply matrices, the physical layout and structure of the elements do play a major role – and this role is not captured by the formal definition describing only the algebraic structure of the process. In this, matrix multiplication is very similar to multiplication using Hindu-Arabic numerals.

In the matrix-case, the mathematical symbols are clearly treated as physical objects, which are arranged in ways that guide and enhance our performance of certain tasks (here multiplying two matrices). The use of the symbols as physical objects is very clear in this case, but other, similar examples could be given, for instance some operations on permutations, the use of group diagrams *etc.*

Finally, it should it should be noted that this use of the physical aspect of representations is not necessarily restricted to mathematical *symbols*. Rhetoric representations of mathematical content could be arranged in similar ways. Matrices for instance can be displayed using number words instead of Hindu-Arabic numerals. Abstract symbols however, will in most cases be better suited for this kind of physical arrangement due to their compact physical size (compared to rhetoric representations); a symbol such as '29' is simply easier to handle and arrange *qua* physical object than a word such as 'twenty nine'.

---

All the uses of symbols as physical objects we have seen so far are purely pragmatic. The physical arrangement of matrices does not reflect or clarify or help us understand the algebraic structure of matrix multiplication. It is an auxiliary structure, which serves the purely pragmatic purpose of making it easier to find out, which symbols to operate on when (although of course, an important part of learning matrix multiplication is learning how to master this auxiliary structure in addition to the algebraic structure). Such pragmatic uses of symbols as physical objects must be distinguished from the epistemic use of symbols as physical objects. Here, the physical arrangement of the symbols is used to serve epistemic, not pragmatic goals.

As an example, I will present Cantor's celebrated (and much debated) 'diagonal argument' for the uncountability of the real numbers. The argument was presented by Cantor in a paper from 1891. It should be noted that Cantor in fact did not prove the uncountability of the real numbers in this particular paper (he had done that already using another method in 1873 (Kline, 1990, p. 997)). In the 1891-paper, Cantor merely proved that uncountable sets, i.e. sets that cannot be brought into a one-to-one relationship with the natural numbers, exist. His argument can however easily and obviously be used on the set of real numbers, and it is this application of Cantor's argument I will present as a case, not Cantor's actual 1891-proof.

The proof is usually presented as a proof by contradiction: If we assume, that the reals in [0; 1] can be enumerated, it must be possible to display them as a list, and assign a natural number to each real number in the list:

$$\begin{array}{llllll} 1 & \leftrightarrow 0, & a_{11} & a_{12} & a_{13} & a_{14} \ \dots \\ 2 & \leftrightarrow 0, & a_{21} & a_{22} & a_{23} & a_{24} \ \dots \\ 3 & \leftrightarrow 0, & a_{31} & a_{32} & a_{33} & a_{34} \ \dots \\ 4 & \leftrightarrow 0, & a_{41} & a_{42} & a_{43} & a_{44} \ \dots \\ & \vdots & & \vdots & & \ \ddots \end{array}$$

Here, the natural numbers are represented using the standard Hindu-Arabic numerals, and the real number are symbolically represented as decimal fractions, where $a_{nk}$ is the $k^{th}$ decimal in the $n^{th}$ real number on the list. Given this list, we can define a real number $b$ in the following way: Let $b = b_1 b_2 b_3 b_4 \dots$, where

$$b_k = \begin{cases} 9, \text{if } a_{kk} = 1 \\ 1, \text{if } a_{kk} \neq 1 \end{cases}$$

Let $a$ be the $n^{th}$ number on the list of all the reals. Due to the way $b$ is constructed, $b_n \neq a_{nn}$, and consequently $b \neq a$. As this applies for all $n \in \mathbb{N}$, the real number $b$ cannot be on our list of all the real numbers, in contradiction with our original assumption. Consequently, the real numbers cannot be collected in a countable list (see for instance Kline, 1990, for a similar version of the proof).

The proof is interesting as an example of symbol use for two reasons. Firstly, the proof is not just at random called a *diagonal* proof. The constructed number $b$ will differ from the numbers on the list precisely on the diagonal digits, i.e. the digits $a_{11}$, $a_{22}$, $a_{33}$ etc. In more pedagogical versions of the proof, this crucial idea is conveyed by typographically highlighting the diagonal, for instance like this:

$$\begin{array}{llllll} 1 & \leftrightarrow 0, & \mathbf{a_{11}} & a_{12} & a_{13} & a_{14} \dots \\ 2 & \leftrightarrow 0, & a_{21} & \mathbf{a_{22}} & a_{23} & a_{24} \dots \\ 3 & \leftrightarrow 0, & a_{31} & a_{32} & \mathbf{a_{33}} & a_{34} \dots \\ 4 & \leftrightarrow 0, & a_{41} & a_{42} & a_{43} & \mathbf{a_{44}} \dots \\ & \vdots & & \vdots & & \ddots \end{array}$$

In such cases, the physical arrangements of the symbols are clearly use as knowledge guiding, i.e. for epistemic purposes.

Secondly, the proof is in fact not about the real numbers as such, but about a particular symbolic representation of the real numbers (as also noted for different reasons in Kerberi & Polleti, 2002). The proof only works, if the real numbers are represented as decimal fractions, i.e. as infinite sequences of singular digits, which can be compared and arranged in space. The proof can be presented using an actual list as above or more formally (e.g. in Schumacher, 1996, p. 167-8), but it will always rely on the possibility of representing any real numbers as sequences of numerals. So in this proof, a particular symbolic representation of the real numbers plays a crucial epistemic role; without a place value representation of the reals, there simply would not be a proof.

I will return to the epistemic use of symbols as physical objects in section 6.10.2.

---

The use and structuring of mathematical symbols treated as physical objects, is a instructive example of one aspect of the embodied nature of human cognition. We do not only use the given environment. We change it and structure it in ways that suit our cognitive and physical profile and enhances our cognitive performance. We deposit things at different physical positions: Pens are in one drawer, binders in another, papers on different subjects are sorted and placed in different piles on the desk and so on. We organize, alphabetize, sort and pile. And sometimes we arrange mathematical symbols, in order to support the operations we are about to perform on them. The digits in real numbers are arranged in a specific way, when we multiply real numbers, and the entries of matrices are structured in rows and columns, when we multiply matrices. This reflects something about the beings we are. Our abilities for arranging and locating things in physical space are highly developed, and these capacities are exploited in certain mathematical operations. For another kind of being with different cognitive and bodily profiles, the picture might be different. A digital computer for instance, might not prefer matrices arranged in arrays.

### 6.4.3 Physical patterns

Finally, the physical pattern made by the symbols, i.e., the way, the symbols physically present them selves to the eye, might be used as a way to discover new theorems. Two different mechanisms have been suggested: Firstly, patterns in the layout of physical symbols might suggest new and more general formulas. Secondly, new formulas might be discovered by simply substituting some of the symbols in a well-known formula by others.

As an example of the first, I will present an example from (Adams, 1995)[8]:

> Find the $n$th derivative $y^{(n)}$ of $y = \frac{1}{1+x} = (1+x)^{-1}$.
> Solution: Begin by calculation the first few derivatives:
>
> $$\begin{aligned} y' &= -(1+x)^{-2} \\ y'' &= -(-2)(1+x)^{-3} = 2(1+x)^{-3} \\ y''' &= 2(-3)(1+x)^{-4} = -3!(1+x)^{-4} \\ y^{(4)} &= -3!(-4)(1+x)^{-5} = 4!(1+x)^{-5} \end{aligned}$$
>
> The pattern here is becoming obvious. It seems that
>
> $$y^{(n)} = (-1)^n n!(1+x)^{-n-1}.$$
>
> We have not yet actually proved that the above formula is correct for every $n$, although it is clearly correct for $n = 1, 2, 3$, and 4. To complete the proof we use mathematical induction [and Adams concludes by performing such a proof].
>
> (Adams, 1995, p. 137)

So, Adams see a pattern in the development of the derivatives, and uses this to reach a general expression for the $n$'th derivative of the function.

In this case, the symbolic representation guides the search for a pattern by presenting the algebraically similar parts of each line in typographically

[8] It should be noted, that I do not claim anything about how the formula was first discovered. The example is merely an example of how the reader of the textbook is supposed to 'see' a general formula – and (as the example is a paradigmatic text-book solution to a problem), how the reader is instructed to find solutions to similar problems on their own. Examples where a similar recognition of patterns in the physical symbols might play a part in the actual discovery can be found in the history of mathematics, see for instance (Euler, 2000).

similar ways; the $(1 + x)$-factor clearly stands out as a typographic block, exponents are lifted *etc.* This typographic appearance makes it easier to find and compare similar elements in different lines; $(1 + x)$ for instance immediately stands out as invariant, and it is easy to find the exponents and follow their development through the four lines. The abstract mathematical pattern is simply matched by a typographic pattern – and it is in fact not completely clear which of these patterns, Adams finds 'obvious'.

So once more, the guidance is offered by the physical appearance and layout of the symbols. The symbols are here guiding *qua* being physical objects, and neither as syntactic or semantic objects.

The contribution of this guidance might be clearer, if we compare the symbolic representation of the calculations with a purely it with a purely rhetoric:

> The first derivative of the function equals minus one plus the argument of the function (taken as a whole) and taken to the power of minus two.
> The second derivative equals two multiplied with one plus the argument (taken as a whole) and taken to the power of minus three.
> The third derivative equals minus the faculty of three multiplied with one plus the argument (taken as a whole) and taken to the power of minus four.
> The fourth derivative equals minus the faculty of four multiplied with one plus the argument (taken as a whole) and taken to the power of minus five.

Although the algebraic pattern might still be derived from the rhetoric representation, the representation does not structure and guide the search. The different parts of each line seem typographically alike; it is for instance difficult to identify and compare the exponents in all the lines, and is not immediately clear, that the factor $(1 + x)$ remains unchanged through the development of the different derivatives. The typographic structure offered by the rhetoric representation is simply inferior to that offered by the symbolic ditto.

---

It is more difficult to find examples, where the typographic layout of the symbols has inspired entirely new theorems, but there are a few interesting cases.

Some commentators take Leibniz' discovery of the formula for differentiating the product of two function as an example of discovery by substitution.

In a letter dated October 1695 to Johan Bernoulli, Leibniz points out an apparent analogy between powers of a sum and differentiation of products. Leibniz begins by expressing Newton's binominal formula in the following way (Gerhardt, 1863, p. 221):

$$^{n}\overline{(x+y)} = x^n y^0 + \frac{n}{1} x^{n-1} y^1 + \frac{n(n-1)}{1 \cdot 2} x^{n-2} y^2 \text{ etc.} \tag{6.1}$$

Notice the inclusion of $y^0$ and division by 1. These elements are syntactically redundant (i.e. they can freely be removed), but they are typographically important as they make the pattern in the development more obvious. From the binominal formula, Leibniz substitutes $(x + y)$ with $(xy)$ and substitutes exponentials with differentials ($d^n$ denoting the $n^{th}$ differential). This gives him the following formula for differentiating products (now known as Leibniz' formula):

$$d^n\overline{(xy)} = d^n x d^0 y + \frac{n}{1} d^{n-1} x d^1 y + \frac{n(n-1)}{1 \cdot 2} d^{n-2} x d^2 y \text{ etc.} \tag{6.2}$$

We do not know exactly what lead Leibniz' to make the substitutions necessary to go from (1) to (2) above, or what gave him confidence in the formula. Michel Serfati sees the derivation of (2) as the result of a"jeu combinatoire" - a game of combination (of symbols) (Serfati, 2005, p. 390). Brendon Lavore sees Leibniz' confidence in the formula as partly resulting from a belief in the syntactic analogy between the well-known result (1) and the new result (2) (Lavor, 2010). I will not try to settle the matter her, but will simple leave the Leibniz formula as a possible example of discovery, based on or helped by the physical appearance of the mathematical symbols. Perhaps, Liebniz' formula (2) was simply the lucky result of his playing around with the symbols.

---

The so-called umbral method is another – and better documented – example of discovery by substitution of symbols (see also Lavor, 2010). The methods close connection to the physical appearance of the symbols is already reflected in its name; 'umbral' is Latin for 'shadow', and allegedly the method got its name because it involves the interchange of exponents and their typographic shadows, suffixes (Di Bucchianico & Loeb, 1995).

The method was first presented by John Blissard in 1861 (Blissard, 1861). The basic idea, in short, is to derive identities involving sequences of numbers by changing the index $n$ of the sequence $\{a_n\}$ to an exponent and subsequently treat the sequence as if it was a sequence of powers $a^n$.

The method is for instance frequently used on the sequence $\{B_n\}$ of Bernoulli numbers. The numbers are defined by the generating function:

$$\sum_{n=0}^{\infty} B_n \frac{x^n}{n!} = \frac{x}{e^x - 1}$$

By changing the index $n$ to an exponent, the following transformation can be performed:

$$\sum_{n=0}^{\infty} B_n \frac{x^n}{n!} \simeq \sum_{n=0}^{\infty} B^n \frac{x^n}{n!} = \frac{x}{e^x - 1} = e^{Bx}$$

, where $\simeq$ is used to symbolize umbral equivalence. Standard algebraic manipulations can then be performed on the given expression, and identities concerning the Bernoulli numbers can be reached by changing the exponents back to be an index (see (Di Bucchianico & Loeb, 1995); (Guinand, 1979) for detailed examples).

From a mathematical point of view, umbral transformations are simply non-sense. It does not give any meaning to change an index to be an exponent and back again. Consequently, the method was simply considered to be a collection of magic rules, that somehow gave interesting results (Di Bucchianico & Loeb, 1995, p. 3), although the method have now been given a thorough mathematical justification (see for instance Roman, 1984)).

We do not know for sure how Blissard found his methods. However, as the transformations used in the umbreal method are mathematically nonsensical, it seems unlikely that they were discovered from considering the meaning of the symbols. The method simply seems to be inspired by the typographical appearance of the symbols; a symbol written thus: $B_n$ can easily be changed to a symbol looking thus: $B^n$, although the actual meaning of this transformation remains in the shadows (so to speak).

Blissard does make some comments on his notation, which could support such an interpretation. In chapter III of the paper "Theory of generic equations", Blissard opens by describing his 'representative notation' as consisting of a single convention:

> Let $U_0, U_1, U_2, \ldots U_n$ be any class or series either of quantities or functions, which are connected by any general law of relations, then $U^n$ is held to be equivalent to, and may in development be replaced by $U_n$.
> (Blissard, 1862, p. 185)

After presenting a long list of theorems, discovered by his new method, Blissard closes the chapter with the following interesting comment:

> The preceding list of theorems and formulæ, most of which I suppose to be perfectly new, has been given for the purpose of exhibiting, in brief compass, a body of results which may serve to recommend the notation through which they have been obtained, – a notation which, I confidently believe must, in course of time, from its perfect simplicity and unrivalled power, come into general use. It may certainly, I think, be regarded as tending to give to analysis the eminently desirable qualities of unity, compactness, and simplicity; and since it is of wholly unlimited application, it is perhaps in the perfecting of this notation, as a proper tool to work with, that a large extension of modern analysis may not unreasonably be expected. If one like myself can obtain by its use, in whatever direction it may be applied, novel results of great generality and symmetry, what may it not prove in the hands of the able and accomplished analysts who adorn our age and country?
> (Blissard, 1862, p. 208).

Here, Blissard explicitly contributes his discoveries to the power of the notation and the convention of transformation, this notation makes possible. So here, we do seem to have a something like an example of discoveries depending on the typographic structure of the symbolic notation used in mathematics.

## 6.5 The impact of the use of symbols

So far, I have mainly tried to explain how the use of abstract symbols allows for a cognitively more economic practice. The use of abstract symbols simply seem to be a very effective cognitive strategy, and that explains why mathematicians have adopted this practice to the extend they have.

Next, I will discuss the possible impact the use of this cognitive strategy has had on our mathematical beliefs. In the examples given above, the abstract symbols seemed to work as neutral tools, merely making it easier for us to do, what we would have done anyway without them. One could perhaps argue, that the Cantorian diagonal argument opened entirely new land (confirming the telescope hypothesis), but as we will remember, the diagonal argument was merely a striking and (to most) convincing way of proving a fact, Cantor had already proven by another technique. In this section, I will more thoroughly discuss whether the use of abstract symbols might in some cases have had a more profound impact on what we take mathematics to be.

### 6.5.1 Extension of the symbol calculus

I will open the discussion by considering the idea that abstract symbols plays a constitutive role in the introduction of particular new mathematical objects. The idea was originally suggested by Augustus De Morgan (1849), but has recently been expanded by Michel Serfati (2005).

According to De Morgan, abstract symbols and the rules for operation on them, are at first introduced in one area in a meaningful way. The rules of operation however, will occasionally lead to symbol-combinations that seem meaningless or contradictory under the original interpretation of the symbols. Nonetheless, in some cases, such meaningless symbolic forms can be accepted as valid new mathematical objects. The examples of such new objects used by De Morgan are negative and imaginary numbers.

> Beginning with *specific* or *particular arithmetic*, in which every symbol of number has one meaning, we have invented signs, and investigated rules of operation. An easy ascent is made to *general* or *universal arithmetic*, in which general symbols of number are invented, the letters of the alphabet being applied to stand for numbers [...]. And thus, [...] we arrive at a calculus in which the actual performance of computations is deferred until we come to the time when the values of the letters are found or assigned.
>
> (De Morgan, 1849, p. 95, emphasis from the original)

In this symbolic calculus, the rules of operation led to symbols such as $a-(a+b)$, which were meaningless or inconsistent when the letters were interpreted as quantities. However:

> So soon as it was shewn that a particular result had no existence as a quantity, it was permitted, by definition, to have an existence of another kind, into which no particular inquiry was made, because the rules under which it was found that the new symbols would give true results, did not differ from those previously applied to the old ones. A symbol, the result of operations upon symbols, either meant quantity, or nothing at all; but in the latter case it was conceived to be a certain new kind of quantity, and admitted as a subject of operation, though not of direct conception.[...] These phrases, incongruous as they always were, maintained their ground, because they always produced a true result, whenever they produced any result at all which was intelligible: that is, the quantity less than nothing, in defiance of the common notion that all conceivable quantities are greater than nothing, and the square root of the negative quantity, an absurdity

> constructed upon an absurdity, always lead to truths when they led back to arithmetic at all, or when the inconsistent suppositions destroy each other.
>
> (De Morgan, 1849, p. 99)

So in other words, although the new symbolic forms were inconceivable in themselves, they were accepted because they formed a conservative extension of a body of meaningful signs and rules of operations. In this description, the introduction of abstract symbols seems to be crucial to the process; abstract symbols and formal rules of operation allow us to disregard meaning and expand the symbolic calculus into new areas. In this way algebra becomes, according to De Morgan, a science of reduction and restoration: "*reduction* of universal arithmetic to a symbolic calculus, followed by *restoration* to significance under extended meanings" (De Morgan, 1849, p. 98).

This idea of extension of symbolic forms has been developed further by Michel Serfati. In his 2005 book *La revolution symbolique*, Serfati discusses several examples, where one or more of the laws governing a symbolic form are used to give meaning to the form, when it is extended onto a domain where it *prima facie* is meaningless. One such examples is the expansion of Descartes' notation $a^p$ of powers (defined as $a$ multiplied with itself $p$ times), to the form $a^{p/q}$. Descartes' symbolic form is perfectly well-defined as long as the exponent is a natural number, but what does it mean to multiply a number ${}^{p}/_{q}$ times with itself? The only way this new and meaningless form could be given meaning, was by extending some of the laws of operation, governing the original notation, to the new domain.

Given the definition of the original form, it can easily be shown that $(a^p)^q = a^{p \cdot q}$ is a meaningful law of operation on the form. From the original definition of $a^p$ it also follows trivially, that if $a^p = b$ then $a = \sqrt[p]{b}$. Using these laws on $(a^{1/p})^p$ we get: $(a^{1/p})^p = a^{p/p} = a$, so $a^{1/p} = \sqrt[p]{a}$, and in general $a^{p/q} = \sqrt[q]{a^p}$, which is a meaningful expression (for all $a \geq 0$). To use Serfati's expressions, the chosen laws of operation (*canons électifs*) formed a bridgehead that allowed the original symbolic form to be extended to a new domain in a meaningful way (Serfati, 2005, pp. 328).

Serfati treats several other similar examples. These includes: the expansion of the factorial function $n! = n \cdot (n-1) \cdot \ldots \cdot 1$ to all positive real numbers, the use of complex numbers and square matrices as exponents, the expansion of trigonometric functions to take complex numbers as arguments, and the expansion of the form $A^{-1}$ to all given complex matrices (including non-square and non-invertible matrices). According to Serfati:

> Dans chaque prolongement en effet, les réquisits symboliques priment

> initialement ceux de sens, puisque, dans la méthodologie canonique, ce sont les significations qui doivent trouver le moyen de s'adapter aux exigences du symbolique et non l'invers.
>
> (Serfati, 2005, p. 377)

So to both De Morgan and Serfati, the symbolic forms and the laws of operation governing their syntactic transformation have a clear and strong impact on the content matter of mathematics. The lawful manipulation of symbols inspire and bring new mathematical entities into being, and subsequently – as Serfati expresses it –, meaning will have to find the means to adapt to the new symbolic forms. This is a very strong claim; if it is true, some of the mathematical objects we use are in fact constructs dictated to us as a consequence of our use of a particular cognitive artifact, viz. abstract symbols.

For the sake of the discussion at hand it is important to be very clear on the distinction between two different scenarios: 1) cases where symbols are used to express content matter that it is hard or even impossible to express by other known means, and 2) cases where a symbolism inspires or determines the conceptual development of mathematics, for instance in the form of the creation of new meaning or new entities. Serfati clearly seems to consider his examples to be of the second type – meaning must conform to symbols, and not the other way around. I on the other hand, see the question as somewhat more open.

Let us return to the development of the notation for powers. As it turns out, a lot of both conceptual and notational development had taken place long before Descartes started to use his $a^p$-notation. As noted in subsection 6.3.2, Diophantus was already using a semi-symbolic notation in the $3^{\text{th}}$ century CE. In the $12^{\text{th}}$ century, the Arab mathematician al-Samaw'al developed a table of powers, including reciprocals (what we would call powers with negative exponents). By describing how to jump from one column to the next in this table, al-Samaw'al was able to explain the content of the law of exponents, viz. that $x^n \cdot x^m = x^{n+m}$, although his rhetoric style, where say, the product of a square ($x^2$) and a cube ($x^3$) would be expressed as a 'square-cube', did not allow him to express the law numerically (Katz, 1998, pp. 252).

In the $14^{\text{th}}$ century Nicole Oresme worked with fractional exponents especially in connection to ratios. He developed a language, where for instance the $\frac{1}{2}$'th power of a given ratio was expressed as 'half' the ratio, the $\frac{3}{4}$'th power as 'three fourth parts' of the ratio, and so on. This allowed him to expressed general relationships, such that "... one third of a whole equals two-thirds of its half or subdouble.", meaning in modern terms: $A^{1/3} = (A^{1/2})^{2/3}$ (Grant 1974, p. 153; see also Cajori 2007, pp. 91). Most remarkably, Oresme even seems to have had an intuition of the existence of irrational exponents, but he was unable to describe his ideas clearly because he lacked the prober notation.

Finally, in the 15$^{\text{th}}$ and 16$^{\text{th}}$ century, several different notations were tried out competing with the one finally used by Descartes. These include Nicolas Chuquet's notation, which allowed him to use negative exponents (see subsection 6.3.2 above).

All in all, the introduction of the Cartesian notation for exponentials and the 'prolongement' of it to include fractional exponents seem quite clearly to be a case, where already existing ideas were given a suitable and usable expression by a newly invented symbolic form. So the symbols did not predate the meaning, as Serfati suggests, rather the symbolic forms presented a way to convey ideas that were hard or even impossible to express without the proper representational means. So this is a case of type 1 of the two types distinguished above.

Something similar can be said of the examples given by De Morgan. Negative numbers were known and operated on long before the introduction of symbolic algebra. Diophantus stated the basic rules for multiplication involving negative numbers (described as 'wantings'), and explains how to transform equations in order to eliminate negative coefficients (Heath, 1910, pp. 130). In the Chinese textbook *The Nine Chapters on the Mathematical Art*, probably composed in the 1$^{\text{th}}$ or 2$^{\text{th}}$ century CE, the rules the for addition and subtractions involving negative numbers (and zero) were explained in connection to the use of the Chinese stick abacus (Yong 1994, pp. 35; Boyer 1989, pp. 222). Finally, all the basic rules of arithmetic, including how to operate with negative numbers and zero were stated in purely rhetoric form by the Indian mathematician Brahmagupta as early as year 628 CE (Katz, 1998, p. 226). Although the Hindu mathematicians did not acknowledge negative solutions to equations involving quantities (such as monkeys), they at least on one occasion accepted a negative solution to a problem involving distance (interpreting it as a positive distance in the opposite direction) (Ebbinghaus *et al.* , 1991, p. 13).

So although the Europeans were indeed very skeptical towards negative numbers for centuries, the development of an abstract symbolic arithmetic does not seem to be a necessary step towards at least some understanding of the concept.

Complex numbers – De Morgan's second example – were also first encountered and operated on in a purely rhetoric context in Gerolamo Cardano's (1501-1576) *Ars Magna* from 1545, where Cardano states and verifies the complex solutions ($5+\sqrt{-15}$ and $5-\sqrt{-15}$ in modern notation) to the problem of dividing 10 into two parts such that the product is 40. His successor Rafael Bombelli (1526-1572) stated the various laws of multiplication for imaginary numbers – still without the use of abstract symbols – and used calculations on complex numbers to find the real number solutions to selected cubic problems (Katz, 1998, p. 335–336).

It should be noted that Cardano, Bombelli and most mathematicians of the time regarded complex numbers as mere sophistry, and not as solid mathematical objects. This leaves some room for the introduction of abstract symbols to play in the acceptance of complex numbers. It can however, be argued, that the most important event in this process was the introduction of an intuitive and meaningful geometric representation of complex numbers in form of the complex plane, and not the introduction of abstract symbols as such. I will return to the role played by the introduction of the complex plane below (in section 6.12.1.2.3)

So in conclusion, De Morgan seems to have overrated the role played by the introduction of abstract symbols in the conceptual development of mathematics. The conceptual development of negative and imaginary numbers was well on its way before the introduction of abstract symbols. In these cases, the central artifact seems to be the algorithms and rules of operation developed in order to solve specific problems; the crucial step in both Cardano and Bombelli's encounters with complex numbers was not abstract symbols, but algorithms developed in order to solve particular classes of problems. Algorithms, which in some cases led to solutions involving square roots of negative numbers. In *The Nine Chapters*, negative numbers and the rules for operating on them were introduced as part of a method for solving linear equations. The method involved both addition and subtraction of large columns of numbers, and such a procedure would inevitably lead to the occurrence of negative numbers. This was handled arithmetically by stating the relevant rules of operation, and representationally by introducing sticks with two different colors – red sticks for representing positive numbers, and blacks sticks for representing negative numbers. Zero was represented by leaving a slot empty (Yong, 1994, pp. 35). So of course, some representational power was needed, but not the kind of abstract, symbolic power, De Morgan refers to. The two cases used by De Morgan in other words seems to be of type 1, using the typology presented above.

The development of pseudo-inverses and factorials of positive real numbers are better candidates to cases of type 2, i.e. cases, where the symbolism played a greater part in the conceptual development. In both cases, the 'bridge' formed by the chosen rules of transformation seems to lead into genuinely new areas of mathematics, and furthermore, there seems to be more of a genuine choice involved in picking out the *canons électifs*.

In the case of the factorials, the chosen rule is the identity $f(x) = x \cdot f(x-1)$, which is obviously true for the factorial function. According to Serfati (2005, pp. 366-8), Euler however, discovered another function – the gamma function $\Gamma(x)$[9] – which satisfies the same relationship, i.e. $\Gamma(x) = x \cdot \Gamma(x-1)$, and furthermore has the value $(x-1)!$ for all integer values of $x$. So in other words, the gamma function (or rather $\Gamma(x+1)$) is both a conservative expansion of the factorial function, and share an important rule of operation with the factorial function. The gamma function was on this ground chosen as a way to generalize the factorial function to all positive real numbers simply by defining $x!$ as $\Gamma(x+1)$. From this development, otherwise meaningless forms such as 3,5! or $\pi!$ were given meaning.

Something similar can be said about the development of pseudo inverses of matrices. Here, according to Serfati (2005, pp. 370-2) four identities, all trivially true for regular inverses, were chosen to define pseudo inverses[10]. The pseudo inverse of a given matrix $A$ then, was simply defined as any matrix $A^{-1}$ making all of the four identities true. Given this definition, any complex matrix $A$ turned out to have one unique pseudo inverse, and in the case of invertible matrices, the pseudo inverse was identical to the regular inverse. So the concept is well-defined and pseudo inverses form a conservative extension of regular inverses. In this case, a clear choice formed the bridge to a place, where the symbolic form $A^{-1}$ is meaningful for all complex matrices $A$.

In these cases the bridges at least seems to lead to genuinely new and uncharted land. It is however, still somewhat unclear precisely what the symbols added to the process. Is the use of abstract symbols essential to such developments, or could we have reached similar results using a purely rhetoric style? To my mind, the method of conservative expansion through

[9] The function was by Euler defined as

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

[10] The four identities are:

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

, where $M^*$ is the adjoint matrix (Serfati, 2005, p. 371) choice of rules of operation does not seem necessarily tied to the use of symbols. In fact, as we saw in the case of negative and imaginary numbers, such expansions can take place in purely rhetoric context as well. I will return to this type of expansion of mathematics again in chapter 7. This time with a focus on the laws of operation, and not so much on the fact that they in some cases happen to be applied to abstract symbols.

### 6.5.2 Impact on ontology and epistemic standards

There are however, other examples, where the use of abstract symbols has had a clear impact on both the ontology and the epistemic standards of mathematics.

As a beginning, I will turn to the discussion over which objects to allow in geometry. As I will discuss at length below (in section 6.7), the framework provided by Euclid's *Elements* was rather restrictive, and in the 17th century, René Descartes, Christiaan Huygens and others discussed how to expand this framework in order to allow more objects into geometry. The discussion however, never came to a conclusion in the given setting. Towards the end of the 17th century, the problem of finding tangents to curves and areas beneath curves became prominent in mathematics, in part because such problems frequently arose in the study of motions in mechanics. These problems could not be handled satisfactory by traditional Euclidian (synthetic) geometry. Instead, a new analytic paradigm, where curves were represented by equations, arose. This paradigm turned out to be highly successful, partly because it offered a way to represent a class of curves that were hard or impossible to express in the previous paradigms, and partly because it offered a way to replace a number of difficult operations and constructions (including finding tangents and areas) with syntactic transformations of the symbols of the equations (i.e. calculus).

This remarkable development however, turned out not only to offer an easier and faster way to accomplish mathematical results; it also changed mathematics in several ways. Firstly, the adoption of the calculus led to a considerable change in epistemic standards. Where synthetic geometry based on the axiomatized Euclidean framework had the very highest epistemic standards, advocates of the calculus more or less had to develop an epistemic blind-spot in order to overlook the severe foundational problems of the theory (see Berkeley 1754 for a contemporary criticism). Although calculus was widely used in the 18th century, it was not given a satisfactory foundation at least until the development of the concept of limits in the early 19th century.

Secondly, the acceptance of any curve that could be stated in analytic form, ultimately lead to the acceptance of a number of curves that were impossible from a geometrical point of view, such as space-filling curves and everywhere continuous but nowhere differentiable curves[11]. In fact, instead of leading to the rejection of these objects, the geometric counter intuitiveness of the curves lead to the rejection of geometric intuition; as the intuition was clearly in contradiction with the symbol based analytic geometry, it could no longer be trusted (see for instance Mancosu 2005).

So in this example a particular embodied cognitive strategy had a clear impact on mathematics: The need and desire to use symbols as a cognitive artifact in a specific area of mathematics led to profound changes both in regard to the objects accepted by and the epistemic standards of the area. Clearly, in this case the cognitive artifact served as more than just a neutral tool. It seems that a particular set of mathematical beliefs was directly formed by our adoption of abstract symbolic representations of geometric objects. The 'monster curves' described above are clear examples of mathematical objects that are constructed as a direct consequence of our use of a particular cognitive tool.

---

As a second example of the impact of the use of abstract symbols, I will mention the use of formalizability as a criterion for the acceptability of a proof. Although proofs are rarely given as rigorous formal deductions, most mathematicians today will only accept a proof, if it is somehow made probable that it could be formalized and given as a series of purely formal transformations in a formal theory (see e.g. Curry, 1954; Tymoczko, 1979). Proofs are simply to be identified with the external, physical, symbolic forms used to represent them. The test of the validity of a proof, amounts to examine whether the manipulations made on the symbols conform to the syntactic moves allowed in the formal system – that is why Thomas Tymoczko in a much discussed paper on computer assisted proofs adds surveyability to formalizability as "major characteristics of [acceptable] proofs" (Tymoczko, 1979, p. 59). It

[11] The first space-filling curve was published by Peano in 1890. The curve is a continuous map from the unit interval $[0;1]$ to the unit square $[0;1] \times [0;1]$. It was defined purely analytically without any geometrical interpretation (Peano, 1890). The first continuous nowhere differentiable function to be published was Weierstrass' function, discovered in 1872 and published in 1875 in (du Bois Reymond, 1875). It was given by the equation: $f(x) = \sum(b^n \cos(a^n x)\pi)$, with $a$ odd $b \in ]0;1[$ and $ab > 1 + \frac{3}{2}\pi$.

must of course be possible to examine the proofs given as external, physical objects, in order to see if they live up to the given standards. This, on the other hand, is something, which can be done by purely syntactic means, and consequently, the symbols do not need to be considered as semantic objects at all. Revealingly, characteristics such as 'conveying understanding' or 'being meaningful' are not considered as being important by Tymoczko. A probable motive for this choice is that meaning and understanding is something subjective, while the properties of a concrete physical object (a list of stepwise, symbolic manipulations) is something which can objectively be determined. Consequently, only by considering the symbols of a given proof *exclusively* as a syntactic objects, is it possible to establish truly objective criteria for determining whether the proof is valid or not.

So here once more, the introduction and use of a particular cognitive artifact – abstract symbols – has facilitated a clear and significant change in the epistemic standards of mathematics, i.e. in the standards used to judge whether a knowledge claim is acceptable or not. Mathematics was done before the introduction of abstract symbols, and proofs were made and accepted as valid. However, with the introduction of abstract symbols, a different standard of validity – believed to be more objective – was made possible, and was eventually adopted by the mathematical society.

So in conclusion, the use of abstract symbols as cognitive artifacts has had a clear and significant impact on mathematics. The artifact is not just a neutral tool. Its use has had consequences for the kind of objects, we belief to be acceptable geometrical objects, and it has changed the way we evaluate proofs to be acceptable or not. This in other words seems to be consistent with the constructivist hypothesis.

The power and enormous influence of this particular cognitive tool is witnessed by the fact that the formalist movement simply identified mathematics with "manipulation of signs according to rules" (Hilbert, 1925a, p. 381) (or in other words, they adopted the identification hypothesis). From my point of view, this is a mistake. Abstract symbols are simply a cognitive tool used in our mathematical practice, and one should not take the tools for the trade. Although this particular cognitive tool is at the heart of modern mathematics, it does not exhaust what mathematics is. As we shall see in the next sections, other cognitive strategies and tools are equally important.

## 6.6 Figures as cognitive artifacts

The spatial and topological properties of external representations can be used in many different ways. The pragmatic use of the physical properties of object symbols described in section 6.4.2 provides one such use. In this dissertation, I will discuss two other slightly different uses; one is exemplified by the use of drawings and figures, and the other by the use of certain types of diagrams. Both of these uses are epistemic, or what I will call *knowledge guiding*; in addition to the purely pragmatic use of the spatial arrangement of symbols e.g. in matrix multiplication, the physical and spatial properties of figures and diagrams can be used to gain knowledge and understanding about the mathematical objects, they represent. As they do so in two very different ways, I will concentrate on the use of figures in this section, and discuss the use of diagrams in section 6.10.1 below.

The main difference between figures and diagrams is, as I see it, the following: Figures are meant to resemble or even have an iconic likeness with the topological structure of the objects, they represents, whereas diagrams do not. Diagrams are of course also meant to resemble something, but what that more precisely is will take some explanation, which I wait until section 6.10.1 to give. The above statement might cause some ontological worries. What does it mean for an abstract object to have geometrical properties? And do such objects exist at all? With more caution, the statement could be rephrased like this: A figure will partially share the geometrical structure of or even belong to the general type of shapes, the mathematical object, the figure is used to represent, is an abstraction of. An actual drawing will of course always be different from an abstract mathematical object – the lines used to draw a triangle will for instance have breath and cannot be perfectly straight in contrast to the lines making up the legs of an ideal and abstract mathematical triangle. But still, the likeness between a drawing of a triangle and an idealized triangle can in some instances be good enough to be used for knowledge guiding purposes, as I will explain in more details below. But first, I will present some examples of mathematical figures.

Figures are of course prototypically used in geometry. The figure used as part of the proof presented in figure 6.4 above can serves as a good and typical example. Figures however, can also be used in analysis, for instance to represent curves and graphs of functions (see figure 6.6 and figure 6.7) and even, one could argue, in some cases in arithmetic (figure 6.8).

From a cognitive point of view, figures allow mental content to be off-loaded into the environment in the form of a concrete, external object, which can be inspected from a third person view and intersubjectively shared and

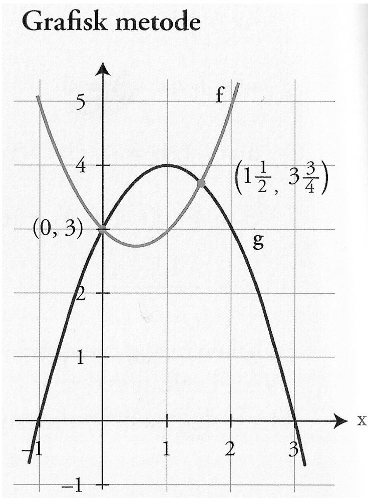



**Figure 6.6:** "The graphic method" from a Danish text book on hi-school mathematics (Antonius *et al.* , 2009, p. 148). The method is used to find the points of intersection of the graphs for the functions $f(x) = x^2 - x + 3$ and $g(x) = -x^2 + 2x + 3$ (so the ideal, mathematical objects represented in the figure are graphs, not functions). In the context of Danish hi-school mathematics, the graphical method is considered valid if the solutions are subsequently validated by insertion in the analytic expressions for the functions.

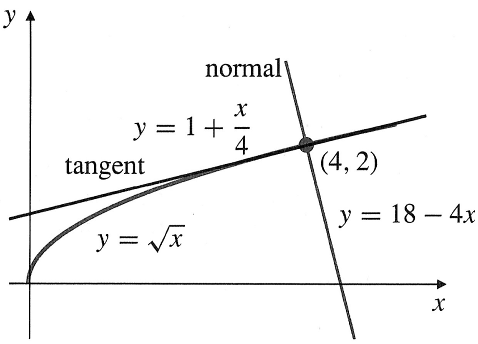



**Figure 6.7:** The tangent and normal to the graph $y = \sqrt{x}$ from a text book on undergraduate calculus (Adams, 1995, p. 97). Here, the figure is used as a way to illustrate solutions found by other (analytic) means. However, the once the graph $y = \sqrt{x}$ is drawn, it *will* have a tangent and a normal at any point (although they can be hard to determine with precision by inspection of the figure alone).

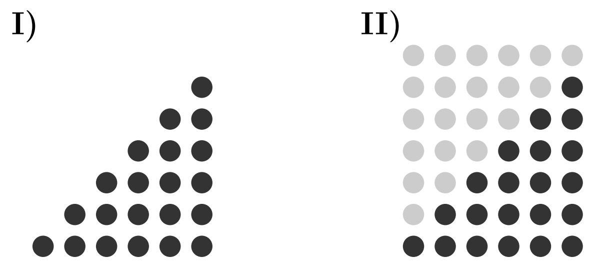



**Figure 6.8:** The elements in these figures are iconic representations of arrangements of discrete objects. In I), the objects are arranged in six columns. The number of objects in the columns corresponds to the first six natural numbers, starting with one object in the first column on the left. The sum of the numbers from one to six can easily be deduced from figure I) by counting the elements, although the sum was not part of the information used to make the figure. In II) auxiliary elements have been added to the figure, showing that the sum of the original elements – and consequently the sum of the numbers form one to six – can be determined as $(6 \cdot 7)/2$. The figure also suggests how a more general argument could be produced.

discussed. This is the advantage of language noted by Andy Clark by (see section 5.3.2 above), but in my view, mathematical figures and most other external representations share this function with rhetoric representations.

Figures however, also offer something more, something the inspection of a rhetorical representation does not. Firstly, figures can serve as material anchors for conceptual structures. The concept *material anchors* was originally designed by Edwin Hutchins only to apply to representations of conceptual blends (see section 5.5.1), but I see no reason why it does not apply to unblended domains just as well. The use of figures as material anchors is for instance clearly evident in figures such as 6.4, where the physical drawing is used to stabilize an elaborate conceptual structure.

Secondly, due to the iconic likeness between the figure and the object it represents, the figure will share more properties of the objects, than those used to create the figure. In the examples given above, the intersection points for the graphs of $f(x) = x^2 - x + 3$ and $g(x) = -x^2 + 2x + 3$ can be read off from the figure, although this information was not used in the construction of the figure (figure 6.6). Similarly, the figure representing the graph for $y = \sqrt{x}$ will have a tangent and a normal at any point, once the figure is drawn, and the properties (such as slope) of the tangents and normals on the figure will approximate the properties of the corresponding abstract objects (figure 6.7). In figure 6.8, there will be a total number of dots in the columns once they are drawn, although this number was not used in the construction of the figure. And finally, returning to figure 6.4, we can add a triangle to the initial figure of the arch length on the unit circle. This physical triangle will have heights $h_1$ and $h_2$ similar to the abstract triangle it represents, and it will have a given area *etc.* None of this information was used in our original drawing of the arch length, but it can nonetheless be used to infer information about the arch length.

The fact, that physical figures can contain additional information about the objects, they represent, allow us to use the figure to gain knowledge. Figures might in other words function as epistemic artifact that allow us to replace calculations and mental imaginary with physical actions, in the form of construction and inspection of the concrete, external figures that are drawn on the paper before our eyes.

---

The fact that figures can be used as epistemic artifacts in ways similar to abstract symbols, has led to discussion about whether there is any difference between the two types of representations. Charles S. Pierce for instance, considered figures and symbols to be the same general type of symbols. Pierce simply defined iconicity functionally; a representation is *iconic*, if it makes it possible to derive more information about the represented than the information used to form the representation (Stjernfelt, 2006). As this is the case for both prototypical figures, as those discussed above, and mathematical symbols, both types of representations are in Pierces' classification iconic symbols.

Marcus Giaquinto seems to advocate a similar viewpoint (Giaquinto, 2007). Giaquinto notices, that since we use the concrete physical shape of both mathematical symbols and figures to gain new knowledge, the traditional dichotomy between algebraic and geometric methods cannot be upheld:

> Symbolic thinking typical of algebra, to wit, rule-governed manipulation of symbols, is just as spatial as geometrical thinking. The rearrangements, additions, and deletions of symbols [...] are operations in two-dimensional space. Moreover, these operations depend on spatial features of the input symbol array and may be performed independently of any semantic content assigned to the symbols.
> (Giaquinto, 2007, p. 241)

Both Pierce and Giaquinto are in a way right; as both mathematical symbols and figures are used as epistemic artifacts, we do create knowledge or information by handling the concrete physical objects on the paper. However, as I see it, we do not create new knowledge in the same way in the two cases, and that is important to notice. In the case of symbols, we manipulate them in accordance with syntactic rules, which must somehow be known or learned in advance. In the case of figures, new knowledge is guide by exploiting the figures' iconic likeness with the objects they represent (although admittedly, the construction of new geometrical shapes and auxiliary lines *etc.* might be restricted by rules, such as the rules restricting possible constructions in the Euclidian framework). The shape of an arbitrary symbol is arbitrary; the physical shape of a symbol does not have any likeness with the object, it represents (and this is why symbols in my mind do not have iconic likeness with that, which they represent), so during the production of new knowledge, the meaning of the symbols might be totally disregarded, as also noticed by Giaquinto. This is never the case with figures. They will always present themselves for us as meaningful, and it is because we understand the figures that we are able to use them to infer new knowledge.

So in sum, figures can produce knowledge *qua* their iconic likeness with the objects they represent, symbols can produce knowledge *qua* their role as syntactic objects. These are two very different processes. The use of figures discussed above, is in other words a genuine new way of using the spatial character of external representations.

---

This use of drawings and figures is clearly an example of embodied cognition. The computational tasks involved in mental imagery are replaced by the creation and inspection of external, physical objects. Furthermore, the inspection of the physical objects can be eased by drawings of additional objects. The construction of the physical drawings can be aided by the use of instruments, such as rulers and compasses - and in resent years the image creating computer programs.

## 6.7 Impact of figures and drawings

I will center the discussion of the impact of the use of figures on the use of drawing instruments, such as compass and straightedge in mathematics.

These instruments serve as tools to draw precise representations of geometrical objects. But the choice of instrument is not innocent, as different objects can be constructed and different problems solved depending on the tools used. To give an example, the angle cannot be trisected in the Euclidean framework, where only constructions by compass and straightedge is allowed, but the problem can easily be solved, if other types of instruments, such as a marked ruler or a special compass, are brought into play.

In the 16$^{th}$ and 17$^{th}$ century discussion of instruments and practical methods of construction frequently entered the debate over which objects to accept in geometry. François Viète, for example, wanted to expand geometry. He did so simply by postulating the existence of a particular construction, the 'neusis', which can be used to solve classical problems such as the trisection of the angle[12]. The use of the neusis was later sought justified by Johannes Molther by appeal to the exactness of the instruments (a marked ruler) and procedures needed to perform the construction (Bos, 2001, chap. 10 and 12).

Instruments and machinery used to trace curves also played an important part in the mathematical reasoning of René Descartes. Descartes also wanted to expand the universe of traditional Euclidean geometry. Instead of restricting himself to the curves, which can be traced by straightedge and compass (viz. straight lines and circles), he wanted to accept more complex curves as well:

> [I]f we think of geometry as the science which furnishes a general knowledge of the measurement of all bodies, then we have no more right to exclude the more complex curves than the simpler ones, provided they can be conceived of as described by a continuous motion or by several successive motions, each motion being completely determined by those which precede; for in this way an exact knowledge of the magnitude of each is always obtainable[13].
>
> (Descartes, 1954, p. 43).

In order to determine whether a curve actually fulfilled this criterion of being constructible in a clear and distinct way, Descartes frequently imagined how and with which instruments a curve could be traced. Descartes for instance envisioned special compasses that could be used to divide an

[12]Given two straight lines, a point $O$, and a line segment $a$, the neusis is the construction of a straight line going through $O$ and intersecting the two original lines in points $A$ and $B$ such that $AB = a$.

[13]In modern terms, Descartes accepted only algebraic curves, but rejected the transcendental once. Although Descartes co-invented analytic geometry, he did not use the equations of curves as a way to distinguish between acceptable and unacceptable ones (Bos, 1981, p. 297).

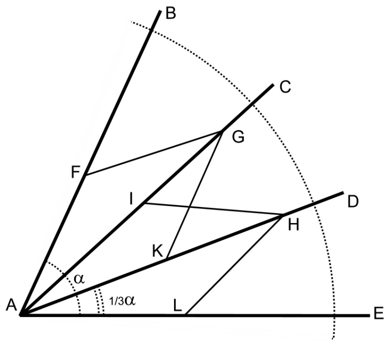



**Figure 6.9:** Trisection compass. The four rulers $AB$, $AC$, $AD$, and $AE$ are pivoted at $A$. The links are fixed at $F$, $I$, $K$, and $L$, but can slide at $G$, and $H$. A given angle $\alpha$ is trisected by opening the compass until $\sphericalangle BAE$ is equal to $\alpha$. This will make $\sphericalangle DAC$ equal to $1/3\alpha$.

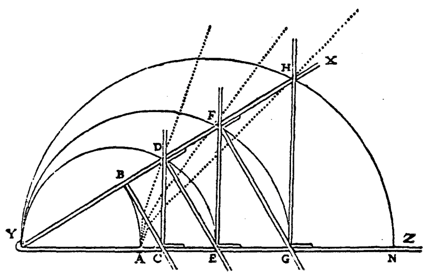



**Figure 6.10:** Descartes' mesolabe. The ruler $YX$ pivots around $Y$. The ruler $BC$ is fixed at $B$, and the following rulers can slide on either $YX$ or $YZ$. When the instrument is opened, $BC$ will push $CD$, which in turn will push DE and so on. The wanted curves are traced by the intersection of $YX$ and the sliding rulers. (Reproduced from (Descartes, 1954, p. 46 (p. 318))

angle into any number of equal part (which is not possible in the Euclidean framework, see figure 6.9) and a very complicated piece of machinery, the *mesolabe* (figure 6.10), which could be used to construct any number of mean proportionals (also something not possible by Euclidean means). These instruments were, in Descartes view, "no less certain and geometrical than the usual compass by which circles are traced" (quoted in (Bos, 2001, p. 349), see also (Descartes, 1954, p. 47)), and the curves traced by them were just as acceptable geometrical objects as lines and circles.

Descartes also considered machinery involving pieces of string. He accepted some such designs, for instance machines used to trace ellipses and ovals (Descartes 1954, p. 90 and p. 122, see also figure 6.11). However, he did not accept a curve, if the string in the machine used to trace it changed from being curved to being straight during the tracing motion. Descartes did not consider such a change in curvature of a string a clear and distinct motion, because the ratio between straight and curved lines was not known (and, Descartes believed, would never be known by human minds (Descartes, 1954, p. 90)).

We do not know exactly what kind of machinery Descartes had in mind in connection to this statement. However, he does mention the Archimedean

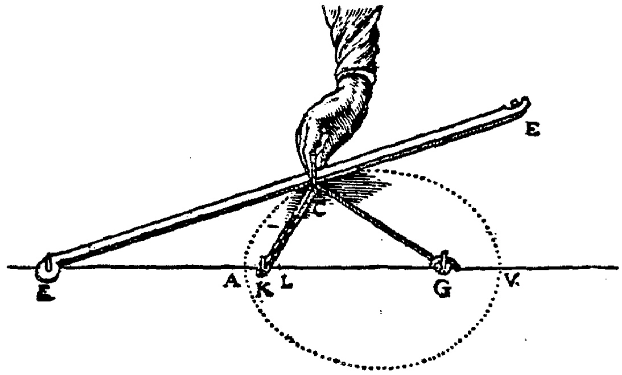



**Figure 6.11:** Descartes' oval tracing machine. A string is fixed at $E$, slung around $K$ and $C$ and fixed at $G$. The string is kept straight to the ruler $FE$ at $C$ by a tracing pin. The oval is traced by pivoting the ruler around $F$ (reproduced from (Descartes, 1954, p. 122 (p. 356))

.

spiral as an unadmittable curve, and perhaps he might have been aiming at something similar to a device sketched by Christiaan Huygens some years later (in 1650). This machine could be used to trace Archimedes spiral by letting a string curl around a pivot/pulley during the tracing motion (see figure 6.12).

The device mentioned above is not the only one imagined by Huygens. In fact, such machinery seems to have played an important part in his mathematical thinking as well. The best example is perhaps his attempt to argue for the acceptance of another controversial curve, the *tractrix*[14], by designing a number of quite ingenious machines, which came, in his opinion "very close to the simplicity of the compass" and allowed him to tract the curve "almost as easily as the circle" (citations from Bos 1988, p. 29). These machines included a small two-wheeled cart, a spherical shell dragged while floating on a fluid, and various designs involving heavy objects being dragged (see figure 6.13). It is known, that Huygens made at least one of the devices and used it to draw the tractrix (Bos, 1988, p. 29).

---

[14]The tractrix is inspired by the curve traced by a heavy object (say a chain watch) being dragged by a string over a smooth surface (such as a tabletop) in a direction perpendicular to the original orientation of the string. For a precise definition and properties, see (Bos, 1988)

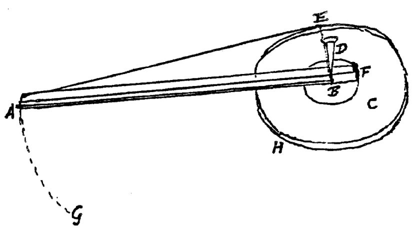


**Figure 6.12:** Huygens' device for tracing the Archimedian spiral: A ruler $AB$ with a drawing-pin attached at $A$ is pivoted counterclockwise around $B$. While the ruler moves, a string $AED$ winds around a disc $EH$ and drags the drawing-pin toward the center $B$ (Huygens, 1908, p. 216)

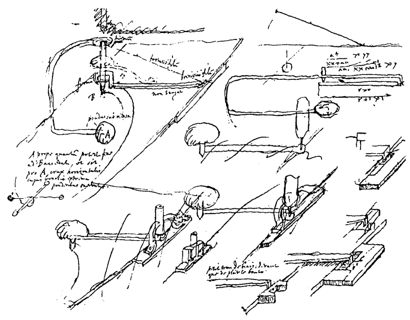


**Figure 6.13:** Huygens' drawings of some of his designs for tractrix tracing machines (reproduced from Bos (1988, p. 30)).

The examples given so far should be enough to establish the following point: In the 16$^{th}$ and 17$^{th}$ century a particular type of argument involving reference to physical representations of geometrical objects entered the mathematical debates over which objects to admit in geometry. This type of argument and the debate as a whole are only meaningful in a setting, where figures are used to represent geometrical objects. So here, we see a clear impact of this particular cognitive practice; in this case it facilitated and made a particular kind of debate over which objects to accept in mathematics, possible. So our use of figures as external representation of mathematical objects is not just a neutral tool. At least in this case, it seemed to influence our beliefs about which objects to accept in mathematics.

This particular episode underlines embodied nature of mathematical cognition. The arguments made by Descartes and Huygens presuppose both very specific bodily capacities – you must for instance be able to grasp and move things – and a set of environmental affordances, such as the existence of strings, rigid bodies, and objects capable of leave a permanent mark. For this reason, the arguments simply does not seem to be available to disembodied cognizers or cognizers having bodies or living in environments markedly different from ours; if dolphins were doing geometry, there is little chance that they would consider the mesolabe or even be able to understand the argument made from it by Descartes. And this is not due to any facts about the imagined dolphin-geometers' brain or mind, but due to the fact that their body and physical environment simply does not support the right kind of interactions.

The episode in other words, serve as an example of the impact the use of a particular embodied strategy can have on our mathematical beliefs, but it also very clearly shows how or mathematical beliefs are constrained by our body and particular way of existing in the world. Although the reference to the complicated machinery above offers a particularly clear example of this influence, it might not be isolated. I see no reason why a similar argument could not be made for Euclidean geometry and other types of constructive geometry. In those practices only other types of machinery (such as compass and straightedge) are involved.

## 6.8 Partial conclusion: The use of externalization in mathematical thinking

I the preceding sections, I have investigated the role played by externalization in mathematical thinking. The use of this cognitive strategy is mainly centered around different types of cognitive artifacts, ranging from purely conceptual artifacts, such as rules of operation and sequences of counting words, to material artifacts, such as the abacus, abstract symbols and figures. As we have also seen, our mathematical practice is highly dependent on such external artifacts in almost anything, from very basic tasks such as handling the numerical aspect of reality with digital precision, to very complicated operations, where mental computations are substituted with symbol manipulation, or complicated conceptual structures are anchored in figures representing the mathematical objects in question. Cognitive artifacts, furthermore, does not seem to be merely neutral tools, that allow us to do, what we would have done anyway in a cognitively more economic way. In some instances, the artifacts are clearly constitutive for the performance of the tasks, they are part of, and in others, our adoption of a particular type of artifact has had a clear influence on our mathematical beliefs. Our use of cognitive artifacts, in other words, cannot be explained with the neutral tool hypothesis. It seems that some of our mathematical beliefs are partially constructs formed by our use of particular cognitive artifacts.

I will now leave the use of use of externalization, and in the following sections turn to the second embodied cognitive strategy used in mathematical thinking; the use of sensory-motor experience in off-line thinking.

## 6.9 The use of life-world experience in off-line thinking

Sensory-motor experiences, especially in the form of what I will call *life-world* experiences, i.e. pre-theoretic perceptual or practical experiences, might influence, be used in or even be constitutive of mathematical thinking in two different ways: 1) Cognitive blends having mathematical objects in one domain and life-world experiences in the other, and 2) Conceptual metaphor having mathematical objects as target and life-world experiences as source domain. These mechanisms are related, and they both fall under the more general category of conceptual mapping.

Some examples of the use of such mappings in mathematics are given in Fauconnier & Turner (1998), Fauconnier & Turner (2003), Lakoff (1987), and Robert (1998). Furthermore, Lakoff & Núñez (2000) contains a thorough and in-depth analysis of the use of cognitive mapping in mathematical thinking. The ideas of this book have been developed with modification in subsequent work by Núñez (for instance in Núñez, 2004, 2008, 2009). The work by Lakoff and Núñez is not only the most thorough, it also contains – by far – the strongest claims about the impact of cognitive mapping on the content of mathematics. For this reason, I will discuss the claims made by Lakoff and Núñez in depth in a separate subsection below. Firstly, however, I will give my own account of the role played by conceptual mapping in mathematics.

## 6.10 The use of conceptual metaphors in mathematical thinking

In the textbook *Basic Algebra* by Nathan Jacobson sets are introduced as arbitrary collections of elements (Jacobson, 1985, p. 3). On the following pages of the book, the basic properties and relations between sets are defined and described in the following way:

> If $A$ and $B \in \mathcal{P}(S)$ (that is, $A$ and $B$ are subsets of $S$) we say that $A$ is *contained in* $B$ or is a *subset of* $B$ (or that $B$ *contains* $A$) and denote this as $A \subset B$ (or $B \supset A$) if every element $a$ in $A$ is also an element in $B$. [...]
>
> If $A$ and $B$ are subsets of $S$, the subset of $S$ of elements $c$ such that $c \in A$ and $c \in B$ is called the *intersection* of $A$ and $B$. We denote this subset as $A \cap B$. If there are no elements of $S$ contained in both

> $A$ and $B$, that is, $A \cap B = \emptyset$, then $A$ and $B$ are said to be *disjoint* (or *non-overlapping*)
>
> (Jacobson, 1985, p. 3–4. All emphasis form the original)

In the text, sets are described as containers; a set $B$ can *contain* another set $A$, and both sets can *contain* elements *etc.* Such descriptions are clearly metaphorical; sets are abstract collections of mathematical objects, and a collection in itself cannot literally contain anything. The descriptions given by Jacobson all draw on a basic metaphorical conception of sets as containers (see table 6.2 for details).

Sets Are Containers

| Source domain: Containers | | Target domain: Sets |
|---|---|---|
| A container | → | A sets |
| Containers inside a container | → | Subsets of the set |
| Objects inside a container | → | Elements of the set |
| Objects contained by two containers | → | Elements shared by two set |

**Table 6.2:** Elements of the Sets Are Containers metaphor

Our experiences of containers are pervasive. We are constantly dealing with containers such as cups, pans, and cupboards. We move them around, put things into them, take things out of them again, and put containers into other containers. We are our self being contained in rooms, buildings cloths, light, darkness *etc*, we experience moving into and out of containers, and we experience their internal relationships (a room might be contained in a building). In fact, one of our most basic experiences is the experience of our body as a container and of the flow of material moving into and out of this container[15].

The conceptualization of sets as containers allow us to recruit all of these past experiences with containment when we deal with mathematical sets.

[15]In cognitive linguistics the container metaphor is counted amongst a few conceptually primitive *image schemas*. In this context, the concept is derived from the concept of *schemata* introduced by Immanuel Kant, and used to describe "embodied patterns of meaningfully organized experience" (Johnson, 1990, p. 19). Image schemas might be perceived, as when we see that the tea is in the cup, but they might also be imposed on perception, as when we see a bird as being *inside* a flock (see Lakoff & Núñez, 2000, pp. 30 for more). In this connection, however, the precise classification of the metaphor is perhaps less important, as long as it is understood that we are dealing with a significant embodied experience.

This grounds our understanding of sets in experience and give us an intuitive grasp of the abstract concept, which enable us to draw inferences about mathematical sets quickly and effortlessly[16]. So for instance, if we are told that a set $A$ is contained in another set $B$, and that $A$ contains a particular element $x$, we immediately and effortlessly infer that $x$ is also contained in $B$.

Here, we seem to have a clear example of a conceptual metaphor, as they were described in the previous chapter: An abstract and unknown domain is conceptualized in terms of a well-known domain of physical experience, and this connection allows the user to transfer structure and expectations from the well-known to the unknown domain.

The use of linguistically expressed conceptual metaphors in mathematics is well described in the literature, so I will not give further examples at this point. In the discussion of the impact of metaphors below, I will give examples of the use of metaphors in arithmetic. The reader is also referred to Núñez (2004) for a thorough analysis of the metaphors of movement and the use of the so-called Source-Path-Goal image schema in analysis.

### 6.10.1 Material anchors for conceptual metaphors

In the example given above, the Sets Are Containers metaphor was expressed by purely linguistic means. But this is not the only way to express metaphors. In mathematics, metaphors are frequently expressed by the use of diagrams, arrangement of symbols and other visual means. As I see it,

[16]It should be noted, that in the context of mathematics, 'grounded meaning' is not the only type of meaning. An expression as $(-3)\cdot(-2)$ can be perfectly meaningful in the sense that one knows the rules for handling it correctly – in this case that $(-3)\cdot(-2) = 6$ – even though the rules themselves do not make sense and are not understood on a deeper level. I will call this more superficial understanding consisting in knowing how to manipulate the terms of a given expression 'Wittgensteinian meaning' following Ludwig Wittgenstein's famous definition of meaning in *Philosophical Investigations*: "For a large class of cases — though not for all — in which we employ the word 'meaning' it can be defined thus: the meaning of a word is its use in the language" (Wittgenstein, 1958, §43).

I am aware, that this brief discussion of the concept of meaning is hardly satisfying from a philosophical point of view. This dissertation however, is a highly cross-disciplinary work. Its aim is to combine knowledge and insights from philosophy, mathematics, and cognitive science in a fruitful way. Unfortunately, this does not leave time or space for an elaborate discussion of concepts of particular philosophical or mathematical interest. It is my privilege – and responsibility – to define the concepts I use with adequate precision for the job at hand. In this case, this rather sketchy account of the meaning of 'meaning' will have to do. I will however, return to the concept of meaning and add to the account in the next chapter such representations are used as material anchors for the metaphors, similar to the way conceptual blends can have material anchors.

Although the use of material anchors for conceptual blends have been described by Hutchins (2005), the use of material anchors for conceptual mapping in mathematics is largely unnoticed in the literature. Furthermore, a better understanding of the use of such anchors might shed light on the different roles played by symbols, diagrams and figures in mathematical thinking. For these reasons, I will give a thorough analyses of several such non-linguistic expressions of conceptual mappings.

As a first example, we can continue to look at the conceptualizations of sets. In textbooks, sets are frequently conceptualized as Venn-diagrams. In Jacobson's *Basic Algebra*, we find such a diagram as part of the explanation of the distributive law, connecting the concepts of union $\cap$ and intersection $\cup$. Jacobson at first describes the law purely symbolically as

$$A \cap (B \cup C) = (A \cap B) \cup (A \cap C).$$

Then he gives a graphically representation of the law with the diagram reproduced as figure 6.14, and finally, he gives a purely symbolic proof of the law.

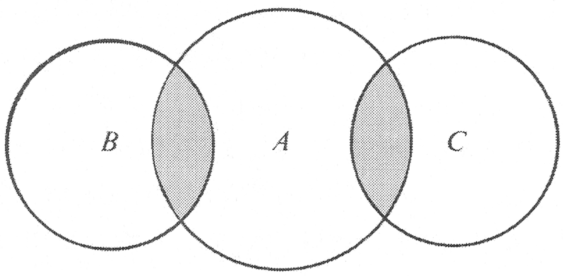



**Figure 6.14:** Venn-diagram illustrating the distributive law for set operations. The shaded areas corresponds to both $A \cap (B \cup C)$ and $(A \cap B) \cup (A \cap C)$, thus illustrating the identity. (Reprinted from Jacobson, 1985, p. 4).

In the diagram, the sets in question are depicted as bounded regions of space. Bounded regions of space are a special type of containers. Unlike some containers, bounded regions of space can overlap, but otherwise they are encompassed by the same basic logic as containers in general. As sets do not have any inherent spatial properties, the diagram is not a literal description of the sets. It is a description of what it would look like, if the sets were bounded regions in space. Consequently, the sets in question are metaphorically conceptualized as bounded regions in space, and the diagram is a representation of the sets conceptualized in this particular way (see table 6.3 for details).

| Sets Are Bounded Regions in Space | | |
|---|---|---|
| Source domain<br>Space | | Target domain<br>Sets |
| Bounded regions in space | → | Sets |
| Points in the region | → | Elements of the set |
| Points in overlapping regions | → | Elements shared by two or more set |

**Table 6.3:** Elements of the Sets Are Bounded Regions in Space metaphor.

The diagram is a material anchor for the metaphor; it allows the conceptual structure established via the metaphor to be mapped onto the material structure of the drawing on the paper. The components of the diagram serve as proxies for elements of the conceptual structure captured by the metaphor. Consequently (and connecting to the discussion about the role of figures in section 6.6 above), the diagram does not have topological iconic likeness with the sets represented as such, only with the sets conceptualized as bounded regions in space.

A diagram serving as material anchor for a metaphor, serves several purposes. Firstly, it simply conveys the particular metaphorical conception and thus ground our understanding of the represented mathematical content in sensory-motor experience. In figure 6.3, we do not need to explain that the sets are to be understood as bounded regions of space, it is immediately clear from the diagram. Secondly, the anchor provides a stable, external representation of the conceptual structure. One can alter parts of the representation, for instance by adding new bounded regions or, as in the case of figure 6.14, by shading some regions, while the rest of the structure remains stable. This stability of the representation allow us to perform more complex manipulations of the conceptual structure. Thirdly, manipulation and inspection of a diagram can be used to draw inferences and give us new knowledge about the objects represented (or rather: about the objects as they are conceptualized under the given metaphor). In the case of figure 6.3, the distributive law can be verified by simple inspection of the diagram, although knowledge of the distributive law was not used in its construction. This inference of the distributive law is relatively intuitive, as we (due to the underlying metaphorical conception of sets as containers) can recruit our experience with containers during the reasoning process[17].

---

It is a well-known fact that inferences drawn from the inspection of figures can lead to false conclusions for a number of reasons, such as lack of generality (in figure 6.8, only the case $n = 6$ is depicted, but how can we describe the general case with a figure), over-specificity (you cannot draw a triangle as such, but will have to draw either a right, an obtuse or an acute triangle) and other types of unintended exclusions (see Giaquinto, 2007, pp. 137 for a review). With the inspection material anchors for metaphors (i.e. diagrams) we must add *inadequacy of the metaphor* to the list of possible errors. Diagrams only have iconic likeness with the mathematical objects under a particular metaphorical conception, but metaphors can sometimes be misleading. In the case of Venn-diagrams for instance, the diagrams only offer a partial description of the properties of mathematical sets. Let me give two examples where the inadequacy of the metaphor might lead to false conclusions, when the diagrams are inspected.

Firstly, mathematics sets cannot only be subsets of other sets, they can also be elements of other sets. This distinction carries important mathematical weight, but when sets are conceptualized as bounded regions in space, the distinction is blurred. If a set $A$ is represented as a bounded region of space and encapsulated in another bounded region representing the set $B$, $A$ will seem to be a subset of $B$. $A$ can of course be represented as a member

[17]In contrast, the reader might try to verify the theorem using the formal proof given by Jacobson:

> [L]et $x \in A \cap (B \cup C)$. Since $x \in (B \cup C)$ either $x \in B$ or $x \in C$, and since $x \in A$ either $x \in (A \cap B)$ or $x \in (A \cap C)$. This shows that $A \cap (B \cup C) \subset (A \cap B) \cup (A \cap C)$. Now let $y \in (A \cap B) \cup (A \cap C)$ so either $y \in A \cap B$ or $y \in A \cap C$. In any case $y \in A$ and $y \in B$ or $y \in C$. Hence $y \in A \cap (B \cup C)$. Thus $(A \cap B) \cup (A \cap C) \subset A \cap (B \cup C)$. Hence we have both $A \cap (B \cup C) \subset (A \cap B) \cup (A \cap C)$ and $(A \cap B) \cup (A \cap C) \subset A \cap (B \cup C)$ and consequently we have [the distributive law].
>
> (Jacobson, 1985, p. 4)

This is not to say that verification of theorems using diagrams is superior to verification via formal proof. In contrast to diagrammatic proofs, formal proof can be characterized as precise and linear; in the formal proof the argument is broken down into a sequence of small steps that can be verified one at a time. In many instances this is an advantage over the use of diagrammatic proofs.

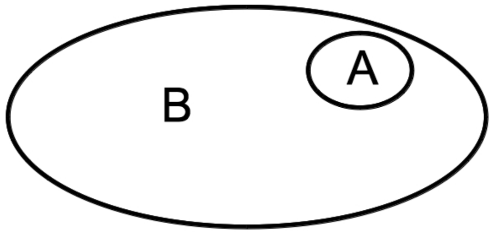



**Figure 6.15:** Is $A$ an element of $B$ or subset of set $B$?

of $B$ by other means, for instance by representing $A$ symbolically as a dot named '$A$' inside the bounded region of space making up $B$, but this would be to go beyond the original metaphor, and it would not make the metaphor in question capture this particular property of sets (see figure 6.15).

Secondly, the metaphor might lead to wrong inferences concerning the relative size of sets. In mathematics, two sets – and this includes sets with infinitely many elements – are considered to be of the same size, if there exists a bijection pairing the elements of the two sets. In the case of infinite sets, this definition of size contradicts our intuitive geometrical experience that the whole is greater than its parts. In figure 6.16, the natural numbers are geometrically represented as a true subset of the rational numbers. A natural and intuitive conclusion from this diagram would be, that the set of rationals is larger than the set of natural numbers. But given the definition of size currently accepted in mathematics, this is not the case. The natural and the rational numbers are sets of precisely the same size.

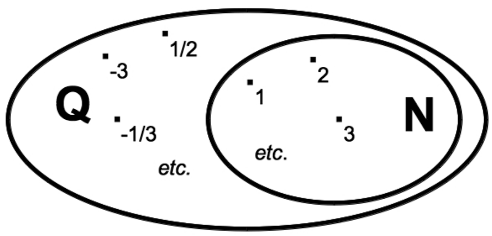



**Figure 6.16:** Diagram illustrating the set $\mathbb{N}$ of natural numbers as a subset of the set $\mathbb{Q}$ of rational numbers.

This underlines the fact that the conceptualization of sets as bounded regions in space is merely a metaphor and not a literal description. It is a well-known property of metaphorical descriptions that they are seldom complete. A metaphorical description might capture some, but not all aspects of a given phenomenon, and not all inferences of a metaphor might hold true. This is exactly why we use many different metaphors to conceptualize highly abstract phenomena such as ideas; each metaphor captures parts of the phenomena and leaves other parts in the dark.

This aspect of metaphor brings the importance of the choice of metaphor in focus. As each metaphor describes the phenomena in question differently, they might have different implications, and consequently the choice of metaphor used to conceptualize a phenomenon is not innocent. It shapes how we subconsciously understand and deal with the phenomenon, and it can determine the outcome of a debate concerning the properties of the phenomenon in question.

In the world of mathematics, this is especially important as the properties of a given object or phenomenon is subjected to – and perhaps even determined through – debate. The size of infinite sets is a case in point. As noted above, the container metaphor for sets leads conclusions considered to be false concerning the size of infinite sets. It would in fact be more adequate to say that a choice of metaphor has been made. As I see it, there is no objective truth about the size of infinite sets. The truth was constructed by the choice to apply the 'pigeon hole principle'[18] metaphorically to infinite sets. Had we chosen to accept the container metaphor as an adequate description of the size of infinite set, we would have reached another truth. It should be noted, that the metaphors themselves did not imply or cause the choice.

The choice was most likely motivated by several other factors, such as values residing in the mathematical community dictating the mathematicians to expand mathematics as much as possible. I will, however, not go further into this point here, only note that there is a limit to the power of metaphors; they cannot cause their own acceptance, but once they are accepted, they do exert a certain power on the way we conceive of things. Interestingly, in recent years mathematicians have successfully developed transfinite set theory based on the conception of size implied by the container metaphor (see Mancosu, 2009, for a discussion). This underlines the fact, that we have a real choice here. We can *chose* to develop transfinite set theory using either the one or the other metaphor to conceptualize the size of sets, but the choice will have real consequences. Different choices will lead to different truths.

[18] It states: 'If you can put exactly one letter in all of the pigeonholes, the number of pigeonholes and letters are the same.'

### 6.10.2 Mixed anchors

Physical anchors for metaphorical mappings can also be created by the use of symbols or a mix of symbols and other forms of representation. As an example, I will take a closer look at the use of commutative diagrams in set theory and algebra.

The visual representation of mappings in set theory is a good example of the use of mixed material anchors for conceptual metaphors. From a strictly formal point of view, a map consists of three sets, a domain $S$, a co-domain $T$ and a set $\alpha$ of ordered pairs $(s, t)$, with $s \in S$, $t \in T$ and such that:

1. for any $s \in S$ there exists a pair $(s, t) \in \alpha$ for some $t \in T$
2. if $(s, t) \in \alpha$ and $(s, q) \in \alpha$ then $t = q$.

So a map in short states a relation between all of the elements of one set and all or some of the elements of another sets.

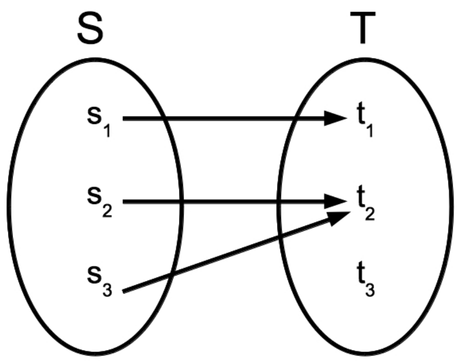



**Figure 6.17:** Diagram with directed paths illustrating the mapping of the set $S$ on a subset of the set $T$. From a formal point of view, the map consists of the domain $\{s_1, s_2, s_3\}$, the co-domain $\{t_1, t_2\}$, and of the following subset of the Cartesian product of the domain and co-domain $\{(s_1; t_1), (s_2; t_2), (s_3; t_2)\}$

.

A map can be represented by adding arrows to Venn-diagrams representing sets, as seen in figure 6.17. The arrow is a conventional sign representing directed movement. Here, the arrows do not only represent a direction (as when they are used on a street sign), they furthermore constitute the actual path between the two points one is to take on the paper. All in all, the diagram represents a layered metaphor, where sets are conceptualized as bounded regions in space, and mappings as directed movement along paths connection the elements of the sets (see table 6.4 for details.). These conceptualizations are deeply metaphorical; sets are not bounded regions in space, and although the concept of mapping has its roots in geometrical operations, the concept as defined in set theory is a purely static object consisting of three sets as described above (Jacobson, 1985, p. 6).

SETS ARE BOUNDED REGIONS IN SPACE WITH MAPS

| Source domain: Space | | Target domain: Sets |
|---|---|---|
| Bounded regions in space | → | Sets |
| Points in the region | → | Elements of the set |
| Points in overlapping regions | → | Elements shared by two or more set |
| A map | → | A set of directed paths connecting the points of two bounded regions |

**Table 6.4:** Elements of the Sets Are Bounded Regions in Space With Maps metaphor

In many cases, the precise relation between the elements of the sets is either inconsequential or too cumbersome to represent explicitly, and maps are simply represented as a movement along a directed path between sets represented as locations in space labeled with letters (this is called *external diagrams* in contrast to *internal diagrams* depiction relations between individual elements). So a map $\alpha$ form $A$ to $B$ can for instance be represented as $A \xrightarrow{\alpha} B$. This type of representation is particularly useful when maps between several sets are involved. In the simplest case, we might have three maps, say $\alpha$ form $A$ to $B$, $\beta$ form $B$ to $C$ and $\gamma$ form $A$ to $C$, represented by the following triangle:

$$\begin{array}{ccc} A & \xrightarrow{\alpha} & B \\ & {}_{\gamma}\searrow & \downarrow{\scriptstyle\beta} \\ & & C \end{array}$$

Interestingly, the metaphor is not adequate in this case. In the diagram, a movement along the composition of $\alpha$ and $\beta$ brings me to the same location on the paper as a movement along $\gamma$. However, maps are not relations between sets, but relations between the elements of sets, and the composition of $\alpha$ and $\beta$ is only equal to $\gamma$ if it takes me to the same element in $C$ for any elements in $A$. If this is the case, the diagram is said to commute.

The diagram in figure 6.18 is an example of the use of such a commutative diagram in a mathematical textbook. The precise mathematical content and relevance of the diagram is not so important here. It will suffice to know, that the diagram illustrates the relation between two sets, their sum and the set 2.

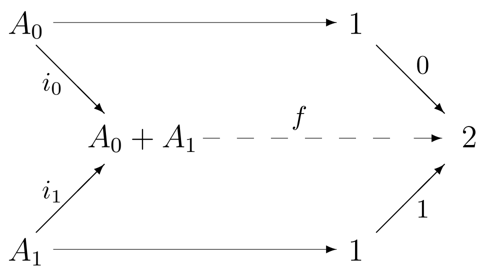



**Figure 6.18:** Diagram illustrating the relation between the sets $A_0, A_1$, their sum set $A_0 + A_1$ and the set $2 = \{0, 1\}$ (redrawn from Lawvere & Rosebrugh, 2003, p. 31)

.

Diagrams of this type are not only used in set theory. Especially commutative diagrams are used in virtually all parts of abstract algebra and category theory to express different types of mappings between different types of mathematical objects. Often the truth of a theorem can be converted to a question of whether a certain diagram commutes.

In group theory, an interesting variant is used[19]. Here, mathematical objects are not only represented as objects on the paper, the location of the

[19]For readers not acquainted with group theory, a group is a set $G$ with a composition $*$ mapping $G \times G$ to $G$ and satisfying the following three conditions:

1. the composition is associative, i.e. for all $x, y, z \in G : (x * y) * z = x * (y * z)$,
2. the group contains a neutral element $e$ satisfying $e * x = x * e = x$ for all $x \in G$, and
3. for any element $x \in G$ the group must contain an inverse element $x^{-1}$ satisfying $x^{-1} * x = x * x^{-1} = e$.

Given two groups $G$ and $G'$, a *homomorphism* is defined as a map $\varphi : G \mapsto G'$ satisfying the condition that $\varphi(x * y) = \varphi(x) * \varphi(y)$ for all $x, y \in G$. An *isomorphism* is a bijective homomorphism objects on the paper is also used to express inclusion: A subgroup $H$ of a group $G$ is placed below $G$.

As an example, we can look at the two diagrams in figure 6.19. The diagrams are taken from a textbook on algebra, where they are used as a way to illustrate Noether's two isomorphism theorems (Thorup, 1998, p. 96)[20]. The maps in question (homomorphisms) are represented with arrows as above, but trivial inclusions are represented with strokes only.

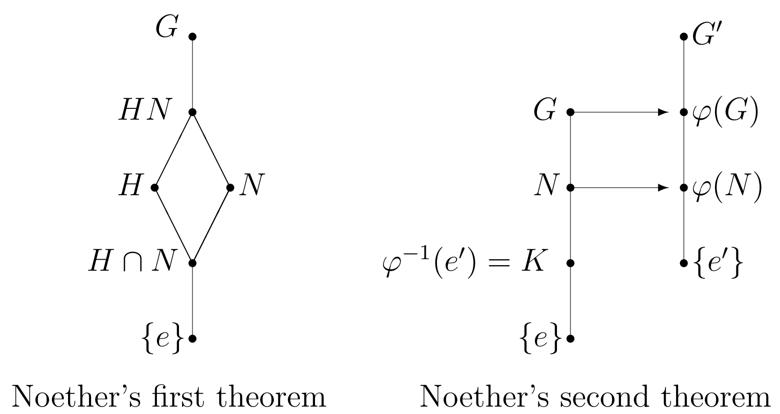



**Figure 6.19:** Diagrams illustrating Noether's two isomorphism theorems (redrawn from Thorup, 1998, p.96)

.

What is represented here, is in fact a very complex conceptual structure,

[20]The precise content of the theorems is not essential for the understanding of this example, and I will not explain it in detail. For those interested, the theorems state the following (see Thorup, 1998, p. 94-95):

Noether's first isomorphism theorem: *Let H and N be subgroup of G, where N is normal in G. Then the subset $HN = \{hn | h \in Handn \in N\}$ is a subgroup of G. Furthermore N is normal in HN and $H \cap N$ is normal in H and there exists a natural isomorphism,*

$$H/(H \cap N) \xrightarrow{\sim} HN/N.$$

Noether's second isomorphism theorem: *Let $\varphi : G \to G'$ be a homomorphism, and let K be the kernel for $\varphi$. The we have: $H \mapsto H'$ defines a bijective map from the set of subgroups H of G which includes K, on the set of all subgroups of $\varphi(G)$. The inverse map $L \mapsto \varphi^{-1}(L)$, for subgroups L of $\varphi(G)$.*

*Under this bijection, a subgroup N of G with $N \supseteq K$ is normal in G if and only if $\varphi(N)$ is normal in $\varphi(G)$. Furthermore, we have that if N is normal in G and $N \supseteq K$ then there exists a natural isomorphism,*

$$G/N \xrightarrow{\sim} \varphi(G)/\varphi(N)$$

where the original metaphor conceptualizing sets (or here groups) as physical objects in space, is used opportunistically to conceptualize inclusion as relative physical location. This type of diagrams resemble technical drawings, where, say, an engine is drawn as dismantled into its constituent parts, and the parts are spread out in a sequence reflecting the sequence of assembly (such as figure 6.20).

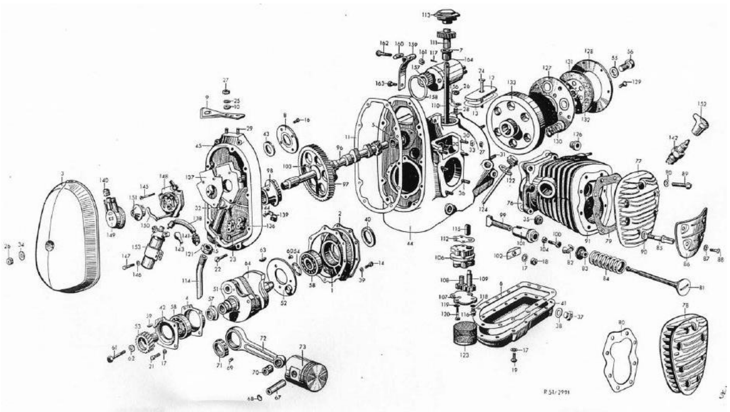


**Figure 6.20:** A BMW R-71 engine. Technical drawing from the factory manual. From:
www.fallschirmjager.net/Vehicles/Motorcycles/BMWTechnical/Drawings.html

This adds to the original metaphor. Groups are not only conceptualized as physical objects, they are conceptualized as objects consisting of parts. Groups can be disassembled, and their parts can be spread out in space in a sequence resembling the way the parts fits together inside the original group. Furthermore, under a given map, parts of one group might have relations with analog parts of other groups.

---

As noted above, material anchors for conceptual metaphors serve several purposes: 1) they ground our understanding of mathematical content in sensory-motor experience by conveying a metaphorical conception of the objects and relations in question, 2) they serve as a stable, external representation of the metaphorical conceptualization and 3) they can be used to infer new knowledge about the objects represented. In the examples above, a mix of mathematical and other symbols are used to create a material anchor for a metaphors, conceptualizing sets and groups as physical objects located in space and maps as trajectories connecting these objects.

These anchors are created by the physical layout and composition of the actual symbols on the paper. This adds to the list of things we can do with abstract symbols; by arranging the symbols on the paper, we can create material anchors for conceptual metaphors and blend. In doing so, the symbols are used both as physical objects and as semantic objects – the symbol '+' for instance has a precisely defined meaning in the sign '$A + B$' in figure 6.18, and the arrows of the diagram are defines as denoting group homomorphisms or maps between sets (relative to content). So the symbols, in other words have a double life here; as semantic objects they represent mathematical objects and operations, and as physical objects they are used to represent elements of a metaphorical conception of these objects and operations.

## 6.11 The use of conceptual blends in mathematical thinking

The number line is a simple example of a conceptual blend integrating mathematical objects and sensory-motor experience. In the blend, numbers are metaphorically conceived as locations in space. This metaphorically constructed domain, is then blended with the geometrical domain of lines to create an entirely new domain, where numbers are conceived as locations on a line.

In the blend, the arithmetic property of 'being greater than' is associated with the geometrical property of relative location, and the arithmetic property of difference is associated with the geometric property of directed distance. A particular number, zero, is associated with a particular location, the origin $O$, and the arithmetic difference of $+1$ is associated with a particular directed distance, the unit. This creates a new conceptual domain, where numbers are conceptualized as locations in directed distance form a particular point $O$. In the blend, structure from both input-domains are integrated, and numbers are simultaneously conceptualized as geometrical points on a line and as mathematical objects obeying the laws of arithmetic. This is why the blend is a *blend*, and not merely a metaphor; numbers are not simply (metaphorically) conceptualized as locations on a line, they are done so in a way that integrates arithmetic properties with properties of space (see table 6.5 for further details, see also Lakoff & Núñez 2000, pp. 278).

| The Number Line blend | | |
|---|---|---|
| Domain 1<br>Numbers as points in space | | Domain 2<br>A directed line |
| A number | ↔ | A point on the line |
| 0 | ↔ | A point $O$ (the origin) |
| 1 | ↔ | The location one unit distance to the right of the origin |
| $a < b$ | ↔ | Point $a$ is located to the left of point $b$ |
| $a = b$ | ↔ | Point $a$ is in the same location as point $b$ |
| Absolute value of $a$ | ↔ | Distance from point $a$ to point $O$ |

**Table 6.5:** Elements of The Number Line blend

The use of conceptual blending is widespread in mathematics, and several such blends have been analyzed and discussed in the literature (see for instance Lakoff & Núñez, 2000; Fauconnier & Turner, 1998, 2003; Robert, 1998). However, most of this work concentrates on the integration of different mathematical domains. As an addition to this work (and in line with the foregoing section), I will focus on the integration of physical and mathematical domains and on the use of material anchors for such mappings.

When drawn in the familiar way, the number line is in fact an excellent example of such an anchor, made up by a mixture of mathematical symbols and geometrical elements. In the following subsection, I will contribute with one more example by giving an in-depth analysis of the various material anchors used for a conceptual blend in the proof of a particular result.

### 6.11.1 Anchoring the countability of the rational numbers

The countability of the rational numbers was proved by George Cantor in 1874. His second proof of the theorem, published in 1895, is the one most widely known and used, so I will only discuss that proof here.

The basic idea of the proof is to establish an algorithm that makes it possible to go through all of the rational numbers one by one. If one can go through the numbers one by one, they must be countable (or more precisely: the algorithm establishes a one-to-one connection between the rational and the natural numbers).

In most presentations of the proof, numeral symbols representing a section of the rational numbers are arranged in a particular pattern in space, and the algorithm is described as a way physically to move through this pattern.

In the presentation of the proof in (Kline, 1990, p. 996) for instance, ten rational numbers are arranged in a triangular array with rows of ascending denominator and columns of ascending nominator (starting with 1/1 in the top left corner) (see figure 6.21). One can imagine that all of the rationals will be included in the array, if the pattern is continued infinitely. We are told, that the sum of the nominator and denominator are constant for all the numbers in any diagonal.

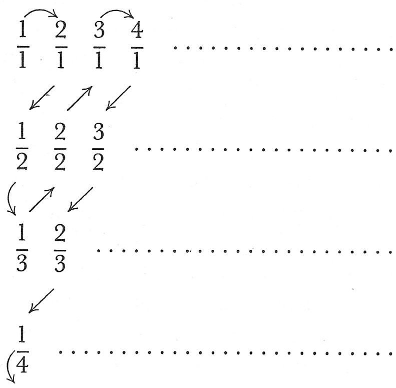


**Figure 6.21:** Diagram proving the countability of the rational numbers (reprinted Kline, 1990, p. 996).

The central algorithm is described by pointing out a particular way through this array of numbers. This is done by the addition of arrows to the array of numbers, and by the following explanatory text: "Now one starts with 1/1 and follows the arrows assigning the number 1 to 1/1, 2 to 2/1, 3 to 1/2, 4 to 1/3, and so on. Every rational number will be reached at some stage and to each one a finite integer will be assigned" (Kline, 1990, p. 996).

The proof as given here is vastly dependent on cognitive mapping. Two metaphors are at play; firstly the rationals are conceptualized as objects located in space (in an array), and secondly, the idea that we can include all of the rational numbers by completing the array is a clear use of the Basic Metaphor of Infinity, which I will not go further into here (see Lakoff & Núñez, 2000, pp. 158 for a description of this metaphor).

This creates a metaphorical domain, where the totality of the rationals is conceptualized as an infinite array of physical objects. In the proof, this metaphorical domain is integrated with a domain of directed movement in physical space to create an abstract domain, where one can actually move through all of the rationals one by one. This is not merely a metaphor, but a blend, where structure from both domains are integrated to create a completely new domain; the rationals are at once conceptualized as numbers (arranged by the value of the nominator and denominator) and as discrete locations on an path of directed movement. So the composition in this case is the placement of the number-objects on a particular path of directed movement giving each number-objects a location on the path. The completion consists in the infinite extension of the array of number-objects and the path going through them. By running the blend, we can infer that all of rational numbers can be reached one by one by following this path, and hence that they must be countable.

The accompanying diagram serves as a material anchor for the conceptual blend. Physical symbols representing the rationals are arranged on the paper forming part of the imagined array of the blend, and arrow symbols are used to indicate the path one is to take through the number-locations. So the material anchor is created by arranging mathematical and other symbols in space, and – once more –, mathematical symbols are at once used as physical objects with a location in space and as semantic objects with a particular meaning. Furthermore, as in the previous examples, the material anchor serves both as a stable, external representation of a complex conceptual domain, and grounds our understanding of the mathematical content represented in physical experience. In this case, the existence of a bijective map between two infinite sets of numbers is inferred by appealing to our knowledge of moving along paths traversing objects (see figure 6.22).

It is debatable whether the conceptual blend is essential for the proof. In Cantors original poof (Cantor, 1895, pp. 492), we find no array of rationals and no arrows indicating a paths of movement through them. Instead, Cantor represents the (positive) rationals as a sequence of ordered pairs $(\mu, \nu)$ of natural numbers. The pairs are ordered in accordance with two principles, 1) after the sum $\rho = \mu + \nu$ of the elements and 2) pairs with same sum are ordered so the first elements of the pairs form an increasing sequence:

$$(1, \rho - 1), (2, \rho - 2), \ldots, (\rho - 1, 1)$$

Cantor continues:

> ... so erhält man sämmtlicher Elemente $(\mu, \nu)$ in einfacher Reihenform:
>
> $$(1, 1);\ (1, 2), (2; 1);\ (1, 3), (2, 2), (3, 1);\ (1, 4), (2, 3), \ldots,$$

> und zwar kommt hier, wie man leicht sieht, das Element $(\mu, \nu)$ an de $\lambda^{st}$ Stelle, wo
> $$\lambda = \mu + \frac{(\mu + \nu - 1)(\mu + \nu - 2)}{2}$$
> (Cantor, 1895, p. 494)

And, Cantor concludes, as $\lambda$ takes on every integer value precisely once, it constitutes a bijective map between the natural and the (positive) rational numbers.

Although less spectacular, the sequence of pairs printed by Cantor is still a material anchor for a cognitive blend. The rationals are conceptualized as objects in space and a path traversing the totality of these objects is imagined. This metaphorical blend is materialized on the paper by a particular arrangement of the physical symbols used to represent the numbers in question. Only Cantor has arranged the numbers as a one dimensional sequence,

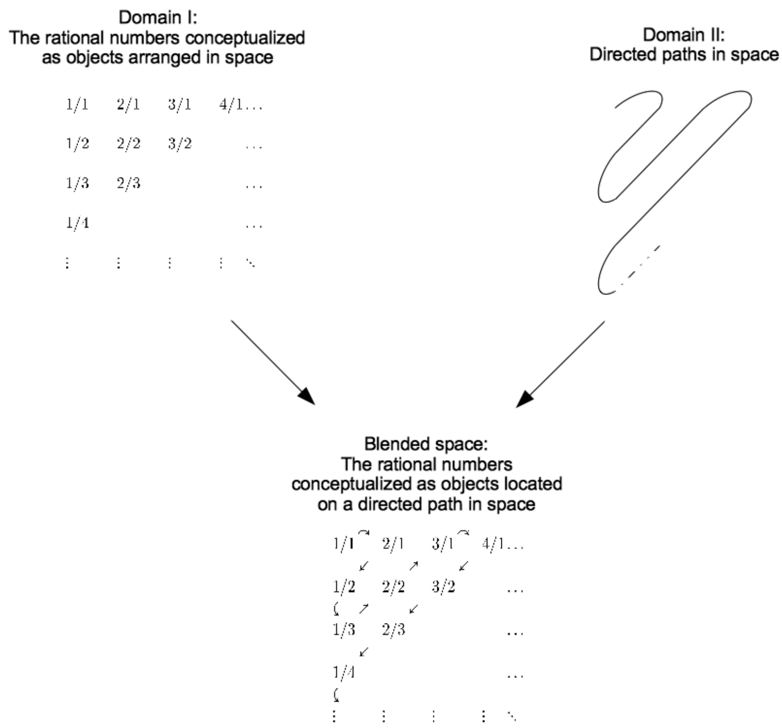



**Figure 6.22:** Two spaces are integrated to create a blended space, where the rational numbers are conceptualized as locations on a directed path.

instead of spreading them out in a two dimensional array. This makes the path traversing the numbers obvious, and eliminates the need to point it out using arrows or other visual cues.

Notice also, how Kline's version of Cantor's proof is actually wrong; Kline's path through the rationals gets the numbers in every other diagonal in the wrong order. If Kline were to follow Cantor's order, his path should have looked like this:

$$
\begin{array}{ccccccccc}
1/1 & & 2/1 & & 3/1 & & 4/1\ldots \\
 & \nearrow & & \nearrow & & \nearrow & \\
1/2 & & 2/2 & & 3/2 & & \ldots \\
 & \nearrow & & \nearrow & & & \\
1/3 & & 2/3 & & & & \ldots \\
 & \nearrow & & & & & \\
1/4 & & & & & & \ldots \\
\vdots & & \vdots & & \vdots & \vdots & \ddots
\end{array}
$$

Kline however, clearly – and wisely – has chosen cognitive over formal accuracy, by choosing a *connected* path going through the rationals in a different order than Cantor's. In other versions of the proof, different layouts of the rationals are chosen and a variety of paths are used (for one more example, see figure 6.23). They are not formally equal to Cantor's proof, but from a cognitive point of view, they all share the same idea of creating a cognitive blend, where the rationals are conceptualized as locations spread out in space, and of showing that there exists a connected path taking you through all of these number-locations one by one. And of course, they all use mathematical symbols (sometimes combined with arrows etc.) to create a physical anchor for the cognitive blend.

Despite their presence in most versions of the proof, the blend and the physical representation of it are in fact not essential. From a formal point of view, all you need is the $\lambda$-function

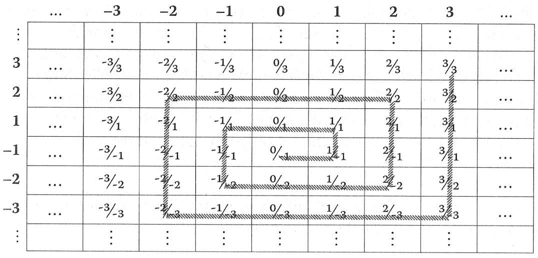
$$\lambda = \mu + \frac{(\mu + \nu - 1)(\mu + \nu - 2)}{2}$$

creating a bijection between the sets $\mathbb{N}$ and $\mathbb{Q}$. Using only the $\lambda$-function might not completely clean the proof for all cognitive mapping, but it will at least remove the cognitive blend under discussion.

Consequently, the cognitive blend and the material anchor for it are not strictly necessary for the proof. We could choose to present the proof purely formally by use of the $\lambda$-function, but we rarely do so. In fact, I have never

| | ... | −3 | −2 | −1 | 0 | 1 | 2 | 3 | ... |
|---|---|---|---|---|---|---|---|---|---|
| ⋮ | | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | |
| 3 | ... | -3/3 | -2/3 | -1/3 | 0/3 | 1/3 | 2/3 | 3/3 | ... |
| 2 | ... | -3/2 | -2/2 | -1/2 | 0/2 | 1/2 | 2/2 | 3/2 | ... |
| 1 | ... | -3/1 | -2/1 | -1/1 | 0/1 | 1/1 | 2/1 | 3/1 | ... |
| −1 | ... | -3/-1 | -2/-1 | -1/-1 | 0/-1 | 1/-1 | 2/-1 | 3/-1 | ... |
| −2 | ... | -3/-2 | -2/-2 | -1/-2 | 0/-2 | 1/-2 | 2/-2 | 3/-2 | ... |
| −3 | ... | -3/-3 | -2/-3 | -1/-3 | 0/-3 | 1/-3 | 2/-3 | 3/-3 | ... |
| ⋮ | | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | |

(a) Physical layout of the rationals



(b) Connected path through the rationals

**Figure 6.23:** Alternative layout of and path through the rationals. (Reprinted from Friend, 2007, p. 18 and 19).

seen the λ-function mentioned anywhere except in Cantor's original proof – the proof seems always to be explained by presenting a material anchor for the blend. So in this case, the anchor and the blend are used as heuristic tools that make the content of the proof intuitively and effortlessly graspable by giving it a concrete physical form (in the material anchor) and relating it to concrete bodily experience (via the blend).

## 6.12 Lakoff and Núñez' radical theory of impact

So far, the conceptual metaphors I have analyzed seem to have a limited impact on the content of mathematics; apparently, the main role of the metaphors is to ground the meaning of mathematical content in sensory-motor experience, which in turn allows for easier and more intuitive reasoning. For the most parts, the metaphors are not indispensable. The reasoning could just as well have been performed purely formally without the use of metaphors, so the metaphors and blends simply seems to be neutral cognitive tools.

In the following sections, I will discuss whether this picture of metaphors as merely heuristic tools is adequate, or whether the use of metaphors might have a more significant impact on the content of mathematics. I will open the discussion by presenting and evaluating the radical theory of impact proposed by George Lakoff and Rafael Núñez in their groundbreaking (and highly controversial) main work *Where Mathematics Comes From.* Here, Lakoff and Núñez explains how our innate arithmetic, covering only the arithmetic properties of collections containing at most four objects, are expanded firstly to full blown arithmetic and ultimately to all of mathematics in a process, driven by the use of conceptual metaphors and blends.

In this presentation of Lakoff and Núñez' work, I will mainly focus on their description of how innate arithmetic is expanded into general arithmetic. The first step in this process is, according to Lakoff and Núñez, the creation of four metaphors, grounding our innate arithmetic skills in everyday sensory-motor experiences. These metaphors are called the four grounding metaphors (4Gs) and with them, innate arithmetic is respectively conceived as (Lakoff & Núñez, 2000, pp. 54):

- object collection
- objects construction
- motion along a path and
- the use of a measuring stick.

The 4Gs offer natural conceptualization of all basic elements of innate arithmetic, *viz.* cardinality, order (greater than, smaller then) and the operations plus and minus. Here, I will only describe the exact content of the Arithmetic is Object Collection metaphor (table 6.6). The reader is referred to (Lakoff & Núñez, 2000) for exact descriptions of the other three Gs.

The conceptualizations of arithmetic offered by the 4Gs are visible in the expressions we use, when we communicate arithmetic facts. Taking the 4Gs one by one, arithmetic is conceived as *object collecting* when we say for instance: "If you add three and four, you get seven" or "If you put two and two together, you get four". *Object construction* is visible in the expressions: "Five is made up of two and three," and "If you take three from seven, how much do you have left?". Arithmetic is conceptualized as *motion along a path* when we say: "The result is around forty," and "4.9 is near 5" (examples from Lakoff & Núñez, 2000, p. 54–74. No examples of the Measuring Stick metaphor is given).

| Arithmetic Is Object Collection | | |
|---|---|---|
| Source-domain: Object Collection | | Target-domain: Innate arithmetic |
| A collection of objects | → | A number |
| The size of the collection | → | The size of the number |
| A bigger collection | → | A greater number |
| The smallest collection | → | The unit (one) |
| Putting collections together | → | Addition |
| Taking a smaller collection from a larger collection | → | Subtraction |

**Table 6.6:** Elements of the Arithmetic Is Object Collection metaphor. Reproduced with small adjustments from (Lakoff & Núñez, 2000, p. 55).

In the limited domain of our innate arithmetic, the structure of all of the 4Gs are isomorphic and corresponds to the structure of our inborn arithmetic. So for instance, according to our innate arithmetic, we expect two plus two to be four. This corresponds precisely to the structure of the source-domains of all of the 4Gs: A collection of two objects added to a collection of two objects results in a collection of four objects, taking two steps down a road followed by two more steps in the same direction leaves you in the same place as taking four steps down the road *etc.*

Once the analogies with the source-domains of the 4Gs are established, the metaphors are used to expand arithmetic beyond its original limits. This is the next step in the creation of general arithmetic. As an example, we know from basic experience that adding a collection of objects to another collection of objects always results in a collection of objects. When this structure is projected onto the domain of arithmetic, we can infer that the addition of two numbers must always result in a number, and consequently $\mathbb{N}$ must be closed under addition. This forces us to expand the domain of arithmetic from the four element limits of our innate abilities, to the unlimited natural numbers.

Similarly, all basic laws of arithmetic, such as the commutativity and associativity of addition, are derived by projecting the inferential structure of the source-domains of the 4Gs to the domain of arithmetic. To take an example, we know from experience that the order is insignificant, when you pile collections of objects; you will end up with three objects no matter whether you add one object to a collection of two objects, or the two objects to the one. When this structure is transferred to arithmetic, you get the law of commutativity for addition ($a+b=b+a$). Similar results can be obtained by projecting structure from the other 3G's.

The projection of structure from the source-domains of the 4Gs also allows us to define new operations. Most importantly, collections of the same size can be pooled into new collections; $A$ collections of size $B$ can be pooled into a new collection of size $C$. When this structure is projected onto the domain of arithmetic, it entails a new operation, multiplication: $A \cdot B = C$. Similarly, the inverse operation, division, is entailed by projection the reverse operation of splitting a collection into a number of equally sized subcollections.

As the final step in the creation of general arithmetic, a number of new mathematical objects such as zero, fractions, and negative and complex numbers are created through projections from basic experience of one or more of the 4Gs (and some times with the addition of other metaphors or conceptual blends). I will discuss the creation of complex numbers at length below. Of the other entities, *zero* is the natural consequence of both the Arithmetic Is Object Collection and the Arithmetic Is Motion Along A Path metaphors; in the first, zero is naturally conceptualized as lack of objects to form a collection (Lakoff & Núñez, 2000, p. 64), and in the second, zero is naturally conceived as the origin point. The conceptual metaphors are in other words entity-creating; they create zero as an actual number.

The *negative numbers* are entailed by a natural extension of the Arithmetic Is Motion Along A Path metaphor; if positive numbers are conceptualized as the points lying to the one side of the origin, negative numbers are simply the points lying to the other side (Lakoff & Núñez, 2000, p. 72). As a consequence of the measuring stick-metaphor, given a unit length, any physical segment is conceived as a number. This conceptual blend leads to the expansion of arithmetic with both the rationals (fractions) and the irrationals (Lakoff & Núñez, 2000, p. 70–71).

All in all, according to Lakoff and Núñez, general arithmetic is created in a process, starting with the experience of a correspondence between our innate arithmetic and four different types of life-world experiences (the source-domains of the 4Gs). This leads to the formation of a strong analogy between innate arithmetic and these four different types of experiences. Once this analogy is established, elements of innate arithmetic are conceptualized metaphorically using elements from the source domains of the 4Gs. Subsequently, structure not in the original analogy is projected from the source-domains of the 4Gs onto the domain of innate arithmetic. This expands

Extensions of innate arithmetic entailed by the Arithmetic Is Object Collection-metaphor

| Source-domain: Object Collection | | Target-domain: Innate arithmetic |
|---|---|---|
| Adding collections of objects always result in a new collection of objects | → | $\mathbb{N}$ is closed under addition |
| Adding a collection to another collection, and then removing it again leaves you with the original collection | → | $(a+b)-a=b$ (addition and subtraction are inverse operations) |
| The order does not matter when you pool collections (you get the same resulting collection whether you add collection $A$ to collection $B$ or $B$ to $A$) | → | Addition is commutative ($a+b=b+a$) |
| Pooling of $A$ collections of size $B$ into a new collection of size $C$ | → | $a \cdot b = c$ |
| Splitting a collection of size $C$ into $B$ collections of size $A$ | → | $c/a = b$ |
| The lack of objects to form a collection | → | 0 |

**Table 6.7:** Examples of extensions of innate arithmetic entailed by the Arithmetic Is Object Collection metaphor. The same extensions can be made for all or some of the other 4Gs.

innate arithmetic adding both new mathematical objects and new operations.

Once fill-blown arithmetic is created, it is, according to Lakoff and Núñez, expanded even further using a host of other conceptual metaphors, so-called 'linking metaphors' (linking different mathematical domains to each other) and conceptual blends (I will not describe the details of this process here, but in section 6.12.1.2.3 I will give an example by presenting Lakoff and Núñez' description of the creation of complex numbers). In the end, the process lined our above leads to the creation of large and important parts of mathematics, such as the real and complex number systems, the concept of infinity, transfinite numbers, limits, and specific theorems, such as Euler's formula $e^{\pi i} + 1 = 0$. If Lakoff and Núñez are right, conceptual metaphors plays a vital part and is the driving force in the development of most of the mathematics known to us today.

---

The picture of mathematics presented by Lakoff and Núñez can be broken down to two different claims. First of all, they clearly make a cognitive claim: The 4Gs are cognitively active metaphors that constantly shape the way we understand arithmetic. Secondly, Lakoff and Núñez also seem to make a genealogical claim about the genesis and expansion of arithmetic, although it is at times difficult to see, whether they are making a historical claim about the actual development of mathematics, or more of a psychological claim about how mathematics (under a particular rational reconstruction) can be made meaningful. So for instance, when they describe how zero is 'created' as a number by a conceptual metaphor, do they refer to actual historical events that led to the invention and acceptance of zero? Or do they only mean to describe how each of us learns to recognize zero meaningfully as being a number? As I see it, Lakoff and Núñez are not completely clear about this point, and statements supporting both positions can be found in (Lakoff & Núñez, 2000). For brevity, I will not discuss Lakoff and Núñez' exact position on this matter any further, but simply discuss the evidence given for the genealogical hypothesis, as that hypothesis makes the strongest claim concerning the impact of conceptual metaphors, and hence is the most interesting from the point of view of the current investigation.

It should also be noted that genealogical and cognitive claims are logically independent. Metaphors and cognitive mechanisms at one point involved in the development of a theory or conceptual system, do not necessarily need to stay active, once the theory is developed. This was for instance the case with the Saturnian system metaphor, which was an active and cognitively important metaphor in the development of the Bohr-Rutherford model of the atom, but quickly played out its role and became a dead metaphor, only useful for pedagogical purposes (Knudsen, 1999, pp. 106). Conversely, metaphors that are active and play a vital part in the modern understanding of a theory need not to have been actively involved in the development of this theory. As I will argue below, this is clearly the case with the metaphors used in the modern conceptualization of complex numbers (subsection 6.12.1.2.3). For this reason, the two different claims must be justified independently.

### 6.12.1 The genealogical claim

Let me begin by spelling out the exact content and radicality of the claim. The claim is in its essence the statement of a particular kind of constructivism: Full-blown arithmetic is constructed by us primarily by projecting the inferential structure of four domains of embodied experiences onto the rudimentary inborn arithmetic. The rest of mathematics is constructed using similar conceptual metaphors or linking metaphors, building new content on top of already created parts of mathematics. This naturally places conceptual metaphor in a central position in the development of mathematics, and ascribes an enormous impact on the content of mathematics to our use of such metaphors. It might in fact be wrong to speak of an impact here. Mathematics is not just influenced by our use of conceptual metaphor; it is simply a product of conceptual metaphors mapping real world experiences onto the abstract domain of mathematics.

This constructivist stance should not be confused with the idea that mathematics is hardwired into our brain or otherwise determined by our biology. Firstly, as Núñez has pointed out in subsequent work (Núñez, 2009), the constructivist theory regards the rudimentary mathematics, we are born having, as being qualitatively insufficient for the creation of full-blown mathematics. Our innate skills simply cannot be scaled up to provide the richness and precision of the natural number system or general arithmetic:

> Explaining the origin of numbers and arithmetic requires an explanation that gives an account of their precision and the highly developed range extension, as well as the specificity and precision of their combinatorial power. Mere training at improving numerosity judgments, whether it is at the level of the individual or the neuron, doesn't provide the answer to the question on the nature of number systems and arithmetic.
>
> (Núñez, 2009, p. 72).

Something else is needed, and this is, according to the strong genealogical claim, the cognitive mechanisms provided by conceptual mapping. So, mathematics is not hardwired into our brain in the form of a pre-given 'number module' or similar, according to Lakoff and Núñez . Rather, it is constructed in an active process, where a small core of hardwired mathematics is articulated and extended using the qualitatively different cognitive mechanism of conceptual mapping.

Secondly, although the conceptual metaphors used in the construction of mathematics to some extend depend on our brain and in general biology, they are not, according to Lakoff and Núñez, *determined* by it. There is room for contingency, as culture-specific ideas (this could be ideas from science, philosophy, religion *etc.*) can be brought into mathematics and shape its development in different ways:

> There is a sense in which mathematics in not culture-dependent and another in which it *is* culture-dependent.

> - Mathematics is independent of culture in the following very important sense: Once mathematical ideas are established in a worldwide mathematical community, their consequences are the same for everyone regardless of culture. (However, their establishment in a worldwide community in the first place may very well be a matter of culture.)
> - Mathematics is culture-dependent in another very important sense [...] Historically important, culturally specific ideas from outside mathematics often find their way into the very fabric of mathematics itself. Culturally specific ideas can permanently change the actual content of mathematics forever.
>
> (Lakoff & Núñez, 2000, p. 356)

As examples of such cultural-specific ideas that have made their way into and have shaped the development of mathematics, Lakoff and Núñez mention the idea that any theoretical building (including mathematics) must have a foundation, and the idea that mathematical reasoning must be a version of logic.

In subsequent work, Núñez allow for an even stronger influence from culture. A study of the Aymara language of the Andes' highlands shows that the speakers of this language, conceptualize time in a way that is inconsistent with our standard conceptualization of the phenomena. In both standard English and Aymara, time events can be conceptualized as locations in unidirectional space – this is for instance, what we do, when we say: "I'm looking forward to the Holidays" or "I'm glad the winter is well behind us now". As indicated by the examples, English speakers conceptualize the future as being ahead of us, and the past as being behind us. The Aymara speakers however, conceptualize the future as behind them and the past as in front of them. So both language groups use the same conceptual metaphor Time Events Are Locations In Unidirectional Space, and both consistently use the inferential structure of the source-domain to describe time-events, but the two groups project the inferential structure onto the target domain in completely different ways, and that leads to two mutually inconsistent conceptualizations; what is true for an English speaker (e.g. "The winter is behind us"), will be false for an Aymara speaker. Consequently, there is no single, transcendental truth about such imaginary structures. The truth will always be relative to the particular metaphorical mapping used to structure the abstract domain in question (Núñez, 2009).

So although our biology to some extend determine which bodily experiences are available to us, it does not determine how we use them. This leaves a lot of latitude for cultural and other ideas to put their mark on any particular abstract conceptualization.

---

Furthermore, although the source-domains of the constituting metaphors are experiences of the physical world, the claims made by Lakoff and Núñez should not be mistaken for a empiricistic claim (such as the one made by John Stuart Mill, that mathematical theorems are inductive generalizations based on direct observation (Mill, 1973)). Mathematics is not in a direct way 'out there' as an objective parts of the external world. There are several reasons for this.

Firstly, what Lakoff and Núñez claim is only that the structure of local experiences is projected onto the domain of mathematic. The difference between these two claims is very visible in the example given above on the closure of $\mathbb{N}$. In Lakoff and Núñez' view, the closure of $\mathbb{N}$ is a consequence of the projection of the *local* observation that we can always add more objects to a collection. It does not matter that we cannot in fact make a pile containing infinitely many objects, due to the finite nature of the Universe. What matters is the structure we experience in our every-day handling of objects, and exhausting the Universe is not part of this experience. In a strict empiricist theory on the other hand, mathematical theorems must be based on actual observations, and consequently infinitistic mathematics cannot be accounted for – and that is one of the major points of criticism raised against classical empiricist theories of mathematics. Lakoff and Núñez however, avoids this type of criticism by focusing on locally observed structure.

Secondly, the experiences used to build mathematics are not neutral or objective observations, as is demanded in an empiristic theory. Rather, they are the results of interactions between a particular kind of being, having a particular bodily and morphological structure, and its environment. This makes the experiences used to create mathematics specifically human and depending on our special way of being in the world. Other intelligent creatures trying to create mathematics might not have the same kind of experiences available to them (unless of course, their morphology and habitat are similar to ours in relevant aspects).

---

This is the main elements of Lakoff and Núñez' strong genealogical claim: Mathematics is constructed by projection life-world experience onto rudimentary inborn mathematical skills. Mathematics is 'neither hardwired nor our there' (as Núñez expresses it (2009)); it is not hardwired, because our innate mathematical abilities are qualitatively too weak to generate full-blown mathematics. It is not out there, because it is not based on inductive generalizations of neutral observations. Mathematics is a particularly human construct, dependent both on our biology and interactions with our environment, and on cultural-dependent ideas and interpretation of our basic life-world experiences.

In the discussion of this claim it will be useful to make a further subdivision between 1) the original genesis of basic arithmetic and 2) the expansion of arithmetic (with new objects and operations).

#### 6.12.1.1 Genesis of arithmetic

Lakoff and Núñez' description of the genesis of basic arithmetic is in many ways attractive and certainly adds to classical empiristic theories. As I see it, however, the main problem concerning the description is the question whether the cognitive mechanisms involved are in fact conceptual metaphors, as claimed by Lakoff and Núñez. A conceptual metaphor is a mapping between two distinct domains, but it is in my view questionable whether innate arithmetic can be counted as an independent conceptual domain. As we saw in chapter 4, innate arithmetic is nothing but the ability to pay attention to the numerical aspects of experience, perhaps added some innate expectations concerning the numerical behavior of small number of objects. By representing small number of objects as object files, human infants might be able to form the expectation that two objects added to one object should result in three objects, but they do not have conceptual knowledge that $1 + 2 = 3$, and to them, the numbers one, two and three are nothing separate from the experience of one, two or three objects (or tones or jumps of a doll *etc.*). For this reason, it is questionable whether innate arithmetic can be counted as a separate conceptual domain that exists independent of our experiences and which might be metaphorically conceptualized in terms of such experiences. What is described as innate arithmetic is more likely the ability to pay attention to particular aspects of experience.

If this is the case, the close connection between arithmetic and the source-domains of the 4Gs cannot be described as a cross-domain mapping, where structure from one domain is mapped onto another. Instead, what Lakoff and Núñez describes, seems to be a process, where the domain of abstract arithmetic is formed by focusing solely on the numerical aspects of experiences involving object collections, object construction and movement along paths. Such a process is more rightly described as a process of *abstraction* than as a process of cross-domain mapping. This being said, the difference between the two types of processes might not be that big. The cognitive mechanism of cross-domain mapping always involves an element of abstraction, as we must always abstract from some aspects of the two domains in order to see a structural likenesses between others. So for instance, when the atom is described metaphorically as a Solar system, we abstract from size, weight, temperature and so forth, and focus on certain structural similarities between the two domains.

Theories explaining the genesis of mathematics with a form of abstraction from experience are not new. They are not unproblematic either, but Lakoff and Núñez do seem to avoid two of the most common objections. Firstly, it is commonly objected that the process of abstraction is obscure. This is true, but by placing the genesis of arithmetic in the general context of cognitive semantics, the process of abstraction can at least be seen as a part of a general and important cognitive mechanism, i.e. conceptual metaphor. We might not know how we perform the abstractions involved in conceptual mapping, but (according to cognitive semantics) we do it all the time. Although this does not explain the process of abstraction, it at least locates it as a normal part of our cognitive life, instead of seeing it as a special process invented especially to explain the genesis of mathematics. Secondly, it is commonly objected that arithmetic, involving infinity or even just large numbers plus the more advanced parts of mathematics, cannot be described by processes of abstraction. This objection clearly does not apply to the theory proposed by Lakoff and Núñez, as they 1) only claim arithmetic to take departure in locally observed structure (which makes it possible for them to account for infinity, as described above), and 2) more advanced mathematics is constructed but other (and truly) metaphorical mappings. The convincing answer to this last objection is in my view one of the main assets of Lakoff and Núñez' theory.

Another and minor problem facing Lakoff and Núñez' theory, is the fact that the genesis of arithmetic can hardly be accomplished with experiences and cognitive abilities existing on a personal level alone. If that was the case, we would expect humans to develop basic arithmetic skills spontaneously. However, the possession of arithmetic skills beyond the rudimentary skills of innate arithmetic is not universal, but seems to be culture-dependent. In this case, the culturally dependent element can hardly be the metaphors (or source-domains of abstraction) as these are universally shared life-world experiences. This suggests, that the development of arithmetic not only depends on the individual, but also on culture-dependent artifacts such as number words or other counting sequences, and material cognitive artifacts such as tally sticks or other types of representational means strong enough to express or even calculate arithmetic facts.

Although the theory proposed by Lakoff and Núñez is not the final answer to the genesis of arithmetic, it should not be dismissed too easily. The theory does offer a new and interesting explanation to the mysterious connection between the real world (or at least our experience of it) and the abstract domain of arithmetic. Furthermore, the theory is an attempt to place the explanation of the genesis of arithmetic on empirical data (from cognitive science), so although the theory might seem somewhat speculative, it is in fact much less speculative than other similar theories (such as the one proposed by Mill). What is completely lacking is historical evidence. The origin of arithmetic is after all a historical event, and a theory explaining it should be backed up by historical data, or perhaps by date from contemporary cultures still in the process of developing arithmetic. This lack of historical evidence turns Lakoff and Núñez' theory into a possible explanation of what could have happened; a model, so to speak, which seems to have a good fit with two types of date: human cognition and the nature of arithmetic.

#### 6.12.1.2 Expansion of arithmetic

Lakoff and Núñez' picture of the expansion of arithmetic (taken in its strong version), is clearly a claim about the historical development of mathematics and the origin of certain mathematical concepts. For this reason, it will need historical evidence as justification, and in this case Lakoff and Núñez do provide some.

In connection to the development of general arithmetic, Lakoff and Núñez primarily present two historical cases as justification: The role played by the discovery of incommensurability in the construction of irrational numbers (Lakoff & Núñez, 2000, p. 70–71), and the role played by the use of the number line in the $16^{\text{th}}$ century in the construction of negative numbers (*ibid.* p. 73-74). I will discuss these two cases in turn, beginning with the discovery of incommensurability.

In connection to the development of other and more advanced parts of mathematics, several case studies are presented. I will here limit myself to present and discuss Lakoff and Núñez' description of the development of complex numbers

#### 6.12.1.2.1 The discovery of incommensurability

According to Lakoff and Núñez, the expansion of arithmetic to include irrational numbers was the direct result of the discovery of incommensurability, in combination with a particular conceptual blend integrating numbers and physical segments. I will describe these two elements one by one, and then give my own comments.

The discovery of incommensurability, in short, refers to the discovery, made by Greek mathematicians sometimes during the 5th century BCE, that the square root of two cannot be expressed as an irreducible fraction $p/q$ for any natural numbers $p$ and $q$. In modern terms, this amounts to saying that $\sqrt{2}$ is not a rational number.

Given the Pythagorean theorem $a^2 + b^2 = c^2$, it can easily be seen that the length of the diagonal of a unit square equals the square root of two. So it follows as an immediate consequence of the discovery incommensurability, that an easily constructible length, i.e. the length of the diagonal of a unit square, cannot be expressed as a fraction $p/q$ of the unit for any natural numbers $p$ and $q$.

The Number/Physical Segment blend

| Domain 1<br>The use of a measuring stick | | Domain 2<br>Arithmetic |
|---|---|---|
| Physical segments consisting of parts of unit length | ↔ | Numbers |
| Basic physical segment, (the unit segment) | ↔ | One |
| The length of the physical segment | ↔ | The size of the number |
| Manipulations of physical segments | ↔ | Arithmetic operations |
| Forming longer physical segments by putting segments together end-to-end | ↔ | Addition |
| Taking a shorter physical segment from a longer | ↔ | Subtraction |

**Table 6.8:** Relevant elements of the Number/Physical Segment blend (Lakoff & Núñez, 2000, p. 68-71)

The second element of the case-study is a particular conceptual blend integrating numbers with physical segments. The blend is called the Number/Physical Segment blend, and is created by integrating the source- and target-domains of the MEASURING STICK METAPHOR (see table 6.8 for details). The blend creates a new conceptual domain, in which there is a one-to-one correspondence between numbers and physical segments (and not only a segment for every number, as entailed by the MEASURING STICK METAPHOR). Consequently, in the blended domain there must be a number corresponding to the length of any line segments, including the length of the diagonal of the unit square.

Now, according to Lakoff and Núñez, the discovery of incommensurability combined with the NUMBER/PHYSICAL SEGMENT blend, forced the Greek mathematician Eudoxus to conclude that $\sqrt{2}$ must exist as a number. Consequently "[i]t was the MEASURING STICK metaphor and the NUMBER/PHYSICAL SEGMENT blend that gave birth to the irrational numbers" (Lakoff & Núñez, 2000, p. 71) (notice the clear expression of the strong genealogical thesis in this quote).

---

There is much to be commented on this case study. Firstly, it eludes me why it is necessary to use the NUMBER/PHYSICAL SEGMENT blend to create the irrational numbers. According to (Lakoff & Núñez, 2000, p. 70), the MEASURING STICK METAPHOR can be used as an object-construction metaphor to create the rational numbers (by mapping the $n$-th part of a physical segment onto the fraction $1/n$) and zero (by mapping the lack of segments onto the domain of numbers), so why can't the metaphor be used to create $\sqrt{2}$ and other irrationals as well?

Secondly, from a strictly mathematical point of view, there is something wrong with the interpretation of the blend. In a geometrical interpretation, the incommensurability of two line segments means that they cannot be constructed using the same basic line segment (of finite length). So when the diagonal is incommensurable with the side of the unit square, it means that I cannot find a basic line segment that allows me to construct both the side of the square and the diagonal by placing copies of the basic line segments end-to-end. As only this kind of construction is allowed in the NUMBER/PHYSICAL SEGMENT blend, it seams that the diagonal is in fact *not* constructible in the blended space (given that we chose a basic length which allows us to construct the unit, i.e. the segment corresponding to 1, which seems reasonable). What is needed in order to entail irrational numbers in this way, is a blend integrating numbers with a continuous domain, as it is the case in the Number-Line blend. A blend integrating numbers with a discrete domain of line segments will not do.

Thirdly, the historical accuracy of the case is somewhat debatable. Although Heath has hypothesized otherwise (Heath, 1921, Vol. 1, pp. 325 ), it is the general consensus that the Greek did not draw the conclusions implied by the Number/Physical Segment blend (or rather as explained above, the Number-Line blend). Greek arithmetic was not simple expanded to include a new type of objects, the irrationals. Instead, the discovery of incommensurability lead to a paradigm shift, where a research program focusing on arithmetic and the properties of numbers was replaced by a geometry-centered research style, better suited to handle incommensurable magnitudes (Dauben, 1984) – although number theory was not given up all together.

As noted above, Lakoff and Núñez acknowledge that culture has some say over which metaphors and ideas we allow in mathematics. But once an idea is accepted, its implications are (according to Lakoff and Núñez) beyond the influence of culture. So the Greeks rejection of the implications of a conceptual blend seems to be a direct contradiction of this theory. There is however, another possibility. It is well-known, that the Greek mathematicians were very careful in distinguishing between numbers and magnitudes (physical lengths) (see for instance Knorr, 1975, p. 9-10). This suggests, that the Greeks might well have rejected the very idea of a Number-Line blend and not only one of its implications. This interpretation would make the episode consistent with Lakoff and Núñez' general theory. So at best, the discovery of incommensurability illustrates the power of culture to accept or reject certain metaphors as valid, and not the power of conceptual mapping as intended by Lakoff and Núñez. At worst, the episode is in direct contradiction to their theory. In any case, the discovery of incommensurability does not give support to the strong genealogical claim.

All of this is not to say that conceptual mapping did not play any role in the process leading to the modern acceptance and understanding of irrational numbers. So for instance, the conceptual integration of the domain of numbers with a continuous line plays a central role Richard Dedekind's (1831–1906) famous attempt to understand the nature of irrational numbers, not least in his definition of irrational numbers as 'cuts' (in a continuous line) (see Lakoff & Núñez, 2000, pp. 292 for a beautiful analysis of the metaphors and blends used by Dedekind). This however, does not prove that such conceptual mappings were the driving force or main motive behind the invention and acceptance of irrational numbers. It should be noted, that the need for irrational numbers does not only arise when numbers are integrated with continuous lines. It also arises in purely arithmetic contexts as the need for a consistent use of certain operations such as square root; without the acceptance of irrational numbers, the square root of a number will in some cases be a number, but in other cases not, which seems odd.

#### 6.12.1.2.2 Negative numbers

Lakoff and Núñez briefly mention another historical case. The ARITHMETIC IS MOTION ALONG A PATH metaphor gives a natural extension of the natural numbers to zero (the origin point) and negative number (locations in the same distance but opposite direction from the origin, as the corresponding positive numbers).

This metaphorical extension was, according to Lakoff and Núñez, made by the 16$^{th}$ century mathematician Rafael Bombelli, who gave the first known representation of negative numbers as locations on a number line (Lakoff & Núñez, 2000, p. 73).

Lakoff and Núñez are right in stating that the number line was probably created by Bombelli and (more well-known) his 17$^{th}$ century colleague John Wallis. But still, negative numbers can hardly be said to be a 16$^{th}$ century European discovery. As mentioned in section 6.5.1 above, negative numbers were represented and operated on by Chinese, Greek and Hindu mathematicians long before Bombelli's introduction of the number line. So negative numbers were clearly not created by the ARITHMETIC IS MOTION ALONG A PATH metaphor or the introduction of a geometrical interpretation in form of the number line. The metaphor was more likely used as a way to conceptualize and give meaning to entities already known and discovered by other means. This is not to be belittled. As Lakoff and Núñez rightly notes (p. 73), the introduction of the number line metaphor offered a uniform conceptualization of all (real) numbers, in contrast to for instance Brahmagupta's conceptualization of negative and positive numbers as respectively debts and fortunes or Diophantus' conceptualization in terms of wantings and forthcomings.

#### 6.12.1.2.3 Complex numbers

Lakoff and Núñez also give a very detailed account of the cognitive origin of complex numbers (Lakoff & Núñez, 2000, p. 420–432). In brief, according to Lakoff and Núñez, our conceptualization of the complex numbers takes departure in the NUMBER-LINE blend, where all real numbers are conceptualized as points on a line. For any number $n$, $n$ and $-n$ are located in exactly the same distance, but in opposite directions from the origin $O$. According to Lakoff and Núñez, this symmetry is conceptualized in terms of a 180° mental rotation around the origin, taking $n$ to $-n$ (and *vice versa*). As the multiplication $n \cdot -1$ takes you to the same point on the number line as a 180° rotation to the symmetry point of $n$, a blend, correlating multiplication by -1 in the domain of arithmetic with 180° rotation in the domain of space, is formed. By combining this blend with the Cartesian plane, a ROTATION-PLANE blend, correlating multiplication by -1 in the Cartesian plane with 180° rotation around the origin, is formed. The expansion to the plane allow us to expand the original blend to include rotations other than 180°. By adding a 90° rotation anti-clockwise around the origin, we metaphorically add a number $i$, such that multiplication by $i$ corresponds to a rotation by 90°. As two rotations by 90° corresponds to one rotation by 180°, it follows that $i^2$ corresponds to -1. Or in other words, $i = \sqrt{-1}$ (see table 6.9 for an overview).

The ROTATION PLANE blend
extended with 90° ROTATION PLANE metaphor

| Domain 1<br>The ROTATION-NUMBER-LINE blend | | Domain 2<br>The CARTESIAN PLANE blend |
|---|---|---|
| The zero point | ↔ | The origin $O$ |
| Rotation by 180° | ↔ | Multiplication by $-1$ |
| Rotation by 90° anti-clockwise | ↔ | Multiplication by $i$ |
| Two rotations by 90° anti-clockwise | ↔ | Multiplication by $i^2 = -1$ |

**Table 6.9:** Overview of the ROTATION PLANE blend

Most importantly, the integration of the ROTATION-NUMBER-LINE with the Cartesian plane allow us to ascribe coordinates to the newly defined numbers. In this extended ROTATION PLANE blend, $i$ is the point (0,1) (the result of rotation a 90° rotation of the point (1,0) around the origin), and $i^2$ is the point (-1,0) (the result of a 180° rotation of (1,0) around the origin). For a real number $b$, $b \cdot i$ is the point (0,b) (the result of rotation a 90° rotation of the point ($b$,0) around the origin), and consequently the complex number $a + bi$ is the point $(a, b)$.

Given these basic identities, addition and multiplication of general complex numbers can straightforwardly be given a geometrical interpretations.

Lakoff and Núñez are aware that complex numbers can be conceptualized in several other ways. One can see complex numbers as points in the usual Cartesian plane (with unusual rules for addition and multiplication) or one can simply see the complex numbers as ordered pairs of numbers. Commenting on these different representations and their presentation of the arithmetic rules (or 'laws') governing complex algebra, Lakoff and Núñez write:

> The last two are the ways the complex numbers are usually taught. But they give very little insight into the conceptual structure on the complex numbers - into *why* they are the way they are. [. . . N]either method explains *why* those laws are there as consequences of the central *ideas* that structure the complex numbers.
>
> (Lakoff & Núñez, 2000, p. 432, original emphasis)

According to Lakoff and Núñez, the conceptualization in terms of the 90° Rotation Plane metaphor is vital for any understanding of complex numbers, because the properties of the complex number system was *motivated* by this metaphor:

> If you want to think of [a complex number] as isolated from the spatial domain, you can. But then you would be losing all the conceptual structure that motivates the arithmetic properties of this number system.
>
> (Lakoff & Núñez, 2000, p. 430)

Lakoff and Núñez furthermore, dismiss the idea that spatial metaphors are merely a pedagogical tool:

> One can compute with, and prove theorems about, $\sqrt{-1}$ without the idea of the complex plane or rotation. Does that mean that those ideas are extraneous to what $\sqrt{-1}$ is? Do they stand outside the mathematics of complex arithmetic per se? Are they just ways of thinking about $\sqrt{-1}$ – mere representations, interpretations, or ways of visualizing $\sqrt{-1}$ – useful for pedagogy but not part of the mathematics itself?
>
> From the perspective of cognitive science and mathematical idea analysis, the answer is no.
>
> (Lakoff & Núñez, 2000, p. 431)

So Lakoff and Núñez clearly seems to hold a strong genealogical thesis regarding the spatial metaphors for complex numbers; complex arithmetic is motivated by and has the properties it has *because* of the spatial metaphors described above.

Unfortunately, this thesis is very hard to back up. In fact, the historical evidence clearly seems to refute it. As noted above, complex numbers were at first encountered in a purely algebraic context (the solution of second and third degree equations) in the 16th century. There is some evidence suggesting that Euler and a few other 18th century mathematicians sporadically visualized complex numbers as points in a coordinate plane (Kline, 1990, p. 629). But apart form a vague attempt by John Wallis (who interpreted imaginary numbers as the sides of negative areas, such as a field swallowed by the sea), decisive geometrical interpretations of complex numbers and complex algebra were first invented late in the 18th century by outsiders to the mathematical community, and geometrical interpretations only became accepted and used by mainstream mathematics in the beginning of the 19th century – when complex numbers had been used and explored for more than two centuries.

All in all it seems very unlikely, that complex numbers and their basic mathematical properties are the result of spatial metaphorical mappings. Imaginary and complex numbers were not motivated by metaphors, but by the need for arithmetic closure, and by the fact that they produced correct results (see Kline, 1990, pp. 251; pp. 592 for a review). The spatial metaphors used to give a geometrical interpretation of complex numbers were chosen to fit the properties of complex algebra, not the other way around.

In this case, however, we have clear evidence suggesting that the spatial metaphors influenced the acceptance of complex numbers as genuine mathematical objects. Although complex numbers were used and manipulated formally in the 17th and 18th century, they were generally believed to be impossible or merely imaginary entities, and only tolerated because of their usefulness.

In an introductory comment to the work *Theoria Residuorum Biquadraticorum*, originally published in 1831, Gauss regrets this state of affairs and sets out to change the general conception of complex numbers (Gauss, 1863, 169–178). He coins the term ‘complex numbers’ for numbers of the form $a + b\sqrt{-1}$ (with $a, b \in \mathbb{R}$), and describes how such numbers can be conceptualized as points on an infinite plane, similar to the way the real numbers can be conceptualized as the points on an infinite line; instead of ordering the numbers in one dimension with $+1$ as a unite distance, the imaginaries must be ordered in two dimensions with $+1$ as the (real) unite of distance in one dimension and $\sqrt{-1}$ as the (imaginary) unit of distance in the other.

In section 38 of the paper proper, Gauss straightforwardly interprets the complex number $x + y\sqrt{-1}$ as the point $(x; y)$ in a plane with a real $x$-axis and complex $y$-axis, and continues to gives a geometrical interpretation of subtraction and multiplication of complex numbers. “Auf diese Weise”, he remarks, “wird die Metaphysik der Grössen, welcher wir imaginäre nennen, in ein ausgezeichnetes Licht gestellt" (Gauss & Maser, 1889, p. 548).

This conceptualization of complex numbers as points on the plane, Gauss imagines, is needed in order for the mathematicians to understand and accept the numbers:

> Wir haben geglaubt, den Freunden der Mathematik durch diese kurze Darstellung der Hauptmomente einer neuen Theorie der sogenannten imaginären Grössen einen Dienst zu erweisen. Hat man diesen Gegenstand bisher aus einem falschen Gesichtspunkt betrachtet und eine geheimnisvolle Dunkelheit dabei gefunden, so ist diess grossentheils den wenig schicklichen Benennungen zuzuschreiben. Hätte man +1, -1, $\sqrt{-1}$ nicht positive, negative, imaginäre (oder gar unmögliche) Einheit, sondern etwa directe, inverse, laterale Einheit genannt, so hätte von einer solchen Dunkelheit kaum die Rede sein können.
>
> (Gauss, 1863, pp. 177)

Although the names here suggested by Gauss never caught on, spatial interpretations of complex numbers (either as points on the complex plane as here suggested by Gauss, or as directed vectors as suggested by other mathematicians) became a vital step in the graduate acceptance of such numbers as genuine mathematical objects. It can be discussed, whether these interpretations corresponds to the rotation plane interpretation given by Lakoff and Núñez, but leaving that aside, it seems that spatial metaphors grounding complex algebra in our experience of two dimensional space played an important role in the history of complex numbers. Not as the driving force behind their invention, but as a way to make operations determined and constrained by other forces, intuitively meaningful and thus acceptable.

As a final comment on this case study, it should be noted that in case of conflict, formal calculations are trusted at the expense of intuitive (metaphorical), spatial interpretations. A clear example of such a conflict is the discovery made by Edward Kasner that for a complex function $f(x)$, the arch length along the curve $y = f(x)$ between two points $P$ and $Q$ on the curve can be *shorter* than the chord (i.e. the straight line) from $P$ to $Q$. This is for instance the case for the complex valued function $f(x) = x^2 + ix$. By calculating the arch length $s$ from $x = 0$ to $x = a$ with the usual formula

$$s = \int_0^a \sqrt{1 + f'(x)^2}\, dx,$$

it can be showed that the ratio $s/c$ between the arch length $s$ and the cord length $c$, limits $2/3\sqrt{2} \approx 0,94$ as $a \to 0$. This means, that the arch gets about 6% shorter than the chord as the point $a$ closes in on 0 (see Nahin, 1998, p. 105–7). Our geometric intuition tells us that a straight line connecting two points is always shorter (or as short as) an arch. We would in other words expect the ration $s/c$ to be $\geq 1$. So here, the formal calculations clearly contradicts the result implied by a metaphorical, geometric interpretation of complex numbers. As mentioned above, modern mathematics does not question the formal result, but counts the mismatch with our geometric intuition as an interesting puzzle. This is a clear example of the limits of the metaphorical conceptualizations of complex numbers.

---

In conclusion, Lakoff and Núñez do not present evidence strong enough to support the strong genealogical claim concerning the 4G's. The metaphors we frequently use to conceptualize negative numbers, complex numbers, and several other mathematical entities did not cause their creation. Rather these entities were created from the consistent use of established algorithms and rules of operation, and only subsequently given meaning by the introduction of the metaphors.

#### 6.12.1.2.4 Metaphors as constitutive for expansions

Although it is hard to find examples where metaphors are the driving force behind a particular extension of mathematics, metaphors might well influence the development of mathematics in more indirect ways. There are examples, where the expansion of mathematics in a particular direction clearly depends on the introduction of new metaphors (although it is not driven by them). The development of analytic geometry is a case in point. As described in section 6.5.2, analytic geometry depends on the use of a particular cognitive artifact, i.e. abstract symbols. However, analytic geometry also presupposes several metaphors and conceptual blends. The most important of these are:

- a linking metaphor (introduced by Descartes and Fermat), linking geometry and algebra
- the conceptualization of the plane and geometric objects as point-sets
- a blend integrating numbers and locations in geometric space.

These metaphors and blends made it possible to identify geometrical objects with sets of ordered pairs of real numbers defined by equations determining the relationship between the numbers.

It should be noted that in this case, the metaphor was not the driving force behind the development – analytic geometry did not force itself upon us as an implication of the metaphors. Rather, the metaphors were chosen in order to reach specific mathematical goals, which could not have been reached without them. So the cognitive tool of conceptual mapping clearly had an impact here: It allowed us to expand mathematics in a way, we could not have done without mapping (which is consistent with the telescope hypothesis). Several similar examples, where linking metaphors are used to create and exploit an analogy between different areas of mathematics, could be given.

The metaphors used in the creation of analytic geometry also have another and deeper impact. As mentioned in section 6.10.1, metaphors have different implications, and consequently the choice of metaphor is not innocent as it might determine the outcome of a debate. Something similar can be said about the introduction of new metaphors. To give a clear example, the introduction of analytic geometry gave rice to a number of 'monster functions' having equally monstrous graphs (as noted above). When the existence of these curves (i.e. the graphs) is debated within the framework of the metaphors of analytic geometry, the answer is more or the less given in advance; the curves must exist. Once the metaphors are accepted, they determine how we understand and deal with geometric objects. In this way, the metaphors have a real, although indirect impact on the content of mathematics. A real discussion of the existence of such the monster functions should address the adequacy of the metaphorical conceptions of geometric objects, they rely on.

### 6.12.2 The cognitive claim

Lakoff and Núñez claims the conceptual metaphors they consider to have a real and ongoing cognitive significance; the metaphors are active and subconsciously structure the way normal human beings think about mathematics. In some cases, such as the 4Gs, the metaphors are even hypothesized to be grounded in actual neural connections linking arithmetic operations (such as addition) and sensory-motor physical operations (such as taking away objects from a collection) (Lakoff & Núñez, 2000, p. 54).

Lakoff and Núñez are not alone in making cognitive claims. Cognitive linguistics is in general committed to the hypothesis that the metaphors they investigate are not just linguistic phenomena, but reflect underlying and significant cognitive mechanisms (which might or might not be neurally encoded as Lakoff and Núñez hypothesize). In the following, I will start by reviewing the evidence given by Lakoff and Núñez. However, as the hypothesis is a more general one, I will also consider other ways to justify it.

Part of the evidence given for the cognitive claim is linguistic; when we talk about mathematical objects, we frequently use a vocabulary taken from the sensory-motor experiences; we might for instance metaphorically speak about numbers as if they were objects (see examples above). As always, this type of evidence is faced with a serious problem. A lot of expressions that entered language as metaphors, have turned into literal expressions, because the original meaning of the expression is forgotten[21]. So the fact that we *talk* about numbers as if they were objects, does not proof that we also *think* about numbers as if they were objects. More evidence is needed.

In (Lakoff & Núñez, 2000) the linguistic evidence is mainly backed up by experimental psychology and the study of the human brain. A number of experiments suggest that at least the natural numbers seem to be encoded in the form of some sort of magnitude. Reaction time experiments for instance, reveal that the time it takes a subject to judge whether a number represented in Hindu-Arabic digits is larger or smaller than a given target, increases with the numerical distance between the two numbers (Dehaene *et al.* , 1990). Furthermore, studies of patients who have lost part of their mathematical capabilities due to injuries of the brain suggest that there is a close connection between basic arithmetic skills, body maps, and spatial maps (Dehaene, 1997, pp. 189). This is used by Lakoff and Núñez to support the view that at least basic arithmetic is closely connected to basic life-world experiences of the body and physical space (Lakoff & Núñez, 2000, pp. 23).

However, such evidence from experimental psychology should be treated with much care. It is questionable to what extend studies of the physical brain can tell us anything about mental phenomena such as thinking and understanding. Does the (supposed) fact that arithmetic is encoded in the same region of the brain as spatial- and body maps really prove that we understand arithmetic using bodily and spatial experience?

Even granted such an intimate relation between the physical brain and understanding, the evidence is still somewhat inconclusive, and it only supports a limited connection between life-world experiences and mathematics. Only arithmetic is (apparently) located in the brain area in question, while other mathematical capacities are not. This was for instance the case in a patient suffering from *acalculia* (Hittmair-Delazer *et al.* , 1995). Due to the

[21] In English this is for instance the case with many of the expressions originating in Latin. To give an example, the word "examine" is in modern English considered a literal expression, but it was originally a metaphor derived from the Latin "examine" meaning "tongue of a weight".

effects of cancer treatment, the patient had lost the ability to solve even basic arithmetic problems, such as $2 + 3$ and $3 \cdot 4$, but he was nevertheless able to solve abstract algebraic problems, such as recognizing that $(d/c) + a$ is not in general equal to $(d + a)/(c + a)$. So the brain area involved in abstract algebra is located in a different place than that involved in general arithmetic and body and spatial maps. Does that prove that abstract algebra is not in any way connected to or conceptualized in terms of body and spatial maps?

#### 6.12.2.1 Gestures

In Núñez (2004) another type of evidence supporting the cognitive claim is tried out. Núñez moves a little away from basic arithmetic and investigates the metaphors of movement used in calculus. Although all central concepts such as function, continuity, differentiability *etc.* are now defined in terms of sets, i.e. discrete and motionless entities, textbooks still describe functions as 'oscillating', 'approaching', 'tending to' and so on (see Núñez, 2004, for an interesting treatment of several such examples).

In order to argue for the real cognitive significance of these metaphors, Núñez turns to the study of gestures. As mentioned above, there seems to be a close link between gestures and speech, and it is believed, that gestures and speech both reflect the same underlying cognitive reality. A study of college professors teaching calculus shows that the words of movement used by the teachers are accompanied by corresponding gestures of movement. This suggests that the metaphors of movement are in fact active; the teachers not only think in terms of movement, they also think in terms of movement (Núñez, 2004, pp. 68). In a revealing example, a professor describes how the partial sum $S_n$ of a convergent sequence oscillates around 1. While he is giving this description, his thumb and index finger are pressed together as if he holds a small object, and he waves his hand from side to side. So in other words, he describes and thinks of 1 as a location in space, and he thinks of the partial sum as an object oscillating around this location.

Núñez' results are confirmed by studies of hi-school teachers. Here, we also find a close relationship between gestures and conceptual metaphors describing abstract mathematical entities in terms of sensory-motor experience (see Frant *et al.* , 2006; Arzarello *et al.* , 2009).

Such studies of gestures give us strong reasons to believe that metaphors, used by the teachers in these cases, actually reflect how they think. Although more studies should be conducted to establish the generality of this effect, the fact that the conceptual metaphors used by hi-school and even college teachers are active *at all* is still a major result. This shows that conceptual

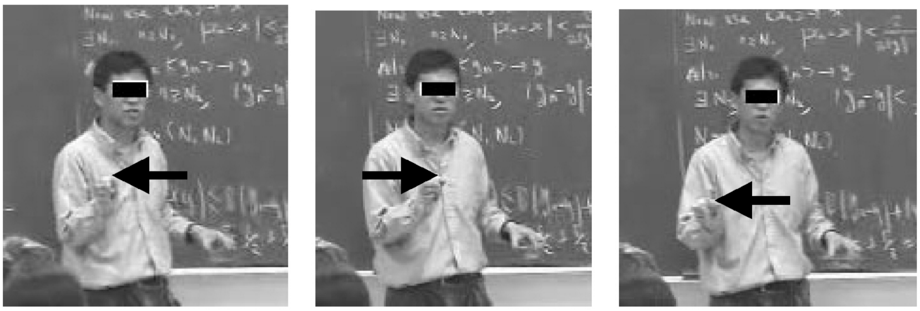


**Figure 6.24:** A professor of mathematics describes how a convergent sequences oscillates around 1 (reprinted from Núñez, 2004, p. 69).

metaphors are not merely linguistic phenomena or figures of speech that has lost their literal meaning long ago. When the metaphors are used in the classroom, they are alive.

The main problem with these studies is the question whether they can be generalized to non-teaching situations, and especially to the problem-solving process of active mathematicians. The teaching situation is a very special situation, and the teachers might well have chosen to use the metaphors in question for purely didactic reasons, although they would never use the metaphors outside the classroom. So the observation that college professors think in metaphors of movement when they teach does not prove that the professors still think in movement, when they return to their offices and start doing mathematical research.

This problem is addressed in a recent study (Marghetis & Núñez, 2010). Here, a group of graduate mathematics students were divided into pairs and asked to prove the following non-trivial theorem from analysis:

> Let $f$ be a strictly increasing function from $[0, 1]$ to $[0, 1]$. Then there exists a number $a$ in the interval $[0, 1]$ such that $f(a) = a$.
> (Marghetis & Núñez, 2010, p. 24)

The students were video filmed while they were working, and their language and gestures were subsequently analyzed. According to the analysis, intuitively dynamic mathematical concepts such as 'increasing', 'continuity' and 'intersection' were accompanied by corresponding dynamic gestures, while other intuitively static concepts such as 'containment' and 'closeness' was accompanied by static gestures. This shows that even highly trained professional mathematicians conceptualize certain mathematical concepts in terms of physical movement when they solve certain problems.

The study is unique both in its topic (advanced problem solving) and in its method (the study is quantitative rather than qualitative). Its basic conclusion that students conceptualize central concepts of analysis in terms of physical movement seems very well founded. The study does however have two possible problems, which should be addressed.

Firstly, although the students clearly understood some concepts in terms of movement, it can be debated whether these dynamic descriptions are in fact metaphorical. According to Marghetis (personal communication), the subjects of the study typically started by drawing and discussing graphs on the blackboard, and only later formulated their reasoning in set theoretical terms. A graph drawn on a blackboard is clearly produced by dynamic processes — the movement of a piece of chalk —, so as long as the students were discussing such physical representations of graphs, dynamic descriptions can hardly be said to be metaphorical. We should keep in mind, that although the formalist movement identifies mathematical concepts exclusively with their set theoretical definitions, such set-theoretical descriptions are often deeply metaphorical. This is for instance clearly the case, when dynamic concepts such as continuity, intersection, and increment are captured with set theoretical descriptions (see Lakoff & Núñez, 2000, pp. 306). As the problem the students were solving in this case, was exactly a problem concerning the properties of a monotonic increasing function, one could argue that the dynamic language used by the students was pre-metaphorical, and that the metaphorical descriptions only began when the situation was coined in terms of static, set theoretical language.

It could also be argued that the dynamic descriptions were in a way pre-mathematics. We are here in a situation similar to the genesis of basic arithmetic. When it comes to the analysis of simple functions, it seems to me that we do not use the physical world as a means to (metaphorically) describe an already existing domain of mathematical objects. Rather, we use (or create or abstract) the mathematical objects as a way to describe properties of the physical world. In this case, functions conceptualized as objects of set theory are used to capture and analyze properties of trajectories in physical space, i.e. graphs drawn on a blackboard. So here, the relationship between mathematical content and physical inscriptions are the exact opposite of what it is, when we use Venn- and commutative diagrams. In the case of diagrams, the physical inscription is clearly used to create a metaphorical description of mathematical content. Here, mathematics (in the form analysis) is used to describe properties of physical inscriptions.

Although it is questionable whether the dynamic descriptions used by the students are metaphorical or not, they are still there. And that in itself in an important result. It shows that the so-called 'discretization program' aiming to eradicate dynamic thinking from mathematics by redefining all mathematical concepts in terms of static sets, has not yet been completely successful. Mathematicians still think of functions as trajectories of movement in space, or rather: they start by thinking about trajectories in space, and only later try to capture the properties of such trajectories by corresponding set-theoretic concepts from mathematical analysis.

The problem addressed above could perhaps be solved by observing students solving problems in a branch of mathematics not so closely related to the dynamics of the real world. There is however another and more fundamental possible problem with the study. In a teaching situation, the teacher is clearly trying to reach communicative goals. For this reason she might be using conceptualizations, she would never use if she were thinking in solitude. In the current study, the students are observed in a problem-solving situation. Although this makes it more likely that the speech and gestures produced by the students correspond to the way they privately conceptualize the concepts in question, this is still a communicative situation. The students work in pairs, and they produce speech and gestures as a way to convey ideas to each other. Consequently, the metaphors might only be used as a way to reach communicative goals; the metaphors might not reflect the way the students think, but only the way they explain their thoughts. The conclusions of the study are only valid, if the students are 'thinking aloud', i.e. directly expressing their own conceptualizations of the problems at hand without adding metaphors or gestures in order to be better understood by their teammate. This is a far more serious problem, as it questions the method of using gesture studies as a way to justify the cognitive claim made by cognitive semantics.

#### 6.12.2.2 Material anchors

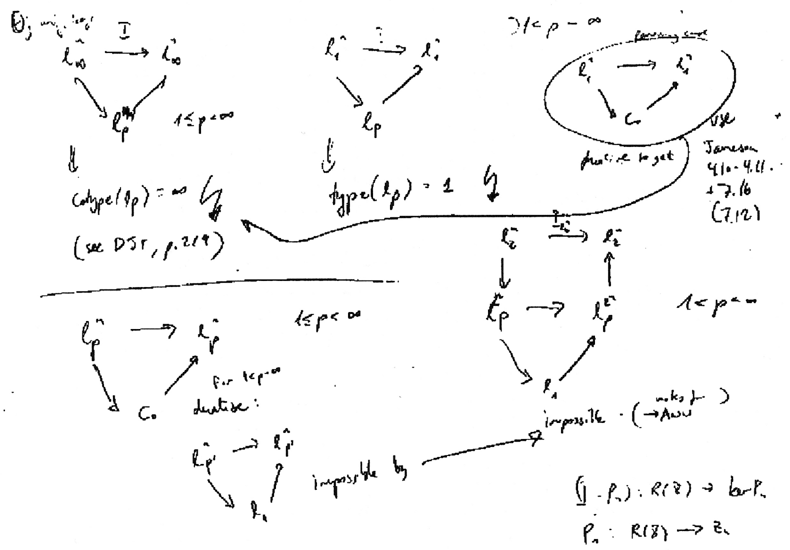
A possible solution to some of theses problems could be to investigate material anchors for metaphors and blends instead of speech and gestures. So for instance, as we saw above, Cantors proof of the countability of the rational numbers is usually communicated using a material anchor for a blend integrating a metaphorical conception of the rational numbers as locations in space with the domain of directed paths in space. The various proofs of the theorem use formally different, but cognitively similar anchors. This indicates that the material anchors reflect a cognitive reality; we understand the proof in terms of the blend materialized in the anchor, and we can express this meaning in formally very different anchors. So in our understanding of this particular proof, the different conceptual mappings are active.

Cantor's proof and most of the material anchors discussed above are taken from communicative situations (i.e. text books and published papers). Consequently, the conclusions drawn from them are limited; we cannot tell, if the anchors reflect how the authors generally conceive of the objects in question, or whether the anchors are specially invented for the communicative situation and merely used as didactic tools. Luckily, and in contrast to language and gestures, material anchors are not in principle confined to communicative situations, but might also be used by mathematicians working in solitude. In a study on mathematical writing, Morten Misfeldt has shown that writing has least five different functions for the working mathematician. Working mathematicians use writing for: 1) heuristic treatment, i.e. getting and trying out ideas, 2) control treatment, i.e. a precise investigation of the heuristic ideas, 3) information storage (for own personal use), 4) communication with colleagues, and 5) production of papers (Misfeldt, 2006, p. 27). So writing is not only used in communicative settings, but also as part of the personal thinking process.

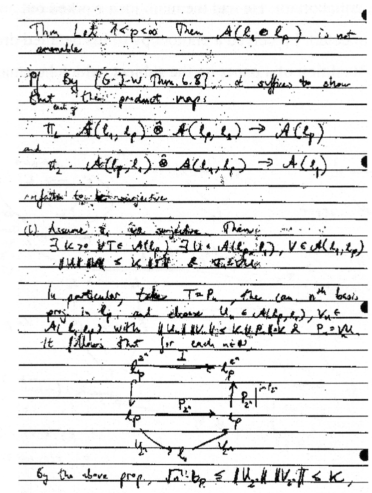
The five functions roughly corresponds to the different phases involved in the production of mathematical knowledge: First you get the idea, then you submit it to tests (control it), you communicate it to others, and finally you produce a paper for publication. Interestingly, some (but not all) of the mathematicians in the study by Misfeldt used diagrams heavily in the initial heuristic phase. As a typical example, the respondent R1 starts an article by working on diagrams drawn on a piece of scrap paper. Then he controls his ideas working mostly with formal calculations on a different type of paper. Finally, he presents his ideas and arguments as a linear narrative using a mix of formal and natural language and only few diagrams (see figure 6.25)(Misfeldt, 2006, p. 23–26 and 34–35).

As it can be see, R1 use commutative diagrams heavily in the initial, heuristic treatment of his ideas. The use of material anchors for conceptual metaphor in a completely private and non-communicative setting shows that at least to R1, these metaphors are active. They reflect the way he in fact conceptualizes the mathematical reality in question, and to the extend such diagrams find their way into his papers, they are not merely communicative tools glued onto the formal treatment as an afterthought. They go to the heart of how he conceptualizes the mathematics in question.

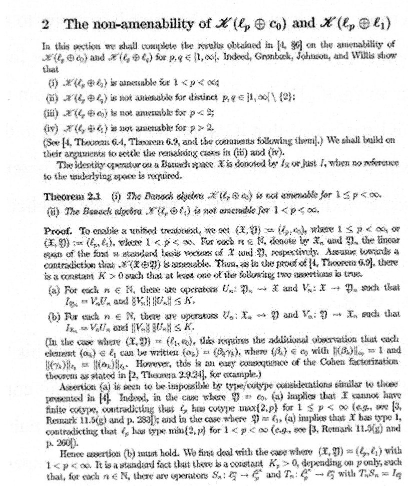
Misfeldt's study was not conducted with the purpose of testing the cognitive claims made in cognitive semantics, and consequently we cannot draw any firm conclusions about this claim from it. However, the study does show that writing, and in some cases diagrams and non-linear arrangements of



(a) Scrap-paper for heuristic treatment



(b) Lined paper used for control treatment



## 2 The non-amenability of $\mathscr{K}(\ell_p \oplus c_0)$ and $\mathscr{K}(\ell_p \oplus \ell_1)$

In this section we shall complete the results obtained in [4, §6] on the amenability of $\mathscr{K}(\ell_p \oplus c_0)$ and $\mathscr{K}(\ell_p \oplus \ell_q)$ for $p, q \in [1, \infty[$. Indeed, Grønbæk, Johnson, and Willis show that

(i) $\mathscr{K}(\ell_p \oplus \ell_2)$ is amenable for $1 < p < \infty$;

(ii) $\mathscr{K}(\ell_p \oplus \ell_q)$ is not amenable for distinct $p, q \in ]1, \infty[ \setminus \{2\}$;

(iii) $\mathscr{K}(\ell_p \oplus c_0)$ is not amenable for $p < 2$;

(iv) $\mathscr{K}(\ell_p \oplus \ell_1)$ is not amenable for $p > 2$.

(See [4, Theorem 6.4, Theorem 6.9, and the comments following them].) We shall build on their arguments to settle the remaining cases in (iii) and (iv).

The identity operator on a Banach space $\mathfrak{X}$ is denoted by $I_{\mathfrak{X}}$ or just $I$, when no reference to the underlying space is required.

**Theorem 2.1** (i) *The Banach algebra $\mathscr{K}(\ell_p \oplus c_0)$ is not amenable for $1 \leq p < \infty$.*

(ii) *The Banach algebra $\mathscr{K}(\ell_p \oplus \ell_1)$ is not amenable for $1 < p < \infty$.*

**Proof.** To enable a unified treatment, we set $(\mathfrak{X}, \mathfrak{Y}) := (\ell_p, c_0)$, where $1 \leq p < \infty$, or $(\mathfrak{X}, \mathfrak{Y}) := (\ell_p, \ell_1)$, where $1 < p < \infty$. For each $n \in \mathbb{N}$, denote by $\mathfrak{X}_n$ and $\mathfrak{Y}_n$ the linear span of the first $n$ standard basis vectors of $\mathfrak{X}$ and $\mathfrak{Y}$, respectively. Assume towards a contradiction that $\mathscr{K}(\mathfrak{X} \oplus \mathfrak{Y})$ is amenable. Then, as in the proof of [4, Theorem 6.9], there is a constant $K > 0$ such that at least one of the following two assertions is true.

(a) For each $n \in \mathbb{N}$, there are operators $U_n\colon \mathfrak{Y}_n \to \mathfrak{X}$ and $V_n\colon \mathfrak{X} \to \mathfrak{Y}_n$ such that $I_{\mathfrak{Y}_n} = V_n U_n$ and $\|V_n\|\,\|U_n\| \leq K$.

(b) For each $n \in \mathbb{N}$, there are operators $U_n\colon \mathfrak{X}_n \to \mathfrak{Y}$ and $V_n\colon \mathfrak{Y} \to \mathfrak{X}_n$ such that $I_{\mathfrak{X}_n} = V_n U_n$ and $\|V_n\|\,\|U_n\| \leq K$.

(In the case where $(\mathfrak{X}, \mathfrak{Y}) = (\ell_1, c_0)$, this requires the additional observation that each element $(\alpha_k) \in \ell_1$ can be written $(\alpha_k) = (\beta_k \gamma_k)$, where $(\beta_k) \in c_0$ with $\|(\beta_k)\|_\infty = 1$ and $\|(\gamma_k)\|_{\ell_1} = \|(\alpha_k)\|_{\ell_1}$. However, this is an easy consequence of the Cohen factorization theorem as stated in [2, Theorem 2.9.24], for example.)

Assertion (a) is seen to be impossible by type/cotype considerations similar to those presented in [4]. Indeed, in the case where $\mathfrak{Y} = c_0$, (a) implies that $\mathfrak{X}$ cannot have finite cotype, contradicting that $\ell_p$ has cotype $\max\{2, p\}$ for $1 \leq p < \infty$ (*e.g.*, see [3, Remark 11.5(g) and p. 283]); and in the case where $\mathfrak{Y} = \ell_1$, (a) implies that $\mathfrak{X}$ has type 1, contradicting that $\ell_p$ has type $\min\{2, p\}$ for $1 < p < \infty$ (*e.g.*, see [3, Remark 11.5(g) and p. 260]).

Hence assertion (b) must hold. We first deal with the case where $(\mathfrak{X}, \mathfrak{Y}) = (\ell_p, \ell_1)$ with $1 < p < \infty$. It is a standard fact that there is a constant $K_p > 0$, depending on $p$ only, such that, for each $n \in \mathbb{N}$, there are operators $S_n\colon \ell_2^n \to \ell_p^{2^n}$ and $T_n\colon \ell_p^{2^n} \to \ell_2^n$ with $T_n S_n = I_{\ell_2^n}$

(c) Final paper written in LaTeX. The argument is formalized and linealized

**Figure 6.25:** Three stages in the creative mathematical process (reprinted from Misfeldt, 2006, p. 23–25)

.

symbols, are used for non-communicative purposes as part of the working process of professional mathematicians. This is in itself an important result. It shows that studies of the initial phases of mathematical working process can give us insight into what the mathematicians think while they work, and not just into how they communicate their ideas. Unfortunately, to my knowledge no studies have be performed specifically investigating the use of material anchors in this phase of mathematical writing, but I propose, that such a study will be a promising way to shed light on the question of the cognitive reality of conceptual metaphor to the working mathematician.

---

It is now time to recapitulate. The main question in the evaluation of the cognitive claim is: When are the conceptual metaphors and blends active, and to whom? The question is particularly complicated, as a metaphor might be active only to some individuals, only in a particular situation and only in a particular historical era. Judging from the evidence evaluated above, the thesis that the metaphors used in mathematics are always active and determines the way mathematicians think, is clearly much too strong to be defended. Neither the study of gesture, language or the human brain gives sufficient evidence to support such a conclusion. The study of gestures does, on the other hand, give strong evidence to the conclusion that at least some metaphors and blends are active at least in communicative situations, such as collaborative problem solving, teaching and communication of mathematical results. This result is supported by the study of material anchors for conceptual mappings (such as diagrams). The study of such material anchors might furthermore be a promising way to prove the role played by conceptual metaphors and blends even in non-communicative situations, such as actual mathematical research.

When this conclusion is combined with the conclusion regarding metaphors' role in the expansion of mathematics, it seems that the major role played by conceptual metaphors and blends is not the role as the creative force driving the development and creation of mathematics. Rather, metaphors and blends are used to give grounded meaning to objects created by the use of symbols and (mainly) the application of established of operation rules in consistent ways.

In this way, the two embodied cognitive strategies I have described, i.e. internalization and externalization, seem to compliment each other neatly. By the help of externalization, mathematics can be performed as a formal game, where external object symbols are treated and manipulated as tokens of arbitrary, Wittgensteinian 'meaning as use'. However, mathematics can also be done as something meaningful, where the meaning, at least in part, comes from conceptual metaphors and blends grounding the meaning of the symbols in life-world experience.

## 6.13 A functional role for grounded meaning?

What remains is to ask, if this latter grounded meaning has a functional role to the practitioner of mathematics. Do we actually need the deeper meaning provided by conceptual metaphor, or is the more superficial Wittgensteinian meaning (i.e. meaning as use) sufficient?

In answering this question, we should distinguish between the broader historical development of mathematics and the problem solving and development for new results within an established theoretical framework.

Starting with the historical development, it should firstly be noted that even the rules of operation used to create Wittgensteinian meaning are not arbitrary. Most parts of mathematics take departure in real world observations and problems. As we have seen, this is for instance the case in arithmetic that takes departure in the manipulation of collections of objects and other real world experiences (I will discuss and defend this position at length in chapter 7 below).

Although the rules take departure in real life phenomena, it is not always possible to completely capture the essence of these phenomena by the rules. The problem of non-standard arithmetic serves as a telling example. Here our intuitive understanding of the natural number system is underdetermined by the formal rules attempting to capture it (see section 2.5). This points to a limitation in formal and un-grounded rule following. The fact, that we are able to recognize a mismatch between the formal axioms and an intuitively given structure shows that there is more to mathematics than rules and axioms. There is an intuitively given and meaningful structure, which we wish to capture with the rules and axioms. The fact that we see the mismatch between rules and as a problem, furthermore shows that in this case, the intuitively given structure has precedence.

So mathematics starts with something meaningful and intuitively given, which might never be fully captured in the rules. In the expansion of mathematics, however, the intuitive or 'grounded' meaning is in many historical cases secondary to the rule based treatment; in case of conflict between grounded meaning and established rules of operation, the rules tend to win, as we have seen in several instances. So here, grounded meaning seems to play a secondary role in the development of mathematics.

This being said, these are examples where the conceptual mapping in general (and this covers more that grounding metaphors and blends) does seem to have a functional role in the development of mathematics, besides creating meaning to rule governed extensions *post factum*. There are examples where we have a real choice over which metaphor to use to conceptualize a concept. And such choices have consequences. As we also saw, there are cases (such as the development of analytic geometry) where a particular set of metaphors and blends are necessary in order to create and exploit fruitful analogies between two separate areas of mathematics.

---

Turning to problem solving, one way to attack the question is to make empirical investigations of the role of grounded meaning in actual problem solving. To the best of my knowledge very few such studies have been made. Furthermore, the few studies that have been made, mainly addresses how basic mathematical problems are solved. It has for instance been shown that elementary school students, who have a grounded understanding of negative numbers (by using the Number/Line blend) are better at solving problems that children, who handle negative numbers using only algebraic rules (Thompson & Dreyfus, 1988).

Another and more direct way of attacking the problem, is to investigate so-called *automated theorem provers*. Automated theorem provers are essentially computer programs that are able – on their own hand – to find the formal manipulations needed to get from a given set of (formally stated) assumptions to a given (formally stated) theorem. In contrast to computer assisted proofs, where the computer is only used to performs long, but trivial calculation, automated theorem provers can be said to simulate an element of creativity; they seem *find* the proof on their own (although it the process in many cases is better described as an extensive search).

Automated theorem provers have successfully proven a long list of theorems. Most of the problems are from the typical undergraduate curriculum, but at least on one occasion, a computer program beat top-level human mathematicians, and became the first to prove a non-trivial result. This was in 1996, when the program EQP ('Equation Prover') proved the so-called 'Robins problem' (McCune, 1997)[22]. As impressive at this might be, automated theorem provers, however, also have severe limitations compared to human mathematicians. At least three such limitations can be pointed out:

- Automated theorem provers cannot form higher order concepts
- Automated theorem provers cannot use knowledge from other areas of mathematics
- Automated theorem provers cannot change the representation and state the problem in another form (which often makes the solution trivial).

I will discuss these three limitations on at a time in the following.

The lack of higher order concepts tends to make the proofs generated by automated systems dense with symbols and trivial calculations. As an example, we can take a closer look at the proof of the Robins problem produced by EQP. The proof consists of only twelve equations of the following type:

$$\neg(\neg(\neg(\neg(\neg(\neg x + y) + x + 2y) + \neg(\neg x + y) + \neg(y + z) + z) + z + u) \\ + \neg(\neg(y + z) + u) = u$$

(Equation number seven of the proof, from McCune, 1997, p. 266)

Such proofs are almost impenetrable for human mathematicians to read and verify. Human proofs in contrast, generally invoke higher order concepts.

So for instance, after the publication of the computer-generated proof of the Robbins problem, several 'anthropomorphized' versions of the proof have been given (see Dahn, 1998). All of these proofs start by introducing higher order concepts, such as $\delta(x, y) = \neg(\neg x + y)$, and a considerable part of the proofs consist in reasoning about these concepts (for instance proving that $\delta(x, \neg y) = \delta(y, \neg x)$). The anthropomorphized proofs are in general longer than the original EQP-proof, but they consist of considerably simpler calculations and higher order reasoning, which makes them much easier for human mathematicians to read and understand.

It could be argued that this contrast between human and computer provers illustrates a shortcoming in the human mathematicians, and not in the computer systems. Computers cannot master higher order concepts, but they do fine without them. Humans, on the other hand, need higher

[22] In brief, the problem consists in showing that a certain set of equations, including the so-called Robbins equation $(\neg(\neg(x + y) + \neg(x + \neg y)) = x)$, can be used as a basis for Boolean algebra. For details, see McCune (1997).

order concepts, because our ability to master formal calculations is limited. Nevertheless, here we are interested in the actual abilities of human mathematicians, and not in how they could have performed, if they had been computers. Given this premise, the contrast between human and computer provers teach us a valuable lesson. Purely formal proofs made by purely formal reasoners (i.e. computers) are simply not accessible to us. We can hardly understand, let alone produce, proofs of this type. When humans construct mathematical proofs, we simply need to introduce higher order concepts that allow us to substitute some of the low-level, formal calculations with higher order reasoning. So higher order concepts clearly play a functional role to human mathematicians.

Furthermore, the work with automated theorem provers has brought out examples that clearly illustrate the advantage of conceptual reasoning in comparison to purely formal reasoning. To give and example from Michael Beeson (2003, p. 98), humans can easily prove the continuity of the function $f(x) = (x + 3)^{100}$ by recognizing it as compound function and appeal to the high-level theorem, stating that functions composed of continuous functions are themselves continuous. This strategy is not open for automated systems. Even recognizing a function as compound is, according to Beeson, beyond the reach of automated systems. It takes conceptual knowledge.

The process of concept formation in mathematics is not well understood, but it is clearly not a unitary process. Some concepts, such as the $\delta$-function mentioned above, are introduced probably as a clever abbreviation of frequently occurring symbolic forms in a given area of mathematics. Others are shaped by the internal mathematical needs of a proof process (these are the 'proof generated concepts' Imre Lakatos famously described in (Lakatos, 1976b)). Such higher order concepts do not necessarily reflect any grounded meaning. Other concepts however, clearly relate to sensory-motor experience. This is for instance clearly the case with concepts such as 'triangle' or 'circle'. Also, some mathematical concepts are shaped by the interaction between mathematics and science. Liouville's introduction of differentiation of fractional order or various mathematicians use of the delta-functions can be briefly mentioned as examples – although I will not go into further discussions here (see e.g. Lützen, 1990, 2006). Finally, the names given to many higher order concepts betray their origin in grounded meaning[23]. Mathematical concepts are not given systematic names (such as newly discovered bodies of the heavens), but are often named in a meaningful way. Such names typically refer to a particular metaphorical representation of the general concepts. Groups for instance have 'centers' and 'kernels', and subgroups can be

[23] I am indebted to Esben Lorentzen for pointing out this fact to me

used to form a 'tower'. Here, groups are clearly conceptualized as physical objects that have parts and can be manipulated in space.

All of this shows, that grounded meaning both in the form of direct experience and in the form of metaphorical mapping plays a role in the formation of higher order concepts. As such concepts play a functional role to human mathematicians, grounded meaning consequently has at least an indirect functional role as well.

---

The second shortcoming of automated provers, i.e. their inability to use knowledge from multiple areas, put clear limitation on the performance of the computer systems, as mathematical theory often consists of a mix of knowledge and concepts drawn from different areas. As a telling example, Michael Beeson reports how one of his master students went through the exercises on ring-theory[24] in a typical text book on algebra (Beeson (2003, p. 97–98; p. 122). The textbook is Jacobson (1985)). Of the 150 exercises, only 14 could be formalized in first order language of rings (and subsequently solved by the automated theorem prover Otter). The rest of the problems involved higher order concepts (such as sub-groups, sub-rings and homomorphisms) and theory from other areas, such as number theory. As an example, Beeson mentions Lagrange's theorem stating that if $H$ is a subgroup of a finite group $G$, then the number of elements in $H$ is a divisor in the number elements in $G$. The theorem involves both the higher order concept of subgroup, and knowledge about natural numbers and the property of being a divisor form number theory. For this reason, the theorem is not in the range of automated theorem provers (Beeson, 2003, p. 122).

The last inability of computer provers, i.e. the inability to change representation, also put clear limitations on such automated systems. Although automated provers are able to prove a vide range of theorems, they have surprising problems with some fairly trivial problems. As an example, David Corfield notes the following theorem of group theory: "If $G1$ has exactly two elements and $G2$ has exactly two elements, then there exists and isomorphism between them" (Corfield, 2003, p. 41). The theorem is part of the problem library TPTP (Thousand Problems for Theorem Provers), which is often used by constructers of theorem provers to test their programs. Although the problem is trivial from a human point of view, at the time of

[24]Rings are a particular type of objects in algebra. There exact properties are not important here.

Corfield's review, it did not have a satisfying automatically generated proof. The reason for this difference between the performance of humans and computers, is that humans do not prove the theorem directly by the use of the group axioms in a formal argument. Instead, a human proof will typically (and simply) consist of filling in the multiplication table for the group (see table 6.10).

MULTIPLICATION TABLE FOR A TWO-GROUP

| $*$ | **e** | **a** |
|---|---|---|
| **e** | $e$ | $a$ |
| **a** | $a$ | ? |

**Table 6.10:** Let $e$ and $a$ be the elements of the group, and $e$ be the neutral element. The first three spaces of the table are filled out as a direct consequence of the neutral element property of $e$. As $a$ according to the group axioms must have an inverse, the element in the last remaining space must be $e$, making $a$ its own inverse. As the multiplication table is completely specified by the group axioms, it must be the same for all groups with exactly two elements, and hence they must all be isomorphic.

The human proof involves a change in representational form; instead of formal deductions, a table is used. According to Raymond Duval mathematicians use four different kinds of representational systems (called 'registers', see table 6.11). Representations can either be 'discursive' or 'non-discursive' and they can either be 'multi-' or 'mono-functional'. Discursive registers are characterized by deduction and linear argument, where non-discursive registers are typically geometrical drawings, graphs or diagrams. In monofunctional registers, processes can be performed by the use of algorithms, whereas in multi-functional registers they cannot (Duval, 2000, 2006). The change between deductions from axioms to filling in a multiplication table used in the proof above, amounts to a shift from a discursive, monofunctional register to a non-discursive, monofunctional register.

The ability to change the representation of a problem and make conversions between different registers has well-established benefits to (human) reasoners (Zhang, 1997; Kerberi & Polleti, 2002). It is however, difficult for many mathematics students to learn to perform such shifts, and they seem in general to be beyond the reach of automated theorem provers (Kerberi & Polleti, 2002). Making such shifts is an advanced process. As noted by Duval 2006, a shift or a conversion is a transformation that changes representational system without changing the conceptual reference. The ability to

| | **Discursive** | **Non-discursive** |
|---|---|---|
| **Multifunctional** (non-algorithmic) | Natural language | Drawings, sketches and geometric figures constructed with tools |
| **Monofunctional** (algorithmic) | Symbolic computations, and proofs | Diagrams and Cartesian graphs |

**Table 6.11:** Duval's typology of registers (redrawn with simplifications from Duval, 2006, p. 110)

perform such a shift presupposes that one does not identify the mathematical concepts with their representations. It presupposes, that one can grasp meaning that lies beyond the rules for operating on the symbols of a given representational system.

As I see it, a conversion can in some cases be performed by a simple change in representational forms (this is the case in Zhang (1997)). In other cases however, the change requires conceptual mapping, either linking two areas of mathematics or grounding mathematical theory in life-world experience. The first (i.e. linking) is the case in analytic geometry, where a conceptual link between algebra and geometry allows powerful conversions from a non-discursive, geometric register to a discursive, algebraic register to take place. The second (i.e. grounding) is the case in commutative diagrams, where a metaphor conceptualizing algebraic objects as objects in space, allows advanced algebra to be represented in diagrammatic form (i.e. conversions from discursive to non-discursive registers).

This great flexibility in reasoning style clearly has a functional role to human mathematicians. As it is in part realized by grounded meaning, it constitutes another example, where grounded meaning has a functional role to human mathematicians.

The comparison between automated theorem provers and human mathematicians helps illustrating the advantages of the cognitive strategies used by humans. Computers have more memory and can perform significantly more manipulations of formal expressions than human mathematicians (EQP for instance searched almost 50.000 equations in its search for the proof of the Robbins problem (McCune, 1997)). Still, automated theorem provers are currently at the level university freshmen (at best) (Beeson, 2003, p. 100). This proves the efficiency of the cognitive strategies used by human mathematicians, and analyzed in this present chapter. Humans form high-level concepts, we integrate knowledge from more areas and we represent a problem in different ways, allowing us to attack it from different perspectives.

All of this involves the ability to form analogies between different cognitive domains, and even ground mathematical meaning in life-world experience. This is how we do mathematics. But it is more than that. Compared to the Wittgensteinian, rule-governed manipulation of abstract symbols, mastered by automated theorem provers, it also proves a very effective strategy. Manipulation of arbitrary symbols according to syntactic rules is but one of the cognitive strategies used by human mathematicians. It is a very efficient strategy, but as the above comparison clearly shows, mathematics should not be identified with this strategy alone. Other strategies, including the grounding of mathematical concepts in real-world experience, clearly have a functional role for human mathematicians.

## 6.14 Life-world experience in mathematical cognition

Allow me to take stock and summarize my conclusions regarding the role played by life-world (and other sensory-motor) experiences in mathematical cognitions. As we have seen in these last few sections, life-world experience clearly enters mathematical cognition in connection to the use of conceptual mapping as a cognitive strategy. Here, life-world experiences can either be used as source-domain for conceptual metaphors or as one of the domains used to form conceptual blends. Our use of diagrams presuppose this kind of blends or metaphors that make it possible for us to conceptualize abstract mathematical content as physical objects that subsequently can be represented and manipulated in diagrammatic form. In communicative settings, such as teaching or cooperate problem solving, conceptual metaphors are used to ground mathematical content in life-world experiences in order to make it intuitively accessible to the listeners. We might even use these techniques when we solve mathematical problems in solitude, as evidenced by the use of diagrams in the heuristic treatment of new mathematical problems.

It is debatable what kind of impact our use of life-world experience in mathematical cognition has on the content of our mathematical beliefs. Judging from the examples given above, conceptual mapping is mainly a neutral tool that helps us to perform in a cognitively more economic way. As suggested by the comparison with automated theorem provers, the use of conceptual mapping and grounding of meaning might even in some cases make it possible for us to reach results, not reachable without this particular cognitive instrument (supporting the telescope hypothesis). The idea (suggested by Lakoff and Núñez) that conceptual metaphors have been the driving force behind the development of central mathematical concepts, such as real and complex numbers, is not supported by the historical evidence. We do see however, that the introduction of a suitable metaphor, grounding otherwise counterintuitive concepts in life-world experience, might have played a part in the general acceptance of such concepts. Also, in some isolated cases, a particular area of mathematics can be conceptualized using different metaphors with different mathematical implications. In such cases, the culture-dependent choice and use of particular conceptual metaphors and blends has had a direct influence on our mathematical beliefs. In these cases, our mathematical beliefs do seem to be constructs, depending on the introduction of particular life-world conceptualization of abstract mathematical content.

## 6.15 Partial conclusion: The embodiment of mathematical cognition

To conclude more generally on the chapter as a whole, we have seen the following. Firstly, externalization of mental content clearly plays an important part in mathematical cognition; we use cognitive artifacts such as symbols and figures, and they allow us to substitute mental computations with epistemic actions and they make it easier for us to handle conceptual complexity by anchoring it in external inscriptions (such as written or spoken language, abstract symbols, figures and diagrams). Secondly, through conceptual mapping, concrete and well-known life-world experiences are use to render highly abstract mathematical content intuitively graspable and understandable. Mathematical cognition, in other words, is clearly *embodied* cognition. It is not objective or abstract reasoning that simply happens to be performed by humans. The cognitive tools we use are essentially anthropomorphic; our body and environment determine both which basic experiences that are available to us as source-domain for conceptual metaphors, and which physical artifacts it is possible for us to produce and operate. Human cognition is in a non-trivial way embodied; it is shaped by the nature and possibilities of our body and physical surroundings. And that goes for mathematical knowledge as well. As all human cognition, mathematical cognition is integrated with our biological nature and our way of existing in the world, and it is influenced and constrained by the kind of beings we are. For that reason, Husserl's claim that mathematics is species-independent does not hold good; at least, the mathematical knowledge available to us, is dependent on the kind of species we are. In theory, this does not exclude the existence of mind-independent, purely objective mathematical truths – but they do not seem to be ours to have. The mathematics we know, is shaped by the kind of beings we are.

As we have seen in this chapter, mathematics furthermore depends heavily on the culture-dependent context of artifacts, instruments and accepted metaphorical conceptualizations. A simple question such as: What is a genuine geometrical object? will always be answered within such a context; Euclid gave one answer, the culture-dependent practice of machine-use led Descartes and Huygens to give another, and finally, the introduction of abstract symbols as a new mathematical technology led to yet another.

There is here, as pointed out by David Kirsh (in personal communication), a great deal of parallelism with the ideas of Imre Lakatos. Lakatos pointed to the lack of certainty of mathematical theorems due to the fact that the intra-mathematical discourse might always lead to new counter-example. The dependency of mathematical thinking on cognitive strategies involving artifacts and metaphors points to another layer of contingency; the introduction of new artifacts or the acceptance or rejection of a metaphor might always lead to revisions of our mathematical beliefs.

# Chapter 7

# The social level

In the previous chapter, I identified the essential part rules play in the development of mathematics. In several cases, new mathematical objects were created simply by extending known rules of operation into new domains. This was for instance how negative numbers and complex numbers were brought into being. This process of expansion naturally brings our ability to follow rules to the center stage, and demands a deeper explanation of what this ability consists in.

So far, I have treated rules and rule following as trivial processes. It is however, somewhat mysterious that we are in fact able to follow rules. This became clear with the later writings of Ludwig Wittgenstein, where seemingly trivial examples of rule following, such as '+2', were examined and brought into questioning. Wittgenstein's central point is that neither training nor the previous use of a rule seems to determine the outcome, when the rule is applied to new cases. The use of a rule seems to be underdetermined by previous use. This being said, there is no consensus on Wittgenstein's general conclusion on the matter. At least three different schools can be identified all – with some right – claiming to be based on different interpretations of Wittgenstein's writings: 1) An individualistic approach claiming rule following to be an essentially individual process, 2) a collectivistic approach claiming social groups to be constitutive to rule following and 3) a 'quietistic' approach claiming rule following to be beyond philosophical and rational treatment, but as long as we do not theorize about it, there is no problem.

In the following, I will limit myself to a discussion of the collectivistic approach. My goal is neither to discuss the correct interpretation of Wittgenstein's philosophy nor to account for rule following in general – I will leave that for others. My goal is solely to shed light on the process of rule following in the context of mathematics, and as I see it, the radical claims made by the collectivistic approach invites the most fruitful discussion.

Furthermore, rule following is closely connected to normativity; when rules are involved, you can do things either right or wrong. Normativity is a salient and important feature of mathematics, as noted in the discussion of Frege and Husserl's anti-psychologism (in section 3.2.4). In mathematics, what you do is almost always either right or wrong. However, neither the biological nor the cognitive level is capable of giving an adequate account of normativity in mathematics. At these levels of analysis, we are confined to describe behavioral dispositions; a monkey might be disposed to choose the bucket containing three pieces of apple over the bucket containing only two, but it is neither right or wrong in doing so. It is only more or less competent in food gathering. Humans might be disposed to follow certain cognitive strategies, such as externalizing, but we are neither right nor wrong in doing so. We are only more or less efficient cognizers. Yet, in mathematics, you are right if you claim $2 + 2$ to be 4, and wrong, if you claim it to be 5, and this aspect of normativity must somehow be accounted for. To give such an account, it is, I believe, necessary to move to a new level of explanation and treat mathematics as a social phenomenon.

## 7.1 Rule skepticism

The collectivistic approach is based on a skeptical account of rule following inspired by Wittgenstein's later writing. I will begin by presenting this account as Saul Kripke proposes it.

Kripke takes departure in the following passage from Wittgenstein: "This was our paradox: no course of action could be determined by a rule, because every cause of action can be made out to accord with the rule" (Wittgenstein, 1958, §201). What Wittgenstein aims at here, is that, under the right interpretation, any action can be made out to be in accord with a given rule. But if that is the case, how do we have the feeling that rules somehow picks out one particular action as the right one? This seems to be a paradox, and at least some kind of explanation is needed. It can be debated whether the paradox applies to all instances of rule following, but in Kripke's treatment, the rule following paradox is mainly taken to problematize the idea that rules can apply to infinitely many cases, although at any given time we have only used it in finitely many cases.

The bulk of Kripke's argument is centered on the familiar arithmetic rule 'plus'. Although the function plus is defined for all the infinitely many pairs of natural number, there exists a number $N$ such that no sums $a + b$, with neither $a \geq N$ nor $b \geq N$ have actually been performed. For convenience, Kripke imagines this number $N$ to be 57.

Given this setting, we can imagine that someone asks me to calculate a new and unknown sum, say $57 + 68$. I confidently give the answer: 125. At this point, Kripke imagines a skeptic, and the skeptic objects: How can I know that 125 is the correct answer to the problem $57 + 68$? The most straightforward answer would be to show the skeptic an arithmetic calculation, but the skeptic does not doubt my ability to calculate. He is doubting whether I can be sure, that I have based my calculation on the correct rule, i.e. whether I am following the same rule in this case, as in all the previous cases of addition, I have so far encountered. As the rule for addition has only been applied to finitely many cases, we can imagine a multitude of functions, all of which accord on all of these cases, but differs on new cases. Kripke's skeptic for instance, imagines the function 'quus' defined in the following way:

$$x \oplus y = \begin{cases} x + y, & \text{if } x, y < 57 \\ 5, & \text{otherwise} \end{cases}$$

If I had been 'quussing' instead of plussing on all previous cases, the correct answer to this new problem of 57 + 68, i.e. the answer according to the rule followed so far, would be 5 and not 125. In fact, *any* answer could be made out to be in accordance with my previous practice, given an intelligent choice of rule (as Wittgenstein noted in the quote above). So when I claim that the answer '125' is in accordance with my previous practice, I am, the skeptic claims, taking a stab in the dark. I have chosen to apply the rule for plussing, and not the rule for 'quussing' (or similar deviant rules) to the new and unknown case, but how is this choice justified?

When I am faced with the new case, I feel like I follow directions, I previously gave myself, and that these directions somehow determine the answer I ought to give in this new case. But, the skeptic asks, what could such directions consist in (Kripke, 1982, p. 10)? What fact about me or my previous behavior establishes that I meant plus and not 'quus' (or another deviant function) in the past? This is the core of the skeptical challenge.

According to Kripke, an answer to this challenge should satisfy two conditions: Firstly, it should establish a fact about me that specifies precisely which rule, I was following in my previous addition practice, and secondly, this fact must furthermore justify the answers I give in the new addition situations (Kripke, 1982, p. 11).

Kripke consider several candidates for such facts or 'self-instructions'. Firstly, I could explicitly have told myself to give the answer '125' when faced with the problem '57 + 68'. This is true, but addition is supposed to work for an infinite number of cases, and at any given time I can only explicitly have given myself the answer to finitely many calculations. So this solution does not capture the depth of the skeptical challenge, Kripke is posing.

As a second candidate, I could explicitly have described the algorithms used to calculate answers to addition tasks. This could for instance be a description of the physical operation of combining piles of discrete objects. I could give myself the direction that the result of $a + b$ is the number of objects in a pile, constructed by combining a pile containing $a$ and a pile containing $b$ objects. This answer however, presupposes the extension of another, more basic rule – the rule of counting – to new cases. But how do

I know what counting a pile of objects means? At any point in time, I have only counted finitely many samples, so the skeptical challenge can be applied to the operation of counting in exactly the same way, it is applied to adding (Kripke, 1982, p. 16). So, I have not removed the problem by describing an algorithm, only moved it to another, more basic level. A similar skeptical answer will apply to any other self-instructions in the form of algorithms; I may try to determine the meaning of a rule by stating another rule, but that does not change anything. The skeptical challenge remains unanswered.

Thirdly, I could answer that the meaning of 'plus' flows from a disposition to give certain answers (Kripke, 1982, pp. 22). When faced with a familiar task, say $7 + 8$, I am simply disposed to give a certain answer, here '15', and the same goes for novel tasks. Especially, I am disposed to answer '125' when posed the task of adding 57 and 68. This account in terms of dispositions, however, does not tell my why I am *justified* in giving the answer '125' to the problem of adding 57 and 68. On the contrary, it merely states that whatever I feel to be right, is right, and if that is the case, we are, as Wittgenstein noted, in a situation where we cannot talk of 'right' or 'wrong' anymore (Wittgenstein, 1958, §258). An account in terms of dispositions dissolves the normativity that surrounds the situations. It fails to acknowledge that there is a difference between questions such as "what would you like for lunch?" and questions such as "what is 57+68?" There is no correct answer to the first question, and any answer I might be disposed to give will suffice. This is not so in the case of the second question. 125 is the *right* answer to $57 + 68$. It is not just an arbitrary answer, I somehow feel disposed to give.

In a variant of the dispositional answer, I can imagine building a plussing machine, i.e. a machine that, given two numbers, answers with their sum (Kripke, 1982, pp. 32). In that case, the machine handles the normativity of the situation; what ever the machine answers, is the *right* answer. Or in other words: The 'disposition' of the machine determines what the correct answer is, and I can test my own answer against that of the machine. This solution to the skeptical challenge is, unfortunately, very problematic. Any mechanical device is finite, just like myself, so at some point the machine will either break down or I will encounter numbers too large for it to handle. What if the machine breaks down and starts answering '5' to all addition tasks involving numbers larger than 56. Has the machine, and consequently myself, been quadding all the time? For this reason, a machine cannot play the role of a standard. It must be build to confine with a standard, and I must be able to judge its answers to be right or wrong, just as my own answers must be either right or wrong. Simply defining the right answer to be what ever the machine comes up with, does not answer the skeptical challenge.

Normativity cannot be accounted for in terms of dispositions, neither of a machine nor of individual persons.

Fourthly, the meaning of 'plus' might be an irreducible experience, such as a headache, a mental image or a similar introspectible sensation that always confront me, when I follow the rule for 'plussing' (Kripke, 1982, pp. 40). Although I might in the past have had such a sensations each time I applied the rules for 'plus', it would not in itself, Kripke notes, tell me how to apply the rule to new cases. The most convincing case for such a introspectible state would be a mental picture of the addition table, but as we are finite beings, even such a picture would be limited, and could not guide my extension of the rule infinitely. So the meaning of 'plus' cannot be captured in introspective sensations or mental pictures.

Finally, the meaning of 'plus' could be a real Platonic object, such as an addition table containing all of the infinitely many addition task and correct answers. Even granting the existence of such immaterial, Platonic objects does not answer the skeptical challenge. My own practice is still finite, and when confronted with a new addition task, how can I be sure, that I in my previous practice was referring to the addition table, and not the equally infinite 'quaddition' table? The introduction of platonic objects does not solve the problem. It merely moves it to another level, where the choice of Platonic object as referent to a term must be justified, instead of a particular answer to a calculation task.

This is the skeptical paradox, as Kripke describes it. In his own solution of the paradox, Kripke focuses on assertability and justification conditions. An individual might believe she is following a rule. When asked about the result of $57 + 68$ I unhesitatingly answer '125' and believe this answer to be in accordance with my previous use of 'plus'. As we have seen, this belief cannot be justified by pointing to any fact about myself. According to Kripke, however, I am nonetheless licensed to give whatever answer I believe to be right, as our language game of speaking of rule following allows a speakers to follow her inclinations without giving ultimate justifications. "All we can say, if we consider a single person in isolation, is that our ordinary practice licenses him to apply the rule in the way it strikes him" (Kripke, 1982, p. 88). So when a person is considered in isolation as a private rule follower, the justification conditions only amounts to the person believing she is acting in accord with the rule.

This of course does not fully capture our concept of rule following. On the contrary, it seems to show that rule following is *not* an individual process. As Wittgenstein put it: "[T]o *think* one is obeying a rule is not to obey a rule. Hence it is not possible to obey a rule 'privately': otherwise thinking one was obeying a rule would be the same thing as obeying it" (Wittgenstein, 1958, §202). In order to grasp what rule following consists in, we must, according to Kripke, widen our gaze and consider the individual, not as an individual, but as part of a community. The community will not in general accept the individual as an authority on her own rule following practice. An answer given by an individual to a rule following problem will be judged as 'correct' if it corresponds to the answer generally accepted by the community, and as 'incorrect' otherwise. This alters the justification conditions radically; I am justified in answering '125' to the problem '57+68', not just because I think the answer is correct, but because this answer is the answer accepted by the community of adders.

I might myself be accepted in the community of adders, if I generally give correct answers to particular addition tasks, such as '57+68'. So the situation is to be seen like this: When I answer '125' to the task '57+68', my answer is not caused by a rule. On the contrary, by confidently answering '125', I show that I am competent in following the rules governing the word 'plus'. So the rule following does not cause the correct answer, instead the correct answer causes my behavior to be in alignment with the rule.

This also determines the assetability conditions of sentences such as "I mean addition by 'plus' ". As soon as my answers to addition tasks are so frequently in accordance with the answers of the community that I get the feeling that I can give the correct answers in new situations, then I am (subject to correction by others) entitled to say that I mean addition by 'plus'. Furthermore, I am (also subject to correction by others) entitled to judge my responses to new addition tasks as 'correct' with the sole justification that the responses are the once I am inclined to give (see Kripke, 1982, p. 90).

This picture of rule following is capable of accounting for the important aspect of normativity. Normativity is simply understood as conformity; as long as my answers agree with those of the community, I am right, and if my answers for some reason begin to diverge, I am wrong – viz. I am no longer entitled to assert that I am following the rule in question.

Kripke's account ties rule following closely to the agreement of a social group. But how does this general agreement emerge? Here, Kripke is remarkably – and deliberately – silent. He turns to the Wittgensteinian concept of 'forms of life': "The set of responses in which we agree, and the way they interweave with our activities, is our *form of life*. Beings who agreed in consistently giving bizarre quus-like responses would share another form of life" (Kripke, 1982, p. 96). In Kripke's view, our form of life cannot be explained any further. An explanation of our form of life would imply an explanation of why we agree that $57 + 68$ is 125, and according to the skeptical paradox, no such fact can be stated. For this reason, our agreement must be taken as a brute fact.

I will return to Kripke's solution and in particular to his stance on explanations of our form of life shortly, but first I will present two slightly different theories, both applying the collectivistic account of rule following directly to the practice of mathematics. So firstly, I will present Andrew Pickering's theory of 'disciplinary agency' and secondly David Bloor's account of mathematics as social institutions.

## 7.2 Rules as institutions

Kripke's answer to the skeptical challenge has been extended by David Bloor (2002). In contrast to Kripke, Bloor takes departure in a conception of rules as *social institutions*. Here, an institution is to be understood as a self-referring system, i.e. a system that is created and upheld by practices referring to the system itself. As an example of such an self-referring system, Bloor states the institution of property (Bloor, 2002, p. 30-31). The fact that people can own property is generated by the very fact that enough people talk about ownership and act as if it is the case that people can own property. It is in other words generated by acts referring to the very institution they constitute, and if everybody stopped engaging in the social institution of property, it would simply vanish. This makes social institutions such as property, money, royalty, marriage *etc.* into something completely different from physical objects. A physical object will exist, whether or not we know about and acknowledge it, but a persons ownership of the object only exists, if enough people acknowledge it. As Bloor remarks, social institutions are almost something magical; a group of people brings something into existence, simply by acting as if it existed (Bloor, 2002, p. 29).

The introduction of self-referring social institutions allows Bloor to give a 'straight' answer to the skeptical challenge, i.e. an answer that shows one of the premises of the paradox to be false. This is to be contrasted with Kripke's 'skeptical solution', where the paradox is accepted, but shown to be consistent with our usual practice of talking about rule following and meaning.

As we will recall, Kripke's skeptic was looking for a fact about me that could determining what I, in my previous practice, meant by the word 'plus'. Kripke claimed that such a fact could not be found. Bloor does not agree. He points out that all the possible facts considered in Kripke's skeptical argument, are individualistic facts. For this reason, the argument does not show that meaning and rule following cannot be accounted for in factual terms, only that it cannot be explained by facts about isolated individuals. But there are other types of facts. Facts, not about isolated individuals, but about social groups and social institutions.

It is a fact that (say) Jones owns a house. But what kind of fact is it? It is not a fact neither about the house taken as a physical object or about Jones taken as an individual. No inspection of either Jones or his house will reveal the ownership. This is because Jones' ownership is a social fact. It amounts to the fact that enough people acknowledge the institution of property and accept Jones as the rightful owner of the house. The ownership is in other words constituted by Jones' contact with and particular role in the institution of property. As Bloor puts it, the ownership is true *of* Jones, not *caused by* him (or the house) (Bloor, 2002, p. 65).

In Bloor's view, a similar type of fact constitutes the meaning of addition. Addition is a self-referring social institution similar to property. It is created by people being trained to, performing acts of and talking about adding. If people stopped talking about, stopped performing and stopped training others in addition, addition would simply vanish, just as ownership or royalty or marriage would vanish, if people stopped acknowledging the existence of these institutions. So, the fact that I mean addition by 'plus' is constituted by social facts about my membership of and participation in the institution of addition, just as Jones' ownership of his house was constituted by his participation and role in the institution of property:

> The (finite) content of the state of meaning [addition], or the act of meaning, derives from the actor's real or perceived contact with the institution of adding. The institution itself only exists in virtue of the whole nexus of similar actions and references and behaviours by the other participants.
>
> (Bloor, 2002, p. 67)

And similarly, the truth that $57 + 68$ is 125, is a truth about this institution. I am right in claiming that $57 + 68$ is 125, because I have a certain perceived contact with the institution of adding, and 125 is the institutionalized answer to $57 + 68$.

In Kripke's story, the ambiguity of the rules for addition arose, because the particular addition task considered was a novelty. If that is the case, how can the answer be embedded in an institution? Will the members of the adding institution still not have to face the ambiguity between – and hence different extensions of – 'adding' and 'quadding'. To this, Bloor simply answers "what ambiguity? There is no ambiguity. Within the practice so described there is no double meaning attached to the word ‘add’. There is no surmise and no doubt. There is just one thing that they rightly do, namely give *this* answer to *this* question, and *that* answer to *that* question”(Bloor, 2002, p. 69). So, as long as the community of adders agrees in their practice, there is no ambiguity. The possibility of ambiguity only arises, if the community of adders becomes aware of the two possible descriptions of their past practice (as either adding or quadding) – and hence of the two possible extensions of the practice in the new case. The community is, however, completely free to decide, and whatever they decide to be correct, is correct. And if they agree that $57 + 68$ is 125, this agreement in itself counts as a proof that they all along meant ‘adding’ and not ‘quadding’ by ‘plus’ (Bloor, 2002, p. 70). So as long as they agree, even on what answers to give to the new problem, there is no ambiguity in their practice.

Bloor readily admits that his institutional account is a form of community-wide dispositional theory. A social institution can simply be seen as “the collective product of the interactions between the dispositions of many individuals” (Bloor, 2002, p. 68). As we will recall, Kripke’s skeptic dismissed individual dispositions as candidates for facts constituting meaning, on the ground that individual dispositions could not account for the perceived normativity. So what about community-wide dispositions? Doesn’t the problem of normativity arise again, only at the community level? It does, Bloor admits, but the community is beyond right or wrong. If the group of people sharing the institution of adding deems “125 to be consistent with their past meaning, then it is consistent – because meaning is an institution, and an institution is what people make it” (Bloor, 2002, p. 70). Normativity only exists within an institution. My individual dispositions and answer to a particular addition task can be judged right or wrong in comparison to the answer deemed right by the others participating in the institution of adding, but ultimately, the institution itself cannot be compared to anything outside itself. There is no golden standard to compare the decisions of the community against. Institutions are self-referring practices. They provide normativity for their participants, and that is all the normativity there is.

### 7.2.1 Institutions as a source of objectivity

The answers given by Kripke and Bloor might seem very similar. They both agree on the basic claim that rules, normativity and meaning cannot be accounted for by looking at isolated individuals, but essentially involves the shared practice of a community. There is however, at least one important difference between them. As Kripke accepts the skeptical challenge, he is forced to give up the idea that our practice of talking about meaning and rule following is fact-stating. This is why he is only able to state assertability and justification conditions, and not truth or correspondence conditions. In Kripke's view, I can be entitled to make statements like "I mean addition by 'plus' ", but I can never claim it to be a *fact* that I mean addition by 'plus'. On Bloor's account on the other hand, it is a *fact* that $57 + 68$ is 125, similar to the fact that Jones owns his house or that Margrethe II is the queen of Denmark. It is a fact, not about me as an individual, but about the social institution I participate in. On Bloor's account in other words, the dichotomy between referring to an independent reality and not referring to facts at all, accepted by Kripke, is false, as it leaves out the possibility of self-reference, i.e. reference to social institutions (Bloor, 2002, p. 68).

The implication of Bloor's acceptance of social facts is fully exploited in one of his earlier works (Bloor, 1991). Here, Bloor addresses Frege's search for the objectivity of mathematics. As explained in section 3.2.4, Frege and Husserl notices the objectivity and normativity connected to mathematical objects and truths. This objectivity can neither be explained, if we take numbers to be properties of physical objects nor if we see numbers as the products of subjective psychological factors. Consequently, Frege and Husserl conjectured the existence of a realm of ideal, mathematical objects having objectively given properties.

Given Bloor's institutional account of rule following and meaning, another possibility becomes visible. The third type of objects, which are neither individual psychological states nor physical objects could just as well be social institutions. Social institutions are neither physical objects nor merely individual, psychological states or dispositions. They are external to us as individuals. They form a social reality, and for that reason they can account for the experience of normativity and objectivity, Frege and Husserl notices so well; we cannot at will create numbers with the properties that suits us. We are restricted by something that is not our own will. So according to Bloor,

> The conclusion is that the way to give substantial meaning to Frege's definition of objectivity is to equate it with the social. Institutionalized belief satisfies his definition: this is what objectivity is.
> (Bloor, 1991, p. 98)

### 7.2.2 Institutions and social causes

There is another essential difference between the answers to the skeptical challenge given by Bloor and Kripke. As mentioned above, Kripke refrains from giving causal explanations of our form of life. He leaves our agreement to follow one rule and not another completely and intentionally unexplained. As we shall see shortly, something similar is seen in the account given by Andrew Pickering. Here, institutions are seen as emerging in a non-reducible way from the interaction between our projects and intentions on the one side, and the already accepted institutions on the other.

To Bloor, however, there is another possibility. Bloor embeds meaning and rule following in social facts, and social facts can be given sociological explanations – or at least one can meaningfully look for them. This allows Bloor to meaningfully apply the ideas of the so-called 'strong program' to the philosophy of mathematics.

The strong program, in brief, refers to a particular approach to the sociology of scientific knowledge, proposed by Bloor in association with Barry Barnes and Harry Collins amongst others (see for instance Bloor, 1991; Barnes *et al.* , 1996). The strong program demands the investigation of scientific knowledge to be *causal*, i.e. "concerned with the conditions which bring about beliefs or states of knowledge" (Bloor, 1991, p. 7), *impartial*, i.e. investigate and explain both true and false, rational and irrational beliefs, *symmetric*, i.e. seek the same types of explanations for true and false, rational and irrational beliefs, and *reflexive*, i.e. "its patterns of explanation would have to be applicable to sociology itself" (Bloor, 1991, p. 7). Although the precise type of causes are not specified in the tenets of the strong program, in the actual application of the program Bloor and colleagues are mainly focusing on social causes and on showing how scientific beliefs are caused by social interests. Consequently, the strong program in its actual realization is part of the sociological program claiming scientific knowledge to be a social construction.

The strong program has been met with stark criticism as basic for investigating and explaining the empirical sciences (see e.g. Collin, 2003). To mention only one point of criticism, the basic idea (of the social constructivistic approach) of using empirical studies to show that the knowledge claims made by the empirical sciences are caused by social facts, and not by correspondence with an independent reality, seems to have insurmountable reflexivity problems. How are we supposed to evaluate such a claims? The claim is made by an empirical science (sociology) and is the result of an empirical investigation, so should we take the claim to be true (i.e. corresponding to the independent social reality they it is meant to describe) or to be itself the product of social facts causing the sociologists of science to make this claim? If we believe the sociologist of science to be right, we simultaneously rob her of her empirical justification. From the outset, the strong program seems to presupposes a fundamental asymmetry between social and other types of facts. We have access to social facts, they can cause our beliefs – and sociologists can describe them rightfully –, but how other types of facts enters the equation is somewhat less clear.

Mathematics, however, is not an empirical science, so the idea of seeking social explanations of mathematical beliefs seems to evade at least the reflexivity problem. There might of course be other problems, but let us at least see some of the explanations given by Bloor, before we evaluate them and his general approach.

I will start by introducing Bloor's conception of social causes, as it is presented in his treatment of four case studies (in Bloor, 1991, p. 110–130). As noticed above, the strong program does not explicitly demand the causes which "bring about beliefs or states of knowledge" to be social. However, Bloor makes his focus on social causes clear before he sets upon the actual cases: "I shall offer illustrations of four types of variations in mathematical thought each of which can be traced back to social causes" (Bloor, 1991, p. 110). This being said, Bloor does seem to allow the possibility that other than social factors can influence mathematics. He mentions for instance, how mathematics can be affected by experiences, habits, patterns of behavior and psychological processes (see e.g. Bloor (1991, p. 154-5); Bloor (2002, p. 20)). However, as it turns out Bloor only describes social causes as explanations for the mathematical beliefs, he describes, and he does seem to consider social causes to be privileged in comparison to the other possible sources of influence mentioned above.

The four cases considered by Bloor in (1991) are: 1) the status of 'one' as a number, 2) number mysticism 3) the proof that $\sqrt{2}$ is irrational and 4) the status of infinitesimals.

The first two cases are connected, and address the fact that mathematicians through the ages have had very different conceptions of what numbers are. In the classical conception, numbers were associated with pluralities of discrete units. From this point of view, it can meaningfully be questioned whether 'one' is a number. Aristotle for instance argues that 'one' is the unit, i.e. the measure of some plurality, whereas 'number' is a measure of the plurality of units. For this reason 'one' cannot be a number, as the unit is by definition not a plurality (*Metaphysics* book N 1087b33-1088a14 Annas, 1976, p. 117). As it turned out, this conception of number changed, and as described in section 6.12.1.2, a different conception of number, associating numbers with locations in space (i.e. on a directed line) were introduced in the 16th century.

In section 6.12.1.2, I were mainly concerned with the cognitive mechanisms in the form of conceptual metaphors and blends involved in this change in the conception of number. Bloor however, draws attention to the social factors also involved in the change. According to Bloor, the Greek mathematicians holding the classical conceptions of number were concerned with mathematics from a purely theoretical or even philosophical point of view, whereas several of the leading mathematicians of the 16th and 17th century were engineers and physicists (Bloor only gives the Dutch inventor of logarithms, Simon Stevin (1548–1620), as an example, but the same holds true of several others of the leading figures). These men were not (or at least not only) interested in numbers for their metaphysical significance. They wanted to use numbers for measurements of distance, movement and processes of change, and according to Bloor, that paved the way for a change in the conception of numbers.

The last two of the case studies concern the variation in the demands on rigor and in the interpretation of pieces of deduction. In both cases, Bloor draws attention to the fact that there are no eternal and objective standards of rigor or of what counts as a valid argument. For this reason, the knowledge claims made in a mathematical proof are not successful only because of the proof in itself, but because the proof makes use of accepted and meaningful forms of reasoning: "Certain conditions have to obtain before a computation has any meaning. These conditions are social in the sense that they reside in the collectively held system of classifications and meanings of a culture" (Bloor, 1991, p. 124).

What Bloor claims here, is that mathematics is always set in broader context of purposes, interests, values, standards, training, accepted interpretations and meaning giving metaphors. This is not unlike the Kuhnian idea of *paradigms*, as Bloor notes – although he does not pursue the analogy further (Bloor, 1991, p. 129). As the cases are meant to show, changes in this broader, culturally shared context might lead to (or facilitate) changes in the content of mathematics. One could perhaps argue that Bloor does not explain why these changes came about; engineers and physicists suddenly became interested in mathematics, the standards of rigor constantly change, but why did they do that? Bloor does not give an explanation. But that is, as I see it, less important in this connection. The important thing is to note, that mathematical practice is always performed in a wider social setting, and properties of this setting can (however they came to be) influence the mathematical practice. That is, as I see it, how Bloor understands 'social causes' at this place.

I will get back to and discuss this conception of social causes, but first

I will point out, that Bloor seems to operate with two other very different conceptions of social causes, both of which are more in line with the social constructivistic ideas, often connected with the tenets of the strong program and the sociology of scientific knowledge.

The second conception of social causes is found in Barnes, Bloor and Henry (1996). Here, Lakatos' idea that $2 + 2$ might equal 5 in some cases is discussed. The idea origins in the dialog of *Proofs and Refutations*, where the dialog partner Kappa makes the following suggestion: "In certain cases two and two makes five. Suppose we ask for the delivery of two articles each weighing two pounds; they are delivered in a box weighing one pound, then in this package two pounds and two pounds will make five pounds" (Lakatos, 1976b, p. 101). Barnes *et al.* see this as a real challenge. Why do we perform what Lakatos calls 'weightless addition', i.e. why do we hold that $2 + 2 = 4$ and not 5 or some other thing? According to Barnes and colleagues, it must be for a sociological or psychological reason. They do, however, not search for psychological reasons, but instead give the outline of a sociological explanation. From a sociological point of view, they explain, "to establish a convention for adding means solving a coordination problem, that is, it means getting everybody to adopt the same procedure" (Barnes *et al.* , 1996, p. 185). Such problems are most easily solved, if there is a 'salient solution', i.e. a solution that is plainly visible to everyone, and weightless addition is exactly such a solution:

> Salient solutions are often extreme solutions, ones which lie prominently at the beginning or the end of the spectrum of alternatives. Weightless addition may be such an extreme and prominent solution. There are therefore pragmatic reasons connected with the organization of collective action that would favour saying $2 + 2 = 4$, rather than $2 + 2 = 5$ or 6 or 7 or .... As a convention it is probably easier to organize than the others, and therefore more likely to arise historically.
>
> (Barnes *et al.* , 1996, p. 185)

So here, social causes are understood as sociological facts about group dynamics and facts about how social groups establish organized behavior.

The third conception of social causes used by Bloor is established in a detailed case study on the different attitudes towards the status of symbolic calculations hold by William Rowan Hamilton on the one side, and the Cambridge school of symbolic algebra, counting members such as Peacock, Babbage and Whewell, on the other (Bloor, 1981). According to Bloor, these different attitude towards algebra were caused by the actor's different political and social ideas.

Hamilton was involved in a movement inspired by German idealism, and following the ideas of Kant, he saw algebra as a synthetic *a priory* investigation of pure time. This idealist movement operated with a hierarchy of mental faculties. In this way, the faculty of understanding was seen as subordinate to the faculty of reason. As science, according to the idealists, belonged to the first-mentioned, and religion and morality to the last, the hierarchy of mental faculties resulted in a similar hierarchy in knowledge forms – science was considered to be subordinate to moral and religion. By fitting algebra into this ontology, Hamilton managed to see it as dependent on the higher faculty of reason, and consequently as subordinate to religion and morality (see Bloor, 1981, for details).

According to Bloor, this localization of algebra in the hierarchy of knowledge forms was not a purely academic exercise. It served a practical purpose as well. Hamilton was a conservative, and by interpreting algebra in the light of idealist metaphysics, he managed to see algebra in a way that was consistent with his social values: "[the idealist interpretation's] practical import was to place mathematics as a profession in a relation of general subordination to the church. Algebra, as Hamilton viewed it, would always be a reminder of, and a support for, a particular conception of the social order" (Bloor, 1981, p. 217). So, Hamilton's conception of algebra was, in other words, caused by his political ideas and values.

The Cambridge school of symbolic algebra on the other hand, advocated a formalist conception of algebra (or rather; what is now with a slight, but common, anachronism classified as a formalist conception of algebra). They saw algebra as a closed system of manipulation with written symbols, which had no relation to anything outside itself; symbolic algebra, in their view, was a self-sufficient and autonomous system. According to Bloor, this conception of algebra also has to be seen as a statement of social values. The members of the Cambridge school were "reformers and radicals" (Bloor, 1981, p. 222). They rebelled against tradition and authority, and by imposing self-sufficiency and independence onto their symbols, they were freeing themselves of external sources of authority and control. As Bloor summarizes:

> Stated in its broadest terms, to be a formalist was to say: 'we can take charge of our selves'. To reject formalism was to reject this message. These doctrines were, therefore, ways of rejecting or endorsing the established institutions of social control and spiritual guidance, and the established hierarchy of learned professions and intellectual callings.
>
> (Bloor, 1981, p. 228)

The rival conceptions of algebra were in other words used to send different political messages. They were used as tools in a game of power and social control. "[I]n our social life we are always putting pressure on our fellows and seeking to evade that pressure ourselves [...]. In order to do these things we try to make reality our ally, showing how the nature of things supports the status quo, or how the established social order is at odds with what is natural" (Bloor, 1981, p. 230-1). So at this place, the social facts causing the different conceptions of algebra are the actors' interests in different types of social order. The different conceptions of algebra are simply used as means of furthering the social interests of their holders.

I will get back to and discuss these different conceptions of social causes below. At first, I will however present the theory of Andrew Pickering, where the origin of mathematical institutions is given yet another explanation.

### 7.2.3 Disciplinary agency

Like Kripke, Andrew Pickering (1995) identifies the normativity of rule following with the responses generally accepted by a community of rule followers. Pickering however, puts emphasis on the role played by training. In his view, rule following is constituted by a process of training, where the individual is *disciplined* by the community into responding mechanically in a particular way. He introduces the term 'disciplinary agency' to describe the force this kind of training can exert on an individual.

Pickering introduces the concept of disciplinary agency as a counterpart to the concept 'material agency'; in the empirical sciences such as physics, the intentions and wishes of the scientist is opposed by the 'agency' of the material world, she tries to handle and get particular responses from. We cannot simply impose our will on the material world and make it behave as we want to. The agency of the scientist seems to meet with another agency – the agency of the material world. As a consequence, science is produced in an open-ended process, where the ideas and hypothesis of the scientists are constantly 'mangled' (as Pickering expresses it) in the encounter with the material agency of the physical world.

According to Pickering, something similar is taking place in the development of mathematics. Mathematicians cannot do just anything. Their ideas and plans are met with some kind of opposition similar to the opposition facing physicists and chemists, and that opposition is precisely the disciplinary agency. So disciplinary agency accounts for the normative aspect of mathematics. There is a right and a wrong in mathematics, and what counts as what is decided by the community. When the general opinion of the social group is internalized by the individuals through training, it takes the form of an agency – disciplinary agency – that gives us a clear sense of what counts as right and wrong in a given situation. In fact, most of the time, it simply makes us act unhesitatingly in conformity with the rule.

In connection to mathematics, Pickering's main contribution is to describe the collective development of mathematics and disciplinary agency. In Pickering's view, mathematics is developed by extending well-known cases into new domains. The extension process has three stages. Firstly, a *bridgehead* must be established. The bridgehead fixes both the domains to be worked on and how the elements of the well-known domain are to be transferred to the new domain. Secondly, a *translation* takes place. Here, structure and rules of operation are transferred (or translated) via the chosen bridgehead from the old to the new domain. Lastly, the rules of operation of the old domain might not fully determine all of the possible moves in the extension. If that is the case, a process of *filling*, where the undetermined moves are fixed, takes place (Pickering, 1995, p. 115–6).

According to Pickering, the three stages of the extension process, not only reflect changes in the type of processes going on, but also (and more importantly) in the distribution of agency. In short, the choice of bridgehead and process of filling are 'free moves', i.e. moves where the mathematician is free to act and make choices of her own. The translation stage on the other hand, is a forced move determined by disciplinary agency.

This interplay of agency is clearly visible in a model-case presented by Pickering (Pickering, 1995, p. 121–41). The case is William Hamilton's development of the system of quaternions. As is well known, the quaternions were the unexpected outcome of Hamilton's attempt to develop a three-place algebraic description of three-dimensional space. As earlier described (subsection 6.12.1.2.3), an analogy between the two-dimensional plane and complex, two-place algebra was generally accepted during the first decades of the 19th century. Following this example, Hamilton wanted to create a three-place algebra that could be used to create an analogy to three-dimensional space. So in other words, Hamilton wanted to extend both the two-place, complex algebra to a three-place algebra, and the corresponding geometrical interpretation from a two- to a three-dimensional model.

Hamilton tried to extended the complex, two-place algebra to a three-place algebra simply by adding another imaginary unit. So where a standard complex number $s$ has the form $s = x + iy$, where $x, y \in \mathbb{R}$ and $i = \sqrt{-1}$, a number $t$ in the three-place algebra should have the form $t = x + iy + jz$, where $x, y, z \in \mathbb{R}$ and $i = j = \sqrt{-1}$. This is the bridgehead of the extension. Hamilton here made a free choice of how he wanted to extend the well-known two-place algebra. So in this phase of the extension, Hamilton was in control.

In the next stage – the stage of translation – Hamilton had to determine what the basic rules of operation for the new three-place algebra should look like – and here, he was no longer in control. As an example, he determined the square of a number $t$ in the three-place algebra to be:

$$t^2 = x^2 - y^2 - z^2 + 2ixy + 2jxz + 2ijyz$$

This equation was not the result of a free choice on Hamilton's side. It was a result produced by applying the well-known and already accepted rules of standard algebra to the expression $(x + iy + jz)^2$. In Pickering's words, "[a]nyone already disciplined in algebraic practice, then or now, can check that Hamilton (and I) have done the multiplication correctly" (1995, p. 129). So the result, in other words, was (and still is) determined by disciplinary agency.

As it might be noticed, one term of the equation above, however, is not completely determined by previous practice. Previous practice does not tell us what the product of the two imaginary units $i$ and $j$ should be. So here we have once more a free move – a *filling* move –, where Hamilton had an actual choice to make.

Apart from the extension of two- to three-place algebra, Hamilton was simultaneously trying to extend the two dimensional interpretation of complex algebra to a three-dimensional interpretation of his new three-place algebra. These two extensions in combination left him with several filling moves, and Hamilton tried to create the desired fit between the geometrical and the algebraic extensions, by filing in the undefined moves in different ways. As a part of this process, Hamilton decided to give up the rule of commutativity. Until then, it had been a standard rule in algebra, that $a \cdot b = b \cdot a$. Hamilton however, decided to define the new and unknown product of two imaginary units in a way, so $i \cdot j = -j \cdot i$. This move solved some of his problems, but eventually the difficulties facing Hamilton proved too great. He abandoned his original goal of extending complex algebra into a three-place algebra, and instead decided to change his bridgehead and extended it into a four-place algebra, by introducing yet another imaginary unit $k$, defined as the product of $i$ and $j$. The imaginary units of this four-place system furthermore embodied the property of non-commutativity, so for instance $ij = k$, but $ji = -k$. This four-place algebra, usually called the *quaternion* system, turned out to have the required geometrical interpretation, but only to four-dimensional space, and not to three-dimensional space, as Hamilton originally wanted (or at least it did not do so in any straightforward way).

According to Pickering, the case shows how mathematics is developed through a 'dance of agencies'. Hamilton did not reach the goal he set out to reach. He achieved something different. His wishes and intentions met resistance in the form of disciplinary agency, and consequently were changed or mangled. Disciplinary agency on the other hand was also mangled in this meeting, as something new in the form of a non-commutative elements were introduced. Furthermore, Hamilton's eventual success led to the stabilization of the choices he had made; they were accepted and considered as being right, because they lead to a success.

What interests us here, is mainly the origin of the conceptual practice embodied in disciplinary agency. Pickering takes this case study to show that the development of a conceptual practice is an open-ended process that is not completely determined by any of the individual human agents involved in it. Rather,

> [c]onceptual practice thus has the quality [...] of a dance of agency, this time between the discretionary human agent and what I have been calling disciplinary agency. The constitutive part played by disciplinary agency in this dance guarantees that the free moves of human agents – bridging and filling – carry those agents along trajectories that cannot be foreseen in advance, that have to be found out in practice.
>
> (Pickering, 1995, p. 139)

So the conceptual structure itself is an "upshot of the mangle" (Pickering, 1995, p. 140). It emerges in a non-reducible way from the intersection of human agency and already accepted conceptual structures. Pickering furthermore stresses, that conceptual practice and culture in broad in both mathematics and science in general, is controlled by nothing outside of itself. The 'nothing' here specifically covers both social factors and the physical world:

> I want to stress that on my analysis *nothing* substantive explains or controls the extension of scientific culture [defined by Pickering to include mathematics]. Existing culture is the surface of emergence of its own extensions, in a process of open-ended modelling having no destination given or knowable in advance. Everything within the multiple and heterogeneous culture of science is, in principle, at stake in practice. Trajectories of cultural transformation are determined in dialectics of resistance and accommodation played out in real-time encounters with temporally emergent agency.
>
> (Pickering, 1995, p. 146)

So according to Pickering, the conceptual practice of mathematics is nothing but an emergent and non-reducible feature of the 'dance' between human and disciplinary agencies.

## 7.3 Impact of the social

Let me start the concluding discussion with a brief recapitulation. I took departure in Kripke's rule skepticism. According to Kripke, rule following cannot be an individual or 'private' activity. Following a rule, is following it either rightly or wrongly. As no fact about me as an individual can determine what is right or wrong, rule following necessarily presuppose contact with the shared practice of a group. Kripke, however, is reluctant to explain how a particular rule for, say, addition, gets accepted as being correct. David Bloor and Andrew Pickering agree with Kripke's fundamental analysis, and they furthermore give two different explanations of how rules gets established in mathematics. To Pickering, rules (embodied in disciplinary agency) are simply the outcome of a process of interaction between already established rules and the intentions of individual mathematicians. This process is not influenced by any external factors. Bloor on the other hand, sees the rules of mathematics (in the form of self-referring institutions) as the product of different types of external, social causes.

I will open the discussion by consider two lines of criticism against the collectivistic approach to rule following, a radical and a less radical. The radical line of criticism accepts that rule governed behavior presupposes some kind of feedback from a source external to the rule-follower, but claims that this feedback does not necessarily need to be from a social group of fellow rule users. It can just as well be from the physical world. The less radical line of criticism accepts that contact with a social group is constitutive for rule following, but rejects the idea, that the rules of mathematics consequently are arbitrarily created social constructs; even though a rule is created by and can only exist due to a social group of fellow practitioners, it can be used to represent a reality other than the social reality of the group, if that is the use of the rule intended by the social group.

Starting with the first line of criticism, it seems that the collectivist approach considers feedback from our social surrounding to be different from feedback from our physical surroundings. But why is that so? It seems to be a basic fact about humans – and most other animals – that we can learn i.e. adjust our behavior in accordance with regularities, perceived in our surroundings (be it social or non-social). Even simple computer programs, such as the so-called *neural networks* modeling some of the basic mechanisms of biological brains, have this capacity (with some limitations, see e.g. Rumelhart & McClelland 1986; Johansen 2003). So our ability to learn from experience appears to be a purely biological fact based in the structure and function of our nervous system.

Let us go back to the experiments on animals discussed in chapter 4. Here, we saw for instance how rats, in the experimental setup used by Mechner, were able to learn to apply a given number of presses to a lever, in order to receive water (Mechner, 1958a,b). Apparently, the rats did not have to be in contact with any kind of social group in order to learn simple rules such as 'press lever $A$ 18 times or more, then press lever $B$'. If a rat can learn rules of this kind only by experiencing regularities in its physical surrounding, why shouldn't I be able to learn rules, such as the rules for counting or adding, by making similar observations of, say, the regularities governing piling of discreet objects?

The answer is, as I see it, that the rat is not really following a rule. It is trying to adapt to its environment. We can describe the rat's behavior in terms of rule following, saying for instance "oh, it got it wrong. It only pressed the lever 17 times, and not 18 as it was supposed to", but by doing so, we bring the rat into contact with our own rule following practice. We measure the rat's behavior by our own golden standard of what it means to count to 18 rightly. The rat on the other hand, is not trying to behave in accordance with any counting standards. It is simply trying to get water by interacting with and investigating its environment. This includes the approximately repetition of patterns of behavior that previously lead to success, but that in itself does not constitute rule following. It is more appropriately described as part of an adaptive strategy of exploiting the possibilities of the environment. Such a strategy can be described as successful or not, but not as right or wrong.

The rat's behavior is completely tied to the dispositions of its environment – in this case to the machine. Rule following proper is, as I see it, something else. As noted above, rule following presupposes an element of normativity; if I follow a rule, I can do it rightly or wrongly, and if I use rules to interact with the physical world, I must be able to judge whether the world follows the rule as expected or not. I cannot simply define 18 as "the number of times I have to press the lever in order to get water". If I operate Mechner's machine by following a rule such as: "press lever $A$ 18 times", then I must be able to say to myself: "the machine is malfunctioning, it didn't give me water as it was supposed to – or perhaps it is following a different rule now". Similarly, I cannot simply define the rules of addition as the regularities observed during particular instances of object piling. I must be able to say, “these objects do not behave in the right way, they are somehow malfunctioning or following another rule than addition” (this could happen, if I was working with, say, drops of mercury). Rule following in other words presuppose an element of detachment from individual instantiations of the rule in the physical world. It presupposes the acknowledgment and use of a standard, which lies beyond any particular instances of regularities observed in the physical world. Unfortunately, I cannot provide this standard myself as an individual – that would lead to the identification of the rule with my accidental dispositions. The only way such a standard, that lies beyond both my own accidental dispositions and particular instances of regularities in the physical world, can be provided is by contact with a social group.

So even though we can learn to repeat patterns of behavior by observing and interacting with regularities in the physical world, that in itself does not turn us into rule followers. Following a rule presupposes the kind of normativity relative to an abstract standard that only contact with a social group can provide. In Bloor’s words (in personal communication on the rat-example): “We can do whatever the rat can do – and then some”; just like the rat, we can learn from regularities in the world, but by using these regularities as the foundation of a social institution, we can also create an independent vantage point that allows us to evaluate the perceived regularities, and not only follow them blindly.

So far at least, I agree with the collectivistic approach to rule following; contact with a community of fellow practitioners is a necessary condition for rule following practice. This brings us to the second line of criticism: Given that contact with a community is a necessary condition for rule following, does it follow that the rules themselves are social constructs? According to Pickering, we cannot really account for the rules – they simply emerge in the dance of agencies. According to Bloor, the rules are (at least in part) produced by social causes of various types.

Beginning with Pickering, Pickering seems to claim that the rules and laws of operation accepted in mathematics is the somewhat arbitrary emergent product of the ‘dance of agencies’. I will not rule out that this could be so in some instances. However, in the only case provided by Pickering, it clearly seems *not* to be the case. Hamilton is tinkering with the rules of abstract algebra, and his moves are partially restricted by the already accepted laws. So far, Pickering is right. Nevertheless, Hamilton is also restricted by something besides disciplinary agency. He is trying to create a mathematical structure capable of representing or corresponding to fundamental structures of the actually experienced three-dimensional space. He is not just tinkering aimlessly, and the rules he arrives at are not arbitrary. They are part of a mathematical structure that is capable of representing properties of the physical world.

Although Pickering claims that the free moves made by Hamilton were 'stabilized' due to his success, it should be noted that the quaternian system was soon abandoned (or rather made obsolete) by the introduction of vector theory. The theory of vectors provided the wanted algebraic treatment of three-dimensional space, and consequently was favored over the theory of quaternians, which does not provide such an interpretation. This shows that the 'agencies' described by Pickering are not just dancing aimlessly around. The dance is constrained by our interest in creating mathematics, in this case in an interest in describing three-dimensional space in a convenient way. On a more general note, it is worth noting that Pickering only considers modeling in the form of extension from one mathematical domain to another. As we saw in chapter 6, there is also another type of modeling going on in mathematics. With meaning creating metaphors, mathematics is connected, not only to previously constructed mathematical theories, but also to structures in the real world. As we also saw, this mechanism has an impact on the development of mathematics by being instrumental in getting a particular piece of technical deduction accepted as valid. So also in this way, mathematics is connected to and constrained by structures experienced in the real world.

Turning to Bloor, he suggests, as noted above, several slightly different types of social causes, which – in his view – shapes the content and rules of mathematics. Starting with the case study on Hamilton, Bloor suggests that the metaphysical conception of mathematics is used as a tool in a power struggle. So the metaphysical conception is other words caused by the wish to propagate a particular set of social norms. This conception of social causes is clearly a variant of social constructivism. Mathematics – or at least the metaphysical conception of mathematics – is mainly used as an instrument for promoting narrow group interests.

In my view, there are two fundamental problems with this case study. Firstly, according to Bloor, Hamilton and the Cambridge group *agreed* on the level of technical detail, although they disagreed about the nature and symbolic significance of algebra (Bloor, 1981, p. 203). Bloor states this fact in order to show that the conception of algebra is underdetermined by the technical practice (and hence, it must be determined by something else, *viz* social interests). So according to Bloor, there is no causal arrow going in the direction from the subject matter of algebra to the metaphysical interpretation of the discipline. However, the fact that mathematicians can participate in and agree on a shared technical practice, despite severe differences in opinions about the interpretation and significance of that practice, also casts doubt about the causal link going the other direction, i.e. from metaphysical conceptions to the technical practice of algebra. Consequently, even if social interests shape the metaphysical outlooks in the way described by Bloor, the causal impact does not seem to reach the subject matter of algebra – at least not in this particular case.

Secondly, as noted by Pickering (Pickering, 1995, pp. 147-156), it is questionable to what extend the metaphysical conceptions in this case is in fact caused by social interests. The problem for Bloor, is that Hamilton changed his metaphysical views over time, and came to a partial acceptance of formalism. Bloor does address this fact, and he explains it by pointing out that Hamilton gradually loosened his political views as well. However, a correlation between two phenomena does not reveal the nature or even existence of a causal link between them, so Bloor's explanation is not fulfilling. Pickering on the other hand tries to examine what actually caused the change in Hamilton's metaphysical conceptions. As it turns out, Hamilton's partial acceptance of formalistic metaphysics came shortly after his discovery and work on the quaternian system. When doing this work, Hamilton struggled to find acceptable interpretations, and consequently had to trust the rules of operation he was working with. In other words, Hamilton was practicing algebra as a rule-governed game with uninterpreted symbols – in accordance with the formalistic conception of the discipline. For this reason, Pickering finds it likely that the transformation in Hamilton's metaphysical position was in fact caused by the discoveries, he had made in his technical practice (Pickering, 1995, p. 155-6). So apparently, metaphysics is not – or at least not only – determined by social interests, it can also be influenced by the technical practice itself.

All of this casts doubt on the influence social causes in the form of political and social values have on mathematics. Judging from the case study provided by Bloor, it does not seem likely that the social interests of the practitioners shape the technical practice of algebra.

The next conception of social causes used by Bloor (*et. al.*), is the identification of social causes with sociological facts about how social groups solve coordination problems. This conception was exemplified by the suggestion, that our accept of 4, and not 5 (or any other thing else) as the correct answer to $2 + 2$, was caused by the fact that 4 is a 'salient' solution to the problem, and hence easier coordinate as the accepted solution. This conception of social causes severs mathematics from any ties to the physical world. Mathematics simply becomes an expression of properties of how social groups organize their behavior. As it is the case with the above type of causes, i.e. causes in the form of political and social interests, accepting this type of causes as the source of mathematics, will lead to a form of social constructivism. The difference is, that in connection to this type of causes, mathematics is not deliberately shaped to suit particular group interests, but unconsciously determined by facts about the organization of social groups. If the laws of mathematics are caused by facts about social coordination, the content of mathematics is clearly separated from any contact with the physical world.

The introduction of this type of social causes is in my view questionable. To stay with the example at hand, the most obvious explanation of our acceptance of 'weightless' addition (in Lakatos' definition), is the fact that the behavior of most of the discrete objects of importance to us in our environment, can be successfully described by weightless addition, and not by the alternative rules suggested by Lakatos. If our environment had been radically different – or we had had radically different interests or modes of interaction – our choice in rules of addition might also have been different. But they are not.

Bloor's response to this type of argument is that physical objects can be arranged in many ways, so even though the laws of arithmetic takes departure in experience of operations with physical objects, there is still an element of choice:

> Of all the countless games that can be played with pebbles, only some of the patterns that can be made with them achieve the special status of becoming 'characteristic ways' of ordering and sorting them. In exactly the same way, all the countless possible patterns that may be woven into a rug are not equally significant for a group of traditional weavers. There are norms for those who would weave carpets just as there are norms for those who would learn mathematics.
>
> (Bloor, 1991, p. 99-100)

And this brings the social back into the equation. Physical nature does not have the authority to give a particular set of patterns the stamp: 'characteristic'. This is an authority the social group can make only for itself.

In a similar vain, Bloor presents the following model of the relationship between our psychology, social interests and the language-games, we accept. Here, box no 1 represents non-verbal, intuitive logical instincts or innate propensities for making certain inferences or accepting something as correct. Box no 3 represents codified and accepted language games. The relationship between these two boxes is mediated by box no 2, consisting of 'interests

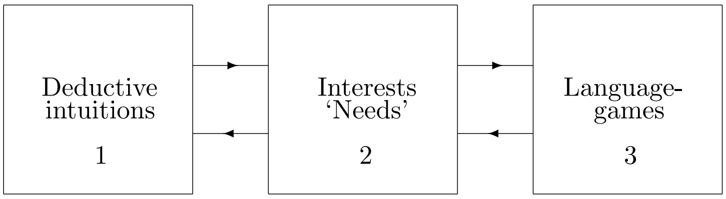



**Figure 7.1:** Relationship between the psychological, the social and institutions. Redrawn from (Bloor, 1983, p. 135)

and needs'. So intuitions only become institutionalized language games after being refracted through our needs and interests. This corresponds to the situation described above, where only some patterns of manipulations of pebbles becomes accepted as significant by the authority invested in the social group. The question of course is, what these interests and needs consist in. Bloor at this place explicitly and exclusively defines them as "social interests", such as the interest a sub-group of the community might have in preserving a particular cultural achievement, such as the propositional calculus. Bloor concludes: "My claim is that a pursuit of the causes that make us deploy our intuitions one way rather than another, leads straight to social variables of this type" (Bloor, 1983, p. 136).

What Bloor brings into battle here is the last conceptions of social causes presented above, i.e. the conception of social causes as the wider context of interests, purposes, values *etc.* mathematical knowledge is always set within. I agree with Bloor so far that only some of the structures we experience in the physical world are used as the basis for institutionalized mathematical rules, and only some of our intuitive ideas about logical inferences are accepted as valid by the community. Social variables and facts do play a part in the development of mathematics; if we want to explaining why the number line got accepted as a valid understanding of the number system, it is not enough to point to the fact that we have the necessary cognitive resources needed in order to create the Number-line blend. We must also look to the factors in the wider context that led to the acceptance of this particular conception of numbers – Bloor has a good and important point here.

Where I disagree with Bloor, is in the (apparent) identification of needs and interests with the narrow interests of social groups. In my view, the context mathematical practice is set within is much too diverse for such a narrow identification to be successful. Firstly, in some cases, our needs and interests simply spring from the kind biological beings we are and the type of environment we live in. If we want to explain why we take $2 + 2$ to be 4 and not 5, we do not need to look at social causes and interests. As noted above, creatures with the biology we have, living in the kind of environment we do, simply gets to see simply gets to see that kind of pattern as significant. As we saw in chapter 4, animals evolutionary close to us seem to share this interests (although it does, as explained above, take contact with a social group to turn the disposition to behave as if $2 + 2 = 4$ into rule following behavior proper). This has nothing to do with social variables, or rather: the social and cultural variables are insignificant in this case. Our belief that $2+2 = 4$ is simply a product of the way we, as biological creatures, interact with our world.

Secondly, in other cases, much more than narrow group interests are involved. This was for instance the case in the debate over which geometrical objects to accept. Here, a sphere of technology entered the equation; the familiarity with different types of drawing instruments led Euclid, Descartes and Huygens to see different types of patterns (in the form of actual or imagined curves) as significant. A similar point can be made for the use of cognitive technologies, such as the introduction of abstract symbols, which radically changed our conception and experience of certain mathematical objects and of the nature of mathematical proof.

Furthermore, the narrow interests of sub-groups of the mathematical community are often (but not always) set in a wider and shared interests, of creating mathematical systems capable of representing structures of the real world correctly. The case of the different number concepts used by Bloor does in fact show this; both the Greek and the Renaissance mathematicians shared a common interest of representing the world correctly by the use of numbers, they only had very different metaphysical ideas about the properties of this world. Mathematics is not the arbitrary product of a social game where narrow interests are negotiated. Although mathematics depend on and is to some extend shaped by a wider context of social ideas, it is largely created instrumentally as a way to represent or help us interact with the world around us – whatever it is perceived to be.

### 7.3.1 Kripke revisited

All of this leads us back to revise Kripke's skeptical argument once more. Kripke is right in pointing out that a rule using practice, covering only a finite number of instances, does not somehow in advance determine all the possible new cases, the rule might be applied to. There is, however, something worrying about the example provided by Kripke. From an intuitive point of view, it is hard to accept $57 + 68$ as being a genuinely new case of addition, even given the setting presupposed by Kripke. There seems to be a difference between adding yet another couple of positive integers, and other cases, such as adding infinitely many integers, adding infinitely large numbers or, say, subtracting 68 from 57 in a setting, where only subtraction involving positive differences have until now been considered. Intuitively, the last examples simply seem to be genuinely new in a way Kripke's addition task does not – and from a historical point of view, they have all led to substantial discussion over which answers to consider as correct (I will give an example below). How can we account for this difference? Why is $57 + 68$ not problematic or debatable (or at lest not perceived as problematic and debatable) in the same way as $57 - 68$? We some kind of explanation.

As I see it, the main difference between the two types of cases lies in the fact that addition of any pair of finite, natural numbers falls within the domain of experience, primarily giving meaning to addition, i.e. manipulation of discrete, physical objects, whereas the other examples does not. Kripke might object that in the given setting, physical operations involving more than 57 pebbles are just as new and unknown as operations involving infinitely many pebbles. He does, however, forget the generality provided by metaphorical mappings. As noted in section 6.12, we do not base the addition of every, individual pair of numbers on different experiences, but on the general experience of pooling two sets containing some (finite) number of discrete objects. So even in the case that nobody has ever added 57 and 68, the task is covered by the meaning giving metaphor.

Furthermore, the algorithm used for adding numbers represented in Hindu-Arabic numerals is completely independent of the number of digits in the addends (similar to the multiplication algorithm described in section 6.3.1). It operates on a completely local basis, where the digits are added pairwise. If you master the addition table for the numerals 1 through 9 and know the proper algorithm, you can also add 57 and 68 – or any other pair of finite numbers. There is nothing new in this particular combination of numerals.

For these reasons the case of adding two positive, natural numbers of arbitrary size does not seem to be a new case that needs to be negotiated. The extension of subtraction to cover negative differences or sums involving infinitely many addends, on the other hand, does seem to be genuinely new. Here, we must both leave the domain of the meaning giving metaphor (seeing addition and subtraction as an operation on finite collections of objects), and we must introduce new symbols, such as $\sum_{i=1}^{\infty}$ or $\ldots$ or negative numbers, whose syntactic properties must be negotiated before they can be used confidently.

As I see it, Kripke have overlooked that not only particular answers to particular problems can get institutionalized as correct. Both conceptual and technical means for handling the more general case can be institutionalized as well. When Kripke asks me to point to a fact that determines my answer to 57 + 68 (given his hypothetical setting, where this particular sum is a novelty), I can point to the fact that I participate in an institution that has accepted this metaphor and this algorithm as the correct way to conceptualize and handle this general type of problem. Of course, nothing is settled for good. The collective might always revise its accept of both a particular metaphor and an algorithm at any given time – the limitations imposed on the metaphorical conception of sets as bounded regions in space might serve as an instructive example.

This being said, we can always find genuinely new extension, where the application of the rules must be negotiated before we can proceed. To give just at glimpse of the actual historical negotiations going on in connection to one of the examples mentioned above, we can look at De Morgan's treatment of an infinite sum. According to Phillips (2005), De Morgan considered the infinite sum $(1 + 2 + 4 + 8 + 16 + \cdots)$. He made the following calculation:

$$N = 1 + 2 + 4 + 8 + 16 + \cdots \tag{7.1}$$
$$N = 1 + 2(1 + 2 + 4 + 8 + \cdots) \tag{7.2}$$
$$N = 1 + 2(N) \tag{7.3}$$
$$N - 2N = 1 \tag{7.4}$$
$$N = -1 \tag{7.5}$$

In the view of De Morgan, the calculation shows that the sum of infinitely many positive numbers can be a negative number. De Morgan defended the result, and used it as a demonstration of the difference between algebra, understood as a purely formal discipline of operating with the symbols, and arithmetic, understood as something meaningful, i.e. operation of numbers understood as quantities; from an arithmetic point of view, an infinite series of positive numbers must be positive, but from an algebraic point of view this is not the case, as the calculation above shows (Phillips, 2005)[1].

From a modern point of view, however, the paradox is resolved by claiming that De Morgan did not extend the rule of subtraction correctly. The step from (7.3) to (7.4), where De Morgan subtracts $2N$ from $N$, is only valid if the series in question is convergent, i.e. if $N$ is a finite number. So effec-

[1]The series considered by De Morgan had puzzled mathematicians before. Euler for instance, reached the same conclusion regarding the sum of the series as De Morgan, but saw this as a proof that negative numbers are larger than infinite (see Kline, 1980, pp. 143).

tively, we say to De Morgan: “what you have done here is not subtraction. The rule you use only applies to finite numbers”.

To formulate the example in parallel with Kripke’s original skeptical challenge, we might say that we have an ambiguity between the two practices, ‘subtraction’ and ‘quubtraction’. The two practices agree on the finite case, but disagree when infinitely large numbers are at play. For quubtraction, we have that $N - 2N = -N$, for all $N$, whereas subtraction is defined as:

$$N - 2N = \begin{cases} -N, & \text{for } N \text{ finite} \\ \text{undefined}, & \text{for } N \text{ infinite} \end{cases}$$

As the two functions agree on all finite cases, how can we know that the practice we have been involved in so far, is subtraction and not quubtraction? The answer is: we cannot. We are here in a situation similar to the one described by Bloor above; one of the practitioners points out a an ambiguity in our practice, and the group will have to make a choice. In this case, the group chose to say: “all along we have been subtracting, not quubtracting, so De Morgan’s calculation is faulty”. This however, is a clear choice. It is not arbitrary – the wish to make algebra consistent with arithmetic constitutes a very good reason for making this choice, but it is nonetheless a choice. Nothing in our previous practice can tell us whether we were subtracting or quubtracting, before the choice was made.

The two examples, $N = 1 + 2 + 4 + 8 + 16 + \cdots$ and $57 + 68$, illustrate in my view, two different things. The first, i.e. $N = 1 + 2 + 4 + 8 + 16 + \cdots$, illustrates Kripke general rule skeptical point; a rule does not and cannot in advance determine how it should be applied in all possible future applications. This observation is also appropriately known as ‘finitism’ (see e.g. Bloor, 1983, p. 23). As the example shows, finitism is not just a purely academic or philosophical problem. It is a real phenomenon that can be observed in the history of mathematics. The second example, i.e. Kripke’s $57 + 68$, illustrates something different. In this case our ‘form of life’ (to use Wittgenstein’s expression) seems to make it possible for us to answer unhesitatingly and with confidence, although the rule is in a way used to cover a new case. The naturalistic investigation performed in this dissertation allows us to put a little more meat to this concept; in this case, our form of life is partially constituted by our use of cognitive tools, such as cognitive artifacts (in the form of object-symbols and algorithms) and meaning-giving, conceptual metaphors, and basic, life-world experiences, which can be used as source-domain for such metaphors. The institutionalized use of this kind of cognitive tools provide the resources necessary to go on, confidently, as long as we are within the domain covered by the tools. The use of the tools is of course subordinate to a social consensus; it might always be decided that *this* metaphor or *this algorithm* is inappropriate or limited in scope, but as long as it is not, we can go on as if the answers we give, were in fact determined in advance.

## 7.4 Partial conclusion

The discussions of this chapter have been centered on two main questions. Firstly, we have been asking into the nature of the rule governed aspects of mathematical practice. As we have seen, the existence of rule following is dependent on the perceived contact with a social collective. The understanding of rules as social institutions furthermore gives a natural explanation of the normativity and objectivity of mathematics. As noted by Frege and Husserl (in chapter 3), mathematical truths seems to be objective in a way than cannot be accounted for neither by tying mathematics to our individual psychology or by seeing mathematics as part of the physical world. Husserl and Frege solve this puzzle by proposing the existence of ideal, mathematical objects. The collectivistic account of rule following however, allows for a metaphysically more economic account, by seeing mathematics as lodged in social institutions. Social institutions can deliver the wanted normativity and perceived objectivity, as they are both external to my individual dispositions; it is because our answers are judged against the consensus of the group that we cannot do just anything in mathematics. Furthermore, the fact that the social group upholds rules, gives the rules the quality of being abstract from particular and accidental regularities perceived in physical nature. Normative rules upheld by a social collective in other words, can function as a 'golden standard' we can judge both our selves and regularities perceived in the surrounding world against. For these reasons, contact with a social group is constitutive of mathematics. If we were not social beings and did not have the ability to coordinate our behavior in social institutions, we would simply not be able to create mathematics.

All of this naturally led to the second main theme debated above; if rule following is constituted by contact with a social group, will the content of the rules (and hence the content of mathematics) then be caused by social factors? As I see it, it is not necessarily so. Although mathematics is created by our collective decisions, it is not done so arbitrarily (as Pickering would have it) or done so solely to serve the social interests of sub-groups of the community (as Bloor seems to imply). Basic parts of mathematics are determined by our biology and way of existing in the world, and furthermore the group have the liberty to chose mathematics to be instrumental for our needs, which at least in some cases requires mathematics to correspond to actually perceived structures of the physical world. For this reason, mathematics is simply constrained by more than just social consensus. This makes mathematics different from purely socially constructed practices, such as, say, fashion or chess.

# Chapter 8

# Recapitulation, answers and final conclusion

## 8.1 Recapitulation

In the material below, I have given a partial answer to the question: What is the origin of mathematical knowledge, and how is it produced? The traditional answer to the question goes along the lines that mathematical knowledge is produced by logical deduction from secure first principles. As we have seen, this answer is severely flawed; it is not possible to find self-evident axioms to serve as secure first-principles, and even if it was, it is not clear what types of arguments to accept as valid logical deductions. Although deductive proofs given in an axiomatic-deductive system does play a part in the mathematical practice, they do not seem to be neither the origin of mathematical knowledge, nor the primary way of producing such knowledge.

These shortcomings of traditional rationalistic accounts of mathematics motivates the attempt to give a naturalistic account of mathematics: if we want to understand why we form the mathematical beliefs we do, we must involve scientific evidence external to the body of mathematical knowledge itself. We must understand how our biology, cognitive style and contact with social groups shape our mathematical beliefs.

In the material below, three different levels of (naturalistic) explanations were investigated. Firstly, I examined the extend to which our evolutionary history can explain our mathematical beliefs. As it turned out, the direct impact made by evolution on our mathematical beliefs is limited. Evolution has provided us with the cognitive and perceptual prerequisites needed in order to do mathematics, but even slightly advanced concepts, such as the number line, or slightly advanced abilities, such as the ability to handle collections with more than a few elements with digital precision, requires more than our innate mathematical skills can handle.

Secondly, the impact made by our cognitive style was investigated. As we saw, mathematics is not exclusively produced by *a priori*, deductive reasoning. Mathematical reasoning is – as all human reasoning – embodied. To a very large extend, mathematical reasoning makes use of embodied cognitive tools, such as cognitive artifacts and conceptual mapping. These tools greatly expand our innate mathematical abilities. Even basic mathematical skills, such as the ability to handle the numerosity of collections with more than four elements, seem to depend critically on our ability to use cognitive artifacts. However, the cognitive tools are not neutral. They not only expand out ability to gain and produce mathematical knowledge, but also have a clear impact on the content of the knowledge produced. Both our epistemic standards and our acceptance of particular mathematical objects, such as space filling curves, are directly influence by our use of particular cognitive tools.

Finally, I examined a social level of explanation. Involving this level makes it possible to explain the crucial element of normaitvity intimately connected to mathematics. Our contact with other individuals of a social collective is crucial for our ability to form and follow rules, and thus, mathematics as we know it would simply not be possible if not set in a social context. The impact of this dependence on social factors is hard to determine precisely, although radical social construtivistic theories clearly overestimates it; although mathematics is connected to social institutions, it is clearly constrained by more than social interests. Mathematics is also constrained by our way of existing in the world and our interest in representing properties of this world with abstract, mathematical structures.

This, in brief, is what we have seen so far.

## 8.2 Answers

A naturalized description should be able to explain at least some of the most salient features of the phenomenon, it tries to describe. In the case of mathematics, two features are in need of explanation: 1) the feeling of dealing with an independent reality, many practitioners of mathematics report, and 2) why mathematics is effective in the description of the physical world surrounding us.

### 8.2.1 Why mathematicians are Platonists

Let me start wit the feeling mathematicians have of dealing with an independent reality. The naturalistic theories presented in this dissertation leave at least two possible explanations. Firstly, Lakoff and Núñez propose that the feeling of reality is a byproduct of our use of conceptual metaphors; when we conceptualize mathematical objects using conceptual maps taking real-world objects as source-domains, we transfer more than just the inferential structure from source- to target-domain (Lakoff & Núñez, 2000, p. 349-50). We also transfer the idea of dealing with an independent ontological reality similar to the physical world. This is a plausible explanation, although in my view, it is not without its problems. The main problem, as I see it, is a problem of generality. Not all mathematical objects are conceptualized metaphorically as physical objects. As we saw in section 6.10.2, maps between sets are typically conceptualized as directed paths. Now, a path is not an independently existing physical object. Does that mean that mathematicians conceive of maps as less real objects than the elements of a set? And what in cases where a meaning giving metaphor has not been established. Does the feeling of dealing with something real disappear? This could be – remember how complex and imaginary numbers were considered to be – well – *imaginary* and hypothetical, until a proper metaphor connecting them to experience of the real world, was established. However, I am not convinced that this is generally the case, and at least, the theory proposed by Lakoff and Núñez should be backed up by more empirical data showing a match between the type of metaphor we use to conceptualize an object, and the feeling of reality we attach to it.

The second explanation takes departure in the normativity of mathematics. Because the rules of mathematics are lodged in social institutions, we cannot do just anything. Our will is constrained by something outside of ourselves, and that can give a feeling of dealing with something real. As Bruno Latour once remarked: “Reality is what resists” (in Latour, 1989, p. 106).

Wittgenstein for instance uses this explanation in the following passage, where he contemplates what is meant by statements of ontological realism, such as: ‘to mathematical propositions there corresponds a reality’:

> [T]o say this may mean: these propositions are *responsible* to a reality. That is, you can’t say just anything in mathematics, because there’s the reality. [...] It is almost like saying, “Mathematical propositions don’t correspond to *moods*; you can’t say one thing now and one thing then.” Or again it’s something like saying, “Please don’t think of mathematics as something vague which goes on in the mind.” Because that has been said. Someone may say that logic is a part of psychology: logic treats of laws of thought and psychology deals with thought. You could get to the idea of logic as extremely vague, as psychology is so extremely vague. And if you oppose this you are inclined to say “a reality corresponds”.
>
> (Wittgenstein, 1976, p. 240)

This explanation is in many ways more convincing; we do encounter a sort of reality when we do mathematics, only it is a social reality and not an independent ontological realm. There are however, also problems. I for one am not completely convinced that the constrains provided by social institutions are enough to impose the feeling of dealing with something real, often reported by mathematicians. In chess for instance, or fashion, we also cannot do just anything. Our own ideas of right or wrong are restricted by those of the community. We cannot move a chess-piece to just any position on the board that suits us. But nonetheless, we do not propose the rules chess to correspond to an independent ontological reality.

As I see it, the difference between these two situations has to do with the fact, that the laws of mathematics are not arbitrary. As explained in chapter 7, mathematics is, in my view, not *only* constrained by the community, or rather: in the case of mathematics, the decisions of the community are constrained by an interest in creating mathematics, such that it captures features perceived as salient in our life-world. For that reason, the basic laws and objects of mathematics are abstracted from regularities and objects, we experience in the physical world, and we experience basic mathematics as being directly applicable to the very same world. This, in my view, is why mathematics is experienced as something real, while chess is not.

### 8.2.2 Why mathematics is efficient in the description of the physical world

the fact that mathematics can be used with empirical success in theories describing the physical world has also been given several explanations, taking departure in the naturalistic theories described above. As noted in section 4.5, the usefulness of mathematics has for instance been described as a product of our biological evolution. As Reuben Hersh has put it: “our mathematical ideas fit the world for the same reason that our lungs are suited to the atmosphere of this planet” (Hersh, 1979, p. 45). And that reason is: if they did not, we would simply not have survived to philosophize about it.

As also explained in (Johansen, 2010), this claim is highly problematic for two reasons. Firstly, at best it only covers a very limited part of mathematics, as most of the mathematics known and used by us today has been developed during the last few thousand years. Not very must biological evolution can have taken place in such a brief span of time, so for that reason alone, there is no reason to believe that our mathematical knowledge fits the physical world, because individuals holding knowledge not fitting the world, did not survive to pass on their genome to future generations. Secondly, as explained in chapter 4, it is furthermore not clear that mathematical knowledge is all that adaptive outside of the highly culturally shaped environment, we inhabit today in most parts of the world. What good would, say, the ability to solve differential equations do hunter-gatherers living on the African savannas 20.000 years ago?

In another but similar theory, the usefulness of mathematics is explained by claiming it to be a product of cultural evolution. This theory has been discussed and dismissed for different reasons in section 4.5, so I will not go further into it here.

Lakoff and Núñez have a different and highly original theory. As they see it, the fact that mathematics so often turn out to be useful in physics (and other empirical sciences one could add), is due to the fact that mathematicians and physicists share the same conceptual system, because they have similar bodies and share roughly the same type of life world:

> Physicists, having physical bodies and brains themselves, can comprehend regularities in the world only by using the conceptual system that the body and brain afford. Similarly, they understand mathematics using the conceptual system that the body and brain afford.
> (Lakoff & Núñez, 2000, p. 344)

This theory might be a way to explain the conceptual match between physics and mathematics, but it cannot, as I see it, explain why the use of mathematics in physics can lead to empirical success. As explained in chapter 6, the metaphors are mainly introduced *post factum* as a way to ground the meaning of mathematical knowledge. Although this might in some cases have played a part in the acceptance of particular theorems or objects, metaphors are not the driving force in the development of mathematics. So mathematics in other words, is not produced by the 'shared conceptual system', Lakoff and Núñez refers to. Rather, the shared system is used to make mathematics, produced by other means, understandable to us on a deeper level.

The idea proposed above, that some mathematical rules and objects are created as abstractions from observations of the physical world, obviously explains a great deal of the empirical success of the application of mathematics to the same type of phenomena. The same can be said about the observation, briefly touched above, that even some quite advanced mathematical objects are created as answers to the demands of physics (as also pointed out by Lützen, 2006). It is no miracle that concepts and theories developed specifically in order to fit the needs of science, later turns out to be effective when used by science. Consequently it is, as pointed out by Mark Steiner (2005), necessary to distinguish between *canonical* and *non-canonical* applications of mathematics in science. Canonical applications are cases, where a mathematical theory is used to describe exactly the class of phenomena, it was developed in order to describe. The successes of mathematics in such cases are in fact not at all mysterious and require no further explanation. Non-canonical applications on the other hand, are cases where a mathematical theory is applied to describe phenomena, foreign to its development. The use of conic sections in the description of the Solar system is a case in hand. Here, the mathematical theory was developed almost two millennia before its use in science – and without any connection whatsoever to the domain of its application. Empirical success in this kind of applications does seem to be harder to explain.

An attempt to explain successful non-canonical applications must, I believe, take departure in a detailed description of the nature of the empirical sciences and especially their relationship with mathematics. Such a description lies outside the scope of this dissertation, so I will restrict myself to propose two ideas, which could prove valuable in the explanation of the 'unreasonable effectiveness of mathematics' (see also Johansen, 2010). Firstly, the successful use of mathematics in science is partially a product of approximation and idealization; the world does not behave strictly according to mathematical law, it only do so approximately, and that takes some of the mystery out of the non-canonical applications. Many of them – including the use of conic sections to describe the Solar system – can simply be explained as the choice of a mathematical theory that has an acceptable fit to data, from several mathematical theories at the scientists' disposal. Secondly, modern science aims to give mathematical explanations, and that might well have guided the types of phenomena taken under consideration. It can be argued that science has chosen to describe only a very specific class of phenomena, namely phenomena that seem ripe for mathematical treatment. And that of course also explains some of the mystery of the applicability of mathematics; mathematics cannot be used to describe the entire physical world. It can only be used to describe a small and carefully selected sample of phenomena, and only approximately so (as also stated in Johansen, 2010).

Much more empirical work is of course needed in order to justify these to claims – and in general to give a full explanation of the relationship between mathematics and the empirical sciences. However, the argument sketched out above do serve as a proof of principle; a full and detailed explanation the mysterious effectiveness of mathematics do seem to be within the scope of naturalistic explanations, although it will take more than just naturalized description of mathematics to deliver it.

## 8.3 The normativity of philosophy of mathematics

So far, this dissertation has been purely descriptive; I have simply tried to describe mathematics as it is currently practiced. It could be asked, whether philosophy of mathematics also holds normative responsibilities, i.e. whether it is also to some extend responsible for telling mathematicians, how mathematics should be practiced.

As with all works of humanities, these two roles cannot completely be distinguished. A careful and fitting description of a practice might also change it, simply because it makes the actors aware of what they are actually doing.

I will not make normative statements about which objects or which proof techniques to accept in mathematics. It is the responsibility of those partaking in the practice of mathematics to make these choices. This being said, I do want to make one clear normative statement: when the practitioner of mathematics make the kind of decisions described above, they *ought* to do it on an informed ground. That is, the practitioners of mathematics have a responsibility to understand the practice they are partaking in and to try to form consistent philosophical position concerning its nature.

In an often quoted passage, one of the leaders of the Bourbaki collective, Jean Dieudonné, made the following statement:

> On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say, "Mathematics is just a combination of meaningless symbols," and then we bring out Chapter 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working with something real. This sensation is probably an illusion, but it is very convenient. That is Bourbaki's attitude toward foundation.
>
> (Dieudonné, 1970, p.145)

In my view, this is no less than a declaration of intellectual bankruptcy. Philosophers like myself have a responsibility to formulate adequate theories about mathematics, and mathematicians have a responsibility to use such theories, in order to inform their own professional practice by meta-level knowledge about this practice. The appeal to ignorance, made by Dieudonné is tempting, but it is not how mathematicians ought to proceed.

My humble hope is that I have contributed to this meta-level understanding of mathematics, by presenting this naturalized description of the biological, cognitive and sociological factors constraining and shaping the practice of mathematics.

## 8.4 Some ideas for future work

The description of mathematics given in this dissertation is far from complete, and consequently could be extended in several directions. Especially, the relationship between mathematics and the empirical sciences is almost completely left out, and a more detailed investigation of this relationship is an obvious and much needed extension. Also, the work by Imre Lakatos, describing how the internal demands on mathematics shape the formation of certain concepts, could be addressed and integrated in the general naturalistic framework offered here. Finally, as noticed in section 6.13, the functional role played by conceptual mapping calls for more empirical work, studying for instance how working mathematicians use diagrams in their research.

Apart from such extensions and addition of further detail, the material presented in this dissertation might also form the basis for further work in the didactics of mathematics. A more precise understanding of the roles played by abstract symbols, figures and diagrams in mathematics, could make it easier learn to manage these cognitive technologies by students learning mathematics at hi-schools level (where most of these strategies are introduced and used in full). As it is, the precise function of the various cognitive tools used in mathematics, is not addressed explicitly in the hi-school teaching of today. The function of the cognitive tools are treated as 'tacit knowledge', the students must somehow pick up during training. In fact, I am afraid that only few teachers would be able to give fulfilling explanations of the various cognitive techniques being used. In my experience as a trained hi-school teacher, this leaves many students puzzled. This points to the need for empirical work, investigating how the role played by cognitive tools and techniques, such as abstract symbols and conceptual mapping, could be introduced to hi-school students, and if such an explicit introduction would enhance the performance and understanding of the students.

On the level of theoretical didactics, the detailed description of the various cognitive techniques and tools used in mathematics could be used to add to the general theory of registers offered by Raymond Duval (see section 6.13).

## 8.5 Final conclusion

Although the account of mathematics given below is far from complete, the material presented in this dissertation gives a rough picture of how our mathematical beliefs are formed. Mathematics – as we know it – is clearly rooted in our biology and our way of existing in the world. Let me take arithmetic as an example. Here, evolution has provided us with the perceptual and cognitive recourses necessary in order to encode and use the numerical aspects of the world adaptively. Furthermore, our environment and culturally mediated ways of interacting with objects in the world, make particular patterns of change in numerosities seem interesting to us. It should be noted that this is not a necessary step; animals and human infants only seem to be interested in the numerical aspects of the world on rare or specially designed circumstances (and the same can be said for several tribes living as of hunter-gatheres (see e.g. Gordon, 2004)). By the use of cognitive artifacts, such as sequences of number signs and different calculating tools, our innate skills are extended to cover more than the original domain. The rules of operation accepted as valid are solidified as social institutions, and finally it is negotiated how the rules should be extended to cover instances beyond the original domain; how should we deal with subtraction of numbers with negative difference, with square roots of such negative differences, with infinite sums *etc.*?. During this negotiation, conceptual mapping relating mathematical content to life-world experience plays a part; the Number-Line blend and the geometric interpretation of complex algebra for instance, provide connections between particular extensions of the rules of algebra and structures experienced in the world. Such connections can help particular extensions to be accepted as valid over others.

Something similar can be said about other areas of mathematics, such as geometry, topology, statistics, probability theory, differential calculus *etc.* They all arise from our interests in particular aspects of the world we live in, their rules and objects are lodged in social institutions and expanded through the use of cognitive artifacts. This is how we do mathematics. This is how our mathematical knowledge is created. There are of course other aspects to it – mathematics is not only inspired and constrained by the needs of daily life, but also but the needs of science, religion and other culturally significant activities, available technologies *etc.* However, including such aspects in the model described above will only strengthen it, not show it to be wrong.

In the picture I here have given of mathematics, our mathematical knowledge is clearly not objective knowledge about a mind-independent realm of Platonic objects. It is a construction made by us and resulting from the way we exist in the world. We can of course never rule out metaphysical assumptions such as the existence of a Platonic realm of mathematical objects (as the assumption by hypothesis is not empirically testable), but we *can* say that the assumption is not necessary in order to explaining our mathematical practice and beliefs. They can be explained by scientific, naturalistic means – in fact, even the belief in realism can be explained as a naturally occurring side effect of the way, our mathematical knowledge is constructed.

Finally, regarding naturalism as a method in the philosophy of mathematics, two main conclusions can be reached based on the material presented below. Firstly, naturalistic methods operating only with one level of explanation have severe limitations. We have, for instance, seen that neither the biological not the cognitive level can explain the normativity surrounding mathematics. The social level, on the other hand, cannot explain why mathematics is not arbitrary, or how our 'form of life' more precisely allows us to extend rules to new cases (such as $57 + 68$), while others (such as $N = 1 + 2 + 4 + 8 + \cdots$) requires negotiation. Mathematics is a highly complex practice, and more than one level of explanation is needed, if we are to understand it on naturalistic terms. On the other hand, this dissertation do demonstrate (performatively) that a naturalistic method involving more than one level of explanation, provides a fruitful way to describe and explain the origin of our mathematical knowledge. Many pieces are still missing before the picture is complete. There is still much work to be done, but luckily, a ph.d.-dissertation is supposed to mark the beginning of a long research career, not the end.

# Bibliography


Adams, C. 1995. *Calculus: A Complete Course*. Ontario, Canada: Addison-Wesley Publishers Limited.

Addessi, E., Crescimbene, L., & Visalberghi, E. 2008. Food and token quantity discrimination in capuchin monkeys (Cebus apella). *Animal Cognition*, **11**(2), 275–282.

Agrillo, C., Dadda, M., & Bisazza, A. 2007. Quantity discrimination in female mosquitofish. *Animal Cognition*, **10**(1), 63–70.

Agrillo, C., Dadda, M., Serena, G., & Bisazza, A. 2008. Do fish count? Spontaneous discrimination of quantity in female mosquitofish. *Animal Cognition*, **11**(3), 495–503.

Aïn, S.A., Giret, N., Grand, M., Kreutzer, M., & Bovet, D. 2009. The discrimination of discrete and continuous amounts in African grey parrots (Psittacus erithacus). *Animal cognition*, **12**(1), 145–154.

Albiach-Serrano, A., Guillén-Salazar, F., & Call, J. 2007. Mangabeys (Cercocebus torquatus lunulatus) solve the reverse contingency task without a modified procedure. *Animal Cognition*, **10**(4), 387–396.

Anderson, P.W. 1972. More is different. *Science*, **177**(4047), 393–396.

Annas, J. 1976. *Aristotle's Metaphysics, books M and N*. Oxford: Clarendon Press.

Antonius, S., Clausen, R., Hansen, H.H., Melin, J., Poulsen, N.H., & Weile, J. 2009. *Matematik C-hhx*. 3. edn. Århus C: Systime.

Arzarello, F., Paola, D., Robutti, O., & Sabena, C. 2009. Gestures as semiotic resources in the mathematics classroom. *Educational Studies in Mathematics*, **70**(2), 97–109.

Azzouni, J. 2006. How and why mathematics is unique as a social practice. *Pages 201–219 of:* Hersh, R. (ed), *18 Unconventional Essays on the Nature of Mathematics*. New York: Springer.

Bächtold, D., Baumüller, M., & Brugger, P. 1998. Stimulus-response compatibility in representational space. *Neuropsychologia*, **36**(8), 731–735.

Balakrishnanl, J.D, & Ashby, F.G. 1992. Subitizing: Magical numbers or mere superstition? *Psychological Research*, **54**(2), 80–90.

Barnes, B., Bloor, D., & Henry, J. 1996. *Scientific knowledge: A sociological analysis*. Chicago: University of Chicago Press.

Barrow, J.D. 2000. *The Book of Nothing*. London: Jonathan Cape.

Barth, H., Kanwisher, N., & Spelke, E. 2003. The construction of large number representations in adults. *Cognition*, **86**(3), 201–221.

Beeson, M. 2003. The mechanization of mathematics. *Pages 77–134 of: Alan Turing: Life and Legacy of a Great Thinker*. New York: Springer.

Benacerraf, P., & Putnam, H. (eds). 1983. *Philosophy of Mathematics: Selected Readings*. Cambridge: Cambridge University Press.

Beran, M.J. 2004. Long-term retention of the differential values of Arabic numerals by chimpanzees (Pan troglodytes). *Animal Cognition*, **7**(2), 86–92.

Beran, M.J. 2008. Capuchin monkeys (Cebus apella) succeed in a test of quantity conservation. *Animal cognition*, **11**(1), 109–116.

Beran, M.J., & Beran, M.M. 2004. Chimpanzees remember the results of one-by-one addition of food items to sets over extended time periods. *Psychological Science*, **15**(2), 94–99.

Beran, M.J., & Rumbaugh, D.M. 2001. "Constructive" enumeration by chimpanzees (Pan troglodytes) on a computerized task. *Animal Cognition*, **4**(2), 81–89.

Berkeley, G. 1754. *The Analyst; or, a Discourse Addressed to an Infidel Mathematician. Wherein it is examined whether the Object, Principles, and Inferences of the modern Analysis are more distinctly conceived, or more evidently deduced, than Religious Mysteries and Points of Faith.* 2 edn. London: Printed for J. and R. Tonson and S. Draper.

BIJELJAC-BABIC, R., BERTONCINI, J., & MEHLER, J. 1993. How do 4-day-old infants categorize multisyllabic utterances? *Developmental psychology*, **29**, 711–721.

BIRO, D., & MATSUZAWA, T. 2001. Use of numerical symbols by the chimpanzee (Pan troglodytes): Cardinals, ordinals, and the introduction of zero. *Animal Cognition*, **4**(3), 193–199.

BLISSARD, J. 1861. Theory of generic equations, chapter I. *The Quarterly Journal of Pure and Applied Mathematics*, **4**, 279–305.

BLISSARD, J. 1862. Theory of generic equations, chapter III. *The Quarterly Journal of Pure and Applied Mathematics*, **5**, 185–208.

BLOOR, D. 1981. Hamilton and Peacock on the essence of algebra. *Pages 202–232 of:* MEHRTENS, H., BOS, H.M., & SCHNEIDER, I. (eds), *Social history of nineteenth century mathematics*. Boston: Birkhäuser.

BLOOR, D. 1983. *Wittgenstein: A social theory of knowledge*. New York: Columbia University Press.

BLOOR, D. 1991. *Knowledge and social imagery*. 2 edn. Chicago: University of Chicago Press.

BLOOR, D. 2002. *Wittgenstein, rules and institutions*. London: Routledge.

BOOTH, J.L., & SIEGLER, R.S. 2006. Developmental and individual differences in pure numerical estimation. *Developmental Psychology*, **42**(1), 189–201.

BOS, H.J.M. 1981. On the Representation of Curves in Descartes' Géométrie. *Archives for History of Exact Sciences*, **24**, 295–338.

BOS, H.J.M. 1988. Tractional Motion and the Legitimation of Transcendental Curves. *Centaurus*, **31**(1), 9–62.

BOS, H.J.M. 2001. *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction*. New York: Springer-Verlag.

BOYER, C.B. 1989. *A History of Mathematics. 2.ed.* New York: John Wiley & Sons.

BOYSEN, S.T. 1993. Counting in chimpanzees: Nonhuman principles and emergent properties of number. *Pages 39–59 of:* BOYSEN, S. T., & CAPALDI, E. J. (eds), *The Development of Numerical Competence: Animal and Human Models*. Hillsdale, NJ.: Erlbaum.

BOYSEN, S.T., & BERNTSON, G.G. 1989. Numerical competence in a chimpanzee (Pan troglodytes). *Journal of Comparative Psychology*, **103**(1), 23–31.

BOYSEN, S.T., & HALLBERG, K.I. 2000. Primate numerical competence: Contributions toward understanding nonhuman cognition. *Cognitive Science*, **24**(3), 423–443.

BRANNON, E.M., & TERRACE, H.S. 1998. Ordering of the numerosities 1 to 9 by monkeys. *Science*, **282**(5389), 746–749.

BROOKS, R.A. 1990. Elephants don't play chess. *Robotics and autonomous systems*, **6**(1-2), 3–15.

BROUWER, L.E.J. 1923. On the significance of the principle of excluded middle in mathematics, especially in function theory. *Pages 334–345 of:* VAN HEIJENOORT, J. (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931.* Cambridge, MA: Harvard University Press, 1967.

BROWN, J.R. 1999. *Philosophy of Mathematics: An Introduction to a World of Proofs and Pictures*. London: Routledge.

BUTTERWORTH, B. 1999. *What counts: How every brain is hardwired for math*. New York: The Free Press.

CAJORI, F. 1916. *William Oughtred: A Great Seventeenth-Century Teacher of Mathematics*. Chicago: The Open Court Publishing Company.

CAJORI, F. 2007. *A History of Mathematical Notations: Vol. I.* New York: Cosimo Classics.

CALLEBAUT, W., & BECHTEL, W. 1993. *Taking the naturalistic turn, or how real philosophy of science is done.* University of Chicago Press Chicago.

CAMPBELL, J.I.D., & EPP, L.J. 2005. Architectures for Arithmetic. *Pages 347–360 of:* CAMPBELL, JAMIE I. D. (ed), *Handbook of Mathematical Cognition*. New York: Psychology Press.

Cantlon, J.F., & Brannon, E.M. 2007. Basic math in monkeys and college students. *PLoS Biology*, **5**(12), e328. doi:10.1371/journal.pbio.0050328.

Cantor, G. 1891. Über eine elementare Frage der Mannigfaltigkeitslehre. *Jahresbericht der Deutschen Mathematiker-Vereinigung*, **1**, 75–78.

Cantor, G. 1895. Beiträge zur Begründung der transfiniten Mengenlehre. *Mathematische Annalen*, **46**, 481–512.

Cantor, G. 1899. Letter to Dedekind. *Pages 113–117 of:* Van Heijenoort, J. (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931.* Cambridge, MA: Harvard University Press, 1967.

Carazo, P., Font, E., Forteza-Behrendt, E., & Desfilis, E. 2009. Quantity discrimination in Tenebrio molitor: evidence of numerosity discrimination in an invertebrate? *Animal Cognition*, **12**(3), 463–470.

Chrisomalis, S. 2004. A Cognitive Typology for Numerical Notation. *Cambridge Archaeological Journal*, **14**(01), 37–52.

Church, R.M., & Meck, W.H. 1984. The numerical attribute of stimuli. *Pages 445–464 of:* H. L. Roitblat, T. G. Bever, & Terrace, H. S. (eds), *Animal cognition.* Hillsdale, NJ: Erlbaum.

Clark, A. 1998a. Magic Words: How Language Augments Human Computation. *Pages 162–183 of:* Carruthers, P., & Boucher, J. (eds), *Language and Thought: Interdisciplinary Themes.* Cambridge: Cambridge University Press.

Clark, A. 1998b. Where brain, body, and world collide. *Daedalus*, **127**(2), 257–280.

Clearfield, M.W., & Mix, K.S. 1999. Number versus contour length in infants' discrimination of small visual sets. *Psychological Science*, **10**, 408–411.

Cloak, F.T. 1975. Is a cultural ethology possible? *Human Ecology*, **3**(3), 161–182.

Cohen, L.B., & Marks, K.S. 2002. How infants process addition and subtraction events. *Developmental Science*, **5**(2), 186–212.

Collin, F. 2003. *Konstruktivisme.* Frederiksberg: Roskilde Universitetsforlag.

Cordes, S., & Gelman, R. 2005. The young numerical mind: When does it count? *Pages 128–42 of:* Campbell, J.I.D. (ed), *Handbook of Mathematical Cognition*. New York: Psychology Press.

Cordes, S., Gelman, R., Gallistel, C.R., & Whalen, J. 2001. Variability signatures distinguish verbal from nonverbal counting for both large and small numbers. *Psychonomic Bulletin and Review*, **8**(4), 698–707.

Corfield, D. 2003. *Towards a philosophy of real mathematics*. Cambridge: Cambridge University Press.

Curry, H.B. 1954. Remarks on the Definition and Nature of Mathematics. *Pages 202–206 of:* Benacerraf, P., & Putnam, H. (eds), *Philosophy of Mathematics: Selected Readings*. Cambridge: Cambridge University Press, 1983.

Dahn, B.I. 1998. Robbins Algebras Are Boolean: A Revision of McCune's Computer-Generated Solution of Robbins Problem. *Journal of Algebra*, **208**(2), 526–532.

Dantzig, T. 2005. *Number: The Language of Science*. New York: Pi Press.

Dauben, J. 1984. Conceptual revolutions and the history of mathematics: two studies in the growth of knowledge. *Pages 81–103 of:* Mendelsohn, E. (ed), *Transformation and tradition in the sciences, essays in honour of I. Bernard Cohen*. Cambridge: Cambridge University Press.

Davis, H. 1984. Discrimination of the number three by a raccoon (Procyon lotor). *Animal Learning & Behavior*, **12**(4), 409–413.

Davis, H., & Bradford, S.A. 1986. Counting behavior by rats in a simulated natural environment. *Ethology*, **73**(4), 265–80.

Davis, H., & Memmott, J. 1982. Counting behavior in animals: A critical evaluation. *Psychological Bulletin*, **92**(3), 547–571.

Davis, H., & Perusse, R. 1988. Numerical competence in animals: Definitional issues, current evidence, and a new research agenda. *Behavioral and Brain Sciences*, **11**(4), 561–579.

Davis, P.J., & Hersh, R. 1992. *The Mathematical Experience*. Brighton: Harvester Press.

De Cruz, H. 2005. Mathematical symbols as epistemic actions - an extended mind perspective. Unpublished on-line working paper, available at `http://pespmc1.vub.ac.be/ECCO/ECCO-papers/DeCruz-Math.Symbols.pdf`.

de Cruz, H. 2007. *Innate Ideas as a Naturalistic Source of Mathematical Knowledge*. Brussel: Vrije Universiteit Brussel.

De Morgan, A. 1849. *Trigonometry and Double Algebra*. London: Printed for Taylor, Walton and Maberly.

Dehaene, S. 1997. *The Number Sense: How the Mind Creates Mathematics*. New York: Oxford University Press.

Dehaene, S., & Changeux, J.P. 1993. Development of elementary numerical abilities: A neuronal model. *Journal of Cognitive Neuroscience*, **5**(4), 390–407.

Dehaene, S., Dupoux, E., & Mehler, J. 1990. Is numerical comparison digital? Analogical and symbolic effects in two-digit number comparison. *Journal of Experimental Psychology: Human Perception and Performance*, **16**(3), 626–641.

Dehaene, S., Bossini, S., & Giraux, P. 1993. The mental representation of parity and number magnitude. *Journal of Experimental Psychology: General*, **122**(3), 371–396.

Dehaene, S., Izard, V., Spelke, E., & Pica, P. 2008. Log or linear? Distinct intuitions of the number scale in Western and Amazonian indigene cultures. *Science*, **320**(5880), 1217.

Descartes, R. 1954. *The Geometry of René Descartes*. New York: Dover Publications Inc.

Di Bucchianico, A., & Loeb, D. 1995. A selected survey of umbral calculus. *Electronic Journal of Combinatorics*, **2**, Dynamic Survey 3, pp.28 (electronic).

Dideriksen, M., Olsen, M., & Witzke, A. (eds). 2009. *AT-Håndbogen. Til eksamen i Almen Studieforberedeslse*. Århus: Systime.

Dieudonné, J. 1970. The work of Nicholas Bourbaki. *American Mathematical Monthly*, **77**, 134–145.

Domínguez, J.F., & Gray, J. (eds). 2006. *The architecture of modern mathematics: essays in history and philosophy*. Oxford: Oxford University Press.

du Bois Reymond, P. 1875. Versuch einer Klassification der willkürlichen Functionen reeller Argumente nach ihren Änderungen in den kleinsten Intervallen. *Journal für Mathematik*, **79**, 21–37.

Dunmore, C. 1992. Meta-level revolutions in mathematics. *Pages 209–225 of:* Gillies, D. (ed), *Revolutions in mathematics*. New York: Oxford University Press.

Duval, R. 2000. Basic Issues for Research in Mathematics Education. *In: Proceedings of the Conference of the International Group for the Psychology of Mathematics Education (PME) (24th, Hiroshima, Japan, July 23-27, 2000)*, vol. 1.

Duval, R. 2006. A cognitive analysis of problems of comprehension in a learning of mathematics. *Educational Studies in Mathematics*, **61**(1), 103–131.

Ebbinghaus, H.D., Hermes, H., Hirzebruch, F., Koecher, M., Mainzer, K., Neukirch, J., Prestel, A., & Remmert, R. 1991. *Numbers. With an introduction by Klaus Lamotke. Translated by H.L.S. Orde*. Berlin: Springer.

Edwards, L.D. 2009. Gestures and conceptual integration in mathematical talk. *Educational Studies in Mathematics*, **70**(2), 127–141.

Emmeche, C., Køppe, S., & Stjernfelt, F. 1997. Explaining emergence: towards an ontology of levels. *Journal for General Philosophy of Science*, **28**(1), 83–117.

Emmeche, C., Køppe, S., & Stjernfelt, F. 2000. Levels, emergence, and three versions of downward causation. *Pages 13–34 of:* Andersen, P.B., Emmeche, C., Finnemann, N.O., & Christiansen, P.V. (eds), *Downward Causation. Minds, Bodies and Matter*. Århus: Aarhus University Press.

Emmerton, J., & Renner, J.C. 2009. Local rather than global processing of visual arrays in numerosity discrimination by pigeons (Columba livia). *Animal cognition*, **12**(3), 511–526.

Euler, L. 2000. *Foundations of Differential Calculus.* Secaucus, NJ.: Springer-Verlag.

Evans, T.A., Beran, M.J., Harris, E.H., & Rice, D.F. 2009. Quantity judgments of sequentially presented food items by capuchin monkeys (Cebus apella). *Animal Cognition*, **12**(1), 97–105.

Fauconnier, G. 1997. *Mappings in thought and language.* Cambridge: Cambridge University Press.

Fauconnier, G., & Turner, M. 1998. Conceptual integration networks. *Cognitive science*, **22**(2), 133–187.

Fauconnier, G., & Turner, M. 2003. *The way we think: Conceptual blending and the mind's hidden complexities.* New York: Basic Books.

Feferman, S. 1998. *In the Light of Logic.* Oxford: Oxford University Press.

Feigenson, L., & Carey, S. 2003. Tracking individuals via object-files: evidence from infants' manual search. *Developmental Science*, **6**(5), 568–584.

Feigenson, L., Carey, S., & Spelke, E. 2002a. Infants' discrimination of number vs. continuous extent. *Cognitive Psychology*, **44**(1), 33–66.

Feigenson, L., Carey, S., & Hauser, M. 2002b. The representations underlying infants' choice of more: Object files versus analog magnitudes. *Psychological Science*, **13**, 150–156.

Ferkin, M.H., Pierce, A.A., Sealand, R.O., & delBarco Trillo, J. 2005. Meadow voles, Microtus pennsylvanicus, can distinguish more over-marks from fewer over-marks. *Animal Cognition*, **8**(3), 182–189.

Fischer, M.H. 2006. The future for SNARC could be stark... *Cortex*, **42**(8), 1066–1068.

Flombaum, J.I., Junge, J.A., & Hauser, M.D. 2005. Rhesus monkeys (Macaca mulatta) spontaneously compute addition operations over large numbers. *Cognition*, **97**(3), 315–325.

Fodor, J.A. 1976. *The Language of Thought.* Wiltshire, GB: The Harvester Press Limited.

Ford, K.M., & Pylyshyn, Z.W. (eds). 1996. *The robot's dilemma revisited: the frame problem in artificial intelligence.* Norwood, N.J.: Ablex Publishing Corporation.

Fraenkel, A.A., Bar-Hillel, Y., & Levy, A. 1973. *Foundations of set theory*. Amsterdam: North Holland.

Frant, J.B., Acevedo, J.I., & Font, V. 2006. Metaphors in Mathematics Classrooms: Analyzing the Dynamic Process of Teaching and Learning of Graph Functions. *Actas del CERME*, **4**, 82–91.

Frege, G. 1974. *Die Grundlagen der Arithmetik, translated by J.L. Austin as The Foundations of Arithmetic*. 2. revised edn. Oxford: Basil Blackwell.

Friend, M. 2007. *Introducing philosophy of mathematics*. Stocksfield: Acumen.

Gauss, C.F. 1863. *Werke*. Vol. 2. Göttingen: Königlichen Gesellschaft der Wissenschaften zu Göttingen.

Gauss, C.F., & Maser, H. (translation). 1889. *Untersuchungen über höhere Arithmetik*. Berlin: Springer-Verlag.

Gelman, R., & Gallistel, C.R. 1978. *The child's understanding of number*. Harvard University Press.

Gelman, R., & Gallistel, C.R. 1992. Preverbal and verbal counting and computation. *Cognition*, **44**, 43–74.

Gerhardt, C.I., editor. 1863. *Leibnizens Mathematische Schriften*. Berlin: A. Asher.

Giaquinto, M. 2007. *Visual Thinking in Mathematics: An Epistemological Study*. Oxford: Oxford University Press.

Gödel, K. 1931. On Formally Undecidable Propositions. *Pages 596–616 of:* Van Heijenoort, J. (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*. Cambridge, MA: Harvard University Press, 1967.

Gödel, K. 1947. What is Cantor's continuum problem. *In:* Benacerraf, P., & Putnam, H. (eds), *Philosophy of Mathematics: Selected Readings*. Cambridge: Cambridge University Press, 1983.

Goodman, N.D. 1990. Mathematics as natural science. *Journal of Symbolic Logic*, **55**(1), 182–193.

Gordon, P. 2004. Numerical cognition without words: Evidence from Amazonia. *Science*, **306**(5695), 496.

Grant, E. 1974. *A Source Book in Medieval Science*. Cambridge: Harvard University Press.

Guinand, A.P. 1979. The umbral method: A survey of elementary mnemonic and manipulative uses. *The American Mathematical Monthly*, **86**(3), 187–195.

Hale, B., & Wright, C. 2001. *The Reason's Proper Study: Essays towards a Neo-Fregean Philosophy of Mathematics*. Oxford: Oxford University Press.

Harris, E.H., & Washburn, D.A. 2005. Macaques' (Macaca mulatta) use of numerical cues in maze trials. *Animal Cognition*, **8**(3), 190–199.

Hassmann, V.M. 1952. Vom Erlernen unbenannter Anzahlen bei Eichhörnchen. *Zeitschrift für Tierpsychologie*, 294–321.

Hauser, M.D., & Carey, S. 1998. Building a cognitive creature from a set of primitives: Evolutionary and developmental insights. *Pages 51–106 of:* Cummins, Denise Dellarosa, & Allen, Colin (eds), *The Evolution of Mind*. Oxford: Oxford University Press.

Hauser, M.D., & Carey, S. 2003. Spontaneous representations of small numbers of objects by rhesus macaques: Examinations of content and format. *Cognitive Psychology*, **47**(4), 367–401.

Hauser, M.D., & Spelke, E. 2004. Evolutionary and developmental foundations of human knowledge. *Pages 853–864 of:* Gazzaniga, M.S. (ed), *Cognitive Neurosciences*, 3. edn. Cambridge, MA: MIT Press.

Hauser, M.D., MacNeilage, P., & Ware, M. 1996. Numerical representations in primates. *Proceedings of the National Academy of Sciences*, **93**(4), 1514–17.

Hauser, MD, Carey, S., & Hauser, LB. 2000. Spontaneous number representation in semi-free-ranging rhesus monkeys. *Proceedings of the Royal Society B: Biological Sciences*, **267**(1445), 829–833.

Hauser, M.D., Tsao, F., Garcia, P., & Spelke, E.S. 2003. Evolutionary foundations of number: Spontaneous representation of numerical magnitudes by cotton-top tamarins. *Proceedings of the Royal Society B: Biological Sciences*, **270**(1523), 1441–1446.

HAYES, J.R. 1973. On the function of visual imagery in elementary mathematics. *Pages 177–214 of:* CHASE, W. G. (ed), *Visual information processing*. New York and London: Academic Press.

HEATH, S.T.L. 1910. *Diophantus of Alexandria - A Study in the History of Greek Algebra*. 2. edn. Cambridge: Cambridge University Press.

HEATH, S.T.L. 1921. *A history of Greek mathematics*. Oxford: The Clarendon Press.

HEATH, T.L. 2006. *The Thirteen Books of Euclid's Elements*. New York: Barnes & Nobel, Inc.

HERSH, R. 1979. Some proposals for reviving the philosophy of mathematics. *Advances in Mathematics*, **31**, 31–50.

HEYTING, A. 1931. The intuitionist foundations of mathematics. *Pages 52–61 of:* BENACERRAF, P., & PUTNAM, H. (eds), *Philosophy of Mathematics: Selected Readings*. Cambridge: Cambridge University Press, 1983.

HILBERT, D. 1918. Axiomatisches Denken. *Mathematische Annalen*, **78**(1), 405–415.

HILBERT, D. 1922. The New Grounding of Mathematics. First report. *In:* MANCOSU, P. (ed), *From Brouwer To Hilbert: The Debate on the Foundations of Mathematics in the 1920s*. Oxford University Press, 1998.

HILBERT, D. 1925a. On the Ininite. *Pages 369–392 of:* HEIJENOORT, J (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*. Cambridge, MA: Harvard University Press, 1967.

HILBERT, D. 1925b. The foundatins of mathematics. *Pages 464–479 of:* VAN HEIJENOORT, J. (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*. Cambridge, MA: Harvard University Press, 1967.

HITTMAIR-DELAZER, M., SAILER, U., & BENKE, T. 1995. Impaired arithmetic facts but intact conceptual knowledge–a single-case study of dyscalculia. *Cortex*, **31**(1), 139–47.

HOLLAND, J.D., HUTCHINS, E., & KIRSH, D. 2000. Distributed Cognition: Toward a New Foundation for Human-Computer Interaction Research. *ACM Transactions on Computer-Human Interaction*, **7**(2), 174–196.

Husserl, E. 1993. *Logische untersuchungen.* Vol. 1. Tübingen: Max Niemeyer Verlag.

Hussey, E. 1983. *Aristotle's Physics, Books III and IV, translated with notes by Edward Hussey.* New York: Oxford University Press.

Hutchins, E. 2001. Cognitive Artifacts. *Pages 126–128 of:* Wilson, R.A., & Keil, F.C. (eds), *The MIT encyclopedia of the cognitive sciences.* Cambridge, MA: MIT Press.

Hutchins, E. 2005. Material anchors for conceptual blends. *Journal of Pragmatics*, **37**(10), 1555–1577.

Huygens, C. 1908. *Œuvres Completes, vol. 11.* La Haye: La Société Hollandaise des Sciences.

Jacobson, N. 1985. *Basic Algebra.* 2 edn. Vol. I. New York: W.H. Freeman and Company.

Jané, I. 2005. Higher-order logic reconsidered. *Pages 781–810 of:* Shapiro, S. (ed), *The Oxford Handbook of Philosophy of Mathematics and Logic.* Oxford: Oxford University Press.

Johansen, M. W. 2003. *Kunstig intelligens: eller hvorfor det er sværere at smøre en håndmad end at blive stormester i skak.* København: Forlaget Fremad.

Johansen, M.W. 2010. Does the usefulness of mathematics prove the existence of God? *Pages 71–93 of:* Kragh, H., & Nielsen, M.V. (eds), *God - a Mathematician?* Proceedings of the Danish Science-Theology Forum, vol. 5. University of Aarhus.

Johnson, M. 1990. *The body in the mind: The bodily basis of meaning, imagination, and reason.* Chicago: University of Chicago Press.

Jordan, K.E., & Brannon, E.M. 2006. Weber's Law influences numerical representations in rhesus macaques (Macaca mulatta). *Animal Cognition*, **9**(3), 159–172.

Kahneman, D., Treisman, A., & Gibbs, B.J. 1992. The reviewing of object files: Object-specific integration of information. *Cognitive Psychology*, **24**(2), 175–219.

Katz, V.J. 1998. *A history of mathematics: an introduction, 2.ed.* Reading, MA: Addison Wesley Publishing Company.

Kaufman, EL, Lord, MW, Reese, TW, & Volkmann, J. 1949. The discrimination of visual number. *The American journal of psychology*, **62**(4), 498–525.

Kerberi, M., & Polleti, M. 2002. On the design of mathematical concepts. *Cognitive Science Research Paper, University of Birmingham*, Available online at ftp://ftp.cs.bham.ac.uk/pub/authors/M.Kerber/TR/CSRP–02–06.pdf.

Kim, J. 1988. What is "naturalized epistemology"? *Philosophical perspectives*, **2**, 381–405.

Kim, J. 2006. Emergence: Core ideas and issues. *Synthese*, **151**(3), 547–559.

Kirsh, D. 1995. The intelligent use of space. *Artificial intelligence*, **73**(1-2), 31–68.

Kirsh, D., & Maglio, P. 1994. On distinguishing epistemic from pragmatic action. *Cognitive Science*, **18**(4), 513–549.

Kline, M. 1980. *Mathematics: The loss of certainty.* Oxford: 0xford University Press.

Kline, M. 1990. *Mathematical Thought From Ancient to Modern Times.* New York: Oxford University Press.

Knorr, W. R. 1975. *The Evolution of the Euclidean Elements: A Study of the Theory of Incommensurable Magnitudes and its Significance for Early Greek Geometry.* Dordrecht: D. Reidel Publishing Company.

Knudsen, S. 1999. *From metaphor to fact.* Roskilde: Roskilde Universitetsforlag.

Koehler, O. 1937. Können Tauben "zählen". *Zeitschrift für Tierpsychologie*, **1**, 39–48.

Koehler, O. 1941. Vom Erlernen unbenannter Anzahlen bei Vögeln. *Naturwissenschaften*, **29**(14), 201–218.

Koehler, O. 1951. The ability of birds to "count". *Bulletin of Animal Behavior*, **9**, 41–45.

Kornblith, H. 1994. Introduction: What is Naturalistic Epistemology? *Naturalizing epistemology*, **2**, 1–13.

Kripke, S. A. 1982. *Wittgenstein on rules and private language: an elementary exposition*. Cambridge, MA: Harvard University Press.

Kusch, M. 2007. Psychologism. *Stanford Encyclopedia of Philosophy*.

Lakatos, I. 1976a. A renaissance of empiricism in the recent philosophy of mathematics. *The British Journal for the Philosophy of Science*, **27**(3), 201–223.

Lakatos, I. 1976b. *Proofs and refutations*. Cambridge: Cambridge University Press.

Lakoff, G. 1987. *Women, fire, and dangerous things: What categories reveal about the mind*. Chicago: University of Chicago press.

Lakoff, G., & Johnson, M. 1980. *Metaphors We Live By*. Chicago: University of Chicago Press.

Lakoff, G., & Johnson, M. 1999. *Philosophy in the Flesh: The Embodied Mind and Its Challenge to Western Thought*. New York: Basic Books.

Lakoff, G., & Núñez, R. 2000. *Where Mathematics Comes From: How the Embodied Mind Brings Mathematics Into Being*. New York: Basic Books.

Lam, L.Y. 1988. A Chinese Genesis: Rewriting the History of Our Numeral System. *Archive for history of exact sciences*, **38**(2), 101–108.

Lancy, D.F. 1983. *Cross-cultural Studies in Cognition and Mathematics*. New York: Academic Press.

Latour, B. 1989. Clothing the naked truth. *Pages 101–126 of:* Lawson, H., & Appignanesi, L. (eds), *Dismantling Truth: Reality in the Post-Modern World*. London: Weidenfeld & Nicolson.

Lavor, B. 2010. Syntactic analogies and impossible extensions. *Pages 197–208 of:* Löve, B., & Müller (eds), *PhiMSAMP. Philosophy of Mathematics: Sociological Aspects and Mathematical Practice*. Texts in Philosophy 11. College Publications.

Lawvere, F.W, & Rosebrugh, R. 2003. *Sets for mathematics*. Cambridge: Cambridge University Press.

Lewis, K.P., Jaffe, S., & Brannon, E.M. 2005. Analog number representations in mongoose lemurs (Eulemur mongoz): Evidence from a search task. *Animal Cognition*, **8**(4), 247–252.

Livingstone, M.S., Srihasam, K., & Morocz, I.A. 2010. The benefit of symbols: monkeys show linear, human-like, accuracy when using symbols to represent scalar value. *Animal Cognition*, **13**(5), 1–9.

Luck, S.J., & Vogel, E.K. 1997. The capacity of visual working memory for features and conjunctions. *Nature*, **390**(6657), 279–281.

Lützen, J. 1990. *Joseph Liouville 1809-1882: Master of Pure and Applied Mathematics*. Studies in the History of Mathematics and Physical Sciences, vol. 15. New York: Springer-Verlag.

Lützen, J. 2006. A Scientific Duo: Reflections on the Interplay between Mathematics and Physics. *Newsletter of the European Mathematical Society*, **60**, 23–27.

Lyon, B.E. 2003. Egg recognition and counting reduce costs of avian conspecific brood parasitism. *Nature*, **422**(6931), 495–499.

Maaswinkel, H., & Whishaw, I.Q. 1999. Homing with locale, taxon, and dead reckoning strategies by foraging rats: sensory hierarchy in spatial navigation. *Behavioural brain research*, **99**(2), 143–152.

Maddy, P. 1997. *Naturalism in Mathematics*. Oxford: Oxford University Press.

Maddy, P. 1998. How to be a naturalist about mathematics. *Pages 161–180 of:* Dales, H.G., & Oliveri, G. (eds), *Truth in mathematics*. Oxford: Clarendon Press.

Mancosu, P. 1996. *Philosophy of mathematics and mathematical practice in the seventeenth century*. New York: Oxford University Press.

Mancosu, P. 2005. Visualization in logic and mathematics. *Pages 13–30 of: Visualization, explanation and reasoning styles in mathematics*. Dordrecht: Springer.

Mancosu, P. (ed). 2008. *The philosophy of mathematical practice*. Oxford: Oxford University Press.

Mancosu, P. 2009. Measuring The Size Of Infinite Collections Of Natural Numbers: Was Cantor's Theory Of Infinite Number Inevitable? *The Review of Symbolic Logic*, **2**(04), 612–646.

Mandler, G., & Shebo, B.J. 1982. Subitizing: An analysis of its component processes. *Journal of Experimental Psychology: General*, **111**(1), 1–22.

Manzano, M. 1999. *Model theory.* New York: Oxford University Press.

Marghetis, T., & Núñez, R. 2010. Dynamic Construals, Static Formalisms: Evidence From Co-Speech Gesture During Mathematical Proving. *Pages 23–29 of:* Pease, A., Guhe, M., & Smaill, A. (eds), *Proceedings of the International Symposium on Mathematical Practice and Cognition.*

Matsuzawa, T. 1985. Use of numbers by a chimpanzee. *Nature*, **315**, 57–59.

McComb, K., Packer, C., & Pusey, A. 1994. Roaring and numerical assessment in contests between groups of female lions, Panthera leo. *Animal Behaviour*, **47**(2), 379–387.

McCrink, K., & Wynn, K. 2004. Large-number addition and subtraction by 9-month-old infants. *Psychological Science*, **15**(11), 776–781.

McCrink, K., & Wynn, K. 2007. Ratio abstraction by 6-month-old infants. *Psychological Science*, **18**(8), 740–45.

McCune, W. 1997. Solution of the Robbins problem. *Journal of Automated Reasoning*, **19**(3), 263–276.

Mechner, F. 1958a. Probability relations within response sequences under ratio reinforcement. *Journal of the Experimental Analysis of Behavior*, **1**(2), 109–121.

Mechner, F. 1958b. Sequential dependencies of the lengths of consecutive response runs. *Journal of the Experimental Analysis of Behavior*, **1**(3), 229–233.

Mechner, F., & Guevrekian, L. 1962. Effects of deprivation upon counting and timing in rats. *Journal of the Experimental Analysis of Behavior*, **5**(4), 463–66.

Meck, W.H., & Church, R.M. 1983. A mode control model of counting and timing processes. *Journal of Experimental Psychology*, **9**(3), 320–334.

Menninger, K. 1992. *Number Words and Number Symbols: A Cultural History of Numbers.* New York: Courier Dover Publications.

Menzel, R., Geiger, K., Chittka, L., Joerges, J., Kunze, J., Mueller, U., *et al.* . 1996. The knowledge base of bee navigation. *Journal of Experimental Biology*, **199**, 141–146.

Messer, R. 1994. *Linear Algebra: Gateway to Mathematics.* New York: HarperCollins College Publishers.

Mill, J.S. 1973. *A System of Logic: The Collected Works of John Stuart Mill.* Vol. 7. Toronto: University of Toronto Press.

Misfeldt, M. 2006. *Mathematical writing.* Copenhagen: Danish University of Education, Learning Lab Denmark. (Online publication avalible at http://www.dpb.dpu.dk/dokumentarkiv/Publications/20051219135403/CurrentVersion/Mathematicalwriting.pdf).

Moore, G.H. 1982. *Zermelo's Axiom of Choice: Its Origins, Development, and Influence.* New York: Springer-Verlag.

Moyer, R.S., & Landauer, T.K. 1967. Time required for judgements of numerical inequality. *Nature*, **215**, 1519–1520.

Murofushi, K. 1997. Numerical matching behavior by a chimpanzee (Pan troglodytes): Subitizing and analogue magnitude estimation. *Japanese Psychological Research*, **39**(3), 140–153.

Nahin, P. J. 1998. *An imaginary tale: the story of* $\sqrt{-1}$. Princeton: Princeton University Press.

Newell, A., & Simon, H.A. 1976. Computer science as empirical inquiry: symbols and search. *Pages 81–110 of:* Haugeland, J. (ed), *Mind Design II. Philosophy, Psychology, Artificial Intelligence.* Cambridge, MA: MIT Press, 1997.

Nissen, H.J., Damerow, P., & Englund, R.K. 1993. *Archaic Bookkeeping: Early Writing and Techniques of Economic Administration in the Ancient Near East.* Chicago: University Of Chicago Press.

Norman, D.A. 1993. *Things that make us smart.* Reading, MA: Addison-Wesley Publishing Company.

Núñez, R. 2004. Do Real Numbers Really Move? Language, Thought, and Gesture: The Embodied Cognitive Foundations of Mathematics. *Pages 54–73 of:* Iida, F., Pfeifer, R., Steels, L., & Kuniyoshi, Y. (eds), *Embodied Artificial Intelligence.* Berlin: Springer-Verlag, 2004.

Núñez, R. 2008. Mathematics, the Ultimate Challenge to Embodiment: Truth and the Grounding of Axiomatic Systems. *Pages 333–353 of:* Calvo, P., & Gomila, T. (eds), *Handbook of Cognitive Science: An Embodied Approach*. Oxford: Elsevier Science Ltd.

Núñez, R. 2009. Numbers and Arithmetic: Neither Hardwired Nor Out There. *Biological Theory*, **4**(1), 68–83.

Núñez, R. (accepted for publication). No innate number line in the human brain. *Journal of Cross-Cultural Psychology.*

Oyama, S. 2000. *The Ontogeny of Information: Developmental Systems and Evolution*. Durham: Duke University Press.

Paseau, A. 2008. Naturalism in the Philosophy of Mathematics. *Stanford Encyclopedia of Philosophy.*

Peano, G. 1890. Sur une courbe, qui remplit toute une aire plane. *Mathematische Annalen*, **36**(1), 157–60.

Pepperberg, I.M. 1994. Numerical competence in an African gray parrot (Psittacus erithacus). *Journal of Comparative Psychology*, **108**, 36–44.

Pepperberg, I.M. 2006. Grey parrot numerical competence: a review. *Animal Cognition*, **9**(4), 377–391.

Pepperberg, I.M., & Gordon, J.D. 2005. Number comprehension by a grey parrot (Psittacus erithacus), including a zero-like concept. *Journal of Comparative Psychology*, **119**(2), 197–209.

Pérusse, R., & Rumbaugh, D.M. 1990. Summation in chimpanzees (Pan troglodytes): Effects of amounts, number of wells, and finer ratios. *International Journal of Primatology*, **11**(5), 425–437.

Phillips, C. 2005. Augustus De Morgan and the propagation of moral mathematics. *Studies In History and Philosophy of Science Part A*, **36**(1), 105–133.

Pickering, A. 1995. *The Mangle of Practice: Time, Agency, and Science.* Chicago: University Of Chicago Press.

Popper, K. 1980. Three Worlds. *Pages 141–167 of:* McMurrin, S.M. (ed), *The Tanner Lectures on Human Values.* Salt Lake City: University of Utah Press.

Proclus, A. 1970. *A Commentary on the First Book of Euclid's Elements, translated by Glenn R. Morrow.* Princeton: Princeton University Press.

Pylyshyn, Z.W. (ed). 1987. *The robot's dilemma: The frame problem in artificial intelligence.* Norwood, N.J.: Ablex Publishing Corporation.

Pylyshyn, Z.W., & Storm, R.W. 1988. Tracking multiple independent targets: Evidence for a parallel tracking mechanism. *Spatial Vision*, **3**, 179–197.

Quine, W.V.O. 1969. *Ontological relativity and other essays.* New York: Columbia University Press.

Ray, S.H. 1971. *Reports of the Cambridge Anthropological Expedition to Torres Straits.* Vol. III. Linguistics. New York: Johnson Reprint.

Reznikova, Z., & Ryabko, B. 2000. Using Information Theory approach to study the communication system and numerical competence in ants. *Pages 501–506 of:* Meyer, J., Berthoz, A., Floreano, D., Roitblat, H.L., & Wilson, S.W. (eds), *From Animals to animats. Proceedings of the Sixth International Conference on Simulation of Adaptive Behaviour.* Cambridge, MA: The MIT Press.

Reznikova, Z., & Ryabko, B. 2001. A study of ants' numerical competence. *Electronic Transactions on Artificial Intelligence*, **5**, 111–126.

Reznikova, Z.I., & Ryabko, B. 1996. Transmission of information regarding the quantitative characteristics of an object in ants. *Neuroscience and Behavioral Physiology*, **26**(5), 397–405.

Richard, D. 1976. *The selfish gene.* Oxford: Oxford University Press.

Robert, A. 1998. Blending and other conceptual operations in the interpretation of mathematical proofs. *Pages 337–350 of:* Koenig, J.P. (ed), *Discourse and Cognition. Bridging the gap.* Stanford: Center for the Study of Language and Information (CSLI) [distributed by Cambridge University Press].

Roberts, W.A., & Mitchell, S. 1994. Can a pigeon simultaneously process temporal and numerical information? *Journal of Experimental Psychology: Animal Behavior Processes*, **20**, 66–78.

Roland, J.W. 2007. Maddy and mathematics: Naturalism or not. *The British Journal for the Philosophy of Science*, **58**, 423–450.

Roman, S. 1984. *The umbral calculus.* New York: Academic Press.

Rumelhart, D.E., & McClelland, J.L. 1986. *Parallel distributed processing: explorations in the microstructure of cognition.* Cambridge, MA: MIT Press.

Russell, B. 1902. Letter to Frege. *Pages 124–125 of:* Heijenoort, Jean van (ed), *From Frege to Gödel: a source book in mathematical logic, 1879-1931.* Cambridge: Harvard University Press, 1967.

Santens, S., & Gevers, W. 2008. The SNARC effect does not imply a mental number line. *Cognition*, **108**(1), 263–270.

Santos, L.R., Barnes, J.L., & Mahajan, N. 2005. Expectations about numerical events in four lemur species (Eulemur fulvus, Eulemur mongoz, Lemur catta and Varecia rubra). *Animal Cognition*, **8**(4), 253–262.

Schlimm, D., & Neth, H. 2008. Modeling Ancient and Modern Arithmetic Practices: Addition and Multiplication with Arabic and Roman Numerals. *Pages 2097–2102 of:* Sloutsky, V., Love, B., & McRae, K. (eds), *Proceedings of the thirtieth annual meeting of the Cognitive Science Society.* Austin, TX: Cognitive Science Society.

Schmandt-Besserat, D. 1979. An Archaic Recording System in the Uruk-Jemdet Nasr Period. *American Journal of Archaeology*, **83**(1), 19–48.

Schumacher, C. 1996. *Chapter zero: fundamental notions of abstract mathematics.* 2 edn. Addison Wesley Publishing Company.

Serfati, M. 2005. *La revolution symbolique. La constitution de l'écriture symbolique mathématique.* Paris: Petra.

Shapiro, S. 2000. *Thinking about mathematics.* New York: Oxford University Press.

Shifferman, E.M. 2009. Its own reward: lessons to be drawn from the reversed-reward contingency paradigm. *Animal cognition*, **12**(4), 547–558.

Sørensen, H. 2010. Exploratory experimentation in experimental mathematics: A glimpse at the PSLQ algorithm. *Pages 341–360 of:* Löve, B., & Müller (eds), *PhiMSAMP. Philosophy of Mathematics: Sociological Aspects and Mathematical Practice.* Texts in Philosophy 11. College Publications.

Starkey, P., & Cooper, R.G. 1980. Perception of numbers by human infants. *Science, New Series*, **210**(4473), 1033–1035.

Starkey, P., Spelke, E.S., & Gelman, R. 1990. Numerical abstraction by human infants. *Cognition*, **36**(2), 97–127.

Steiner, M. 2005. Mathematics-Application and Applicability. *Pages 625–650 of:* Shapiro, S. (ed), *The Oxford Handbook of Philosophy of Mathematics and Logic*. Oxford: Oxford University Press.

Stevens, J.R., Wood, J.N., & Hauser, M.D. 2007. When quantity trumps number: discrimination experiments in cotton-top tamarins (Saguinus oedipus) and common marmosets (Callithrix jacchus). *Animal Cognition*, **10**(4), 429–437.

Stjernfelt, F. 2006. Two Iconicity Notions in Peirce's Diagrammatology. *Lecture Notes in Computer Science*, **4068**, 70–86.

Sulkowski, G.M., & Hauser, M.D. 2001. Can rhesus monkeys spontaneously subtract? *Cognition*, **79**(3), 239–262.

Temple, E., & Posner, M.I. 1998. Brain mechanisms of quantity are similar in 5-year-old children and adults. *Proceedings of the National Academy of Sciences USA*, **95**(13), 7836–7841.

Thom, R. 1971. 'Modern' Mathematics: An Educational and Philosophical Error? *American Scientist*, **59**(6), 695–9.

Thompson, P.W., & Dreyfus, T. 1988. Integers as transformations. *Journal for Research in Mathematics Education*, **19**(2), 115–133.

Thorup, A. 1998. *Matematik 2AL: Algebra*. 2. edn. København: Matematisk Afdeling, Københavns Universitet.

Tomonaga, M. 2008. Relative numerosity discrimination by chimpanzees (Pan troglodytes): evidence for approximate numerical representations. *Animal Cognition*, **11**(1), 43–57.

Tóth, I. 1969. Non-Euclidean Geometry before Euclid. *Scientific American*, **221**(11), 87–98.

Trick, L.M., & Pylyshyn, Z.W. 1994. Why are small and large numbers enumerated differently? A limited-capacity preattentive stage in vision. *Psychological Review*, **101**, 80–102.

TYMOCZKO, T. 1979. The four-color problem and its philosophical significance. *The Journal of Philosophy*, **76**(2), 57–83.

ULLER, C., & LEWIS, J. 2009. Horses (Equus caballus) select the greater of two quantities in small numerical contrasts. *Animal cognition*, **12**(5), 733–738.

ULLER, C., HAUSER, M., & CAREY, S. 2001. Spontaneous representation of number in cotton-top tamarins (Saguinus oedipus). *Journal of Comparative Psychology*, **115**(3), 248–257.

ULLER, C., JAEGER, R., GUIDRY, G., & MARTIN, C. 2003. Salamanders (Plethodon cinereus) go for more: rudiments of number in an amphibian. *Animal Cognition*, **6**(2), 105–112.

VAN HEIJENOORT, J. (ed). 1967. *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931.* Cambridge, MA: Harvard University Press.

VAN KERKHOVE, B. 2006. Mathematical Naturalism: Origins, Guises, and Prospects. *Foundations of science*, **11**(1), 5–39.

VIÈTE, F. 1983. *The Analytic Art, translated by T. Richard Witmer.* Ohio: Kent State University Press.

VLASAK, A.N. 2006. Global and local spatial landmarks: their role during foraging by Columbian ground squirrels (Spermophilus columbianus). *Animal Cognition*, **9**(1), 71–80.

VON GLASERSFELD, E. 1982. Subitizing: The role of figural patterns in the development of numerical concepts. *Archives de Psychologie*, **50**(194), 191–218.

WALLIS, J. 1685. *A Treatise of Algebra: Both Historical and Practical, Showing the Original, Progress, and Advancement Thereof, From Time to Time; and by What Steps It Hath Attained to the Height at Which Now It Is.* London: Playford.

WANG, H. 1949. On Zermelo's and Von Neumann's Axioms for Set Theory. *Proceedings of the National Academy of Sciences*, **35**(3), 150–155.

WARD, C., & SMUTS, B.B. 2007. Quantity-based judgments in the domestic dog (Canis lupus familiaris). *Animal Cognition*, **10**(1), 71–80.

West, R.E., & Young, R.J. 2002. Do domestic dogs show any evidence of being able to count? *Animal Cognition*, **5**(3), 183–186.

Whitehead, A.N., & Russell, B. 1910. *Principia mathematica.* Vol. 1. Cambridge: Cambridge University Press.

Wilson, M. 2002. Six views of embodied cognition. *Psychonomic Bulletin & Review*, **9**(4), 625–636.

Wilson, M.L., Hauser, M.D., & Wrangham, R.W. 2001. Does participation in intergroup conflict depend on numerical assessment, range location, or rank for wild chimpanzees? *Animal Behaviour*, **61**(6), 1203–1216.

Wilson, M.L., Britton, N.F., & Franks, N.R. 2002. Chimpanzees and the mathematics of battle. *Proceedings of the Royal Society of London-B-Biological Sciences*, **269**(1496), 1107–1112.

Wittgenstein, L. 1958. *Philosophical Investigations. Translated by G.E.M. Anscombe.* Oxford: Basil Blackwell.

Wittgenstein, L. 1976. *Wittgenstein's lectures on the foundations of mathematics. From the notes on R. G. Bosanquet, Norman Malcolm, Rush Rhees, and Yorick Smythies. Ed. by C. Diamond.* Hassocks, Sussex: The Harvester Press Limited.

Woodruff, G., & Premack, D. 1981. Primative mathematical concepts in the chimpanzee: Proportionality and numerosity. *Nature*, **293**, 568–570.

Wynn, K. 1990. Children's understanding of counting. *Cognition*, **36**(2), 155–93.

Wynn, K. 1992a. Addition and subtraction by human infants. *Nature*, **358**, 749–50.

Wynn, K. 1992b. Children's acquisition of the number words and the counting system. *Cognitive Psychology*, **24**(2), 220–251.

Wynn, K. 1996. Infants' individuation and enumeration of actions. *Psychological Science*, **7**, 164–169.

Wynn, K. 1997. Competence models of numerical development. *Cognitive Development*, **12**(3), 333–339.

Xia, L., & Emmerton, J. 2001. Pigeons (Columbia Livia) learn to link numerosities with symbols. *Journal of Comparative Psychology*, **115**(1), 83–91.

Xia, L., Siemann, M., & Delius, J.D. 2000. Matching of numerical symbols with number of responses by pigeons. *Animal Cognition*, **3**(1), 35–43.

Xu, F. 2003. Numerosity discrimination in infants: Evidence for two systems of representations. *Cognition*, **89**(1), 15–25.

Xu, F., & Arriaga, R.I. 2007. Number discrimination in 10-month-old infants. *British journal of developmental psychology*, **25**(1), 103–108.

Xu, F., & Spelke, E.S. 2000. Large number discrimination in 6-month-old infants. *Cognition*, **74**(1), 1–11.

Xu, F., Spelke, E.S., & Goddard, S. 2005. Number sense in human infants. *Developmental Science*, **8**(1), 88–101.

Yong, L.L. 1994. Jiu zhang suanshu (nine chapters on the mathematical art): An overview. *Archive for History of Exact Sciences*, **47**(1), 1–51.

Zahavi, D. 2004. Phenomenology and the project of naturalization. *Phenomenology and the cognitive sciences*, **3**(4), 331–347.

Zermelo, E. 1908a. A new proof of the possibility of a well-ordering. *Pages 183–198 of:* Van Heijenoort, J. (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931.* Cambridge, MA: Harvard University Press, 1967.

Zermelo, E. 1908b. Investigations in the foundations of set theory I. *Pages 198–215 of:* Van Heijenoort, J. (ed), *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931.* Cambridge, MA: Harvard University Press, 1967.

Zhang, J. 1997. The nature of external representations in problem solving. *Cognitive Science*, **21(2)**, 179–217.

Zhang, J., & Norman, D. A. 1995. A representational analysis of numeration systems. *Cognition*, **57**, 271–295.

Zorzi, M., Priftis, K., & Umiltà, C. 2002. Brain damage: neglect disrupts the mental number line. *Nature*, **417**(6885), 138–139.