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Michael Toppel

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Abstract (English)
The so-called Ramsey-Carnap-approach, ramseyfication, syntactical or received view in theory-reconstruction has yet not been fully studied in its relation to mathematical entities. While most prominent authors, like Psillos [51] and Demopoulos [22], subsumed mathematical entities mainly as references for theoretical terms, I will point out that Carnap's original approach advanced that mathematical terms were part of the (extended) observational language. Their references are, therefore, distinct from the theoretical entities, i.e. those a theory ontologically presupposes. This will lead into a carnaperian solution of the *Application Problem of Mathematics* in the sciences, when embracing the structuralism immanent in ramseyfication. In order to make this proposed solution convincing, the relations of formalisation and explification to a given scientific theory have to be reconsidered in the light of new developments in the philosophy of formal languages. To keep formalisations informative in the relevant sense, the views that formalisations are linguistic models or translations are dismissed in favour of a *Kreisel's squeezing.*

Abstract (German)
Die Ramsey-Carnap Methode, Ramseyfikation, syntaktischer Ansatz oder der Received View auf dem Gebiet der Theorierekonstruktion wurde bisher nur unzureichend in seinem Verhältnis zu mathematischen Objekten untersucht. Prominente Autoren wie Psillos [51] und Demopoulos [22] betrachten mathematische Objekte als Referenzen des theoretischen Vokabulars einer Theorie. Ich werde aber herausarbeiten, dass Carnaps ursprüngliche Methode mathematische Begriffe als Teil einer (erweiterten) Beobachtungssprache auffasst. Ihre Referenzen sind damit von theoretischen Objekten, d.h. jener Ontologie, die die Theorie voraussetzt, zu unterscheiden. Der immanente Strukturalismus dieser Methode führt dadurch zu einer carnapschen Lösung des *Anwendungsproblems der Mathematik* in den Naturwissenschaften. Um diese Lösung geltend zu machen, muss das Verhältnis von Formalisierung und Explikation zu einer wissenschaftlichen Theorie im Lichte der neuen Entwicklungen in der Philosophie der formalen Sprachen bewertet werden. Um die Anwendbarkeit von Formalisierung überhaupt zu garantieren, werden jene Ansätze, die Formalisierungen als Modellbildung für oder Übersetzungen von natürlichen Sprachen verstehen, zurückgewiesen. An ihre Stelle wird ein *Kreisel's Squeezing* gesetzt.

Contents

Introduction

Even though this book is written for the purpose to be handed in as a Master Thesis for the current HPS master program of the University of Vienna, it might not be seen as a usual thesis. I wrote the current text in a relatively advanced stage of my intellectual development as a logician and philosopher. Hence one should be careful when starting to read this text. Those readers who expect an overview over the Application Problem of Mathematics, the most popular work on the problem or a list of attempts to resolve it will be disappointed, for I am not aiming to give any of these. The current text is rather of a hands-on conceptual nature and aims to develop a single stance in the debate about formal methods and their application in general. That these will also solve the application problem of mathematics lies in the nature of this project. Also, if the solution seems beneficial to the reader, she might take this book as an advertisement for my stance or at least parts of it.

The content of this book is in some respect to my overall development fairly new, but at its core a position I was trying to develop in the last decade. Besides my deep devotion to Rudolf Carnap's work, promoting proof-theoretical methods in philosophy and logic was always my main concern. However, during my PhD in mathematics I almost gave up on academic philosophy. It was the rage which I developed while reading John MacFarlane's PhD Thesis and my passion for Medieval Philosophy that brought me back. Of course, my first focus was proof-theoretical semantics as the most natural choice. However, I always found the pragmatistic semantics, with which this stance is often combined, very unconvincing. After some failed attempts to tackle pragmatistic semantics and change the focus on proof-theoretical methods by publishing critical papers, I realised that I have to come up with more than just criticism; the referees were frequently wondering what I am aiming at, when advising against the publication of my work. In effect, I used the opportunity, which the need of handing in a Master Thesis gave me, to develop a syntax-based position that is not rooted in a pragmatist's semantic of natural language. However, I struggled a lot to interpret and use the formal methods, which I collected and developed myself, in a convincing way. The

game changer, though not delivering much of substance for the current text, was Timothy Williamson's Book *The Philosophy of Philosophy*. And I want to thank Paul Tucek, who recommended the book again and again to me, for pointing towards it. However, the ideas presented here are mainly developed through reading Catarina Dutilh Novaes work and my attempts to stay in proof-theoretician's mainstream of the Solomon Feferman kind, when doing philosophy.

I also want to thank Georg Schiemer for risking his reputation as a supervisor by letting me fly into my own skies. Since I already thanked my parents twice, in a previously write Master and PhD Thesis, and I am a big boy now, I will spear the reader this. Also, since I am single and lacking a loving and supporting husband, or kids for this matter, the acknowledgements end here naturally.

A last warning must be given. I am suffering from a strong version of **dyslexia**. Due to the nature of the task of proofreading a text such as this book, it was virtually impossible to find a proofreader, who could deal with the technicalities of a philosophical text and also limit herself to minor corrections. These highly professional proofreaders are trained to start a back and forth process reaching at a text which is indistinguishable from a native speaker's work and ready for publication. Since starting such a process was off limits regarding deadlines and budget, I acted as my own proofreader. Since I know that offending people's aesthetics for written language might cause strong feelings, I want to trigger warn here. Also I want to apologise for any inconveniences which are caused by my writing skills.

Outline

This book is aiming towards a solution for the Application Problem of Mathematics in the natural sciences. The word “aiming” is rather descriptive, because I will not introduce the Application Problem before Chapter 3. Hence it seems demanding to sketch the general structure of the text and the line of argumentation at the beginning. This enterprise started with my realisation, that many authors in the Carnap exegesis fail to recognise that basic mathematical notions were considered observational in Carnap’s diction. This changes quite a bit, when thinking about the use of mathematics in the sciences. In a nutshell the idea is to use the mathematical notions occurring in the observational language as a fixture for mathematical structures, while using the weak structuralist stance for theoretical truth, that is immanent in Carnap’s approach, to tie the mathematical structures to the content of the theoretical part. This view on things is obviously at odds with any position that claims continuity between mathematics and the theoretical sciences, because it gives a picture of two parallel tales told by the sciences. However, I claim that this is more accurate to the sciences anyway. For those who disagree, the details can be found in this book, and for those who are straight away tempted by this idea: beware, severe problems are emerging. Hence, I must clear a path to reach this solution of the Application Problem. This is done in the following two chapters.

Chapter 1 is a little textbook-like introduction to logic, the philosophy of logic and the Ramsey-Carnap-approach, which leaves the reader with two problem:

First, it was famously claimed by Carnap, that the theoretical terms of a language lack a specified meaning, instead their semantical values are only partly and indirectly given through the observational language by the so-called correspondence rules. Hence the theoretical sentences of a theory lack a specified semantical value as well. As Rozeboom emphasised in [58] this is troublesome for any formalisation of such a theory, because we therefore lose the basis for deciding whether a formalisation is accurate to the target theory. This is especially a problem for my enterprise here, because I put a heavy burden on the structural content of the theory. But this content is accessed by the structures satisfying the used formalisation. Hence, if the formalisation departs from the theory, then it might be that the structure which is given through the formalisation differs from the one presupposed by the theory.

The second problem is related to the analytic-synthetic distinction, which is heavily discussed since Quine famously attacked it in *Two Dogmas* [56]. It is a recurring criticism on the Ramsey-Carnap-approach, which was recently put forward by Otsuka in [50] again, that Ramseyfication cannot get theories right, because it allows the impossible distinction between analytic and synthetic.

I counter both problems in Chapter 2 in one fell swoop by arguing against a linguistic view on formalisation. By using arguments borrowed from Dutilh Novaes [25] and Williamson's *Philosophy of Philosophy* [74] I argue against a (strong) connection between a (natural) language and a formalisation. Since formalisation lacks such a connection, Rozeboom's problem disappears, because the adequacy criterion which is presupposed by him is irrelevant for formalisation. Moreover, since there is no straightforward connection between a formalisation and the semantics of the (natural) language in which a theory is presented, the classical notion of analyticity is not explicated by the Ramsey-Carnap-approach. For knowing the adequate formalisation is an additional epistemologically loaded requirement for a sentence to be true in virtue of the theory and, therefore, cannot be presupposed without troubles. Hence, the problems with the analytic-synthetic distinction cannot be transported to this approach. This leaves open what analyticity means in the context of the Ramsey-Carnap-approach; a problem I am not tackling in the present text. Of course, purging the metatheory of formalisation from linguistics is calling for a different link between a formalisation and the theory it is formalising. I establish this connection by a Kreisel Squeezing as it was developed by Kreisel in [38]; i.e. a formalisation is adequate, if it was successfully used in a squeezing argument.

In the final Chapter 3 I start by telling a story about the development of

mathematics in relation to the sciences as told by Maddy in [47], which gives strong support to my view that the theoretical part of a theory and mathematical applications are not continues with each other. After introducing the application problem of mathematics properly, I distinguish between quantification (in the sense of using number terms), formalisation and mathematisation. Then I give a formalisation of the theory of infrared spectroscopy; this is a measurement technique with wide applications in organic chemistry. Then I go on to mathematise this formalisation and, therefore, the theory itself. I then try to draw general conclusions from this example. I close by taking opposition to Otsuka's very recent book [50].
There will be no separate discussion of structuralism. Because I see no reason to adopt any genuine claims from it. Wearing structuralist's glasses is only helpful to see the means by which mathematics is glued onto scientific theories; I do not understand the structural content of the Ramsey-sentence epistemologically or metaphysically in my approach. In fact, the function I assign to the basic mathematical notions occurring in the observational language, i.e. data given in very different units, with the later demanded by the measurement techniques, undermines an ontological or epistemological reading of the structuralism immanent in the Ramsey-Carnap-approach. (See Footnote 31 in Section 1.2.6)

Chapter 1

The Ramsey-Carnap-Approach

This chapter serves as a small technical introduction to logic and Carnap's techniques for analysing scientific theories.[1] It is necessary to start from "the beginning", because I will break with the mainstream in some respects. In fact, some readers might have already noticed that I dropped Lewis' name from what is frequently called the Ramsey-Carnap-Lewis-Approach. This is intended. For I will adopt a view that might be called externalistic and is close to Williamson's view as expressed in *The Philosophy of Philosophy* [74]. However, what I mean by this will not be clarified before Chapter 2. For some this might come across as an oddity, because Carnap's techniques are usually seen as a version of internalism or inferencialism by influential figures like Putnam [53, pp. 183-190], Lewis [40] and Boghossian [9, pp. 244-248]. However, it should not come as a surprise that most formal methods can be applied in competing stances. But in this case, whether my attempts of turning away from internalism are defensible, affects whether my solution to the application problem of mathematics is of any significance at all. Also, in order to follow Williamson, I am avoiding the word "semantics" when it comes to several formal techniques which are used in logical theory. Since this is only a name for the algebraic methods of model-theory or the calculations in proof-theory to structure valid arguments, my renaming might come across as an unnecessary alienation of well-known notions. But one should be consequent.

[1]Hence, it is an introduction to what is called *Wissenschaftslogik* in German.

1.1 Needed Technicalities

In the following two subsections I am going to define a second-order logical system in a standard way.[2] But my emphasis on Williamson's stance and following Feferman's view on logic made me present the technical parts in a way that might come across as an oddity. Also, I consider sequents and not formulas as the basic entity of logical theorising. In [69, Ch.1] it was argued that this makes a substantial difference, when it comes to proof-theoretical semantics. However, I am not touching the debate about proof-theoretic harmony in this book.

1.1.1 Syntax of Formal Logic

In order to define the set of formulas, the available symbols must be fixed. Those are separated into logical and non-logical symbols. The logical symbols comprise the following:[3]

1. Auxiliary symbols: (,) and ,[4]
2. The standard set of logical connectives: $\neg$, $\vee$, $\wedge$, $\rightarrow$
3. Quantifiers: $\forall$, $\exists$
4. Countably infinitely many free first-order variables: $a_1, a_2, a_3, ...$
5. Countably infinitely many bounded first-order variables: $x_1, x_2, x_3, ...$
6. Countably infinitely many free second-order variables: $U_1, U_2, U_3, ...$
7. Countably infinitely many bounded second-order variables:$X_1, X_2, X_3, ...$
8. The identity sign: $=$

[2] A logical system is a formal system put forward for the purpose of formalising a consequence relation which is in use. This is just a nominal definition. What the definiens means is extensively explained in Chapter 2.

[3] For readability I omit the necessary quotation mark. In general, I will adopt the convention that formulas are also used as names for themselves.

[4] Of course, the first comma and the second comma are different.

To show true colour, I want to emphasise that I reject any of the proposed characterisations of an interpreted symbol as logical. I consider such characterisations as pointless. I am following Feferman and announce myself as a debunker. The word "debunker" was introduced by John MacFarlane to make a general distinction of the aims in logical theorising. He calls a debunker a person who holds the following:

> The logician's method for studying validity is to classify arguments by their forms, but these forms (and the logical constants that in part define them) are logic's tools, not its subject matter. [45, Sec.8]

However, I am not going to emphasise, why this is a good stance to take. Viewing things like this, one might even circumnavigate this debate by calling one part of the alphabet fixed and the other variable. Because being fixed is the actual function of this part of the alphabet, while the following part differs with the scientific theory under consideration. Hence the non-logical alphabet might comprise non or finitely many of the following symbols.

1. Names: $c_1, ..., c_n$
2. Function symbols of different arities: $f_1, ..., f_m$
3. Predicate symbols of different arities: $P_1, ..., P_k$

Since the set of non-logical symbols is variable, I will emphasise its variability by denoting it with an α as a metavariable. Having clarified on the symbols, we can use these to form expressions, i.e. finite sequences of symbols. I use φ and ψ as variables ranging over expressions. It is not the case that any expression is meaningful. Hence we have to separate these which carry a meaning form those which do not.[5]

Definition 1. Term

[5] It is commonly overlooked, that this definition is already a part of semantics. Because it gives sufficient and necessary conditions for which expressions carry content. This distinguishes formal languages from natural languages. In the latter syntax-trees are introduced after one agrees on the set of meaningful sentences by an external criterion.

1. Any free first-order variable is a term.

2. Any name in α is a term.

3. If $t_1, ..., t_n$ are terms, $f \in \alpha$ and is of arity n, then $f(t_1, ..., t_n)$ is a term.

Definition 2. Atomic Formula
If φ has the form $P(t_1, ..., t_n)$, where $P \in \alpha$ is of arity n and $t_1, ..., t_n$ are terms, then φ is an atomic formula.
If φ has the form $U(t_1, ..., t_n)$, where U is a free second-order variable which is of arity n and $t_1, ..., t_n$ are terms, then φ is an atomic formula.

In the following I will use x, y, z as variables for bounded first-order variables, a, b, c as variables for free first-order variables, X, Y, Z as variables for bounded second-order variables and U as a variable for free second-order variables on the metalevel. By $\varphi[\frac{x}{a}]$ I mean that in the expression φ any occurrence of an a has been replaced by an occurrence of x.

Definition 3. Formula
The set of formulas for the alphabet α, denoted by $\mathcal{F}(\alpha)$, is inductively defined as follows.

1. If φ is an atomic formula, then $\varphi \in \mathcal{F}(\alpha)$.

2. If $\varphi \in \mathcal{F}(\alpha)$, then $(\neg\varphi) \in \mathcal{F}(\alpha)$.

3. If $\varphi \in \mathcal{F}(\alpha)$ and $\psi \in \mathcal{F}(\alpha)$, then $(\varphi \vee \psi) \in \mathcal{F}(\alpha)$.

4. If $\varphi \in \mathcal{F}(\alpha)$ and $\psi \in \mathcal{F}(\alpha)$, then $(\varphi \wedge \psi) \in \mathcal{F}(\alpha)$.

5. If $\varphi \in \mathcal{F}(\alpha)$ and $\psi \in \mathcal{F}(\alpha)$, then $(\varphi \rightarrow \psi) \in \mathcal{F}(\alpha)$.

6. If $\varphi \in \mathcal{F}(\alpha)$ and a is a free first-order variable, then $(\forall x\varphi[\frac{x}{a}]) \in \mathcal{F}(\alpha)$.

7. If $\varphi \in \mathcal{F}(\alpha)$ and a is a free first-order variable, then $(\exists x\varphi[\frac{x}{a}]) \in \mathcal{F}(\alpha)$.

8. If $\varphi \in \mathcal{F}(\alpha)$ and U is a free second-order variable, then $(\forall X\varphi[\frac{X}{U}]) \in \mathcal{F}(\alpha)$.

9. If $\varphi \in \mathcal{F}(\alpha)$ and U is a free second-order variable, then $(\exists X \varphi[\frac{X}{U}]) \in \mathcal{F}(\alpha)$.

A $\varphi \in \mathcal{F}(\alpha)$ which does not contain any free variables of either order is called a *sentence*.

Definition 4. Sequent
If $\Phi, \Psi \subset \mathcal{F}(\alpha)$ and finite, we call the pair $\langle \Phi, \Psi \rangle$ a sequent. We will denote it by $\Phi \vdash \Psi$. If Φ and Ψ contain sentences only and Ψ is a singleton containing ψ, then we call $\Phi \vdash \psi$ an *argument*. In this case we call the elements of Φ the *premises* and ψ the *conclusion*.

The inclusion of the last definition is affected by the latest transformations in logic. From a philosopher's perspective, logic changed a bit since Frege. Because logical truth lost its priority over arguments. Philosophically orientated logicians have started to put the focus on the consequence relation instead. (See [4]) I am very fond of this attempts to strip the last bits of rationalism off and reunite with the scholastic tradition. But it is not just my personal performance as a traditionalist that made me include Definition 4. Making arguments the objects of logical theory leads naturally to a Gentzen-style calculus rather than to Natural Deduction. As it was shown in [69, Ch.1], this makes a difference for the interpretation of logical techniques in contexts of philosophy.[6]

1.1.2 Characterising Valid Arguments

As I emphasised in the previous section, I consider the main task of logic to characterise the consequence relation. The notion of logical consequence stayed essentially the same, since Aristotle defined it more than 2000 years ago. The following formulation is taken from Beall and Restall.

> **(V)** A conclusion, ψ, follows from premises, Φ, if and only if any case in which each premise in Φ is true is also a case in which ψ is true. [4, p. 476]

[6]Because it makes a difference for proof-theoretical semantics when studying whether introduction- and elimination-rules are in harmony.

To cover all mutually exchangeable ways of speaking, we say that ψ follows from premises Φ, if and only if ψ is a consequence of Φ, if and only if $\Phi \vdash \psi$ is a valid argument.

Formulating a logical theory is to clarify on the range of the variables Φ and ψ as well as to explicate what counts as a case.[7] There are several ways of doing this; many of them are non-equivalent. A historically interesting way is to view a case as a (metaphysically) possible world. This makes the consequence relation a relation for necessary truth preservation. This is close to what scholastics had in mind.[8] However, in this explication of the consequence relation the argument $B(b) \vdash C(b)$, which is read as "Brownie is brown, hence Brownie is coloured", is valid.[9] This is unpleasant, for even though it is a full-fledged logical theory, it is basically just a reduction to metaphysics or natural philosophy. Not even the medieval theorists considered this explication very fruitful for logical studies. They therefore developed, what is sometimes called, a *formal consequence* relation, i.e. an explication of the consequence relation which can be analysed by taking only the sentences occurring in an argument into account.[10] The reader should be careful about drawing epistemological conclusion too quickly; this is not enough to assure an a priori or analytical nature of logical relations. Because the explication has to be known and accepted before validity can be recognised.[11] But explicating the consequence relation as a formal consequence, has the additional advantage that logic becomes independent of other academic disciplines. And at least for the purpose of doing philosophy of science or studying scientific theories, as I plan to do in the following chapters, this seems like a good desideratum in order to avoid circularities or strange dependencies.

In the next two subsections I will give two explications of (V). These are fairly standard and some readers might want to skip the following passages, but I will use both explications later in certain arguments.

[7] Readers who are not familiar with the term "explication" shall not worry, it is properly introduced later in Chapter 2. For now, the reader might read it as a fancy way of saying "explained".

[8] See John Buridan's definition of logical consequence in [11, p. 67].

[9] See [4] for details.

[10] Reader who are interested in medieval theories are referred to [11, p. 68].

[11] This is one of the main points in Williamson's *The Philosophy of Philosophy* [74].

Tarski's Referential Approach

At this point the canon is usually talking about a semantics of a logical system or a so-called formal language. Since I will reject the view that formal systems are languages and clarify about their relations to a (natural) language in Chapter 2, I do not talk about a semantics at this point. But even if someone wants to see $\mathcal{F}(\alpha)$ as a full-fledged language, then this is not the point where semantics starts. It has already started with Definition 3. Because it is Definition 3 that gives necessary and sufficient conditions for carrying a meaning, i.e. being a sentence. This is very different to the syntax-semantic distinction in contexts where (natural) languages are studied by using structures from formal semantics. In these contexts, syntax-trees are assigned to sentences and Frege's idea of functional application does not play a role before these syntax-trees are related to the structures of formal semantics. But in logical systems, the idea of functional application is already in place when the syntax is stated. Of course, there is a difference between a symbol and the abstract object it refers to, but this is not the same division as the syntax-semantics distinction for (natural) languages.
Since I am planning to analyse theories that actually occur in the sciences, we ran across the phenomenon of names which might not name anything, i.e. lack a reference. In order to account for this phenomenon, one must alter the definition of a model-theoretical interpretation from those commonly given in the literature, as Lewis pointed out in [40]. A possible way to do so is to use a technical apparatus Lewis took in [40, p. 430] from Dana Scott. Also note that these troubles are only occurring in the case of names, because the empty set is a predicate extension as well as a function. Hence the rest of α does not need special treatment. However, I decided to stick to the ordinary model-theoretical definitions to formalise reference and will not extend it to a more general setting including reference lacking names. [12]

[12] In an earlier version of this book, I started with presenting my own account of treating names which lack a reference. I gave up on this, for I did not see any benefits. Aiming at a Kreisel's Squeezing in Chapter 2, some might wonder whether my lack of awareness of reference-lacking names is jeopardising the argument. I do not think so. Since a formalisation is not a translation or a model, nothing is lost, when the truth value has changed in the process of formalisation, as long as the logical properties are preserved, i.e.

Going back to (V), the idea of Tarski's approach is to identify the cases from (V) with different choices of a reference for the symbols. Hence any symbol of α gets an entity assigned to it and by this assignment a notion of truth can be defined. Hence, changing the assignments changes the truth value. Therefore, an assignment can be taken as a case. To make this approach formal the assignments of the logical vocabulary must be kept fixed. The totality of these assignments is sometimes called a model-theoretic interpretation. This notion is defined in the following. As it was just said, restricting the reference changes *to a subset of the symbols* is what makes this approach formal. For by fixing the assignments of some symbols but vary those of others, distinguishes the first part of the vocabulary. It makes these symbols the constitutive parts of the, in virtue of this distinction, persistent form of an argument.[13]

Definition 5. Model-Theoretical Interpretation
Assume $\mathcal{U}$ is a non-empty set. (I call this the universe.) An interpretation $\mathfrak{M}$ of α is a function satisfying the following conditions:

1. For any name $c \in \alpha$ either $\mathfrak{M}(c) \in \mathcal{U}$.
2. For any predicate symbol $P \in \alpha$ of n-arity $\mathfrak{M}(P) \subset \mathcal{U}^n$.
3. For any function symbol $f \in \alpha$ of n-arity a function

$$\mathfrak{M}(f) : \mathcal{U}^n \rightarrow \mathcal{U}.$$

In order to formulate the next definition, we need assignments for free variables; these are functions that assign elements of $\mathcal{U}$ to free first-order and subsets of $\mathcal{U}^n$ to second-order variables:

$$\mathfrak{s}(x) = \begin{cases} u \in \mathcal{U} & : \quad x \in \{a_1, a_2, a_3, ...\} \\ U \subset \mathcal{U}^n & : \quad x \in \{U_1, U_2, U_3, ...\} \text{ and } x \text{ has n-arity} \end{cases}$$

those properties of a sentences which affect its behaviour in the consequence relation.

[13] One should probably add here, that the choice of the logical vocabulary is not completely arbitrary, since some logicians try do characterise logicality as being invariant under assignments. However, even though this characterisation managed to identify logical connectives, it also makes infinite quantification a logical notion and, therefore, versions of arithmetical induction (see [30]). The later makes the characterisation useless, at least in my opinion.

Also we have to expand assignments for free-variables $\mathfrak{s}$ to assignment for terms $\bar{\mathfrak{s}}$, by running an inductive definition on the length of terms.

1. $\bar{\mathfrak{s}}(a) = \mathfrak{s}(a)$, if a is a free first-order variable.
2. $\bar{\mathfrak{s}}(c) = \mathfrak{M}(c)$, if $c \in \alpha$ and is a name.
3. $\bar{\mathfrak{s}}(t) = \mathfrak{M}(f)(\bar{\mathfrak{s}}(t_1), ..., \bar{\mathfrak{s}}(t_n))$, if t is the term $f(t_1, ..., t_n)$.

In the next definition I give the standard definition of truth in accordance to an interpretation.

Definition 6. Model-Theoretical Truth
The truth of $\varphi \in \mathcal{F}(\alpha)$ in $\mathfrak{M}$ under $\mathfrak{s}$ is inductively defined on the length of φ and denoted by $\mathfrak{M}, \mathfrak{s} \models \varphi$.

1. $\mathfrak{M}, \mathfrak{s} \models P(t_1, ..., t_n) \quad \Leftrightarrow \quad \langle \bar{\mathfrak{s}}(t_1), ..., \bar{\mathfrak{s}}(t_n) \rangle \in \mathfrak{M}(P)$.
2. $\mathfrak{M}, \mathfrak{s} \models U(t_1, ..., t_n) \quad \Leftrightarrow \quad \langle \bar{\mathfrak{s}}(t_1), ..., \bar{\mathfrak{s}}(t_n) \rangle \in \mathfrak{s}(U)$.
3. $\mathfrak{M}, \mathfrak{s} \models \neg\varphi \quad \Leftrightarrow \quad \mathfrak{M}, \mathfrak{s} \not\models \varphi$.
4. $\mathfrak{M}, \mathfrak{s} \models \varphi \wedge \psi \quad \Leftrightarrow \quad \mathfrak{M}, \mathfrak{s} \models \varphi$ and $\mathfrak{M}, \mathfrak{s} \models \psi$.
5. $\mathfrak{M}, \mathfrak{s} \models \varphi \vee \psi \quad \Leftrightarrow \quad \mathfrak{M}, \mathfrak{s} \models \varphi$ or $\mathfrak{M}, \mathfrak{s} \models \psi$.
6. $\mathfrak{M}, \mathfrak{s} \models \varphi \rightarrow \psi \quad \Leftrightarrow \quad \mathfrak{M}, \mathfrak{s} \not\models \varphi$ or $\mathfrak{M}, \mathfrak{s} \models \psi$.
7. $\mathfrak{M}, \mathfrak{s} \models \exists x\varphi \quad \Leftrightarrow \quad$ There is a $\mathfrak{s}'$, that satisfies $\mathfrak{s}'(y) = \mathfrak{s}(y)$ for any $y \neq a$, such that $\mathfrak{M}, \mathfrak{s}' \models \varphi(\frac{a}{x})$.
8. $\mathfrak{M}, \mathfrak{s} \models \forall x\varphi \quad \Leftrightarrow \quad$ For any $\mathfrak{s}'$, that satisfies $\mathfrak{s}'(y) = \mathfrak{s}(y)$ for any $y \neq a$, holds that $\mathfrak{M}, \mathfrak{s}' \models \varphi(\frac{a}{x})$.
9. $\mathfrak{M}, \mathfrak{s} \models \exists X\varphi \quad \Leftrightarrow \quad$ There is a $\mathfrak{s}'$, that satisfies $\mathfrak{s}'(y) = \mathfrak{s}(y)$ for any $y \neq U$, such that $\mathfrak{M}, \mathfrak{s}' \models \varphi(\frac{U}{X})$.
10. $\mathfrak{M}, \mathfrak{s} \models \forall X\varphi \quad \Leftrightarrow \quad$ For any $\mathfrak{s}'$, that satisfies $\mathfrak{s}'(y) = \mathfrak{s}(y)$ for any $y \neq U$, holds that $\mathfrak{M}, \mathfrak{s}' \models \varphi(\frac{U}{X})$.

Just to be complete, the following theorem holds by the standard proofs as, for instance, given in [28, Ch.4].

Theorem 1.1.1. *For any sentence φ and any two assignments $\mathfrak{s}$ and $\mathfrak{s}'$ holds that:*

$$\mathfrak{M}, \mathfrak{s} \models \varphi \quad \Leftrightarrow \quad \mathfrak{M}, \mathfrak{s}' \models \varphi.$$

Therefore, in the case of a sentence, there is no need to mention assignments.

Note that logic has not started yet. I presented this approach in full detail, because I must talk about reference, interpretations and models of certain theories in future chapters. The relation to logic is given in the next definition, where interpretations and assignments are used to explicate the phrase "any case" in (V).

Definition 7. T-Valid Argument

1. A sequent $\Phi \vdash \Psi$ is satisfied by an interpretation $\mathfrak{M}$ and an assignment $\mathfrak{s}$, if and only if there is one $\varphi \in \Phi$ such that $\mathfrak{M}, \mathfrak{s} \not\models \varphi$ or there is one $\psi \in \Psi$ such that $\mathfrak{M}, \mathfrak{s} \models \psi$.

2. A sequent is T-valid, if and only if it is satisfied by any interpretation and assignment.

3. An argument is T-valid, if and only if it is T-valid as a sequent.

Note that The metaquantifier over all assignments assures, that

$$\varphi_1(a_1), \varphi_2(a_2) \vdash \psi(a_3) \text{ is valid } \Leftrightarrow \forall x \varphi_1(x), \forall y \varphi_2(y) \vdash \forall z \psi(z) \text{ is valid.}$$

In this sense, there are no free variables in arguments and, therefore, the cases of (V) are just the interpretations without the assignments.

Gentzen's Approach

Another way to tackle (V) is to deliberately leave an explication of "case" out. This is how one might read Gentzen, even though this is probably not how Prawitz and Gentzen understood their work by themselves. (See [54]) In this approach, one recognises that, whatever a case might be, (V) surely recognises $\varphi \vdash \varphi$ as a valid argument. Having this stated, one might wonder, whether there are certain properties of the consequence relation we agree on such that, if the sequent $\mathcal{S}_1, ..., \mathcal{S}_n$ are valid, then also is $\mathcal{S}$. This can be written as

$$\frac{\mathcal{S}_1, ..., \mathcal{S}_n}{\mathcal{S}}$$

and is usually called an inference rule. I will stress this point in Chapter 2 in more detail, but the reader should not interpret these rules as guiding the use of logical notions in an idealised setting for argumentations (straight away).[14] Several logicians have proposed different sets of these properties and, therefore, proposed different theories about the consequence relation. A prominent one is denoted by **LK** and is called (first-order) *classical logic*. In addition to $\varphi \vdash \varphi$, it takes the consequence relation to enjoy the following properties over the formulas of $\mathcal{F}(\alpha)$. I denote them by their common names in inferential semantics.

1. Weakening:

$$\frac{\Gamma \vdash \Delta}{\varphi, \Gamma \vdash \Delta}\text{ (left)} \quad \frac{\Gamma \vdash \Delta}{\Gamma \vdash \Delta, \varphi}\text{ (right)}$$

2. Cut:

$$\frac{\Gamma \vdash \Delta, \varphi \quad \varphi, \Pi \vdash \Lambda}{\Gamma, \Pi \vdash \Delta, \Lambda}$$

[14] Some readers might be unhappy with this way of seeing things. However, I want to stress here that I do not have a linguistic view on logic, i.e. I neither see a calculus as a language nor a model of a language. The benefits of a syntactic approach over a referential are, in my view, not a modelling of the usage of logical notions, but the possibilities it opens to use a Kreisel Squeezing.

3. Negation:

$$\frac{(\neg\varphi), \Gamma \vdash \Delta}{\Gamma \vdash \Delta, \varphi}\ (\neg\text{-left}) \quad \frac{\Gamma \vdash \Delta, (\neg\varphi)}{\varphi, \Gamma \vdash \Delta}\ (\neg\text{-right})$$

4. Conjunction:

$$\frac{\varphi, \Gamma \vdash \Delta}{(\varphi \wedge \psi), \Gamma \vdash \Delta}\ (\wedge\text{-left}_1) \quad \frac{\varphi, \Gamma \vdash \Delta}{(\psi \wedge \varphi), \Gamma \vdash \Delta}\ (\wedge\text{-left}_2)$$

and

$$\frac{\Gamma \vdash \Delta, \varphi \quad \Gamma \vdash \Delta, \psi}{\Gamma \vdash \Delta, (\varphi \wedge \psi)}\ (\wedge\text{-right})$$

5. Disjunction:

$$\frac{\varphi, \Gamma \vdash \Delta \quad \psi, \Gamma \vdash \Delta}{(\varphi \vee \psi), \Gamma \vdash \Delta}\ (\vee\text{-left})$$

$$\frac{\Gamma \vdash \Delta, \varphi}{\Gamma \vdash \Delta, (\varphi \vee \psi)}\ (\vee\text{-right}_1) \quad \text{and} \quad \frac{\Gamma \vdash \Delta, \psi}{\Gamma \vdash \Delta, (\varphi \vee \psi)}\ (\vee\text{-right}_2)$$

6. Implication:

$$\frac{\Gamma \vdash \Delta, \varphi \quad \psi, \Pi \vdash \Lambda}{(\varphi \to \psi), \Gamma, \Pi \vdash \Delta, \Lambda}\ (\to\text{-left}) \quad \frac{\varphi, \Gamma \vdash \Delta, \psi}{\Gamma \vdash \Delta, (\varphi \to \psi)}\ (\to\text{-right})$$

7. Generalisation:

$$\frac{\varphi(t), \Gamma \vdash \Delta}{\forall x \varphi(x), \Gamma \vdash \Delta}\ (\forall^1\text{-left}) \quad \frac{\Gamma \vdash \Delta, \varphi(a)}{\Gamma \vdash \Delta, \forall x \varphi(x)}\ (\forall^1\text{-right})$$

where t is a term and a is a free variable not occurring in the lower sequent.

8. Existence:

$$\frac{\varphi(a), \Gamma \vdash \Delta}{\exists x \varphi(x), \Gamma \vdash \Delta}\ (\exists^1\text{-left}) \quad \frac{\Gamma \vdash \Delta, \varphi(t)}{\Gamma \vdash \Delta, \exists x \varphi(x)}\ (\exists^1\text{-right})$$

where t is a term and a is a free variable not occurring in the lower sequent.

After having these properties recognised, one can set up a calculus by stipulating them as derivation rules. Then one can generate valid arguments by starting with a $\varphi \vdash \varphi$ as an initial sequent and using these derivation rules to generate more complex sequents. Such calculations are often called *derivations* and have tree form.

Definition 8. Derivation

Assume $\mathfrak{D}$ is a finite set of derivation rules.[15] A $\mathfrak{D}$-*derivation* is a finite, ordered, rooted and labelled tree. Such that:

1. Each leave of the tree is labelled with a sequent of the form $\varphi \vdash \varphi$.

2. Any other note of the tree is labelled with a sequent such that the labels of adjoined notes satisfy the derivation rules of $\mathfrak{D}$.

This is already enough to give an account of validity.

Definition 9. G-Valid Argument Assume $\mathfrak{D}$ is a finite set of derivation rules.

1. A sequent $\mathcal{S}$ is $\mathfrak{D}$-valid, if and only if there is a $\mathfrak{D}$-derivation whose root is labelled with $\mathcal{S}$.

2. An argument is $\mathfrak{D}$-valid, if and only if it is $\mathfrak{D}$-valid as a sequent.

Since this approach characterises certain properties of the consequence relation through the structure of elements from $\mathcal{F}(\alpha)$, the approach is at a whole formal in nature. For, as before in Tarski's approach, the initial choices, e.g. which symbols are used to formulate the properties, give an account of form of an argument in virtue of this choices. But of course, there are several

[15] A derivation Rule can be defined as the primitive recursive set of sequences $\langle \mathcal{S}_1, ..., \mathcal{S}_n, \mathcal{S} \rangle$ which satisfy the schema given by

$$\frac{\mathcal{S}_1, ..., \mathcal{S}_n}{\mathcal{S}}$$

.

important differences to Tarski's approach, which occur through leaving out to explicate the notion of case in (V).
First, note that I did not include any rules of the second-order quantifiers in **LK** above. In Gentzen's approach this is not a problem. The form of a sub-formula which occurs in the scope of a second-order quantifier plays no role in a deduction, i.e. they act like variables for formulas. In the Tarski approach, on the other hand, one must deal with any occurring symbol. Viewing this virtue from a differing angle, Gentzen's approach is an open-end approach: one can expand the extension of the consequence relation by simply adding new rules to a sufficiently rich set of formulas.
This leads to a methodological benefit; the methods of studying logics do not differ much, when the set of rules changes. On the other hand, studying model-theoretical interpretations needs a broad variety of methods from algebra and set-theory.[16] If one wants a notion of G-validity that recognises second-order quantification, one simply adds rules for these quantifiers and comprehension rules. To give an example for such a set of rules I define $\mathbf{LK}^2$.

Definition 10. $\mathbf{LK}^2$ comprises all rules which are included in **LK** with the following additional rules for quantifiers:

1. Generalisation:

$$\frac{\varphi(\psi(\vec{t})), \Gamma \vdash \Delta}{\forall Y \varphi(Y(\vec{t})), \Gamma \vdash \Delta} \ (\forall^2\text{-left}) \quad \frac{\Gamma \vdash \Delta, \varphi(U(\vec{t}))}{\Gamma \vdash \Delta, \forall X \varphi(X(\vec{t}))} \ (\forall^2\text{-right})$$

2. Existence:

$$\frac{\varphi(U(\vec{t})), \Gamma \vdash \Delta}{\exists X \varphi(X(\vec{t})), \Gamma \vdash \Delta} \ (\exists^2\text{-left}) \quad \frac{\Gamma \vdash \Delta, \varphi(\psi(\vec{t}))}{\Gamma \vdash \Delta, \exists Y \varphi(Y(\vec{t}))} \ (\exists^2\text{-right})$$

Where Y has the same arity as the sub-formula ψ has occurrences of distinct terms in all cases.

[16]Of course, this has metatheoretical benefits, when finding an appropriate metatheory. However, this is not the place to discuss such matters.

The rules ($\forall^2$-left) and ($\exists^2$-right) are the Gentzen-style version of the full comprehension axiom. (See [68, pp. 165-172]) These are extremely strong, logically complex to handle and are, therefore, usually restricted to a certain subset of possible ψ's. However, explaining such matters here would exceed the scope of this book. I rather state how G-validity is related to T-validity. Those who want to see T-validity as the more basic notion, would call the following result a *completeness theorem* for G-validity. To give a smooth formulation of this result, I use the term *first-order Interpretation* to denote a function very similar to those given in Definition 5 and Definition 6. One must only exclude the cases for second-order quantifiers and variables. Analogously to Definition 7 one can then define first-order T-validity. Also the first-order fragment of $\mathcal{F}(\alpha)$ is the subset of $\mathcal{F}(\alpha)$ which comprises only those formulas that include no occurrences of second-order quantifiers or variable.

Theorem 1.1.2. *For any sequent $\mathcal{S}$ whose formulas are only taken from the first-order fragment of $\mathcal{F}(\alpha)$ holds that:*

*$\mathcal{S}$ is first-order T-valid if and only $\mathcal{S}$ is **LK**-valid.*

Proof. The proof is standard, goes by constructing a reduction tree and can be found in [68, pp. 40-45]. □

Even with techniques that can handle full comprehension, one must restrict Theorem 1.1.2 to the first order fragment. Because any notion of G-validity is much weaker than T-validity. This is a well-known fact, because equivalence of these two notions would decide the halting problem.

1.2 Ramsey and Carnap Sentences

We reach the point where we develop a logical method which can be applied to scientific theories. Since we mentioned Carnap's notion of explication already, it is important to keep in mind that the current formalisations are **not** supposed to give an explication of the notion of *scientific theory.* This was argued for by Lutz in [44, pp. 99-110]. What I describe in the following is only presented in a schematic way to give the means of its application most effectively. In other words, the following techniques have been developed in order to answer certain questions about the logical properties of particular theories. They are not and do not aim to give an account on what scientific theories are or should be. Neither do these methods presuppose that they capture all or most of what can be said about a scientific theory. Anyone will straight away agree that social, sociological or political aspects are not covered by the following methods; and tackling such issues is not the aim of this approach. But I want to emphasise that also many epistemological and some genuine issues from philosophy of science are not covered as well; and do not have to in order to make the Ramsey-Carnap Approach useful or enlightening. But more on this later.

1.2.1 Theories

In all what fallows, by a theory I simply mean a finite set of sentences of $\mathcal{F}(\alpha)$. Since most (or probably all) scientific theories, that are currently in use, are either finite or schematically defined, keeping theories finite does not restrict the application of our methods and does not require artificial reconstructions. However, I chose a second-order formalisation through using $\mathcal{F}(\alpha)$. From a technical perspective one has therefore to assure that, when using a G-validity approach, enough comprehension is available. Most authors do not care much about whether theories are finite ore infinite objects. But there might be a lot at stake, when it comes to instrumentalism or other metaphysical questions. Because the benefits a theory gets by including further assumptions might be related to whether the theory is finitely axiomatisable or not. The idea is that existence claims of merely instrumental entities shorten or simplify arguments in a theory. A good approach to measure simplicity is the study of the complexity of the related deductions of an argument. Such complexity decreasing effects are called speed-ups. But a speed-up of a theory over another theory, seems directly related to whether a theory is a finite or an infinite object.[17] However, I do not want to take a stance whether speed-ups

[17] See [12] for related research in the philosophy of mathematics.

are a good way to measure the instrumentalistic benefits of assumptions and we will not touch this matters in the following.
For now, I leave open how one obtains the finite set of sentences that we call a “theory” here. I am also not clarifying on the relation which a theory in our sense has to a theory outside in the fields. I will come back to this extensively in Chapter 2.
Note that a theory does not come with a logic. Hence, we must add one additionally. We can do this by choosing one of the formalised explications of (V). Note, that we benefit from our assumption that a theory is a finite object.

Definition 11. Assume $\mathcal{T} = \{\tau_1, ..., \tau_n\}$ is a theory.

1. We say that a formula φ is a T-consequence of $\mathcal{T}$, in symbols $\mathcal{T} \vdash_T \varphi$, when the sequent $\tau_1, ..., \tau_n \vdash \varphi$ is T-valid.

2. Assume that $\mathfrak{D}$ is a finite set of derivation rules. We say that a formula φ is a $\mathfrak{D}$-consequence of $\mathcal{T}$, in symbols $\mathcal{T} \vdash_{\mathfrak{D}} \varphi$, when the sequent $\tau_1, ..., \tau_n \vdash \varphi$ is $\mathfrak{D}$-valid.

Since we are going to apply this definition later, it should be emphasised that it does not matter which version of logical consequence is used. Carnap was quite liberal in this respect as Lutz pointed out in [44]. I have a strong tendency to use a Gentzen-style approach, because proofs are easier and straighter forward. However, as implied above, $\mathcal{T} \vdash_T \varphi$ is in general much stronger than $\mathcal{T} \vdash_{\mathfrak{D}} \varphi$. But argumentation in the sciences is finite as well, so if we want to study actually occurring theories, we do not lose much when using a Gentzen-style approach.

1.2.2 Theoretical and Observational Terms

What became characteristic for Carnap’s approach, which is sometimes also called the *received view* by protagonists of American analytical philosophy, is the separation of the vocabulary into theoretical and observational terms as described in [17]. Formally this is just a split of α into two distinct subsets α_O

and α_T, i.e. $\alpha = \alpha_O \cup \alpha_T$ and $\alpha_O \cap \alpha_T = \emptyset$. Roughly stated, the observational part is considered to be epistemologically and ontologically unproblematic, while the theoretical vocabulary is questionable in these respects. The separation into these two sets induces a division of the sentences of $\mathcal{F}(\alpha_O \cup \alpha_T)$ into three subsets: sentences containing no α_O, which are called theoretical sentences, sentences containing no α_T, which are called observational sentences, and sentences that contain elements of both parts of α. The later are called *correspondence rules* and link the two parts of α together. The idea behind this is that from an empiricist point of view observational statements can be more or less directly verified, while the theoretical sentences are only partially confirmed through the correspondence rules. Carnap himself famously held a verificational semantics, i.e. the meaning of a sentences is identified with the means by which it is recognised as true. Adopting a verificationalistic view implies then that the observational terms have a defined meaning, while the theoretical terms are only partly interpreted through the correspondence rules. If such a view is specified by a referential semantics, e.g. as given by Definition 6, then observational terms have a defined reference, while the reference of a theoretical term is only partly given. This is a consequence of the compositional nature of any referential semantics: through the indirectly given truth value, the options to choose a reference for a theoretical term are only restricted by the correspondence rules. I will explain this in more details in the following sections. As I said, I am not advocating the view, that semantics as a feature of (natural) language plays a role here. However, Carnap's picture reappears on the level of logical properties, but without strong commitments for meaning, communication or the use of (natural) languages.

1.2.3 The Observational-Theoretical Distinction

Carnap's methodology was prominently criticised by Putnam in [53], through the vagueness of the observational-theoretical distinction. The more successful critique was later given by Suppe in [65], where Putnam's critique was re-evaluated as unfair. However, Carnap's methodology was still considered inadequate through its tendency to cover the interesting aspects of a scientific theory. Suppe is willing to grant Carnap, that it is part of his methodology to clarify on the observational-theoretical distinction case by case and to draw the line where it suits him, but he thinks that the more interesting aspects of scientific theories are not affected by this choices. (See [65, p. 9]) A rather detailed description of the debate was given by Lutz in [40] where most of the objections were identified as misreads or confusions. We are not going deeper into this debate, but I want to use a particularly influential critique on Carnap to clarify on yet another confusion. Because this confusion will turn out crucial for the solution of the application problem of mathematics when resolved. As a very influential offspring of logical positivism, Bas Van Fraassen's antirealism became very influential and criticised Carnap's approach heavily on many levels. While for many Van Fraassen's antirealism is a model-theoretically boosted refinement of Carnap's approach, it is its referencialism that caused a misrepresentation of Carnap's observational-theoretical distinction. In [71, p. 1067] Van Fraassen criticised the observational-theoretical distinction as a confusion, because for him a term is always theoretical in virtue of being a term occurring in a theory. For him the distinction must be between whether the entity a term refers to is observable or unobservable. This at first convincing argument is missing the point in Carnap's distinction. Carnap is, as usually, rather ignorant towards ontological questions; what Carnap cares about is the context of verification of a sentence which occurs in an academic discipline. And for him, there are only two contexts: either the verification proceeds by observations, e.g. direct observations ore measurements, or by an argument drawing from the theoretical background of the academical discipline. But this distinction is made parallelly to that between observable and unobservable entities. Because terms which refer to unobservable entities, like abstract objects, occur frequently in observational context. Hence, he includes them in observational sentences as well.

> It is important to understand [...] that Ramsey's approach cannot be said to bring theories into the observation language if "observation language" means (as is often the case) a language containing

> only observational terms and the terms of elementary logic and mathematics. [15, p. 252]

It is clarified below what Ramsey's approach is. However, the important part here is that Carnap (usually) considers elementary notions of mathematics a part of the observational language. It might be debatable what an elementary notion of mathematics is here, but whatever the answer is, some number terms must be a part of it. Earlier at [15, p. 225-226], Carnap stresses the point that this might be hard to swallow for some. There he compares sensual observational statements like “This is hot” with statements that presuppose easy inferences from a measurement, e.g. the use of a ruler or a thermometer. He admits that in a narrow sense of observable, the latter would not be included. But he argues that the verification of a sentence stating a length which is measurable by a ruler is in a wider sense observable in contrast to certain theoretical claims concerning length, e.g. the distance between two electrons in an atom. He then claims that “observable” is a vague notion and clarifies:

> Empirical laws, in my terminology, are laws containing terms either directly observable by the senses or measurable by relatively simple techniques. [15, p. 226]

The reader should be careful here, because we have entered a messy situation. It seems uncontroversial that to measure the temperature with a thermometer is a case of “measurable by relatively simple techniques”. Hence, at least some sentences that include “temperature” must be considered empirical laws. And indeed, Carnap himself does. (See [15, p. 227]). But in [15, p. 232] he states that only observational terms occur in empirical laws. But this contradicts himself, because he counts “temperature” as a theoretical term. (See [17, p. 48]) To clarify on this, one has to separate the debate between sensualism vs. empiricism from the question whether we are speaking about a formalisation of a theory or about a sentence actually occurring in a scientific context. Empirical laws are not identical with observational sentences in the technical sense, even though he defines empirical laws as empirical observable. For Carnap an empirical law is something like “the temperature of this rock is 20°C".[18] Technically, after formalisation, this would turn into a correspondence rule, for it has an occurrence of the observational term "20" and of the theoretical term "temperature".[19] However, when we only want to argue for the inclusion of "20" into the observational part of the vocabulary, then there is an observational sentence (in the technical sense) that includes 20 and helps us: "the mercury of the thermometer reaches the 20 mark". Of course, here we meet again a line of critique that was already taken by Putnam in [53]; why should we dismiss an empirical law as a non-observable sentence and stick to the separation of the vocabulary into observational and theoretical terms? Since obviously there is a matter of degree included. To counter such worries, we do not re-invoke what Suppe admitted to Carnap in [65], i.e. the received view has to be granted the freedom to choose the observational-theoretical line case by case and in accordance to the fruitfulness of their method, but we can see how Demopoulos' much stronger response works.

> Carnap frequently emphasized that virtually every unreconstructed sentence of the language of science and everyday life has both factual and nonfactual aspects, so that a sharp separation of our language into factual and nonfactual sentences is meaningful only related to a reconstruction of that language. [22, p. 121]

Hence it should not be surprising that the technical notion of an observational sentence is not coextensive with the notion of an empirical law, which is used prior to formalisation, in Carnap's terminology. Therefore, we must distinguish between an easily empirically justifiable claim, i.e. an empirical law, and an observational sentence in a proposed formalisation in the sense of the received view. having this mess cleared up, we can establish that an observational sentence might refer to unobservable entities, e.g. abstract or mathematical entities.

[18] Carnap uses the example "a temperature of [...] 80 degrees centigrade". (See [16, p. 225])

[19] Carnap directly implies that this is a correspondence rule in [17, p. 48].

1.2.4 Separating the Theory

Now that we have introduced the main idea of the received view, we can apply it to the notion of a theory as it was already introduced. As we said, the bipartition of the vocabulary into observational and theoretical terms induces a tripartition of the set of sentences from $\mathcal{F}(\alpha)$ into observational sentences, theoretical sentences and correspondence rules. Hence, we can specify our notion of a theory as follows.

Definition 12. A theory in the sense of the received view (RV-theory) is a finite set of theoretical sentences and correspondence rules from $\mathcal{F}(\alpha_O \cup \alpha_T)$.

Since RV-theories are always finite, we can reformulate them as a single sentence, i.e. instead of RV-theory $\mathcal{T} = \{\tau_1, ..., \tau_n\}$ we can take $\tau \equiv \tau_1 \wedge ... \wedge \tau_n$. It does not make sense to say that this single sentence is equivalent to $\mathcal{T}$ itself, because we have not presupposed, and wont at this stage, that a RV-theory comes with a logic attached to it. Hence, there is no notion of equivalence available. But in all the logics which are commonly in use, a RV-theory is equivalent to such a sentence. Therefore, I treat them as the same object in the following.

More importantly, τ includes occurrences of theoretical as well as observational terms. I emphasise this by using the following notation: $\tau(\vec{O}, \vec{T})$, where $\vec{O}$ contains elements from α_O and $\vec{T}$ from α_T. It is important to note that the elements in $\vec{T}$ might be names, predicates or functions. However in order to keep the definitions readable, I assume that there are no function symbols in α_T.[20]

Definition 13. Assume $\tau(\vec{O}, \vec{T})$ is a sentence obtained from a RV-theory $\mathcal{T}$. The Ramsey-sentence of $\mathcal{T}$ is

$$\mathfrak{R}(\mathcal{T}) \equiv (\exists \vec{X})\tau(\vec{O}, \vec{X}).$$

[20]In principle excluding function symbols entirely does not cause any harm when it comes to mere formulations, one can simply use predicate symbols to substitute for functions. However, if one aims to study the instrumentalistic benefits of certain theoretical assumption with a G-validity approach, then a formulation via predicates causes a lot of damage. For instance, for some theories the provability of Kreisel's conjecture depends on whether the theory is formulated with or without function symbols.

We call this Ramsey-sentence also the Ramseyfication of the RV-theory $\mathcal{T}$. The Carnap-sentence of $\mathcal{T}$ is

$$\mathfrak{C}(\mathcal{T}) \equiv \mathfrak{R}(\mathcal{T}) \rightarrow \tau.$$

To study these sentences we have to combine them with a logic.[21] As already mentioned, there are several ways to add a logic to a theory. However, to keep proofs standard, I will take the T-validity approach.
Note that $\mathfrak{R}(\mathcal{T})$ does not include any occurrences of theoretical terms any more. But still, it has the following property.

Theorem 1.2.1. *For any observational sentence φ holds that:*

$$\mathcal{T} \vdash_T \varphi \textit{ if and only if } \mathfrak{R}(\mathcal{T}) \vdash_T \varphi.$$

Proof.

1. The direction from left to right is trivial. For any Interpretation that satisfies $\mathcal{T}$ one can use the interpretation for the theoretical terms as witnesses of the existential quantifiers occurring in $\mathfrak{R}(\mathcal{T})$.

2. The proof from right to left is taken from [58, pp. 291-292]. For any interpretation $\mathfrak{M}$ only two cases can appear: either $\mathfrak{M} \models \varphi$ or $\mathfrak{M} \not\models \varphi$.

    (a) Assume that $\mathfrak{M} \models \varphi$. Then Definition 7 is satisfied for $\mathfrak{R}(\mathcal{T}) \vdash_T \varphi$ and we are done.

    (b) Assume that $\mathfrak{M} \not\models \varphi$. To satisfy Definition 7 we have to show that also $\mathfrak{M} \not\models \mathfrak{R}(\mathcal{T})$. Since we can assume $\mathcal{T} \vdash_T \varphi$, we therefore have $\mathfrak{M} \not\models \tau$. Now we proceed indirectly. Assume that even though $\mathfrak{M} \not\models \tau$ we still have $\mathfrak{M} \models \mathfrak{R}(\mathcal{T})$. In this case we can find a new interpretation $\mathfrak{M}'$ such that $\mathfrak{M}' \models \tau$ as follows:

    - $\mathfrak{M}'$ and $\mathfrak{M}$ agree on α_O.
    - $\mathfrak{M}'$ maps α_T to the witnesses of the existential quantifiers occurring in $\mathfrak{R}(\mathcal{T})$ which are given by $\mathfrak{M} \models \mathfrak{R}(\mathcal{T})$.

[21] To be precise, when using a Gentzen-style approach, it is not necessary to allow only rules that are considered as logical. One might use any kind of derivation rules that make sense in the study of the particular theory and the academic discipline which accommodates it.

Since $\mathfrak{M}'$ and $\mathfrak{M}$ agree on α_O, we have $\mathfrak{M}' \not\models \varphi$. Because we had $\mathfrak{M} \not\models \varphi$ before and φ contains only occurrences of symbols of α_O. But then $\mathfrak{M}'$ contradicts $\mathcal{T} \vdash_T \varphi$. Hence we have established that $\mathfrak{M} \not\models \mathfrak{R}(\mathcal{T})$ as required by Definition 7.

$\square$

We just showed that $\mathfrak{R}(\mathcal{T})$ implies the same observational sentences as the original theory $\mathcal{T}$, without any needs for the theoretical vocabulary. It is therefore said that $\mathfrak{R}(\mathcal{T})$ projects $\mathcal{T}$ into the observational part of the language. $\mathfrak{C}(\mathcal{T})$ on the other hand does the opposite, it does not prove substantial observational sentence, i.e. it stays heuristically in the theoretical part of the language.

Theorem 1.2.2. *For any observational sentence φ holds that:*

$$\text{If } \mathfrak{C}(\mathcal{T}) \vdash_T \varphi, \text{ then } \emptyset \vdash_T \varphi.$$

Proof. We show the contrapositive. Assume that $\emptyset \vdash \varphi$ is T-invalid. Hence there is a $\mathfrak{M} \not\models \varphi$. To show that also $\mathfrak{C}(\mathcal{T}) \vdash \varphi$ is T-invalid, we have to show that there is a $\mathfrak{M}'$ such that $\mathfrak{M}' \models \mathfrak{C}(\mathcal{T})$ and $\mathfrak{M}' \not\models \varphi$. If already $\mathfrak{M} \models \mathfrak{C}(\mathcal{T})$, then we take $\mathfrak{M}$ as $\mathfrak{M}'$. If however $\mathfrak{M} \not\models \mathfrak{C}(\mathcal{T})$, then we can conclude that $\mathfrak{M} \models \mathfrak{R}(\mathcal{T})$ and $\mathfrak{M} \not\models \tau$ by Definition 6. In this case (as in the previous proof) we can find a new interpretation $\mathfrak{M}'$ such that $\mathfrak{M}' \models \tau$ as follows:

- $\mathfrak{M}'$ and $\mathfrak{M}$ agree on α_O.
- $\mathfrak{M}'$ maps α_T to the witnesses of the existential quantifiers occurring in $\mathfrak{R}(\mathcal{T})$ which are given by $\mathfrak{M} \models \mathfrak{R}(\mathcal{T})$.

Since $\mathfrak{M}'$ and $\mathfrak{M}$ agree on α_O and no symbol of α_T occurs in $\mathfrak{R}(\mathcal{T})$ and φ, we still have $\mathfrak{M}' \models \mathfrak{R}(\mathcal{T})$ and $\mathfrak{M}' \not\models \varphi$. Hence $\mathfrak{M}'$ shows that $\mathfrak{C}(\mathcal{T}) \vdash \varphi$ is T-invalid. $\square$

This is why $\mathfrak{C}(\mathcal{T})$ is sometimes called a meaning postulate in the sense of [18], i.e. a statement that merely implicitly defines the theoretical terms and makes no substantial claim about reality. Also, heuristically it says something along the line: if there are certain things, then they are denoted by the symbols in α_T. Lutz called it also a background assumption about the observational part of the theory. (See [42, p. 243]) He does so, because the following theorem can be easily proved.

Theorem 1.2.3. $\mathfrak{C}(\mathcal{T}) \vdash_T \tau \leftrightarrow \mathfrak{R}(\mathcal{T})$

These three properties made Carnap claim that $\mathfrak{R}(\mathcal{T})$ expresses the observational content (or synthetical content) of a theory (see [15, p. 254]), while the Carnap-sentence $\mathfrak{C}(\mathcal{T})$ expresses its analytical content. For it follows from Theorem 1.2.3 that $\mathfrak{R}(\mathcal{T}) \wedge \mathfrak{C}(\mathcal{T})$ is logically equivalent to τ itself. Hence, these two sentences separate a theory into two epistemologically very different parts. It is Carnap's qualification of $\mathfrak{C}(\mathcal{T})$ as analytic in which I will have an interest in Chapter 2 and a reason why I introduced logical consequence so rigorously. Authors like Thomas Uebel try to keep this view on the Ramsey-Carnap-Approach as a good explication for the notions of analytic and synthetic. (See [70]) I am not so keen to do so, as I will explain in Chapter 2. In this context a theorem proven by Jane English is very interesting.

Theorem 1.2.4. *If two theories (even though inconsistent) logically imply the same observational-sentences, then their Ramsey-sentences are consistent with each other.*

Proof. See Jane English' short paper [29]. The proof is using Craig's theorem. □

The theorem shows that an inconsistency must be induced by the related Carnap-sentences. This has also interesting metaphysical consequences, if certain features of more modern theories are considered. But more on this later. However, speaking of metaphysics, structuralism offered its own interpretation of the Ramsey-sentence. They are not so much interested in the fact, that the Ramsey-sentence gets rid of the theoretical vocabulary, but rather that it eliminates their specific references. Structuralism holds that what is the truth-maker of a theory, is not the existence of the objects and their properties which the theory presupposes, but the structure of the universe that is described by the theory. For instance, that genetics or Newtonian physics was successful, and still is, even though it was falsified by molecular biology and relativity theory respectively is explained through the super-structure of reality this theories were able to get a grip on. Later theories were just able to refine these structures, i.e. they found out that genes do not exist, hence the implicit existence claim in genetics is wrong, and offered a new explanation by fanning the structure to include large molecules like the DNA. Proponents of this stance view the Ramsey-sentence as their ancestor. For them the Ramsey-sentence gets rid of an existence claim about the references of theoretical terms and supposes *something that constitutes a structure instead*. For them the Ramsey-sentence shows the actual structural content of the theory, while the Carnap-sentence just offers a heuristic. For an exploration of this view see Demopoulos' [22]. I will not take this thought further in this book.[22]

1.2.5 Observations

We have not yet talked about observational sentences. I excluded them from theories by Definition 12. The idea behind this exclusion is that the theory is linked through its correspondence rules to a not yet fully specified set of relevant observational sentences. It is beneficial to leave this set unspecified by excluding it from the theory, because there are still new observations to make. However, how this set must look like is what concerns us here. As we have already mentioned in Section 1.2.3, this set is very different from what most commentators of Carnap's work take it to be. Because as we have argued before, its formulas include more symbols than just names for sensually given objects and predicate symbols for sensually verifiable properties. For instance, number terms occur frequently in the observational language. However, while these might not cause troubles, because one might consider them as referring to printed signs on a scale or appearing symbols on a display, the Ramsay-sentence forces us to allow even more. To re-quote Carnap:

> It is important to understand [...] that Ramsey's approach cannot

[22]However, I am sceptical as Footnote 31 in Section 1.2.6 will explain.

> be said to bring theories into the observation language if "observation language" means (as is often the case) a language containing only observational terms and the terms of elementary logic and mathematics. [15, p. 252]

Why is this an issue? It is sometimes said that the observational language shall only quantify over unproblematic entities, i.e. individuals or, according to the most liberal reading, very small or uncontroversial sets. But the liberal use of existential second order quantifiers in the Ramsey-sentence violates this ideal. Hence, if one wants to consider the Ramsey-sentence as a projection of the theory into the observational language, one is forced to extend the observational language by allowing it to refer to non-elementary mathematical objects, like arbitrarily complex sets or functions on the reals. Carnap is willing to do so and, therefore, talks about an *extended observational language*. He therefore must extend the realm of observable entities by a ramified set-theoretic hierarchy that acts as a source of reference for the quantifiers. Carnap writes:

> Suppose that, in the logical part of this extended observational language, we provide for a series $D_0, D_1, D_2, ...$ of mathematical entities such that:
>
> 1. The domain D_0 contains the natural numbers $(0, 1, 2, ...)$.
> 2. For any domain D_n, the domain D_{n+1} contains all classes of elements of D_n.
>
> The extended language contains variables for all these kind of entities. [15, p. 253]

Note that $\mathcal{F}(\alpha)$ is already such an extended language by its second-order nature. We can therefore adapt some definitions Lutz gives in [43].

Definition 14. The *observations* $\mathcal{O}_\mathcal{T}$ of a RV-theory $\mathcal{T}$ are a finite set of sentences from $\mathcal{F}(\alpha_O)$.

Note that the definition does not impose any syntactical restrictions on these sentences. They can have arbitrarily long quantifier alterations of any sort.

Also note, that other authors require that observations are at most pure first-order existential claims with an observational matrix, while I do not.[23] Moreover, this opens the space to add the mathematics which is needed to perform data analysis and statistics.
Is this an issue? Does the received view jeopardize its significance for epistemological or ontological debates in the philosophy of science, when extending the observational language in such an excessive way?[24] I believe not. Imagine there is a debate about the ontological status of a newly introduced entity in the sciences. Two protagonists of this debate start to argue about the existence of the newly introduced entity. Then a carnaperian-influenced Structuralist comes along and suggests to stay neutral about the ontological status of this newly introduced entity. He suggests to the existence-supporter to stick to his theory and to the existence-denier to use its Ramsey-sentence instead.[25] He then quotes Theorem 1.2.1 to suggest that no one has to give up grounds when acknowledging the structural content, because the ontological basis of this structure is still left open to further considerations. Independent whether any of the proponents is convinced by this suggestion, we can be rather sure, that none of them will question the carnaperian structuralist on the basis that he still presupposes the existence of mathematical objects. Because mathematical objects where never questions in this debate anyway. This proceeds parallelly in an epistemological context. If there is a similar debate going on about the epistemological accessibility to the properties of a certain entity in question, where the Ramsey-Carnap-Approach might be able to clarify, it is hard to believe that the applicability of the approach is countered by pointing out that it presupposes the epistemological accessibility of mathematical entities. Because the possibility to do mathematics is usually unquestioned throughout the sciences.
However, it will be a consequence of my view as presented in Chapter 3 that

[23] The matrix of a sentence is what is left, when after brought into prenex-normal form the quantifiers are cut away.
[24] Psillos seems to argue along these lines, when he argues against Carnap's neutralism in [51].
[25] After Chapter 3 we could add here "to appreciate its Ramsey-sentence in order build a mathematical model just for calculational purposes".

the separation of the ontology and the epistemology of mathematics from those of the sciences is actually justified.
In the previous section I used T-validity to model the properties of the consequence relation of a theory $\mathcal{T}$. I did that to keep things simple. However, when it comes to the consequence relation in $\mathcal{O}_\mathcal{T}$ we must be more conscious. Some might (justifiably) want to assure that the mathematics used in $\mathcal{O}_\mathcal{T}$ is constructive. Also, it might be handy to build in a statistical theory of causation for sensually accessible entities that does not presuppose a microstructure. Moreover one surely wants to add some combinatorial principals and methods for statistical analysis to the consequence relation, when the theory in question demands a lot of quantified data in its $\mathcal{O}_\mathcal{T}$.[26] For such a task T-validity is notoriously messy and gets very fast rather complex, hence the way to go is to adopt a Gentzen-style approach, where the consequence relation can be easily extended to a non-logical relation by adding theorems of statistics as derivation rules. Doing so, I want to define the following usage of phrases.[27]

Definition 15. Assume $\mathcal{T}$ is a theory, $\mathcal{O}_\mathcal{T}$ its observations and $\mathfrak{D}_\mathcal{O}$ a Gentzen-style approach on consequence for $\mathcal{O}_\mathcal{T}$.

1. I say that $\mathcal{T}$ is *empirical adequate* in respect to $\mathcal{O}_\mathcal{T}$, if there is no $\varphi \in \mathcal{F}(\alpha_O)$ such that $\mathcal{T} \vdash_T \varphi$ and $\mathcal{O}_\mathcal{T} \vdash_{\mathfrak{D}_\mathcal{O}} \neg\varphi$.

2. I say that $\mathcal{T}$ is *empirically supported* by $\mathcal{O}_\mathcal{T}$, if there is a $\varphi \in \mathcal{F}(\alpha_O)$ such that $\mathcal{T} \vdash_T \varphi$ and $\mathcal{O}_\mathcal{T} \vdash_{\mathfrak{D}_\mathcal{O}} \varphi$.

As already mentioned, for many rather modern theories $\mathcal{O}_\mathcal{T}$ might be entirely constituted by a data-model generated from an extensive number of measurements. Also, there might be a source for confusion here. I talked about the needs for mathematics in the extended observational language in

[26] For certain academic fields, this might look very natural, because their theoretical notions are very different from the data they gather in order to support their theories. But in other subjects my presentation might cause a controversy. An example for such a subject is evolutionary biology. I am considering this case in Section 3.7.

[27] I will not heavily use them, but it is a good way to give the reader an idea how I see the relationship between a theory and its observations.

order to eliminate the theoretical terms. Then I talked about the needs of having the mathematics present in the observational part to build a data model. These are two very different needs, but they support each other and support the claim that mathematical notions should not be excluded from the (extended) observational language.

It is a pity that the proponents of constructive empiricism neglect the interesting parallels between the received view in these respects and their own theory, because it resembles their view on such matters as presented by their founder Bas Van Fraassen in [72]. By combining the received view with what was said in [72] and using a Gentzen-style approach, it might be possible to use methods from proof-theory to analyse the correspondence rules in relation to the data-model represented by $\mathcal{O}_\mathcal{T}$. This might lead to interesting conditions which certain mathematical entities must satisfy in order to empirically support $\mathcal{T}$. However, this is highly speculative. But we will clarify on correspondence rules on a more elementary level in the next section.

1.2.6 Correspondence Rules

Personally I consider Carnap's explanation of what correspondence rules are in [17, pp. 47-49] superior to what he is explaining in length in [15, pp. 232-240]. This is mainly due to the effect the more formal approach in [17] has on the messy situation with empirical laws as explained in Section 1.2.3.

> There is no independent interpretation[28] for [α_T. The theoretical sentences for an RV-Theory are] in itself uninterpreted postulate[s]. The terms of [α_T] obtain only an indirect and incomplete interpretation by the fact that some of them connected by the [correspondence rules] with observational terms, and the remaining terms of [α_T] are connected with the first ones by the [other theoretical sentences of the RV-Theory]. Thus it is clear that the [correspondence rules] are essential; without them the terms of [α_T] would not have any observational significance. [17, p. 47]

[28]"Interpretation" is taken here in the technical sense formalised in Definition 5.

From a formal perspective this is rather simple. Assume a RV-theory $\mathcal{T}$ in the language $\mathcal{F}(\alpha_O \cup \alpha_T)$. What this means is, that we restrict Definition 5 to α_O, while the terms in α_T are simply ignored, when constructing a model-theoretical interpretation. To explain further on "obtain only an indirect and incomplete interpretation" I want to explain how a possible interpretation for $\mathcal{T}$ can be chosen. First we have to choose interpretations for $\mathcal{F}(\alpha_O)$ from the class of all possible interpretations of $\mathcal{F}(\alpha_O)$, i.e. $\mathcal{F}(\alpha_O)$'s logical space. Of course we do that in a standard way, i.e. the number terms get their actual number assigned to and the names for sensually given physical objects exactly these objects.[29] We now must single out those interpretations that make all the sentences of $\mathcal{O}_\mathcal{T}$ true. Note that this is in general not a single interpretation (not even up to isomorphism). However, if it would, then the old positivist doctrine, that theoretical terms are definable by observational terms would be true for this particular theory. But, since this is not true for any interesting theory, the chosen interpretations will have many universes $\mathcal{U}$ of different cardinalities. In the next step we can choose references for the symbols in α_T. Hence, we get quite an extensive class which contains different interpretations for α_T. Which of these interpretations is a possible interpretation for $\mathcal{T}$ is then decided by the correspondence rules, i.e. only interpretations that make the correspondence rules true are allowed interpretations. This is the sense in which the correspondence rules choose an interpretation in an indirect way. Moreover, this choice is incomplete. Because this leads only to a class of possible interpretations and not to a single (up to isomorphisms) interpretation for $\mathcal{T}$.

Of course, from a technical perspective this is very close to treating the symbols in α_T as variables, when it comes to the evaluation of sentences that include theoretical terms. This makes this search for references to the model-theoretic counterpart of forming the Ramsey-sentence; the latter might be seen as syntactical in nature. The effects on the semantics of the unformalised theory, in a linguistical sense of the word "semantics", are therefore

[29] Of course, not the actual object but an abstract entity chosen in virtue of representing this object.

unclear.[30] We will come back to this in Section 1.2.7.
What is missing in this explanation, which is taken from [15], but can be found in [17], is the additional condition that correspondence rules can be added to a theory as a science develops further. This reflects the public view on science summarised in a quote often attributed to Galileo Galilei.

> Measure what is measurable and make measurable what is not so.

It is important to keep in mind that the set of correspondence rules can be extended, because it hints towards how they are related to measurement techniques. In this respect there are two possible views on measurement techniques one must choose from. First, the notion of theories is understood rather brought, hence it includes the theory of a measurement device as well. In this case the correspondence rules are used to derive the relations between a theory and the data gathered by the measurement device. In the second view there is an isolated theory and its correspondence rules are generated by an external measurement theory. Here the correspondence rules encode the means by which the theory is related to the generated data through the measurement technique. It might be tempting to choose the first approach, because it seems very honestly holistic. It stays true to the picture that all our theoretical hypotheses are put forward to the trial by reality. However, I prefer the second approach. The first reason for this is practical in nature. It is somehow dishonest to speak from a super-theory, because no one will ever present an analysis of such a theory. Also, the brought view is closer to a weltanschauliches framework rather than a hands-on approach to solve a concrete (philosophical) problem in the sciences. The second reason concerns the picture presented above by the Galilei quote. From a naive and superficial point of view, it seems like how science would actually proceed, i.e. there are distinctly developed theories and several scientists develop in addition methods to measure certain magnitudes occurring in them. The third reason is logical in nature. Sometimes the notions occurring in the target theory and the language of the measurement theory are quite different. For instance,

[30]At least when using extensional techniques, as we do here.

consider an example I want to keep as a leading example in later chapters as well: infrared spectroscopy. Quantum mechanics tells us that chemical bonds in a molecule can only reach certain and discreet energy levels which correspond to their qualitative properties. Hence, it is possible to identify a chemical by measuring what energy levels are reached. Because knowing the energy levels means knowing the quality of the bonds and these might allow a grip of the molecular structure. The measurement device is very different to this. Theoretical chemists tell us, that these energy levels are equivalent to infrared radiation. Since this is heat, the measurement theory is about how to heat up a ceramic stick that radiates the right wave lengths. Then there is some optics included in order to separate the radiation into distinct wave lengths. I claim that an analysis of the notions that occurre in theoretical chemistry should not be affected by a caloric theory about ceramic. Also, certain optical laws do not seem rather relevant, when the notions in questions are related molecular orbitals. So even if, in an epistemological sense, the truths or justification of these theories cannot be entirely separated from each other, it still makes sense to separate them, when we are concerned with logical relations between certain scientific notions. This gets further support by the condition that the set of correspondence rules can be extended as a science proceeds. For it seems highly implausible that a current relation of a theory to its data should be affected by the relation of other data to a not yet developed measurement device.

Note also that, therefore, in such an approach, the correspondence rules are, in a logical sense, stellated. Assume there is a theory which certain measurement devices associated with it. In the view I want to adopt, there is a separate set of correspondence rules for each of the very different measurement devices. For instance, in our example with the chemical bonds in a molecule, there is a set of correspondence rules translating solutions from a Schrödinger-equation to a spectrum coming from infrared spectroscopy as well as a very different set that translates the same solutions to a spectrum coming from magnet-resonance-spectroscopy. Hence very different sets of data coming with very different units.[31]

1.2.7 A Semanticist's Struggle

The received view has faced many critics since it was presented. I already talked about the debate going on in philosophy of science between the possibility to distinguish theoretical and observational terms in principle in Section 1.2.3. There is a parallel and partly related debate happening which concerns whether theories should be formalised with syntactical methods at all. For details about the syntax-semantics debate see the correspondence between Halverson and Glymour via [35], [32] and [34]. I do not want to engage (further) into these two debates here. However, there is another struggle with the Ramsey-Carnap-Approach which is equally prominent but not as widely debated in relation to the Ramsey-sentence; it concerns the adequacy of a formalisation. I must touch this debate, because formalisation and using mathematics is at least sociologically closely related. The question, whether formalising with the Ramsey-Carnap-Approach is adequate, branches into two different problems. The first is the very prominent general critique on the analytic-synthetic distinction. It goes back to Quine's *Two Dogmas of Empiricism* [56]. The critique which traces back to Quine tackles the effects of the received view. Quine argued that any distinction between analytic and synthetic truth is either impossible to make ore useless. But since the received view allows to make such a distinction or presupposes it, it must get it all wrong. A very recent protagonist of this line of argument is Otsuka in [50].[32] A more refined argument, which emerged from the debate about Quine's critique, is given by Boghossian in [8]. Boghossian tries to save the analytic-synthetic distinction by splitting analytic further apart. He argues for two versions of analyticity: *metaphysic analyticity* and *epistemic analyticity*.[33] He argues that the old (Carnap's) notion of analytic is conflating

[31] This might be bad news for those, who consider the Ramsey-sentence an ancestor of structuralism. Because, since this data is formulated with very different units, there might not be much structure which two Ramsey-sentences of the same theory at different stages of development resemble.

[32] I am examining his view in Section 3.7.

[33] I will explain these notions in Section 2.6.

these two notions, but only metaphysic analyticity is defective, while epistemic analyticity can be re-established. The defect he found in metaphysic analyticity is that he cannot make sense out of the claim that a sentence is true independently of reality. Because, as he argues convincingly, truth is when the meaning of a sentence is matching reality. This is opposed to Carnap, because as we explained in Section 1.2.2 theoretical terms lack a defined reference and, therefore, theoretical sentences cannot be true, because they lack a truth value in a referential account; at least when a semantics along the lines of Definition 6 is used. But for Carnap they are held to be true; which is the notion of truth Boghossian argues away. This undermines the status of the Carnap-sentence. However, one might be able to rescue the use of the Carnap-sentence by reducing it even further to less than a background assumption. Another possibility is to reformulate the problem of the references for theoretical terms epistemologically: theoretical terms have a defined reference in virtue of being stipulation as true, i.e. they get their reference by being stipulated as true. What distinguished them from observational terms is the knowledge about which reference they got. In the case of theoretical terms, we can only specify a variety of possible references for which the correspondence rules set the limits.

But the lack for reference leads us in the second branch of occurring problems, when it comes to an adequacy criterion for a formalisation, which was pointed out by Rozeboom in [58]. Assume Carnap's view on the Ramsey-Carnap-Approach is convincing and theoretical terms are only partly interpreted; hence, they lack a reference and, therefore, theoretical sentences lack a defined truth value. In this case a formalisation is highly dubious. There are two main branches in the metatheory of formalisation: either formalisations are models of a language, i.e. a formalisation is a good enough semantical theory about the target language, or they are translations, i.e. the meaning of the formalisation shall match the meaning of the original sentence. But when they lack meaning, then there is neither something to have a theory about nor is there something to match. In such a case the formalisation via the Ramsey-Carnap-Approach is uninformative. Because, contrary to its claim to analyse the notions of the theory or the theory itself, it only stipulates relations between and properties of them. The actual semantical form of the theoretical sentences could be radically different and, therefore, their metaphysical commitments ore presupposed structure on reality. Also note that we cannot solve this by claiming again, that what is lacking is not a semantical value but the knowledge about it. Because in such a case the actual semantical form of the (natural) language sentence might relate theoretical terms with observational terms differently, e.g. observational terms might simply occur in the actual semantical form of a theoretical sentence. This might either induce strange dependencies ore even contradict the correspondence rules.

The way out, which I want to take, is to get rid of meaning once and for all. I do this by separating meaning and logical properties as Williamson did in [74]. Doing so, the critique coming from the first branch disappears, because there is no notion of analyticity in this strong semantical sense, as it is currently used in analytic philosophy, left. What is stated in Theorem 1.2.4 is enough to account for our intuition that there is something like analyticity in the theoretical part of the language. Whatever this is. Also, Rozeboom's worries get countered, because when the relation between a theory and its formalisation is not semantical in nature, then the lack of semantical values do not have to worry us.

But what is the Ramsey-Carnap-Approach then good for? Well, solving the application problem of mathematics in an informative way for instance. Whether this is enough will this book show. How to establish such a meaning-free metatheory of formalisation is the topic of the following chapter.

Chapter 2

What does it mean to Formalise a Theory?

As we saw in Section 1.2.7, there is a pressure to clarify on the relation between a formalisation and a theory presented in (natural) language. This pressure comes from the critique on the analytic-synthetic distinction and from Carnap's claim that theoretical sentences lack semantical values. Therefore, I will tackle several questions concerning how logic, scientific theories and (natural) languages are related to each other. I am not so much interested in giving an overview on the literature, but more in presenting my own views. Also, because, there is not much around concerning these questions, that is worth considering. However, there are four main branches of this discussion which I will draw on to some extent.

The theory of medieval logic is one of the most fruitful and also most ignored source for this debate. This started back in the day, when modern logic was young and the distinction between analytic and continental philosophy settled. The scholars of medieval philosophy found themselves between the fronts; too catholic for the progressive analytical philosophers but to well educated in the theory of logic to stand with the continental philosophers. Philotheus Boehner's book from 1952 [7] is a first account on how to compare modern formalism to working formal with and in (natural) languages. For many currently working philosophers this might sound like an oxymoron;

there is a certain tendency to call (natural) languages informal because they are not a formal system in the sense of modern proof-theory. Coming from the study of medieval logic, young philosophers like Catarina Dutilh Novaes and Sarah Uckelman are constantly trying to correct this view. I will heavily draw from Dutilh Novaes' discussion as presented in [25] and [27].

The paradox of analysis is a paradox from metaethics which was first introduced by C. H. Langford in [39] while discussing G. E. Moore's [48]. Later it infamously entered theoretical philosophy, where it affected the very influential later idea of explication in Carnap's philosophy.[1] The paradox is currently heavily studied by Michael Beaney. (See [5] and [6]) Also Dutilh Novaes together with E. Reck draw on this tradition to some extent. (See [24])

The syntax vs. semantics debate in the philosophy of sciences is a rather rich source for this topic. This is because the protagonists of the semantic view try to get rid of logical formalism. The replays from Hans Halvorson, who is a protagonist of the syntactic view, touch the topic of formalisation frequently. (See [35])

The philosophy of mathematics is not a very rich source for such questions as one might belief at first. However, there is an important text concerning axiomatisation from Dirk Schlimm [62]. Moreover, I draw heavily on Feferman and Kleene by my uprising as a proof-theorist. But most of these influences are very implicit and casually conveyed to me in personal conversations by their academic offspring. This is rather a mood, when looking at such things than a citable position. Because proof-theorist find themselves often under siege and must justify their methods to other mathematicians. For most of them prefer model-theoretic approaches through the close resemblances model-theoretic interpretations show to algebraic structures. Proof-theorist are therefore often in need to give accounts of formalisation and clarify on the relations of formal systems to mathematics as practised by working mathematicians. Hence, they question the nature of formal systems frequently; but not necessarily in their publications.

As the last branch already emphasised, I am not interested in a historical

[1] Carnap and Langford worked together on [59].

study of the developments of accounts on formalisation from whichever point in time on. I neither care much about how formal-systems in the proof-theoretical sense are older than the later occurring semantic view triggered by Tarski's work. Rather than giving a historical account, my aim is to give a (maybe) novel account on formalisation which is conceptual and systematic in nature to overcome problems triggered by philosophy of language, e.g. the analytic-synthetic distinction and the problems mentioned by Rozeboom. However, some might still wonder why I am tackling the problem of theory formalisation from the rather far away point of the philosophy of logic. For I could have started with the philosophy of media or an account on cognitive tools. There are two reasons for this. First, it is not possible to talk about the formalisation of a theory without even touching the formalisation of the logic which is attached to it. And second, to block problems imported form the philosophy of language into the philosophy of science, I have to offer a novel view on formalisation of a theory which is not semantical in nature but still gets some grip on the target theory. The approach I am offering views formalisation of a theory as the result of a logical study of its properties in the same way as a logical formalism is a logical study of a consequence relation. Of course, I presuppose that logical properties are not, or at least not primarily, linguistic or semantic phenomena.[2] I will argue for this by showing that a formalisation is neither a semantical theory of a piece of (natural) language, i.e. modelling this piece of language, nor a translation from a (natural) language into a formal language. After these prominent views are out of the way, the actual work will be done by a Kreisel's squeezing argument.

2.1 What does "Formal" and "Formalized" mean?

There is an important difference between the adjective "formal" and the participle "formalized". The former expresses the property of something the other expresses the result of a process. I consider it unproblematic what it

[2]That these two things have to be distinguished is argued for in Section 2.2 in the part about the model view of formal systems.

means to formalise a formalisandum, because I do not think that many problems can occur in the process of formalisation. As far as I can see, it always means the same thing: somebody comes up with a formula of a previously given set of formulas that is supposed to stand for the formalisandum. The phrase "standing for" does not involve more than that a person decides to use this formula as standing for the formalisandum. The following example is supposed give an intuitive picture of that there is no notion of translation involved in the notion of formalisation: Assume the situation of a student in high school who is supposed to solve the following math problem.

> Show that the difference in height between the Mariana Trench (11,034m) and the Mount Everest is (8,848m) is 19,882m.

The solution to this problem is given by student via formalisation. She gives the formula:

$$(*) \qquad 8,848 + 11,034 = 19,882$$

The difficulty for the student here is, to see that even though it is asked for a difference, the numbers must be eventually added up. Note two aspects here. First, the formula is true, while the original problem is false, e.g. the Mariana Trench has a depth of not more than 10,984 meters. Hence a translational account that analyses meaning-similarity as a necessary condition for adequacy is failing, because the connection between $(*)$ and the problem is not truth preserving. Second, there is also no other meaning-condition involved, because $(*)$ might as well count as a formalisation for another problem involving the same numbers, i.e. there is no notion of sense or intension involved here. One might insist that these numbers in the calculation represent lengths, because the unit "meter" is present. This can be granted. However, in $(*)$ the umber terms are referring to unspecified lengths. The following information is given in the problem:

The hight of the Mariana Trench is 11,034m.

$$h(mt) = 11,034m$$

But only the number term (with the unit) occurs in $(*)$ and not the term with the function-application.[3] Hence, there is no connection to height or the Mariana Trench in $(*)$ left. Hence, taking the leading ideas from this very naive example, viewing a formalisation as a translation seems unnatural.

To block some confusions right at the start, I want to clarify on several things. I do not deny that there is an aspect of language in a formula. There must be a certain grammatical structure in any symbolic representation in order to be understandable. But this is not sufficient to support a relation on the level of semantics. Second, I do not deny that there are narrower notions of formalisation that are fruitful in philosophy or linguistics. If one uses a formalisation in order to grasp some aspects of semantics better, then of course it makes sense to ask which formalisation is more adequate in analogy of asking for an adequate translation. But there are other cases of a good formalisation, where such aspects do not play a role.

However, there is another problem with my example; it is not only a formalisation, it is also a mathematisation. I will talk about the differences between mathematisation and formalisation later in Chapter 3. But there is room for a position which holds, that the reason why mathematisations are not translations is because they essentially involve a change of reference, i.e. the symbols occurring in the original sentence lose their reference and, instead, numbers are assigned to them. Hence, such a position can still hold, that the process of formalisation which precedes such a mathematisation must be seen as (or in analogy of) a translation. Hence, my naive guiding example is indeed not sufficient to clarify on formalisation. However, that I take such a view as unconvincing, will become clear when we start the already promised discussion about why formalisations cannot be models ore translations in Section 2.2.

[3] I granted before, that the unit might stay with the number term to make it a length. I granted this, because it does not affect the conclusion. But I am opposed to this view. The unit does not go with the number term. It acts more like a modal operator on the equation; it affects both: the function h and the number term. In the sense that it tells us in which unit-world the sentence must be interpreted. Hence, it looks more like this:

$$\text{Meter}[h(mt) = 11,034]$$

That the logic of the unit-operators does not have to be spelled out, will be identified as a benefit of applying mathematics in the sciences.

As a last point, I want to clarify on a common misunderstanding. Some might want to view formalisation from a semiotic point of view, as the distinction between symbolic languages and word languages. This is not going anywhere as further developments originating in the philosophy of media showed.[4] There is no distinction between symbolic languages and word languages. The distinction present is between languages, which are spoken, and symbolic systems, which are used for notational purposes. Some of these symbolic systems were devolved to act as a notation system for a spoken language, while other symbolic systems do not target a spoken counterpart. This is extensively discussed in [25, Ch.2]. One might try to rescue the symbolic-word-distinction by arguing for its vagueness. But, given this vagueness, I cannot see how this distinction should be drawn in order to make it informative, when mere representations of sentences are considered. It is for sure informative when there is an interest in the cognitive capacities of a reader in relation to such a representation, e.g. less symbols are faster to read while words are more familiar. But I cannot see a fruitful application of this distinction in the analysis of the logical properties of a scientific theory. Because, in this respect, I see no effect a more complex notation system has on the relevant logical relations in virtue of being of a higher complexity.[5]
Now that we discussed the participle "formalisation" let me clarify on the adjective "formal". In [27] Dutilh Novaes distinguishes two groups of meanings of "formal": the formal as pertaining to forms and the formal as pertaining to rules.[6]

[4] See [25] which draws from thoughts expressed by Krämer in [37].

[5] Complexity here means either that the used symbols are rather long sequences of signs or show a structure by themselves, i.e. they have a certain grammar like "a piece of furniture" in the following example concerning schemas.

[6] Some might have noticed that I have not mentioned John MacFarlane's incredibly influential PhD Thesis from 2000. (See [46]) I must say that I will continue to do so. I do not consider his work as bad philosophy. On the contrary, there are many interesting taxonomies and clever readings included. But it is evaluated on and follows a path which is implicitly defined by his intuition about the formality of logic, which is always in the background without being properly motivated. Hence, when reading his thesis I found myself in the position to either sign up to this or just accept that he speaks about something else than I want to talk about. For instance, as someone who studied logic as an academical discipline, I cannot see why logic should or shall be related to the epistemology of individual humans. Also I wonder why a clear demarcation criterion for logic should be available

The formal as pertaining to forms splits into five usages of "formal" in relation to logic: schematic, independence from particulars, topic neutrality, content-free and de-semantification.

We already came across the interpretation of formal as *schematic* in Chapter 1; it was how I used "formal" there. As a reminder, the forms, to which this notion of formality is referring to, are created by a previously given distinction of the used symbols. Some symbols in a sentence are considered to be logical and, therefore, constitute the form of a sentence. The others are supposed to give the matter of the sentence. In this version of formal two sentences have the same form, if one can be obtained from the other by just substituting the logically irrelevant symbols. A schema in this diction is a proto-sentence. It is obtained by substituting variables for all the logically irrelevant symbols that occur in a sentence. For instance, the sentences

Any vixen is a fox.

and

Any table is a piece of furniture.

can be seen as exhibiting the schema

Any X is Y.

Here we presuppose that "any" and "is" are considered to be logically relevant.

I want to come back to the distinction of a notation system and a spoken language. The sequence of signs "a piece of furniture" is a symbol for a not further specified piece of furniture.[7] Since it contains proper subsymbols, it is a complex symbol and it shows a grammatical structure. But it is nevertheless a single symbol. Depending on the logical theory, it is either a symbol

or should be a desideratum at all. At this point I hold it more like Gillian Russell, when she commented on MacFarlane's position by comparing it to the demarcation from mathematics from physics: where shall we put applied mathematics and what do we expect from such a distinction in the first place? (See [60]) Hence I refused to develop his line of thought here just to block it later on by calling it misplaced. Therefore I rather continued with Dutilh Novaes more descriptive account on the topic.

[7] I do not want to heavily burden the symbol sign distinction. This should be rather intuitively clear loose talk, when it comes to semiotics.

for a piece of furniture or the set containing pieces of furniture. More information about the internal grammar of the symbol, i.e. that it is composed of two nouns and an English genitive-construction using "of" is of no interest to many logical theories.

The second meaning of formal is *independence from particulars*. The best examples for this notion are given through definitions like Definition 5 and Definition 7. Because what is essentially given by Definition 5 is a way to change the individual objects a sentence is talking about. By varying these, hence being independent of them, an account of a valid argument is given in Definition 7.

Somehow related to independence from particulars is the much more general use of formal as *topic neutrality*. Here "formal" means that an argument is valid in any topic there is. This is sometimes companioned with the view that logic is the study of the laws of truth preservation in analogy of natural laws formulated by the sciences. When considering logical truth, i.e. the consequence of a valid sequent with an empty premise-set, rather than the consequence relation, one can formulate this position as finding sentences that are true in any topic or in non. These are called tautologies.

Diametrical to this more realistic use is the idealistic use of "formal" as *content-free*. Here "consequence" is not seen as standing for a relation between sentences, propositions or statements. In this view logic rather tries to describe valid transformations exhibited by a (human) mind.[8]

The youngest meaning of "formal" is *de-semantification*. It goes back to the Hilbert school of proof-theory. What is novel about de-semantification is its unusual treatment of symbols. Symbols are means of communication which stand for something else. They exhibit a natural or conventional relation to something other than themselves. Traditionally symbols were studied by giving different theoretical treatments for this relation. Hilbert gave up on this,

[8] As my ignorance towards MacFarlane and my outing as a debunker shows, I am not very keen to use "formal" in this sense and will not do so in the following. This is not just a matter of taste. Many philosophers seem to belief that there is explanatory power in contexts of philosophy of logic, when reducing logic to the philosophy of mind or the psychology of reasoning. I cannot see where this explanatory power is coming from or that it is even in action.

symbols are stripped from their semantical properties, i.e. they are studied as individual objects.

Formal as pertaining to rules splits further into three different usages of the word “formal”: being algorithmic, regulative rules and constitutive rules. *Regulative rules* can be seen in analogy to the rules of a game. Behind this is the view that logic stands in an intimate relation to an external aim which gives normative means. Like the rules of a game, which force a player to act in a certain way in order to win, a logic forces a user to follow its rules in order to reach truth, reason correctly, justify a claim or, more importantly, to stay in its domain.

Formality as having *constitutive rules*in place credits that, in order to be precise, one has to clarify what one is talking about. Or even more important, one has to specify what is not considered. In the sense of an infinite domain, like the domain of sentences, one can do this by giving explicit rules which generate the considered domain. This is what we did in Definition 3.

To be *algorithmic* is a surprisingly old meaning of “formal” and dates back at least to medieval logic.[9] It credits, that the application of the constitutive and regulative rules should be simple and effortless even to the extent that a machine can do it. This is one of the main aspects why people ascribe to logical truths a relatively innocent epistemological status and call them “analytic”.

2.2 What is a Formalisation of a Scientific Theory?

In the previous section I tried to emphasise that to formalise is nothing else than putting a formula forward. But of course, a formula never stands alone. It is generated by a set of constitutive rules like those given in Definition 3. Also, one is usually choosing a formalisation with some operative rules in mind, which shall act upon it. Hence, a formalisation of a sentence always

[9] See [25] for a historical description of algorithms in logic and see [26] for the proof that Ockham’s 14th century theory of supposition gives a fully functional algorithms in the modern sense of the word.

comes with a rule based symbolic system. Therefore, a formalisation does always put forward a formal system as well.

Definition 16. A *formal system* is a triple $\langle \mathcal{F}, \mathcal{A}, \vdash \rangle$, where

1. $\mathcal{F}$ is a primitive recursive set, whose elements are called formulas,
2. $\mathcal{A} \subset \mathcal{F}$ which is primitive recursive as well, whose elements are called axioms, and
3. $\vdash$ is a set of any complexity, that contains sequents. The premises of these sequents are taken from $\mathcal{A}$.

With this definition in place I can give my approach of formalisation.

> The formalisation of a (natural) language sentence (or sentences) is putting forward a $\varphi \in \mathcal{F}$ from a $\langle \mathcal{F}, \mathcal{A}, \vdash \rangle$ as the formalisation of this sentence (or these sentences).

This covers the techniques which were presented in Section 1.2. Because I defined a theory as a finite subset of formulas of $\mathcal{F}(\alpha)$. Hence we are able to say that formalising a scientific theory, as it is out there in textbooks, is to put forward a RV-theory $\mathcal{T}$ in the technical sense of Section 1.2. We can take $\vdash$ as either G-validity or T-validity. Because, note that $\langle \mathcal{F}(\alpha), \mathcal{T}, \vdash \rangle$ is for both notions of validity a formal system.
I said before that I do not want to consider formalisation as a kind of translation. One might counter my opposition by defining the term "translation" in the same spirit as did formalisation as putting forward a sentence of another language as a translation for a sentence of the original language. One can define what a good translation is later then. Hence, one might say that, since I must continue by distinguishing good from bad formalisations, that I only verbally depart from a translational account. I disagree. However, it is hard to argue for this without defining the complex notion of translation and then argue that certain features are not satisfied. I am not able to do this through my lack of expertise in linguistics. Hence, what I am trying to do is something different. I will give, what I belief, are the two main views

about how formal systems stand to a target theory. Both are linguistic (or semantic) in nature. Then I will try to argue them away. After that I will formulate my own position, which will not establish the relation between a theory and its formalisation by linguistic (or semantic) means.

In [25, pp. 97-100] two views concerning the relation between formal systems and (natural) languages are given. I agree with Duthil Novaes that these are the main tracks, when the nature of formal systems is questioned. In the following I call this two views the *modelling view* and the *language in its own right view* on formal systems. Both views are intrinsically semantic or linguistic in nature, hence sometimes I call both translational.

Formal Systems as Models of (natural) Languages

The view that formal systems are models of (natural) languages is rather popular, for instance it is stated by Stewart Shapiro.[10]

> A formal language is a mathematical model of natural language (of mathematics), in roughly the same sense as, say, a Turing-machine is a model of calculation, a collection of point masses is a model of a system of physical objects, and the Bohr construction is a model of an atom. In order words, a formal language displays certain features of natural language, or idealization thereof, while ignoring or simplifying other features. [63, p. 137]

R. Cook tides formalisation even more to linguistic phenomena, when he sees similarities to the empirical sciences.

> On the Shapiro/Cook view, a logic is not meant to be a perfect representation of the linguistic phenomenon being studied. Instead, a logic is a model of that linguistic phenomenon, and as a

[10] To be absolutely clear about the strength of such a position, most authors do not feel very strong about these view. In a personal conversation about this topic Steward Shapiro waved his hand and told me to just use whatever notion of “model” I want. And I am certain that none of his claims rest on the particularities of this view. But it is a prominent way of talking about formal systems, so I will treat it like a full fetched position for the sake of argument.

> result, all of the advantages and limitations that are present in modeling elsewhere (such as in the empirical sciences) should be expected to reappear in the study of logic. [21, p. 500]

To clarify on this view it helps to recall (V) from Chapter 1. I explained there, that giving a logical theory is to fix the range of the variables and to explicate what counts as a case. The model view holds that the variables in (V) range over (the grammatical structure of) sentences from (natural) language. It also holds that the cases in (V) are given through the semantical relations a sentence from (natural) language has to a phenomenon which it is about. Hence, whatever is the right (or good enough) linguistic meaning theory for (natural) language will explicate "case" in (V) and clarifies on "true in a case". This can then be used to define *the* consequence relation of (natural) language. However, such a meaning theory has certain negative features: it would give a very complex semantics and it is not yet found. Since it seems reasonable to assume that one does not need such a meaning theory in full detail anyway, we can use more handy models of (natural) language. Formal systems and their relatively simple semantics are such models, which we have constructed for this purpose. This is seen in analogy to a scientist who uses Newtonian physics even though she knows that modern quantum physics or relativity theory gives a better account on spatiotemporal objects.

Whether this view is convincing depends on how far one is willing to follow the progression of linguistics in matters of semantics. Because linguistic research is still going on and, therefore, linguists present better accounts on meaning and therefore better semantics. For instance, formal semantics stands in this tradition and is developing ways of more or less successfully dealing with the features which are blamed for the complexity of (natural) language, e.g. high variety of sentence structure, names without a reference, vague notions and many more. Following the model view on formal systems, the protagonists of formal semantics can rightfully claim, that one should use their theories in order to understand the consequence relation or the logical properties of certain sentences better. Of course, this can reach the extent that formalisations can be, and indeed are, evaluated based on empirical findings in linguistics, e.g. the usage of certain words or concepts by competent

language users.
However, the claim that a logic is something that jumps out of a (natural) language's semantics is not as unproblematic as this tradition pretends. What if logicians, who are competent users of a language, are not willing to follow linguists onto their grounds? This question was stated by T. Williamson in [74]. And this is indeed frequently happening, e.g. most prominently when the ontological status of an entity to which a sentence is referring is questioned. This echoes Rozeboom's critique as described in Section 1.2.7. We will come back to Rozeboom very shortly. Williamson elaborates extensively on the flaws of many competing theories and stances in this context. But since his critique on the model view is only a by-product of his critique on Boghossian's notion of epistemic analyticity, most of his argument should not concern us here. His core stance in opposition to the model view, however, is very easily stated: It is not possible to use linguistic arguments in logical disagreement, because semantical properties where never in questions, when competent language users offer different accounts on logical properties of sentences they already understood. (See [74, p. 91]) For they all agree on the semantical properties anyway in virtue of being competent users of the language.[11] Hence, logical properties must be distinguished from semantical properties.
We are on heavily contested grounds here. And I will not enter this debate. Here I take Williamson's view as the more convincing.[12] But in the context of scientific theories, something more must be said, when it comes to the challenge offered by Rozeboom. Assume there is a theory taken from the natural sciences formulated in English. Now assume a formal semantician applies an approach of formal semantics, defines a notion of truth for natural language sentences and therefore creates via (V) a notion of logical consequence.[13] Then assume a formal philosopher is taking up the task to formalise the very

[11]Without the linguists being extremely patronising towards other fields. But then they argue circular anyway.
[12]For more on analyticity see [9] and for an argument concerning the metaphysical neutrality of logic, when extracted from language use, see [23].
[13]It is important to note that (V) is not questioned here, what is questioned is whether "case", as used in (V), should be explicated by a theory of meaning for a (natural) language.

same theory by using the Ramsey-Carnap-Approach with T-validity. This raises the question: Is there a sequent that turns out valid with the formal semantics' logic for (natural) language but is proven invalid when identified with a sequent in the formal-system offered by the philosopher? I see the following relevant possibilities:

1. Such a sequent is impossible.
   (a) Because (natural) languages are very mysterious. Therefore, in the case of ontologically questionable theories, linguists must take guidance from formal logic and philosophy of science anyway in order to offer an account of (natural) language semantics. Hence, their account cannot differ much from the logician's formalisation.
   (b) Even though semantical and logical properties have to be distinguished, semantical properties should not overrule logical properties. Hence, those linguistic theories that lead into another logic are wrong in virtue of being inadequate to the logic explicated by the logician.
2. Such a sequent is possible.
   (a) It is possible, because of Rozeboom's worries. But since these worries are semantical in nature, the linguistic theory is falsifying the logician's formalisation.
   (b) The sequent shows the limits of formal semanticians on logical theorising. Since scientific theories are highly technical, linguists must realise that their methods, which are developed on the vernacular are not suited for higher academic discourse.
   (c) Such a sequent is possible, but the debate about it does not resolve or most protagonists do not care whether it does.

As will become clear in Section 2.3, I am considering (2b) to be the most adequate case. However (2c) is probably the most possible outcome. (1b) and (2a) must be separated, because it highly depends on the way how certain linguists build their theories at the first place. The same is true for

(1a) and (2b). Also note that (1a) and (1b) would make several debates in philosophy of logic and metaphysics superfluous or presuppose their resolution. Therefore, I consider them as uninteresting. I take it as uncontroversial that protagonists of the model view consider like (2a) as the most adequate cases. Williamson's [74] does not help here, because he does not consider cases which are underdetermined in their ontological or semantical aspects. However, there is another line of argument coming from the philosophy of sciences attacking the model view in virtue of the analogy to physics and engineering as explained above. We do not have to clarify on how different formal systems might or might not be evaluated by new or better meaning theories for (natural) languages, because whatever formal systems model, if they model at all, this something cannot be a linguistic phenomenon of a (natural) language. Because whatever account of *modelling* one favours, in order for a model to be a model of something else, the supposed model must show a certain resemblance to the target phenomena; a model may be simplifying, disturbing or even be partly plainly wrong about the target phenomena, but in some aspects it must simulate what it models. However, as Duthil Novaes points out in [25, p. 98], it is not clear which linguistic phenomena formal languages are modelling, when they are put forward as a model for (natural) languages. It is common for the model view to claim that the departure of the model from the represented phenomenon is through idealisations and simplification. But this is most of the tame false conciseness. Because formal systems also, and most prominently, depart also in respect to those words they are supposed to model, e.g. words as "if", "and" and "or". In the case of "if" there might be a debate whether it is understood causal, conditionally or modal. The word "and" is order sensitive in (natural) languages, but commutative in formal systems. Also, the word "or" is exclusive in (natural) languages, but inclusive in formal systems. Granted, those features can be taken as a matter of theoretical elegance. But, if the meaning of the logical connectives is not the resemblance which makes formal systems to a model of natural languages, then what is?

Some might still disagree by pointing out, that logical properties play a role when semantical considerations are made. And this might be true. However, this does not make a logical theory to (a part of) a semantical theory. It just establishes a non-trivial relation between two properties, which might be still very different. However, I see the strong tendencies to build a semantical theory out of a logical theory or the other way around. For many analytic philosophers these two things are essentially combined. But there is one further distinction to be made. We have to ask: what counts as a counterexample for the presenter of the theory, when her theory is presented? Because these possible counterexamples show what the target phenomenon of the presented theory actually is. It might be hard to swallow for some, but if a theory is not proposed as a theory covering (natural) language, then counterexamples taken from competent language user do not affect the theory. But as a matter of fact, many logicians do not feel forced to alter their logical theories because of counterexamples which are given by competent language users. This might be the case, because these logicians had exclusive theoretical insights about logical relations through their research. However, their ignorance shows that these theories were proposed for a variety of problems which are disconnected to any worries a mere competent language user might have.

Formal Systems as Languages in Their Own Rights

The second commonly held view is that formal systems are just another kind of language which one can use in the same way as any other type of communicational device. The idea tracks back to Frege's Begriffsschrift and his famous example about the advantages and disadvantages of the microscope in comparison to the naked eye [31]. As Frege explains, natural language is like the eye; it performs in a wide range of different applications in all sorts of human life situations. On the other hand, a formal system is like the microscope in that, it is particularly designed to perform for specific scientific purposes, to match scientific needs and the phenomenon at hand. However, the comparison does not go through that smoothly, because one cannot speak in a formal system. Therefore, in order to make this comparison work, many contemporary philosophers take to calculate in a calculus, e.g. a version of

G-Validity, in analogy to speaking in a natural language, i.e. calculating in accordance to the rules of a formal system is like speaking grammatically in a natural language.

I am not a specialist in the Frege exegesis and therefore do not presume to judge whether Frege would have gone so far in his comparison. He may simply have meant that the expressible power of formal languages is higher than that of natural languages, i.e. an ambiguous sentence from natural language has several unambiguous counterparts in Frege's formal system. But I want to point out two problems with the picture just described. First, the analogy between calculating in a formal system and speaking in a natural language does not match how we use formal languages anywhere in the sciences or in philosophy. The possibility to program a computer to make calculations for us and the enormous amount of time one needs to make calculations by hand made philosophers believe that our reason for using the computer is a lack of resources. That view is plausible, but I want to offer a different interpretation. I think we neither make such calculations by hand nor by a computer, because we do not actually care about them.[14] This is a substantial difference to speaking. What we care about is whether a certain argument is valid according to a logic. Hence, we only care which sequents are derivable.

Second, many philosophers view the rules of a calculus for a formal language as implicit definitions, i.e. they implicitly give a semantics for the logical connectives, as Gillian Russell does. (See [61, p. 165]) Her argument is that logical rules show the semantics of logical connectives. She is seeing this in analogy to using a name in a conversation: the usage of the name "Sally" in a conversation about a fish eventually clarifies by itself that "Sally" refers to this fish. But it is false that an inference rule is the same as talking about one's fish by using the name "Sally", so that another person might guess that "the name of Gillian's fish is Sally" is a true sentence. The rules of a calculus do not express themselves, rather they are stipulated to allow certain moves in the calculus in order to show that a certain set of arguments is valid. Hence, if they are connected to a semantics at all, they belong to meta-semantics,

[14] Here the semantical value of "we" obviously does not include proof-miners from proof-theory like Ulrich Kohlenbach.

i.e. stipulating them might create some properties of a term of the language, but they do not show these properties.[15] Therefore, the calculus becomes the device to check whether an argument in question is valid or not, in a similar way as a truth table does. This is not countered by the truism that we can reason in a calculus. Reformulating of a problem by a shift to a higher level or to its semantics is always possible, when one wants to solve the problem. For instance, whether abortion is murder can be clarified by reformulating the question as: "what does 'murder' mean and does abortion satisfy the requirements of this notion?". Or stating it more logically: it is not much of a surprise that $T(\varphi)$, saying "The sentence φ is true", and $\mathrm{Proof}(\varphi)$, saying that "φ can be derived by the rules of a calculus", are logically stronger than φ.

Consequences when abandoning both views

What both views have in common is a strong relation to the semantics of the (natural) language which is used to formulate the unformalised target theory. Because for the model view there is a proper logic induced by the semantics of the language to which the formalisation can be hold account to. While for the formal system as a language view the straightest forward way to picture formalisation is to talk about a translation from one language into another. Since I tried to emphasise that both views are not worth taking a stance for, the idea of formalisation as holding account to the semantics of a (natural) language must be given up. Because neither can a formalisation be judged by semantic similarity, like in a translation, nor can it be judged by the means of an externally given semantics, i.e. in the sense that it is a model of it. However, before I can introduce my adequacy criterion for a formalisation, I must clarify on explication.

[15] Of course, this view is only convincing, when the model view on formal logic is already out of the way.

2.3 Explication

To assure that my readers are not completely confused, I must introduce the notion of explication in detail at some point in the book and this seems to be the best place. However, since it was already mentioned in Chapter 1 and will become important later on in the book again, I want to introduce it as broad as possible. I will not apply it to formalisation before the following subsections. So, the reader should not try to find links to what was said earlier in this chapter straight away, because there are none. I will come back to formalisation after this detour to explication took place.
Carnap's notion of *explication*, as it is introduced in [16], can be seen as an attempt to solve the paradox of analysis.[16] Here the paradigmatic case for "analysis" is conceptual analysis, i.e. a concept or notion is analysed by giving a definition that relates the notion in question (called the analysandum) to other (more basic) notions. These basic notions and their relations to each other are then called the analysans, i.e. this what was put forward as an analysis for the analysandum. The classical case is Aristotle's analysis of knowledge: knowledge is justified true belief. Hence one might identify a general form of an analysis for an analysandum A by more basic concepts $B_1, ..., B_n$ as:

$$A \text{ is } ...B_1...B_n...$$

The paradox can then be stated as follows. Either the analysandum and the analysans mean the same, then the analysis is uninformative. Or the analysandum and the analysans do not mean the same, then the analysis is incorrect. The most obvious way out is to separate the notion of meaning into (at least) two types of meaning. A first attempt was given by Frege after he distinguished *Sinn* and *Bedeutung*:[17] one might say that an analysis is correct, when analysandum and analysans have the same reference or

[16]We already mentioned some historical details about the upcoming of explication as a solution for the paradox at the beginning of Chapter 2.
[17]See [5] for details and quotes.

extension, and informative, when they have a different sense, meaning or intension. The most straightforward counter to such a solution of the paradox is Quine's famous counterexample "creature with a heart" and "creature with a kidney". Both notions have different meaning and the same reference, but still none of them is an analysans for the respective other. Michael Beaney tries to resolve the paradox by retreating from the philosophy of language to the philosophy of mind in [5]. He is following Husserl by introducing a time dimension to analysis and drawing from explication. For Beaney analysing analysis includes comparing the pre analysed stage with the post analysed stage. He points therefore in the direction of what Herman Cappelen is describing in [13]. Cappelen is talking about conceptual engineering, i.e. a frequently occurring phenomenon when language users are conscious about the notions they are using. In his view philosophical methods like Carnap's explication and conceptual analysis are just special cases of a more general tendency to improve notions by replacing them by those better suited. He also gives several arguments how conceptual replacement can be justified in [13]. Roughly stated, he wonders why using such methods is not question bagging, i.e. one might think that changing the notions also changes the topic, especially in philosophy where the notions itself are under consideration. In a nutshell his solution is: the topic of an academical debate, inquiry or discourse is much more robust than the references of their notions, hence the topic can stay the same even though the references (or extensions) of the words in use change.
Carnap was not as critical about these things as Cappelen is. He just offered his notion of explication in [16] as an improvement of the notion of analysis and stated that this is what happens in the sciences.[18] The guiding examples for Carnap in [18] are the biological notion of *fish*, i.e. *pisces*, and the everyday notion of *fish*. Here one sees the difference to analysis, because the two notions obviously do not have the same extension. For in a fish-restaurant one would expect to find scallops on the menu even though scallops do not satisfy the definition of *pisces*. But *pisces* and *fish* are similar enough to take

[18]Of course Peter Strawson was criticising Carnap for this and Carnap responded, but this is exactly where Cappelen's book is starting from. So I refer again to [13].

pisces as a more precise notion of *fish*, which can be fruitfully used in biology. Hence instead of giving us an analysandum for an analysans, Carnap tells a story how pre-scientific notion are replaced by a scientific one that satisfies certain features in the sciences. In this sense Carnap's explication takes place in time and does replace a previous notion. But explication differs from Beaney's preferred Husserlian story and Cappelen's conceptual engineering in the why how he links an explicatum to the explicandum.[19] For Carnap the link is not a history of a replacement process, neither as a historical fact as for Cappelen nor as a personal development in a human mind as for Beaney. For Carnap the link is contextual, i.e. one is used in scientific contexts, while the other in everyday life. Hence the original notion might be parallelly kept in use for unscientific purposes.[20] Carnap gives features that link the scientific to the pre-scientific notion and are the means by which the latter is replaced for scientific purposes. These features are exactness, fruitfulness, simplicity and similarity.
Concerning **exactness** Carnap writes about a proposed explicans:

> The characterization of the explicatum, that is, the rules of its use (for instance, in the form of a definition), is to be given in an exact form, so as to introduce the explicatum into a well-connected system of scientific concepts. [16, p. 7]

It is important to note, that exactness is a primitive notion, which is taken from the sciences and not proposed in order to explain the explicatum's exactness. Also, one should not over-interpret Carnap's mentioning of rules. Introducing a notion through a formal system is for sure a possible way to go. But as Carnap's guiding example "pisces" shows, formalisation or mathematisation are not required in order to fulfil scientific exactness requirements.

[19] I want to emphasis here, that I follow Cappelen's arguments how to counter accusations of question bagging, but I do not follow his program of conceptual engineering. I am not that much of a proponent of the Enlightenment as he seems to be. I follow Carnap here: science and looser talk both have their respective places. As an effect I do not buy Cappelen's claim that coming up with a scientific notion is the same as a severe cultural change, like the extension of "marriage" to homosexual couples.

[20] This becomes clear, when looking at his examples "fish" and "warm", which have not been totally replaced by "pisces" and "temperature". Even though the fruitfulness of the latter is unquestioned.

Also, it is important to note that exactness is in tension with the feature of fruitfulness. For instance, it is arguable misguided to formalise anatomy. Because, formalising anatomy is likely to complicate matters to an extend that does not justify the gained exactness of biological notions. In this respect it is also in tension to **simplicity**; a feature I will not consider further. It seems clear enough that a notion should not be unnecessary hard to handle.
The difference of explication to analysis is explicit in Carnap's writing concerning **similarity**:

> The explicatum is to be similar to the explicandum in such a way that in most cases in which the explicandum has so far been used, the explicatum can be used; however, close similarity is not required, and considerable differences are permitted. [16, p. 7]

Here one can see that Carnap is willing to give up quite a bit in order to fulfil the other requirements.[21] All he requires is that the extensions of the explicatum and the explicandum share most of their elements. One might add that they should also share the most obvious of their references. Carnap also adds that one should first know the explicandum very well before trying to propose an explicatum. Hence something like an analysis might help in order to give a good explicatum. However, there are no strong adequacy requirements added, hence the paradox of analysis does not reappear at this stage.
The last requirement is **fruitfulness** and it seems to be the strongest, i.e. other requirements can be weakened in order to fulfil it. Carnap writes:

> The explicatum is to be a fruitful concept [...] useful for the formulation of many universal statements (empirical laws in the case of a nonlogical concept, logical theorems in the case of a logical concept).[16, p. 7]

I am not very interested here in clarifying what can count as a fruitful explicatum. I guess it should be clear enough. More details to all these features

[21] Of course, here start the concerns about question bagging.

are given in [24]. However, as I already mentioned in my discussion of exactness, I do not see a special focus on formal systems, when it comes to explication. As I see it, an explicatum is not a formalised notion but a technical notion. It can be introduced by setting up a formal system, but can as well be introduced by presenting the rule of its use in a (natural) language. The reader should keep that in mind, when reading the following tabular. Both of its columns follow the same iterative explication process, they do not affect each other according to explication.
Beside "fish" Carnap gives another guiding example in [16]: the explication of warm by temperature. Following Carnap's explanation in [16] the case proceeds as follows. The pre-scientific notion of warm becomes refined through its use in the sentences for the purpose of scientific investigations in four steps. I added formulas to show that it can parallelly be formalised in a certain way. I will discuss the relations between the sentences and the formulas later in Section 3.3.

Step 1.	This rock is warm.	$W(r)$
Step 2.	This rock is warmer than that rock.	$W'(r_1, r_2)$
Step 3.	This rock has a higher temperature than that rock.	$H(t(r_1), t(r_2))$
Step 4.	This rock has temperature 40°C and that rock has temperature 20°C.	$t(r_1) = 40 \wedge t(r_2) = 20$

There are several important logical changes happening, which are revealed by the formalisations which I added on the right side in the tabular. The pre-scientific notion is a monadic predicate, which is explicated by a relation and gets ultimately explicated by a function, when the scientific notion temperature is introduced.[22] This function maps objects to unspecified entities which are called "temperatures". In the last step these theoretical entities which are called temperatures get evaluated on a scale and are identified with numbers. In this very last step, the relational character of the sentences is lost; it is

[22]This change of grammatical kinds during the explication is sometimes overseen in the literature as several proponents of the syntactic view in the debate about theory reconstruction critically observed.

handed over to a silently added mathematical apparatus which completes the notion. Because it would be superfluous to state that 40 is a bigger number than 20. It is important for Chapter 3 that the sensual predicate "warm" is replaced by a theoretic term "temperature", which behaves like a function, and is evaluated in relation to an externally given scale, and by this means connected with a set of measurement procedures, e.g. a thermometer.

2.4 Kreisel's Squeezing

The last method we must clarify on before we can go on to talk about formalisation is Kreisel's famous Squeezing argument. As formulated in [38] by Kreisel himself, it is supposed to show that the intuitive notion of consequence matches classical first-order logic. However, P. Smith and C. Dutilh Novaes pointed out that Keisel misuses the term "intuitive". What Kreisel is actually referring to is an unformalised notion of consequence. (See [1, p. 394] and [64, p. 26]) Their arguments will become important in Section 2.5. Here I only state the general idea of the squeezing. Following [1], it can, in its most general form, be seen as a method to clarify on the relation between an unformalised concept $\mathfrak{U}$ to two formalisations $\mathfrak{D}$ and $\mathfrak{S}$.

Assume there is a concept $\mathfrak{U}$ which is theoretically robust but presented in an unformalised way. It is hard to explain what a concept must satisfy in order to count as theoretical robust; also, it is unnecessary to clarify on this, because it lies in the nature of the squeezing that it establishes theoretical robustness. However, I mean something along the line that it is in use in a critical academic context and that participants have at most minor quarrels about its extension. Moreover, this quarrels should either easily resolve or lead into a quick exclusion of an individual from a community.[23] The concept might be formal, but it is still unformalised. This might be, because there are no explicit closure conditions given and, therefore, the concept might seem to have vague borders. In such a case consider two concepts $\mathfrak{D}$ and $\mathfrak{S}$, which are given in a formalised way and

[23] The notion of proof as used by working (classical) mathematicians is an example.

can be predicated of (roughly) the same category of entities e as $\mathfrak{U}$[...]. [1, p. 394][24]

Moreover, assume that the following statements are considered uncontroversial in the most favourable sense.

(1) If e is $\mathfrak{D}$, then e is $\mathfrak{U}$.
(2) If e is $\mathfrak{U}$, then e is $\mathfrak{S}$.

In such a situation, a squeezing argument tries to prove that the formal systems that give $\mathfrak{D}$ and $\mathfrak{S}$ are extensionally equivalent.
Since (1) gives that the extension of $\mathfrak{D}$ is a subclass of the extension of $\mathfrak{U}$,

$$\text{i.e. } |\mathfrak{D}| \subseteq |\mathfrak{U}|,$$

and (2) the analogous for $\mathfrak{U}$ and $\mathfrak{S}$,

$$\text{i.e. } |\mathfrak{U}| \subseteq |\mathfrak{S}|,$$

the equivalence proof *squeezes* these extensions to the same class,

$$\text{i.e. } |\mathfrak{D}| = |\mathfrak{U}| = |\mathfrak{S}|.$$

In [38] Kreisel used such an argument with a deduction system, like $\mathcal{LK}$ from Chapter 1, as $\mathfrak{D}$ and a Tarski-interpretation, as defined in Definition 5, as $\mathfrak{S}$. The squeezing was then performed by Gödel's completeness theorem. From this Kreisel concluded that not only is a first-order formalisation adequate to grasp logical consequence, but also that it is mistaken to take unformalised reasoning as inferior.
A more recent example is given by Peter Smith in [64]. He tries to shed light on the Church-Turing-Thesis about computability notions. He reinterprets the well-known results, that all formalised computability notions are equivalent, as a mutual squeezing argument on the unformalised notion of computability. More precise: since all these formalisations are equivalent

[24] I cited this directly, because it will become crucial in the already mentioned discussion in Section 2.5 about that $\mathfrak{U}$ is not pre-theoretical or intuitive.

and can be mutually used as $\mathfrak{D}$ and $\mathfrak{S}$ in several squeezing arguments, the unformalised notion of computability is robust and does not need any further specifications.

Very critical on squeezing are E. Andrade-Lotero and C. Dutilh Novaes following Smith in [1]. Explaining why their criticism does not affect my later use of squeezing will also shed light on my project. Hence I will use the rest of this section to counter the objections presented in [1]. Andrade-Lotero and C. Dutilh Novaes are interested in giving the right semantics for Aristotelian Syllogistic. They use a very uncontroversial deduction system for syllogistic as their $\mathfrak{D}$ and offer four candidates for $\mathfrak{S}$ which can also directly function as a semantics for their $\mathfrak{D}$. For any of these four candidates they offer a completeness prove in accordance to their deduction system. Hence, every one of these can be used in a squeezing argument. Additionally, they argue conclusively that premise (2) is uncontroversial for all these semantical candidates. Then they argue that it is implausible that all these candidates are equally good, because some lose their capacity to be used in a squeezing, when modal or temporal syllogistic is considered. For some of their examples lose completeness. Therefore, semantic aspects stay under-determined by a squeezing. According to them this is due to that a squeezing depends too heavely on the claim that premise (2) is uncontroversial.

> [T]hese extensions of the language should in principal not affect the connection between target phenomenon and semantics, which is presumably what grounds premise (2) of the squeezing argument. [1, p. 415]

However, they offer a way out, which they block with a question.

> [O]ne may contend that the role of a semantics when formulating a logic is not that of being a representation of different possible configurations inasmuch as relevant for the target phenomenon. But what is its role then, if not meant to be a model, in some sense or another, of the target phenomenon?[25] [1, p. 415]

[25] Note that Dutilh Novaes is not conflicting here with her own claim, that formal lan-

I have an easy answer to this question: to show the adequacy, i.e. that it covers all of syllogistic, of the deduction system that acts as the $\mathfrak{D}$. That might look boring in the case of syllogistic as it is presented in the Prior Analytics. But this is only the case, because what Aristotle developed there is almost a formal system. The authors would promptly respond, that they made explicit that what they took as the target phenomena of the squeezing argument, was the definition of "syllogism" and not Aristotle's own account to explicate it. Hence the semantical property of being a syllogism and not the syllogistic figures. But I must note here that the adequacy of the syntactic part, i.e. the deduction system, was already unjustifiably tricked into their application of the squeezing argument right at the beginning. The problem condenses at premise (2) again. As they realised themselves, the actual premise was formulated contrapositively:

> (2') If an argument from K to d is not $\mathfrak{S}$-valid, then the argument
> is not $\mathfrak{U}$-valid.
> [1, p. 395])

But (2') is not the contrapositive of premise (2). It only seems like the contrapositive, when one allows the formulation "can be predicated of (roughly) the same category of entities e as $\mathfrak{U}$" from above to trick one's reading of (2'). Let me explain. Assume there is an argument presented in syllogistic form in ancient Greek $\mathcal{S}$. Moreover, take an uncontroversial formalisation of it, say e, from the deduction system $\mathfrak{D}$ as presented in [1]. I consider premise (1) first; it is essential here that e can be the same in the antecedence and the consequent due to e being so very uncontroversial.

> (1) If e is $\mathfrak{D}$, then e is $\mathfrak{U}$.

The reason for this is, that using Greek words instead of the original symbols in e leads to a mere notational variance of the same symbolic object, because $\mathcal{S}$ was already rather technically formulated. Since there is only a change of notation happening, there is no need for translation, transformation, regimentation or formalisation. But if viewed in this way, it is clear that (and

guages are not a models of natural languages, as presented in [25], since there she talks about notational devices and here about semantics.

way) (1) is uncontroversial. One can do the same thing, when using (2') through the fact that the antecedence is the formalised version. But this is not possible in (2). It must be stated then as:

(2) If $\mathcal{S}$ is $\mathfrak{U}$, then e is $\mathfrak{S}$.

Which is only the contrapositive of (2'), when the antecedence is restricted to syllogisms which are already in uncontroversial form. I mean, when one uses fake Greek by just pronouncing one signs symbols greekish, as we sometimes do, when pronouncing "$\rightarrow$" as "if..., then...". But this shows that the target phenomenon is not what was claimed in [1]. What is squeezed, is not some semantical value of $\mathcal{S}$, it is the, so to say, logical form of $\mathcal{S}$ which is represented by e. I cannot see how the authors of [1] are able to justify their claim, that their target phenomenon is some semantical value which characterises a syllogism as a syllogism. Does my criticism aim the squeezing argument in general? Does that not mean, that squeezing is always circular, because premise (2) presupposes what it tries to show? No.

Firstly, because fixing the set of formulas is not the same as fixing the consequence relation, i.e. the consequence relation can still be questioned, after one agreed on the formulas. Note that in (V) we separated the explication of "cases" from the fixing of the range of the variables. Moreover, in my argument against the model view on formal systems the main target is the assumption that the variables range over (the grammatical structure of) sentences of (natural) language. Hence, this is not a minor aspect when studying squeezing techniques.

Secondly, the heavy burden on premise (2) is not a problem, when the semantic counterpart, i.e. $\mathfrak{S}$, is not understood as a description. As I said, what I take the squeezing to show, when applied to a theory of logical consequence, is that the syntactical description of the consequence relation as given by $\vdash$ in $\mathfrak{D}$ is adequate in virtue of the logical form which is given by the choice of the logical vocabulary. And the latter is given or represented by the formulas of the formal system which accommodates $\vdash$. Let me put the same differently: If we chose $\mathfrak{D}$ as a syntactical formalisation of the consequence relation, then its accommodated notion of $\vdash$ does not give an explication of "case" in (V). But it offers deduction rules governing $\vdash$ which are formulated in a close relation to the choices made to constitute $\mathcal{F}$, i.e. the choice of the logical vocabulary. What the squeezing shows is that the more or less educated choice of $\mathcal{F}$ was enough to give an adequate account of the consequence relation without explicating "case". But still the question, whether the logical form is adequately represented by the formulas, i.e. whether the guess was good enough, is an additional (an previous) issue to the adequacy of the deduction rules constituting $\vdash$ in $\mathfrak{D}$. That both choices, i.e. choosing an $\mathcal{F}$ and choosing an $\vdash$, are justified in one fell swoop does not merge them to one. But since $\vdash$ was formulated without grounding it in an explication of "case", one is left with justifying one's composition of deduction rules. And this justification is given by the success of the squeezing due to the necessarily present theoretically robust unformalised notion. The only pressure on the semantical counterpart $\mathfrak{S}$ is a demand for its clear description. Because the only functions of the semantical counterpart is to make premise (2) uncontroversial by offering a weak as possible model-theoretical or semantical notion of logical consequence. Therefore, if a $\mathfrak{S}$ loses its function in a squeezing due to an extension of the syntactical $\mathfrak{D}$, we can simply pass on to another candidate.

> [F]rom the syntactic point of view, validity corresponds to an existancial claim (there is a derivation [of $\mathcal{S}$]), while invalidity corresponds to a (negative) universal claim (no derivation is a derivation [of $\mathcal{S}$]). In contrast, from a semantic point of view, validity corresponds to a universal claim (all interpretations satisfy[$\mathcal{S}$]) while invalidity corresponds to an existential claim (there is an interpretation that [does not satisfy $\mathcal{S}$]). Now [...] dealing with existential claims is far more manageable then dealing with universal claim, it seems prima facie plausible that there is indeed a tight connection between the notion of validity and derivability, on the one hand, and invalidity and counterexamples, on the other hand. [1, p. 396]

Approaches which include an universal claims might be less manageable, but they are quite good in covering things. I view this umbrella property as the primary function of a semantics in relation to a squeezing. I see no reason to extend its function to match a semantic value. We already established quite a bit, when we showed that the more manageable approach is chosen broad enough, i.e. that the syntactical formalisation is adequate.

2.5 How is a Formalisation related to a Theory in (natural) Language?

To summarise what was said so far: I was trying to argue away that formalisations stand in a linguistic relation to (natural) language. I did this by claiming that formalisation is just to give a formal system that is supposed to act as a formalisation. I then argued two prominent accounts of formalisation away that link a formula to a (natural) language sentence through a meaning relation. One might call both accounts translational or linguistic in nature, for meaning stability stays in the centre of attention.
In this section I will present my alternative to these linguistic accounts. Instead of linking a formalisation to a sentence in a (natural) language by means of their meaning, I will establish the adequacy of the formalisation through a successful squeezing argument. Since a squeezing establishes a close relation without accommodating a notion of resemblance, such an account is not a modelling. Also, since [1] successfully argues that a squeezing is not suitable to justify the choices made from a semantical perspective, the relation established by a squeezing is not grounded in meaning stability, i.e. it is neither translational nor linguistic.
However, by some means the relation or link must be established. This will be what was called a "theoretical robust concept" above. As a reminder, we drew on Smith in [64], who argued that such a theoretically robust concept must be present in an unformalised theory in order to allow a squeezing to get a grip at something at all. Since this makes the presence of a theoretically robust concept a necessary condition for a successful squeezing argument, we have shown its presence and robustness by running such an argument success-

fully. I claim that this robust concept was, therefore, previously formed by a process of explication which was articulated in a (natural) language setting. I consider this uncontroversial, when it comes to Carnap's example *pisces*. This concept has been developed by zoologists throughout the centuries in a process of explication. The idea is that, if we formalise the textbook definition of *pisces* and squeeze it against a referential standard semantics, we should get a grip of the notion, because it has already been fully developed through its process of explication.
However, what we are still lacking is an explanation in which sense this concept is already robust. In the case of logical consequence, I must explain the claim that the consequence relation is as formal and has the same logical properties as its formalisation. We need this in order to draw strong conclusions from working with the formalisation, when studying an unformalised theory and its consequence relation(s). We will clarify on these things by using the notion of explication and what was said previously about the notion of being formal.

Even though predominant in such discussions, Aristotle's presentation of Syllogistic in the Prior Analysis is a bad starting point, for it is already quite close to a full fletched formal system. The other natural place to look for a theoretical robust concept of logical consequence that is formal, while still being unformalised, are fully developed mathematical theories. Smith did this in [64] when applying squeezing to computability theory. I take it as granted that these theories are given in an axiomatic way. We will take therefore a closer look on axiom systems as they are used in mathematics and physics. A remarkable paper on this is Schlimm's [62]. Schlimm's topic is axiomatisation or the use of axiom systems in general. What is novel about his analysis, beside his rich description of the function of axiom systems, is his view on presenting such an axiom system. For him an axiom system always comes with a language and an explicitly or implicitly given consequence relation, but his definition of axiom systems in [62, p. 40] is essentially different to my Definition 16 of formal system. Because a natural language is allowed when presenting an axiom system. Schlimm therefore distinguishes three aspects of presenting an axiom system, which are then further separated into modi of presentation. The first aspect is concerning the language in which the axiom system is presented, which separates into these three modi: (natural) language, symbolic languages and formal languages. The second is weather the consequence relation is given explicitly or implicitly.[26] In this sense formal systems, as I defined them in Definition 16, are axiom systems presented in a formal language with an explicitly given consequence relation. But the important part about Schlimm's taxonomy is, that there are more axiom systems present than what I call a formal system.

Schlimm does not say much about how to link these modi of presentation to each other. But there is much we can say by using his taxonomy as a basis and draw on what I have introduced so far. Unfortunately, Schlimm is not very explicit about what "formal" means in a presentation of axioms by a formal language. To be fair, he does not have to, because for his purposes it is clear enough. But here I have to specify, by using what was said in Section 2.1. This might therefore alter Schlimm's original thoughts. Also, we have to clarify on the notion of a symbolic languages (again).

Schlimm's notion of formality is narrower than mine and can be seen, in Dutil Novaes taxonomy, as a language with closer conditions, hence a language that is formed through constitutive rules which are algorithmic in nature, like my formal systems. What causes troubles is his notion of a symbolic language, for there is no such thing as a symbol-free (written) language. Granted, what Schlimm talks of a presentation of an axiom system which uses the familiar symbols taken from logic and mathematics. He gives them a special name in order to keep the reader on track about where to put several axiom systems. But there is no essential difference between the three-sign symbol "and" and the one sign symbol "$\wedge$". The reasons why we use "$\wedge$" in formal systems are readability and clear a distinction between object and metalanguage. However, there is an aspect of symbolic languages in Schlimm's sense, which causes a distinction from natural languages, but in another sense than in Schlimm's original more general idea. The notion of a schema is, as we know

[26] He presents this taxonomy in [62, p. 45] together with two other aspects of axiom systems which do not concern us here.

from the history of logic, strongly related to the introduction of variables into the language.[27] And variables are the kind of foreign symbols many have in mind when it comes to symbolic language. Because they are additionally introduced to the familiar symbols of a (natural) language. Some want to contrast this by using "symbolic language" and "natural language" as opposites. But this is not very essential, because one could just introduce new many-sign symbols without a special meaning in order to advice the same function of these new symbols. The reason why one is not doing this is simplicity. I do not think that this aspect of adopting the praxis of using one-sign symbols from the mathematics of Aristotle's time is essential here. To some this might sound like dotting is and crossing ts, but I want to emphasise (again) that I draw the line between (natural) languages and formalisations not between the use of one-sign symbols or variables, but between introducing closer conditions. This means using constitutive rules and giving an explicit consequence relation. To elaborate this further, let me give an example.

There is a common figure of speech in the language of logicians; we use to ask questions like "Is that proof formal or informal?". I use this figure of speech as well and it is very handy and perfectly understandable. But I want to clarify that in my usage of this words, the question must be phrased as "Is that formalized or not?". Consequently, a sentence like "we define force as $F = m \cdot a$" is not more formal as "we define force as the product of mass and acceleration". A total formalisation would be to write "$F := m \cdot a$" in a system that give explicit formation rules for ":" and "=". This is done in order to exclude copies of this statement with other syntactic form like "$F = m \cdot a$ is how we define force" or "$F = m \cdot a$ is our definition of force".[28] And this is done to control all possible cases in order to exclude counterexamples to claims which might have been stayed unnoticed.

Coming back to explication, I want to focus on Carnap's feature of exactness. It does not seem much of a controversial position to view axiomatisation as

[27] See [25] for details.

[28] Of course, the usage of the symbol ":" is not essential, as explained above. For we could just agree on to rather use "we define" instead and would be as formalised as when using ":", as long there are certain rules for this phrase.

the peak of exactness. What Schlimm's publication emphasis more impressive than Carnap's own example of *pisces* is, that exactness does not include formalisation, and hence a formal system is not needed in order to guarantee a peak of exactness.
However, I produced a mess here, because axiomatisation assures exactness of the notions (or concepts) of a theory and not of the notion of logical consequence; like *pisces* in the example I gave before, when talking about the presence of a theoretically robust concept. But the sciences rarely give explications of logical notions. Hence one might say that formalisation is an explication of logical notions (or concepts). But I think this is wrong, because it requires that these notions have been replaced. But the squeezing shows that no such thing happened. Support for my position comes from an already cited quote from [64]:

> [Prevenient informal sharpening i]s surely necessary if the squeezing argument is to have any hope to success. For there just is no pre-theoretical 'intuitive' notion of valid consequence with enough shape to it for such an argument to get a grip. [64, p. 26]

Generalising this idea, even though a consequence relation might be implicitly given, does not mean that this consequence relation is not extensionally specified enough in order to be fully in place. (Formal) scientist have a pretty good grasp of formal argumentation even though their arguments are not formalised (yet). It would be wrong to presuppose therefore a mere intuition, because their ways of argumentation are, and have always been, extensively taught. Moreover, students must put a lot of effort into learning them. Historically speaking, logic is around for quite a bit and formal reasoning is happening in (natural) languages as well. For example, mathematicians and physicists follow regulative rules when giving unformalised proofs. For instance, they are trained to start with an arbitrary candidate of an entity in question, when a general statement must be proven. They take an "and" to be commutative in their (natural) language proofs, even though their usage of "and" outside of mathematics is not in general commutative. They use schematic reasoning as well. Even though their reasoning does not follow exactly schematic pattern, because several words might sneak in, it is obvious to themselves and others that they rearrange notions through their reasoning independent of the particular notions under consideration. Some might still want to counter that with bringing up the notion of linguistic or logical intuitions. After heaving read [14], I am very renunciative to use the notion of *intuition* in theoretical philosophy. In most cases it acts more like the notion of unformalised, which separates the notion of any explanatory power in our context here. But we do not have to deal with intuitions at this stage anyway. There might be some intuitions at place. Also, some application of an implicit consequence relation might be guided by logical intuition (whatever that is). But others are not. Some are clear cut cases of applying regulative rules, schematic reasoning or even algorithmic rules application. But it is the astonishing finding of a successful squeezing that these clear cut cases are already enough, when put into $\mathfrak{D}$, to have a theoretical robust concept of logical consequence in place.

However, as we said in Section 2.4, premise (2) of a squeezing argument is most problematic. But before we come to discuss this let me set up how I think a squeezing works here precisely in order to clarify why premise (2) does not turn out as a problem at all. In fact the reader will realise that it fits rather nicely in the bigger picture.

Assume an unformalised theory $\mathfrak{U}$, which is somewhere out in the literature. Since we want to argue for the premises (1) and (2), we also consider an unformalised argument A, which is contained in the theory. Moreover, we identify A with the sequent $\mathcal{S}$, which is taken from a formal system $\langle \mathcal{F}(\alpha), \mathcal{T}, \vdash \rangle$. Also assume that it is somehow uncontroversial or irrelevant whether the names occurring in A or $\mathcal{S}$ refer and therefore are taken as they would refer.[29]

We assume that all the argumentative moves in the theory, that are clear cut formal in the sense of Section 2.1, are represented, and therefore formalised, in $\langle \mathcal{F}(\alpha), \mathcal{T}, \vdash \rangle$.[30] Hence, we take $\langle \mathcal{F}(\alpha), \mathcal{T}, \vdash \rangle$ to act as $\mathfrak{D}$ and can establish

[29] I will come back to this assumption later.

[30] With possible additional rules which are considered uncontroversial or were introduced for technical reasons. I am not contradiction myself here. That the moves are formalised does not mean, that they are represented by the rules, but only that they are deducible from the rules.

the first premise.

(1) If $\mathcal{S}$ is $\mathfrak{D}$-valid, then $\mathcal{S}$ is $\mathfrak{U}$-valid.

The next three steps are: to find a $\mathfrak{S}$, argue for the second premise, i.e.

(2) If A is $\mathfrak{U}$-valid, then $\mathcal{S}$ is $\mathfrak{S}$-valid,

and then prove a completeness result. Let me assume that we have already proven this result.
Hence, as before, we are left with arguing for premise (2) and the choice of $\mathfrak{S}$. My claim her is that in our context of *Wissenschatslogik* this is not much of an issue, because theories from the modern sciences are logically rather highly developed and that the working scientist should rather easily agree on a suggested semantics. By their deeply into classical logic rooted theories, in most cases it will be a standard semantics anyway. Similarly for the logical form of the argument A in question, i.e. the sequence $\mathcal{S}$ in $\langle \mathcal{F}(\alpha), \mathcal{T}, \vdash \rangle$, the guideline is what the working scientist accept or suggest. So here the target phenomena, to stay in the diction of [1], is the logical status of an argument as the scientists take it to be. Moreover, as promised, we end up with a debunker theory of logical relations.

2.6 Consequences and Clarifications

To clarify further on what $\mathfrak{S}$ in premise (2) is not supposed to be, I will clarify on the two most possible misreads.

$\mathfrak{S}$ is not a model of a linguistic phenomenon, if "linguistic" means the study of the semantical properties of the (natural) language in use, when the theory is presented. This might be criticised, because some might think, that these is exactly what a formalisation should cover. It should be a modelling of the semantic relations, which the junk of language that we call a "theory" has. My response in a nutshell was: no, it does not model the semantics of the (natural) language in use, it only works as a small as possible umbrella to reach the logical properties of the propositions made in the sciences. And these logical properties are established by the working scientist.[31] And as I stated earlier, these are not given by the semantic properties of the (natural) language. This is not just a plain de-punker position, it is also the best we can hope for anyway, if one takes Williamson's position seriously.

> But to stop our logical debate with Peter and Stephen in order to explain to them what the word "every" means means in English would be irrelevant and gratuitously patronizing. We cannot understand them better if translate their word "every" by some non-homophonic expression, or treat it as untranslatable. The understanding they lack is logical, is not semantic. Their attitudes to ["every vixen is a vixen"] manifests only some deviant patterns of belief. Since there clearly could have been, and perhaps are, people such as Peter and Stephen, we have counterexamples to[: Necessarily, whoever understands the sentence "Every vixen is a vixen" asserts to it.] [74, p. 91]

What I want to take from Williamson is the view, that the logical properties of a proposition expressed by a (natural) language sentence are not implied by the semantical properties of this sentence (or related sentences). Presupposed that the "semantics" is understood as the meaning of a (natural) language sentence and not the outcome of a meaning-theory that already confounds it with logical properties. The effect of this is that there is no such thing as epistemic analyticity in my view, i.e. derivations in a formal systems do not justify to call their outcome an analytic truth.[32] Because neither is justifying the premises (1) and (2) epistemic innocent nor is the ascription of logical properties to propositions by the scientists. For some this might ridiculous the hole project, because the Ramsey-Carnap-approach is seen as an explication of this very notion of analyticity. (See [70]) Let me go further into this. The currently used taxonomy for analyticity was developed by Boghossian in [8], where he distinguishes between *metaphysic analyticity* and *epistemic analyticity*.

[31] Note, that this leaves open whether these properties are given to the propositions or found during the scientific process.

[32] At least not in the later explained epistemological sense.

> On this understanding, then, 'analyticaly' is an overtly epistemological notion: a statement is 'true by virtue of its meaning' provided that grasp of its meaning alone suffices of justified belief in its truth.
> Another, far more metaphysical reading of the phrase 'true by virtue of meaning' is also available, however, according to which a statement is analytic provided that, in some appropriate sense, it owes its truth value completely to its meaning, and not at all to 'the facts'.[8, p. 363]

Boghossian's strategy in a nutshell is to separate these two meanings of "analyticity" and then blame the metaphysical reading, or a conflation of it with the epistemic, for the defectiveness of analyticity found by Quine and others. He then tries to establish a version of epistemic analyticity.[33] Coming back to the Carnap-Ramsey-approach, the loss of epistemic analyticity, even though surely very important for Carnap himself, is not much of a problem, because Theorem 1.2.4 gives us a good notion of analyticity. Analytical truths are those which cause an inconsistency between equally empirical adequate theories (if one occurs). This notion is obviously not epistemic analyticity. It is a version of metaphysic analyticity which is not directly targeted by Boghossian, because it is not relying on the notion of meaning but on logical properties. Unfortunately, this text is not the right place to debate such matters further. However, since the analytic-synthetic-distinction was an initiator for this Chapter and critiques on the Ramsey-Carnap-Approach use it (like [50]), I had to clarify up to some point.

$\mathfrak{S}$ is not a model of a (philosophical) concept, like *logical consequence* or *a syllogism*. As we mentioned before, in [1] the authors explained what they take as the phenomenon their squeezing is targeting: the notion of syllogism Aristotle gave by himself at the beginning of the Prior Analytics and not aristotelian syllogistic, which they view as a first attempt to explicate the notion of syllogism. This matches with the view of many on what we

[33]In fact Williamson is trying to dough these arguments in favour of the epistemic readings in his [74] which arguments like the one cited above.

said back when we discussed (V) in Chapter 1.

> **(V)** A conclusion, ψ, follows from premisses, Φ, if and only if any case in which each premise in Φ is true is also a case in which ψ is true. [4, p. 476]

For many this stays at the centre of the enquires of philosophical logic. As I explained when I discussed premise (2) of the squeezing argument, I cannot see how a squeezing argument is able to say something interesting about a notion as (V) that extends the insights which are already given by the explication that is implicit in the unformalised talk of scientists, philosophers or mathematicians. The reason for this is the highlighted (by the authors of [1]) under-determination of a suitable $\mathfrak{S}$ for the squeezing. The squeezing itself cannot separate good semantics from bad semantics unless the syntax gets extended, i.e. the squeezing argument works even with an $\mathfrak{S}$ that does not match the original idea of (V) and an extension of syntax is needed to show this. What I tried to argue for is, that this is the case, because the syntax plays an essential role when arguing for premise (2). Moreover, squeezing is in no respect special. A resent debate in the philosophy of science about theory representation lead to a similar view on such matters: Hans Halvorson gave his paper [34], which is an answer to a respond Clark Glymour gave in [32] to Halvorson's earlier text [35], the self-explanatory title "The Semantic View, If Plausible, Is Syntactic".

That does not mean that I want to move (V) out of the centre of attention of logical theorising. What I doubt is that it is a prototype of a(n unique) semantic value, which acts as an characteristic, of a relation between sentences. There is a certain fetishism in the philosophy community towards algebraic and model-theoretical methods through a homonymous use of the term "semantics". It conflates positions that claim that (V) is a semantical notion with the name "semantics" as a common noun for algebraic methods in logic.[34] In addition, I already emphasised that I do not even follow such views on the matter and rather following Williamson's [74]. Of course, this

[34]Critique on such a conflation was already given by W.V.O. Quine in [55] and the literature of proof-theoretic semantics, e.g. see M. Dummett's [23].

does not mean that we cannot study (V). I mentioned in Section 2.4 that studying (V) through the syntactical approach $\mathfrak{D}$, i.e. using formal systems where $\vdash$ is characterised as G-validity, is in many respects the better option anyway. We just have to view (V) as a schematic definition and not as giving a semantic value ready to explore. I enumerated several advantages of such an approach in Section 1.1.2.

I am aware that explaining how I see the relation of explication, formalisation, $\mathfrak{D}$ and $\mathfrak{S}$ would be helpful for my readers. Unfortunately, since $\mathfrak{S}$ stands to an explication as $\mathfrak{D}$ stands, as a formalisation, to a theory, just on the metalevel as a formalisation of a theory of consequence, this would be already the solution to the application problem of mathematics. Which I am going to tackle in the following and last chapter of this book.

How can we answer Rozeboom's worries? Rozeboom pointed out that, using a compositional semantics, when theoretical terms lack a semantic value, then so do the sentences including them. Therefore, there is no basis to judge the adequacy of a formalised sentence to the original one. I can counter any attempts to make a problem out of this by pointing out, that semantics is not the basis on which a formalisation shall be judged as adequate. For neither are the logical properties of the sentences related to the (natural) language in which they are presented nor is it the aim of the Ramsey-Carnap-Approach to give an account of the linguistic properties of the language in which scientist speak or write. Since we are interested in the former, when analysing a scientific theory by logical means, there is no problem. Still, many might argue that, at least when a Tarski-approach to logical consequence is used wherever in an argument, i.e. where references play an essential role, the existence or non-existence of the references of the formalisation shall match those of the theory. But as we saw in the discussion of the squeezing argument before, logical semantics is under-determined and is merely an algebraic device to study the consequence relation. I use the analogy of an umbrella. Hence, what hast to resemble is this consequence relation and not the references. Therefore, in order to dismiss a formalisation by the Ramsey-

Carnap-Approach one has either to show, that the theory acts substantially different when using a logic that allows names which lack a reference, or, that the logic is substantially different to the logic present in the sciences. The first is highly doubtful and the second is ruled out, when a squeezing argument was successful.[35] Note that we followed this already when analysing the squeezing argument, i.e. at some point I was assuming that names shall refer.

[35] In fact, I was planning to start off with a logic including names which lack a reference. I gave this idea up, because it did not change much, but made things more complicated.

Chapter 3

The Application Problem of Mathematics

Before I can present my solution to the appliance problem of Mathematics, I have to differentiate four notions: Formalisation, Mathematisation, Quantification[1] and Explication. In Chapter 2 I clarified that the process of explication has to be more or less already be finalised before formalisation takes place and that formalisation does not add much essential to it. I need this to assure that a formalisation is informative when it comes to study the logical properties of a theory given in (natural) language. I also explained and justified that I use "formal" broader and, therefore, not synonymously with formalised. Hence, there is room for mathematics without formalisation. I felt the need to be very elaborative about it, because in prominent places in the literature this is obscured. For authors tend to mathematise in the process of formalisation, which strips formalisation of its explanatory power by presupposing what could have been shown. For instance see [66, Ch.12.5], where Suppes gives his formalisation of Newtonian physics by including explicit definitions for functions on real numbers straight away. However, before we move on with our discussion let me introduce the actual topic of this book: *The Application Problem of Mathematics*. This will happen in the second section of this chapter. To assure that the reader understands

[1] In the sense of coding information into numbers and not in the sense of bounding free variables.

this problem correctly, I will start with a historical survey. Then we will continue our discussion, which started in previous chapters, and thereby give a solution.

3.1 Mathematics and the Natural Sciences

Penelope Maddy gives in [47] a historical argument about why further developments or general heuristics from the sciences have no impact on developments in pure mathematics, because the latter is gradually disconnecting from the sciences. Even though related, this is of no interest here for us. However, in this process, she gives a rather impressive, informative but also brief historical description of mathematical developments since the antique. I will follow her and summarise this part of [47] here.

Maddy distinguishes three phases of how mathematics was related to the sciences since the antiquity. Phase one is roughly from "the beginning" of western thought until the beginning of the 17th century. Following Plato, mathematics was the science generating truths about abstract and time independent entities; it was science proper and generated real knowledge. While natural philosophy or the sciences where mere opinion about a continuously chancing physical world. This changes with the work of Galileo Galilei, Rene Descartes and Isaac Newton. In their times a synthesis of mathematics and the sciences emerged. They stopped asking for causes of change, as scholastic natural philosophy did, and practised science as aiming for mathematical descriptions of phenomena. Hence there were neither affords to keep those two apart nor could any question of applicability be asked. For science was the mathematical description of the physical world. This changed rapidly in the 19th century, which induced the third phase, when science became the predominant model for reliable knowledge. Mathematics on the other hand became a linguistic tool to express this knowledge. This view was challenged by Gödel with his incompleteness theorem, which opened up the possibility for a version of Platonism again. Nowadays several debates whether mathematical objects are handy fictions, existing entities, (mutually instantiated) structures, linguistic constructions a.s.o. have started. We will not go into

these metaphysical debates here.
Then Maddy gives three strands of development, which induced the transformation from the second to the third phase. The first strand is the smoothest and based on the increasing professionalism in mathematics as an academic discipline. In antique and during medieval times mathematics and astronomy were virtually the same discipline, because in a neo-platonic reading of Aristotle's natural philosophy the sublunar realm was the realm of mathematics or simply because the dominant application of mathematics was astronomy; the latter is accompanied by the view that the abstract notion of a point seems to be realised by a fixed star.[2] As it was already said, the synthesis of mathematics and the sciences was the predominant view for intellectuals and scholars from Galileo Galilei onwards, therefore mathematical development was guided by scientific enquiry. But later mathematicians started to ask genuine mathematical questions for their own sake. In the second half of the 19th century the notion of group emerged an was applied to several mathematical entities; it was then applied in physics roughly 50 years later. But notions like *group* where mere taxonomies of known or already applied structures. Hence their usefulness was metaphysically and epistemologically unchallenging, because reducible to the already known.
The second strand of development was leading into bigger struggles. Because with Riemann's geometry having been developed, there was a new geometry around contradicting classical Euclidian geometry. Therefore, people were faced with the choice in which world they want to live in. Since this choice seems ridiculous, one rather gave up the idea that mathematical structures have strong metaphysical consequences. Mathematics since then looks for many like a warehouse, i.e. it discovers truth about an abstract realm of mathematical structures and provides these structures for applications in physics.
The third strand leads into the same picture of mathematics by focusing on another aspect of mathematical development. While Newton was arguing

[2]This is a messy topic and Maddy is not covering it in [47]. It is strongly related to the ways how neo-platonic Arabic philosophy read Aristotle and how the Latin-west adopted the Arabs.

mainly by using ancient methods of synthetic geometry, later generations started to develop symbolic calculi that lacked a clear notion of proof, e.g. Leibniz and Euler. The departure from the rigorous system of ancient synthetic geometry was liberating for the newly emerging sciences. But it was also only possible because of the strong relations to these sciences. Because the external standards of scientific enquiry, i.e. explaining a phenomenon adequately, were therefore reflecting onto mathematics to support mathematical claims indirectly without the needs for a direct proof. Hence the lack of a notion of a proof for the novel symbolic systems was not damaging. However, there were two competing traditions following this path: those who focused solely on the phenomenon and those who postulated a hidden structure. For instance, the competing theories on heat advocated by Fourier, who developed his theory by heuristics using infinitesimal layers of a physical body, and Poisson, who presupposes the existence of 'molecules' aggregating to the physical body.

> [T]he familiar battle lines were drawn between those who explained phenomena by appeal to hidden structures and those who describe what they see in terms of differential equations.[47, p. 28]

Further development in chemistry made the hidden structure approach more appealing and modern statistical thermodynamics is using it quite frequently. It was this time, when the atom-theory spread from physical chemistry to all of physics. However, since there is no clear and straight forward way in which sense the model-structure and the real world structure are isomorphic, i.e. modelling the hidden structure involves idealisation and distortions, the mathematical truth of the model, even if mathematically proven, is not straightforwardly coinciding with a truth about the phenomenon. Maddy draws a central conclusion from this.

> Paradoxical as it may sound, it now appears that even applied mathematics is pure. [47, p. 33]

Moreover, this leads to a similar distinction of truth about abstract entities and real-world truth about spatial temporal objects as before in the case of geometry.

This is what made the logical empiricists to locate truth about abstract entities in a linguistic phenomenon. The others, who argue for a platonic stance, e.g. by drawing conclusions from Gödel's incompleteness result, face severe epistemological issues:

> [I]f the cognitive machinery of human beings works as we think it does, how can we gain knowledge of nonspatiotemporal, causal entities?[3] [47, p. 18]

Another interesting conclusion Maddy draws from the separation of mathematics from the sciences concerns the Duhem-Quine-Thesis, i.e. the thesis that the existence of any unobservable entity (including mathematical entities) must be accepted by those who hold a theory postulating these entities. However, Maddy counters this argument by claiming the opposite with an equally persuasive force.

> One well-known metaphysical argument - that the role of mathematics in science confirms the objective existence of abstract mathematical entities - must seem less compelling in light of what we have seen: if the scientist's actual claim is that some aspect of the world resembles a given abstract model in some respects, to some degree, within certain bounds, then it is hard to see why that abstract model must 'exist', whatever that comes to; all that matters for this sort of claim to have content is that the mathematical item be precisely characterized. [47, p. 35]

This does not concern the application problem of mathematics directly, but I want to emphasise that, therefore, my solution does not imply Platonism directly. Moreover, and contrary to what some Carnap scholars have claimed previously in the literature, Carnap had a similar view that is also justifiable. Beside this I will not make any further metaphysical claims in this book.

[3] However, insurmountable problem concerning the truth of mathematics are predominantly articulated as a problem for constructivists by proponents of a realistic view. (See [41, Ch. 1] and [20, p. 269])

Moreover, and very impotent for my argument, I take Maddy's description as a justification to not distinguish between applied and pure mathematics, when it comes to the application problem.

3.2 Formulating the Application Problem

My description of the application problem draws mainly from Mark Colyvan's paper [20]. Colyvan has different aims as I. He wants to show that the application problem of mathematics is in fact a set of problems occurring in different versions for any ontological stance in the philosophy of mathematics and, therefore, affecting realism and anti-realism alike.[4] The reason why he wants this is, because the original formulations of the application problem are phrased in an anti-realist language. Colyvan gives three formulation. The first was giving by Eugene Wigner (1960):

> The miracle of the appropriateness of the language of mathematics for the formulation of laws of physics is a wonderful gift which we neither understand nor deserve. [20, p. 265]

The second formulation goes back to Steven Weinberg (1993):

> It is very strange that mathematicians are led by their sense of mathematical beauty to develop formal structures that physicists only later find useful, even where the mathematician had no such goal in mind. [...] Physicists generally find the ability of mathematicians to anticipate the mathematical needs in the theories of physics quite uncanny. It is as if Neil Armstrong in 1969 when he first set foot on the surface of the moon had found in the lunar dust the footsteps of Jules Verne. [20, p. 265]

As a third version, he adds a younger formulation by Mark Steiner (1995):

[4] In [20] for Colyvan a realist seems to be solely a realist towards the existence of mathematical objects. He does not say anything about realists towards epistemological values of mathematical sentences.

> [H]ow does the mathematician - closer to the artist than the explorer - by turning away from nature, arrive at its most appropriate description? [20, p. 265]

However, as we have seen in the previous section, coming from a realistic position, leads to similar problems. But even with the rather complete story told by Maddy in [47], there is still a more detailed story to tell how it is possible that mathematics fits into the sciences. Quoting Steiner, Colyvan points out that there are two aspects of the application problem: the problem of the applicability of mathematical theorems in the sciences and the role mathematics plays in the discovery of the theories of these sciences. The latter is especially troublesome, because mathematicians are not guided by the needs of particular sciences. For Steiner the crucial role of mathematics in predicting future events must be especially addressed. (See [20, pp. 266-267]) Later in his discussion of how the problem appears in Hartry Field's fictionalism Colyvan notes:

> Field explains why we can use mathematics in physical theories - because mathematics is conservative. He also explains why mathematics simplifies calculations and the statement of thee theories. What he fails to provide is an account of why mathematics leads to simpler theories and simpler calculations. [20, pp. 272-273]

I disagree with Colyvan and Steiner, when they say that the application problem of mathematics must lead to an answer how mathematics plays a role in the predictions of scientific theories. I see no reason why one should answer the case of a theory predicting by mathematical means and the case of a theory predicting without mathematics separately. If one solves the other aspects of the application problem, then these questions towards predictions coincide with the single questions how any kind of theory, mathematical or not, can lead to true predictions. But even if one is not following my reductive suggestion here, it is unclear in what respect solutions to the problems concerning predictions should differ whether a theory includes mathematics or not. Therefore, I am rather tackling the other challenges stated above:

1. How does the mathematician, by turning away from nature, arrive at its most appropriate description?

2. Why does mathematics lead to simpler theories and simpler calculations?

The way how this book was written so far already shows that I intend to tackle these problems from a logical point of view. This might of course rise opposition. Many might think that the most informative or interesting way of tackling these problems is historically, sociologically or psychologically.[5] This might be true. I do not feel very strong about such questions, because I doubt that they are of any methodological significance. Of course, there is a long going debate of, for instance, how formal methods change the subject matter of a philosophical inquire, e.g. the already mentioned Carnap-Strawson-dispute, or how we really must look upon science to unwind its nature. But those debates are rather specific or proceed in a relation to a specific goal. For the rather broad and lose question, which is risen here, whether a logical story about the means and possibilities of applying mathematics in the sciences is interesting or informative depends solely on the interests my readers and I have. I intend to tell a logical story, because I am a logician and interested in logical properties and relations. Does that lead to anything interesting to a more general audience? I do not know, but I hope you can tell me in twenty years or so.

3.3 Four Central Notions

Unfortunately, the i dotting and t squaring cannot stop here. Before I can talk about the use of mathematics in theories, I must be very careful to keep a mathematisation, a quantification and a formalisation apart. In order to explain my use of these notions, it is handy to consider Carnap's example of the explication process from "warm" to "temperature". As I explained in

[5] Compare Dutilh Novaes' view on the use of formalisms as a cognitive tool in [25, Ch. 5].

Section 2.3, I see this as proceeding in four steps.

Step 1.	This rock is warm.	$W(r)$
Step 2.	This rock is warmer than that rock.	$W'(r_1, r_2)$
Step 3.	This rock has a higher temperature than that rock.	$H(t(r_1), t(r_2))$
Step 4.	This rock has temperature 40°C and that rock has temperature 20°C.	$t(r_1) = 40 \wedge t(r_2) = 20$

I will now explain what I mean by the three notions, when an explication is already given.

Quantification

I talk of quantification in the very narrow sense of measurement data. Quantification is happening in the transition from step 3 to step 4. It is the identification of certain terms with numbers by the means of a previously introduced scale. Therefore, relations can be handed over to primitive arithmetic. I do not think that quantification is directly related to the application problem of mathematics. As explained in Section 3.1 by Maddy: the interesting questions arise, when mathematical structures are used. Also Colyvan, as explained in the previous section, formulates the application problem as the tension between a mathematician, who is led by aesthetic considerations ignoring empirical needs, and a physicist, who is held accountable to observable phenomena. Therefore, I cannot see a philosophical problem concerning quantification. Because, there are neither structural considerations made nor is there any extra-empirical guidance present. In fact, it does not seem puzzling that finitely many terms can be paired with finitely many concrete numbers. Especially when this is happening by the means of a measurement method that includes a scale. Because, this is just following an everyday-routine of using such a device. Note, that this is not affected by any question concerning an isomorphism between the natural numbers and the real world, characterisation up to isomorphisms of a structure exemplifying these numbers or any other position currently available in the literature. Because all there is, is a particular finite ordered set of actually given outcomes of measurements.[6] Since there are neither empirical nor metaphysical questions at stake, one might take number as whatever one wants them to be; even just being identical with their number-term. Hence, if problems occur, then these are left to solve for the science of the particular measurement techniques in place; this is no business for philosophers (of mathematics).[7] I am aware of being here in opposition to a certain branch of mainstream philosophy of science, starting with Alan Baker's work. (See [3]) However I want to requote Carnap.

> "[O]bservation language" means (as is often the case) a language containing only observational terms and the terms of elementary logic and mathematics. [15, p. 252]

Since Carnap allows mathematical terms in the observational language, we are in line with him in this respect. Because also for Carnap, actual mathematics enters the stage later; for instance, with the Maxwell-Ampere law in electromagnetism (see [20, p. 268]) or with Fourier's work on caloric theory (see [47, pp. 25-28]). But I do not see much tension with Baker himself. In [2] Baker's aim was not to say much about applicability, but to show that mathematical notions are not indispensable from physics. Following Carnap's quote, Baker's stance is not affected by ours, because we do not want to get rid of the observational language. However, we are not trying to answer ontological questions of indispensability here.[8]

Formalisation

As explained in Chapter 2, I take formalisation to be a connection of formulas with sentences which is not linguistic in nature. It is, in my view, most importantly unrelated to explication, because as one can see in the tabular:

[6] The set is finite, because no measurement devise is infinitely precise. (See Section 1.2.3)

[7] In fact, viewing things as I do here is rather traditional. Because, for ancient Greek philosophy paring numbers with other things was a matter of $\tau\epsilon\chi\nu\eta$, while mathematics happened somewhere else.

[8] Of course, I do not say that this is all what there is to say about quantification from the perspective of the philosophy of science. Big Data, the cognitive advantages, the political effects a.s.o are of course worth studying. But they simply are not relevant when it comes to the application problem of mathematics.

formalisation can be done in any of the stages of an explication process and does neither mark the end of it nor leads to a final explication. Weather a formalisation is possible, informative or adequate depends on the success of a squeezing argument to get a grip on the explicated notion, which has already been fully specified in the (natural) language.

Mathematisation

Of course, my notion of mathematisation is going to do the work later, when I give my solution of the application problem. Hence I will briefly recall the important parts of what was said so far. The application problem as it was formulated above says: how can an abstract object that is studied without scientific applications in mind be applied successfully in science? Maddy's description of the history of mathematics assures us that this is the right question to ask as well as that these objects are structures.[9]

In my understanding mathematisation is replacing the theoretical entities and concepts of a theory by mathematical objects.[10] These mathematical objects form, are embedded in or constitute a mathematical structure. By construction, the structure presupposed by the theoretical terms of a theory is a substructure of this new structure.[11] This new structure can then enrich the structure previously given by the theoretical sentences.

Here I must clarify certain things. First, the word "theoretical terms", as a vernacular terms, does not *a fortiori* favour any particular technical notion which explicates it nor do I see it in relation to a particular formalisation. The identification of the theoretical terms can be done by whatever one might come up with. I just prefer Carnap's approach to perform this task. Second,

[9]It does not matter what these structures are metaphysically; they might be *in rebus* structure, *ante rem* structures, abstractions of particular structures, and whatever else structuralist might come up with in the future. So far a particular position can give an account on isomorphism, it can be adopted here.

[10]I was never very keen to keep concept and notion apart. I was talking very loosely about such things. There was and is no need to introduce the notion of a concept. Here is the only occurrence where it somehow counts that it is the concept (in a Lewis' diction), i.e. the characteristic function of a predicate, rather than the less specific notion of notion, which is going to be replaced.

[11]This is actually the only place, where the structuralism which is immanent in the Ramsey-Carnap Approach plays a role.

the separation of the theoretical terms from the others does not have to be clear cut, to spare artificial adjustments or to be guided by *general* principles. I feel strongly justified to claim this, since Demopoulos assures that this is Carnap's position:

> Carnap frequently emphasized that virtually every unreconstructed sentence of the language of science and everyday life has both factual and nonfactual aspects, so that a sharp separation of our language into factual and nonfactual sentences is meaningful only related to a reconstruction of that language. [22, p. 121]

And because Suppe grants Carnap this.

> How would one show that such a division is impossible? To show that any [division of a observational and theoretical language] will be an "artificial" convention won't do, since Carnap and others admite that it will be. To discuss problems about borderline cases won't do, since someone like Carnap can admit these and make conventional conservative decisions about how to handle these cases. And to consider various proposed divisions and attack them cannot demonstrate the impossibility of drawing such a distinction. In fact, it seems that the only way one could show such a division impossible would be to show either that no finite characterization of the division is possible or else that any possible division which clearly makes observable occurrences directly observable will result in such an impoverish stock of observable occurrences that most of science could not be confirmed. The changes of successfully demonstrating either of these contentions seems quite remote.[...] This [...] is sufficient warrant to conclude that Achinstein and Putnam are urging the rejection of the received view for the wrongs reasons. [65, p. 9]

Third, as before I am not bonded to a particular notion of replacement. The word "replacing", as a vernacular terms, does not *a fortiori* favour any particular technical notion which explicates it. I will us "change of assignment"

by drawing on the notion of assignment from Chapter 1. But it might as well be explicated in (natural) language without any formalisations. Moreover, my notion of mathematisation is informative, even when the notion of replacement stays in a stage where it is not fully specified.
But, to explain what I mean, I will give an example. Mathematisation can happen in all stages of the previously described explication process of warm to temperature. But I will have a closer look at stage 4.

> This rock has a temperature of 40°C.

It is uncontroversial in the natural sciences to see this sentence as equivalent to the following:

> The temperature of this rock is 40°C.

Hence it is uncontroversial to formalise it as:

$$T(r) = 40°C.$$

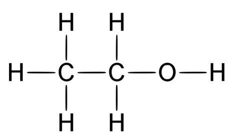
I will continue describing my understating of mathematisation with this formula. However, I claim that nothing is lost or missed, when analysing the original sentence. First, note that this is not a first-order formula, because of the occurrence of a unit. The unit ties the "40" to a scale and a set of measurement methods. Otherwise the sentence would be wrong, because it has the form of an identity statement. But no temperature, which is a configuration of particle movement of the rock r, is identical to the number 40. In fact, the unit "$°C$" acts on the identity-formula to assure truth; it is an operator whose logical properties have not yet been spelled out. One of the main aspects of mathematisation is that it saves us from spelling out the logic of a unit. By chancing the reference of T from a temperature function to a function mapping into the real numbers, we drop the unit and, therefore, get an ordinary identity statement between real numbers. The choice of this new function is only partly restricted by the condition to map the reference of r to the number 40. Note that this restriction is not by accident reminiscent of correspondence rules, because "$T(r) = 40°C$." has the syntax of a correspondence rule. (See Section 1.2.6) Moreover, there are more beneficial aspects of this reference change than the avoidance of spelling out the logic of units: the new references come with a structure ready to use. As before as an aspect of quantification, now the structural properties of the original theory are extended by the structural properties enjoyed by functions mapping the reals into the reals. Of course, to use this additional structure new axioms must be added to the Ramsey-sentence which express its structural properties. Here a characteristic of the syntactic view, which is often considered to be a malfunction, turns out to be beneficial. Because it is the disability of a syntactic approach to characterise a structure up to isomorphisms which allowed a reference change to a richer structure at the first place. I will study this idea on an example in the next section.

3.4 To Mathematise a Theory by Ramseyfication

Since the notion of mathematisation was made clear and carved out from other notions to which it is often related, I will put it into action to clarify on the application problem. In order to do so I will first use Ramseyfication on a bit of scientific theory. It is of course virtually impossible to actually do this for the entirety of a scientific theory. This is also not necessary. It is enough to specify the method, as we did, then justify its use and then emphasise how it can be done. Results are then obtained by general results about this method ore partly given formalisations of bits of the theory.

Infrared Spectroscopy

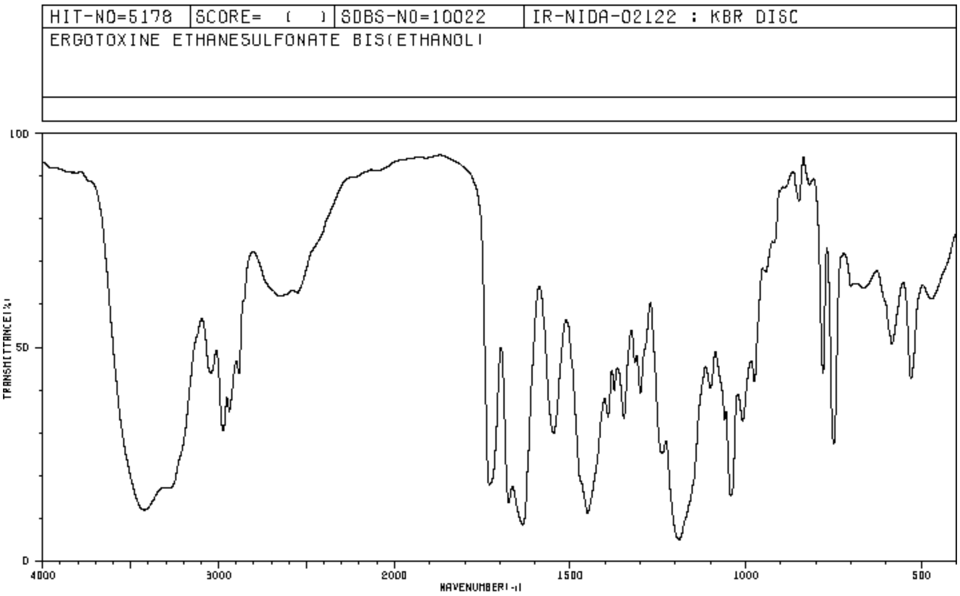
The bit of science I want to examine is the theory behind infrared spectroscopy. Infrared spectroscopy is a standard measurement technique with many applications in organic chemistry. It is mainly used for the qualitative analysis of a substance, i.e. to identify the substance or the components of a mixture. The basis of this technique is, that heat causes movement and that atomic bonds in a molecule stay intact below a certain temperature. A chemist, informed by quantum physics, knows that submolecular structures can only stay in discrete quantum stages and that these correspond to the energy levels which they are able to absorb. She also knows that energy corresponds to the wavelength of radiation and that heat corresponds to infrared radiation. Hence, wavelengths correspond to submolecular structures. The idea behind infrared spectroscopy is now, to measure those wavelengths not absorbed, in order to conclude on those which were absorbed. By that data the chemist can obtain certain properties of the submolecular structure. Since the bond must be stable in order to move, this method works best wit atomic bonds, e.g. bonds between two carbon (C-C), a carbon and a hydrogen (C-H), a carbon and an oxygen (C-O) and an oxygen and a hydrogen (O-H) atom. That is why it is widely applicable in organic chemistry. The type of movement infrared radiation induces in a molecule is vibration of the atoms composing it along their bonds. One can distinguish six types of vibrations in a molecule. Each includes three distinct atoms and groups further into two categories: stretching, i.e. the angles between the bonds stays the same, and deformation, i.e. the angles change. Stretching can be symmetrical or asymmetrical while deformation vibrations follow the following taxonomy: rocking and scissoring, for those vibration moving in the plane, and twisting and wagging, for those vibrations where the parts move out of the plane.[12] The data the measurement devices produce are points which pair a wavelength (λ) in centimetres (cm) with the intensity (I) of the corresponding radiation. However, it proves handy to rather consider related magnitude: the wave-number ($\frac{1}{\lambda}$) in cm^{-1} and the transmittance ($\frac{I}{I_0} \cdot 100$, with I_0 as the original intensity). By printing the date into a suitable diagram and connecting the lines, a curve is obtained; the one given below is the curve for ordinary alcohol (ethanol), which has the following structure:



The (downward) peaks corresponds to an atomic bond. For instance the

[12]The names can be taken pictorial in accordance to arm movements in corny dancing styles, when taking the central atom as body and the two at the edges as arms.

massive peak at the beginning, i.e. between 3500 and 3000 cm^{-1}, is a sign for the stretching of O-H. The sharper one close to 3000 cm^{-1} is a sign for H-C-H.



3423	11	1636	8	1300	37	976	41	686	49
3044	42	1547	28	1239	23	857	84	530	41
2972	29	1451	10	1189	4	848	81	516	49
2939	33	1392	32	1100	38	819	84	472	68
2885	42	1374	38	1060	32	780	42		
1730	17	1347	32	1043	14	750	26		
1676	13	1316	44	1009	31	666	62		

Source: Spectral Database for Organic Compounds SDBS,
https://sdbs.db.aist.go.jp/sdbs/cgi-bin/landingpage?sdbsno=10022

I consider it important and essential to the theory behind this measurement method that a chemist does not have to know any quantum physics in order to be a competent user of this measurement technique.[13] Also, there is no mathematics needed so far. All there is, is a taxonomy of chemical bonds and their possible vibrations which correspond to wavenumbers. Hence, there is a junk of scientific theory out there in the fields, which is in use, an essential part of modern science, links molecular theory to measurement data and is rather easy to formalise. Therefore, I found a nice toy-example for my approach.

Let me counter certain arguments against the choice of this example before they manifest; I can think of two. The most common is probably, that I am

[13]In fact even many experts of this techniques do not know much more about it, than what we said about quantum physics above.

ignoring the fact, that the theory of infrared spectroscopy is deducible from quantum mechanics. However, this is wrong. The taxonomy of the vibrations must be used, when formulating and solving the Eigenwertequations. Therefore, the unmathematical theory is logically prior to the mathematical one. (See [10]) Granting this, others might argue, that the theory I have presented so far is only a degenerative form of a more advanced theory, which is highly mathematical and basically applied quantum mechanics. I agree. But this does not make the theory, as I present it, less of a theory in its own rights. Because, it is taught, used and applied in academical context in this form. As I already mentioned, it is all a certain type of scientist knows about such matters. Moreover, that a theory becomes more advanced, when embedded in (and therefore reduced to) another, more mathematical, theory is the effect we are interested here. Hence, that my theory seems to be an unnatural choice for theoreticians of chemistry is part of the reconstruction, i.e. it is part of an explanation that new distinctions must be drawn.
Another worry with this theory might be, that it is only the theory of a measurement device and, therefore, might seem secondary behind the big world chancing revolutions triggered by quantum mechanics or string theory. Again, I disagree. I found it always quite exiting, that with the data of infrared spectra one can build a molecular theory in classical physics. One just must recognise the spectra as a matter of fact and using discrete version of harmonious oscillation to give a theory of the strength of chemical bounds. We will come back to this point later; it will be my toy-reinterpretation for the application of mathematics. Moreover, coming from the Ramsey-Carnap-Tradition, looking into spectroscopy is not that unnatural. It is one of the closest connections between the unobservable micro-structure of the world and data scientist gather about it. Hence, it is the natural place to look for correspondence rules.

Formalising the Theory of IR-Spectroscopy

I will not give a full formalisation of the theory of IR-Spectroscopy. But I will give the set of terms which can be used to formalise it and then give

some hints how I believe that a formalisation should look like. That should be enough to show what are the benefits of the Ramsey-Carnap-Approach for the application problem of mathematics.
The observational vocabulary α_O includes:

1. Number terms for all natural numbers from 0 to 4000.
2. Function symbols for addition, multiplication and their inverses.
3. One predicate symbol for each known chemical from organic chemistry.[14]
4. A predicate symbol $\mathbb{N}$ for "number".
5. A predicate symbol S for "spectrum".
6. A function symbol of arity two p for "the wave number of the xth peak of y".

As it was elaborated extensively in Section 1.2.3 the occurrence of number terms does not make the observational vocabulary less observational. Hopefully, this example clarifies this even further. There is no epistemological accessibility problem in recognising that the first peak is at 3423 cm^{-1} in the IR-spectrum of ethanol, i.e. verifying that

$$\exists x[S(x) \wedge Ethanol(x) \wedge p(1, x) = 3423]$$

is true is epistemologically unproblematic and, therefore, calling it observational is justified. Also, as my example makes clear, the variables in observational sentences are supposed to refer either to concrete spectra (printed on paper) or numbers (number terms). However, I must clarify what we take the spectra to be. I am not identifying the spectra with the graph; the graph is just beneficial for human cognition. A spectrum is a set of natural numbers between 400 and 4000, i.e. the set of peaks, like the one at the bottom of the diagram.

[14] Chemist will realise that I could do more. Like chemicals made up from silicate. But I want to keep it simple.

I want to make sure that basic arithmetic can be done in the observational language; but I am not sure whether this is necessary. However, our example is particularly nice, because 4001 is prime. Hence the congruent classes $\mathbb{Z}/4001\mathbb{Z}$ are forming a field. Therefore, we can get all of arithmetic up to 4000 without needing a infinite set of numbers. Of course, any observations (see Definition 14) will have also an infinite model that satisfies them, but that is not a problem. What we want is that there is at least one finite model, which we can identify with the actually given data at hand.[15] And this is assured by 4001 being prime even when we add all basic arithmetic for addition and multiplication to the observations.
On the other hand, the theoretical vocabulary α_T includes:

1. A Predicate Symbol A stating “is an atom”.

2. A Predicate Symbol M stating “is a molecule”.

3. Predicate symbols H, C, O, N, P stating that the atom is of the element corresponding to the symbol in the period system of elements.

4. Predicate symbols B^1, B^2, B^3 stating that two atoms are forming a single, double or triple bound.

5. A Predicate symbol of arity two Com stating “x is a compound of y”.

6. Predicate symbols of artiy three $V_1, V_2, V_3, V_4, V_5.V_6$ stating that the three atoms are vibrating according to the six types of vibration.

Hence, in this formalisation molecules and atoms have the same ontological status. Note that I cannot just use the chemical predicates from α_O in the theoretical vocabulary as well. Because this would make sentences with an occurrence of them to correspondence rules. Hence, if one wants to use these names in the theoretical part of a theory, then they must be duplicated. I will not do that. I rather suggest to put the structure of the molecules in question in the antecedence, when referring to a particular chemical. Also note that

[15] I do not feel very strong about keeping it finite, but let me keep it as safe as possible.

the usage of Com forces one to add for any component of a molecule an existential quantifier in such a description of the molecular structure, therefore we have a natural formalisation of "number of atoms in a molecule". Therefore, we do not need number terms in the theoretical vocabulary in order to state chemical formulas of the form $C_xH_yO_z$.
So, I gave a nice formalisation with the particularity that mathematics only occurs in observational sentences. It also enjoys a clear and uncontroversial distinction between epistemological problematic and epistemological unproblematic sentences, when it comes to verification or accessibility. Note that we only need three types of entities:

1. Natural numbers between 0 and 4000.

2. Spectra.[16]

3. Atoms and molecules.

We do not need chemical substances as an ontological basis in addition to their spectra and their molecular structure. I am not concluding that, therefore, the material basis of the measurement technique is reducible. I want to recall here that we just picked out one identifiable junk of theory from a collective of scientific theories in organic chemistry. This theory is not supposed to stand on its own.
As it was described in Section 1.2.4 and defined in Definition 12, an RV-Theory is a set of theoretical sentences and correspondence rules. I also said in Section 2.2, that adding to this a notion of logical consequence makes an RV-Theory to a formal system in the sense of Definition 16. It should not be difficult, just long and unnecessary, to formulate an RV-Theory in order to formalise the theory sketched above and presented in ordinary textbooks (like [36]). Doing this means to formalise the theory of IR-Spectroscopy in the sense of the Ramsey-Carnap-Approach. But since this is an unnecessary long task, I will only sketch such a formalisation. The theoretical sentences should comprise the following information:

[16]Their type token distinction is epistemologically and metaphysically not more problematic than that of any entity able to be printed.

1. They should state that any component of a molecule is an atom, that a molecule with only one component is an atom and that anything that has a component is a molecule.

2. They should state which bonds can appear in a molecule, which types of vibrations these bonds can perform and certain connections between these vibrations.

3. They might also include more advanced parts of the theory, i.e. stating how vibrations change when intermolecular changes happen, including a theory of hydrogen bonds a.s.o.

I think it should be easy to see, that this information can be formulated in the theoretical vocabulary. Because, after all it is very similar to graph theory. Moreover, we see that the theoretical part of the theory is entirely talking about atoms, molecules and their bonds. Hence, there is no reference to mathematical objects.
To formulate the correspondence rules is rather easy. We just have to do the opposite of interpreting a spectrum. What the textbooks give as interpretations devices for spectra must be read the other way around as hypothesis for a possible spectrum. I will give an example to clarify on this. The textbooks say (see [36, p. 51]) that stretching of two hydrogen atoms which share a carbon atom will lead to a peak somewhere between 2950 and 2850 cm^{-1}. Hence, we can formulate a correspondence rule as a sentence:

$$\forall(x_1, x_2, x_3, x_4)[M(x_1)\wedge$$

$$Com(x_1, x_2) \wedge Com(x_1, x_3) \wedge Com(x_1, x_4) \wedge Cx_2 \wedge Hx_3 \wedge Hx_4$$

$$\wedge[V_1(x_2, x_3, x_4) \vee V_2(x_2, x_3, x_4)] \rightarrow$$

$$\exists x_5, x_6, x_7[S(x_5) \wedge \mathbb{N}x_6 \wedge \mathbb{N}x_7 \wedge 2950 \leq x_7 \leq 2850 \wedge p(x_6, x_5) = x_7]]$$

There are certain particularities of this formula which I want to highlight. There is no other possibility than formulating “being a spectrum of” as a function without trivialising the attempt. It would jeopardise the status of

a correspondence rule, if we define a relation “being a spectrum of”. Because it would hand over the role of the correspondence rule to this relation and would leave the former uninformative.[17] It is not very important to mention the molecule and its components separately in the rule; one might just talk about the atoms and leave it to the theory to deconstruct a molecule, when needed. But it will make the mathematisation smoother later. I hope this example makes clear enough how I take the correspondence rule to look like for any possible vibration in a molecule.
The logical forms of the correspondence rules clarify on the logical forms of observations. Because they need to match each other in order to fulfil our notions of empirical adequate and empirical supported.[18] As our example correspondence rule shows: it is all about the existence of spectra and peaks in spectra; hence, they must be of the form: “there is a spectra including a peak at...”. This is in accordance to scientific practice.

Mathematising the Theory of IR-Spectroscopy

The formal system which I sketched in the previous subsection can, as a next step, be ramseyfied. I describe this in Section 1.2.4. For our example correspondence rule this leads to:

$$\exists(X_1, X_2, Y_1, Y_2, Z_1, Z_2)\forall(x_1, x_2, x_3, x_4)[X_1(x_1)\wedge$$

$$X_2(x_1, x_2) \wedge X_2(x_1, x_3) \wedge X_2(x_1, x_4) \wedge Y_1 x_2 \wedge Y_2 x_3 \wedge Y_2 x_4$$

$$\wedge[Z_1(x_2, x_3, x_4) \vee Z_2(x_2, x_3, x_4)] \rightarrow$$

$$\exists x_5, x_6, x_7[S(x_5) \wedge \mathbb{N}x_6 \wedge \mathbb{N}x_7 \wedge 2950 \leq x_7 \leq 2850 \wedge p(x_6, x_5) = x_7]]$$

When we view spectra as types and not tokens, we can build the theory in a way such that the rameyfied theory has a finite model.[19] However, even in this case, where such a finite model is assured, there will be models of any cardinality which satisfy the theory as well. It is usually considered a

[17] Note that this correspondence rule is Π_2.
[18] See Definition 15 for clarification on this notions.
[19] Most radically by restricting the theory to known chemicals.

malfunction of formal systems that they cannot specify their models up to isomorphism. Here it is the main benefit of a formalisation. Since the theory can have models of any cardinality, we can construct a structure based on the real numbers that satisfies the theory but adds more structure. Then we can use this added structure for theoretical purposes. In this stage we have a choice. We can either go with classical physics by using a quantified version of the harmonic oscillator like in [36] or we can add the entire mathematical apparatus contemporary quantum physics has to offer as in [10]. To my view on the application of mathematics, it should be enough to work with the classical version as it is described in [36].

We start by talking about light. Note that, since we neglected wave-lengths (λ) in favour of wave-numbers $\tilde{v} = \frac{1}{\lambda}$, we use a magnitude which is directly proportional to the energy transported by radiation.

$$\Delta E = h \cdot c \cdot \tilde{v}$$

With h taken as the Planck constant and c as the speed of light.[20]
Next we must qualify the strength of a bond between two atoms. We do this by Hooke's law. We denote the strength of the bond by k. This is the value we are interested in and I will sketch a way to characterise it. We view a bond between two atoms like a spring between two masses m_1 and m_2. We denote the Force, which is needed to stretch the bond a distance Δr, by K. Hooke's law now states:

$$K = -k \cdot \Delta r$$

This stretching causes a vibration and this vibration has a potential energy, which is denoted by V. The formula to calculate this potential energy is:

$$V(r) = \frac{1}{2} \cdot k \cdot r^2 = 2 \cdot \pi^2 \cdot \mu \cdot v_{osc}^2 \cdot r^3$$

Here $\mu = \frac{m_1 \cdot m_2}{m_1 + m_2}$ and v_{osc} is the frequency of the oscillator. Absorption of radiation in this model means that the frequency of the oscillator, which is

[20]This follows from $\lambda \cdot v = c$ and $\Delta E = h \cdot v$, with v denoting the frequency.

modelling the bond, is identical with the frequency of the radiation which is going to be absorbed. Physicists tell us that using $\lambda \cdot v_{osc} = c$ leads to the following formula for k. This is the formula I was aiming at.

$$k = \mu \left(\frac{\tilde{v}}{1302} \right)$$

The unit of k is $[N/cm]$. As a consequence of this formula, by identifying the position of the peak in the spectrum, one can calculate the strength of the bond which absorbs at this wave-number. Since this is not accurate in molecules with more than two atoms, we must expect certain limits of variation.

Of course, as a matter of fact only certain peaks occur in the spectrum, which induces a quantisation of the energies being absorbed. Hence, my claim that no quantum mechanics is needed is justified. Well, if one is happy with a good to go theory of infrared spectroscopy and does not press on the accuracy of the peaks' locations to much.

Now I am going to use this to give new references for our example correspondence rule. Note that after Ramseyfication the theory lost its intended references for the theoretical part of its vocabulary. And we are left with the second-order existential statement:

$$\exists(X_1, X_2, Y_1, Y_2, Z_1, Z_2)\forall(x_1, x_2, x_3, x_4)[X_1(x_1)\wedge$$

$$X_2(x_1, x_2) \wedge X_2(x_1, x_3) \wedge X_2(x_1, x_4) \wedge Y_1 x_2 \wedge Y_2 x_3 \wedge Y_2 x_4$$

$$\wedge[Z_1(x_2, x_3, x_4) \vee Z_2(x_2, x_3, x_4)] \rightarrow$$

$$\exists x_5, x_6, x_7[S(x_5) \wedge \mathbb{N}x_6 \wedge \mathbb{N}x_7 \wedge 2950 \leq x_7 \leq 2850 \wedge p(x_6, x_5) = x_7]]$$

Choosing new references is, in my chosen explication, to build a new second-order model-theoretic interpretation that provides witnesses for this existential claim. We choose the reals $\mathbb{R}$ as the universe of our interpretation. Of course, we choose the canonical interpretations for number terms. The interpretation of S is unproblematic. It can stay the same as it was before, i.e. finite sets of natural numbers between 400 and 4000. The same can be done

for the xth-peak-function p. The unary predicate variables Y_1, Y_2, which were substituted for C and H, get the singletons $\{12\}$ and $\{1\}$. Hence, they are stating the same as an identity statement with these numbers. The reason for choosing 12 and 1 is the molecular weight of carbon and hydrogen. Hence being an atom is nothing else then either being the number 1, 12, 14, 16 or 31. I choose to view molecules and atoms on the same ontological basis; therefore, I must use coding techniques. However, before considering this I stipulate that bonds are triples, where the first entrance is a k characterising the bond and the other two entrances are either 1, 12, 14, 16 or 31. We can therefore use the same interpretation for the variables which were substituted for B^1, B^2 and B^3. Note that these occur only indirectly in our example correspondence rule trough V_1 and V_2. We can use the same interpretation for these three relations, because the k already encodes whether a bond is single, double or triple. Molecules can therefore be taken as Gödel-numbers of sets containing such pairs and possible other information. The binary predicate X_2, which was substituted for Com, is based on the graph of a decoding function. My use of codes for molecules is not a problem, because molecules have no special status in the correspondence rules. Interpretations of Z_1 and Z_2 are far more complicated and not easy to give, because they depend heavily on the way how one is coding and decoding molecules as well as on the formulation of the actual theory, which I only sketched above. But I hope that my readers do not press to heavily on this hole in my description.
This finished the process and the theory enjoyed a reference change. But we are not done yet of course. We want to use the new structural possibilities this change gives to us. By adding the formula

$$k = \mu \left(\frac{\tilde{v}}{1302} \right)$$

and new technical axioms, which link the k to the bonds and the μ to the atoms, and possible some similar links between certain ks and types of vibration, we obtain a useful mathematisation, i.e. a mathematical theory of infrared spectroscopy. Whether this mathematisation is accurate depends now on the consistency of the newly extended and mathematised theory.

Because, note that the correspondence rules already include an estimated position of a peak previously to this process. Hence, the accuracy of the mathematisation depends on whether the calculated wavenumbers are in the range which the correspondence rules demand.

3.5 Applying it to the Application Problem

What have I done in the previous section? I used a rather peculiar example, because it had two very important features. First, the observational and theoretical part of the theory were most different to the picture the current Carnap exegesis tries to promote; the example had no mathematical notions in the theoretical part while its observational part was exclusivity given by numbers and numerical functions. This is remarkable, because the example was a natural choice, when studying theoretical entities like atoms and molecules. Fot it is the direct link between the theory about these objects and the data gathered by measurements.

Second, even though the theory is rather modern, i.e. uses entities from modern sciences, it was not at all mathematical. This is somehow the case because it is a chemical theory. But never the less it sits right on the edge between modern theoretical chemistry and experimental methods in the laboratory in the net (so to say) of modern sciences.[21]

This makes it to a good example to show what I was arguing for in Chapter 1. However, it also makes it to a rather straight forward example for what I was trying to show in relation to the application problem. Nevertheless, it shows the basic idea of how I think that mathematics finds its application in the sciences.

Returning to what was said about explication and quantification; I take it as given, that introducing scales and using magnitudes are essential to modern

[21] Arguing that this is, because chemistry had atoms before physics did, is a red herring. Because, historically, infrared spectroscopy was a direct product of modern quantum mechanics and methods from spectroscopy are used in order to falsify theories from theoretical chemistry or quantum chemistry.

scientific theories.[22] I called this quantification before. As was explained in Chapter 1 this part of the scientific language is part of the observational language. It remains the same, after Ramseyfication took place. Since the theoretical vocabulary was substituted by variables it is open to reinterpretation. We reinterpreted it by mathematical objects which are adjacent to the references of the observational language. In a last step, we added additional axiom to the Ramseyfication to let the original theory enjoy the structural properties of these new objects.
In Section 3.2 I identified two questions. I am now answering them by drawing from my example of infrared spectroscopy.

1. How does the mathematician, by turning away from nature, arrive at its most appropriate description?
2. Why does mathematics lead to simpler theories and simpler calculations?

Depending on how the second question is understood, its answer is both: easy to give and impossible to give in this setting. A mathematised theory enjoys stronger structural properties. Hence, argumentation is stronger, i.e. easier to find, in the mathematised theory than in the original theory. Sometimes certain structural links between notions are even just established through the mathematisation. For instance, in our example, we can calculate from the molecule straight away the estimated wavenumber without even going through the correspondence rule, because, since the theory is consistent, we cannot contradict it anyway. However, whether that *makes* calculations, i.e. arguments, easier is also partly also a cognitive question, which we are not able to answer here.
The first question can now be answered by negating its grounds. The mathematician does arrive at nature's most appropriate description. It does arrive at a structure, which is free enough to allow an embedding of a description developed by the experimental sciences. But this is not just isomorphy; choosing a structure is highly interpretative. Recall that infrared spectroscopy

[22] Of course there are interesting stories to tell from humanistic science up to modern days. But none of this concern us here, where we are interested in a logical description.

really is a set of experimental gatherings. No one calculated the wavenumber of these peaks in advance and was happy to find them. Chemists were putting chemicals in the measurement device, found these peaks and where afterwards happy do explain them with a mathematical theory. There are three reason way choosing a mathematical structure to describe this gatherings is neither arbitrary nor passive with an unexpectedly wondrous result. First, those observational terms that are mathematical must be reinterpreted to the same (or to the most natural choice for a) reference as they had before in the original interpretation. Second, the new interpretation must be faithful to the relations the original theory demanded from its notions, because the Ramseyfication is faithful to structural properties. Third, this orientates the chosen structure in an interpretative way. There is not just structure, there is a structure combined with a certain view on it. A view that stipulates on which of its elements it rests. In the Outlines I used the picture of fixing a structure through the observational terms and tying it onto the structure given by the theoretical sentences. Note that the third reason is strongly related to the previously made separation of the vocabulary into an observational and a theoretical part.

3.6 Criticism and Benefits

The most obvious criticism is that I take a shortcut. Some might say that I am identifying atoms with their mass and, therefore, folding a complex physical theory to a mathematical model. Hence, leaving out important aspects between the notion of mass and mathematics. I disagree. First, infrared spectroscopy does not explicitly talk about mass in addition to qualifying an atom as belonging to an element. Therefore, this does not seem important for its mathematisation. If it is missed out, then it is not because I took a shortcut, but because it is reducible. Mass is itself a theoretical term which is connected by a correspondence rule to a scale given by a physical scale; like temperature was before in Section 3.3. One can say, in many contexts (not just in infrared spectroscopy), that "being a carbon atom" and "being a particle of mass 12" is interchangeable, hence one can reduce one notion in

favour of the other. This is what theoretical chemists do as well, when they use mathematical theories. Then after reinterpretation both “mass of” and “carbon atom” becomes “is 12”.
There might be some technical arguments which easily trivialise my construction. It is important to recognise that I only allow *natural* reinterpretations, where “natural” should at least be taken as it is used in mathematics. Otherwise it seems like a boring trick to me. Since mathematicians already only offer structures to physics which are natural in their eyes, we should at least stay above their notion of naturality when reinterpreting theoretical terms.
Suppe famously said that Mathematics cannot be fully separated from the theoretical sciences.

> There is no theoretical way of drawing a sharp distinction between a piece of pure mathematics and a piece of theoretical sciences. [67, p. 33]

Whether this claim is true depends highly on how one understands “theoretical” here. If theoretical means the theoretical part of a theory, then surely, I can. Because I just did that. If it means mathematical physics or theoretical chemistry, then this claim is empty. Because mathematical modelling is trivially not separable from mathematical modelling. One must separate between what the theoretical part of a theory is and what theoreticians of an academic discipline are doing. There is a lot of theory out there which is not at all mathematical, but using mathematics in combination with these theories improves the academic discipline once it is ready to be mathematised. Structural analysis in organic chemistry is one example.
Some might hold that what I call a theory is in fact just a bundle of metaphors and intuitions and that such things do not really count as a theory. What counts is the mathematics. Again, I strongly disagree, and spectroscopy is a good counterexample for this. As I said before, it is the taxonomy of the vibrations which determines the mathematical formulation of the Hamiltonian in an eigenvalue equation for a quantum mechanical description of a molecule. This taxonomy and the structure it presupposes is logically prior to the differential equations the theoretical chemists offer. (See [10]) And it

is also this taxonomy which guides the practical work in the laboratory. (See [36]) The theoretical core of this theory is the stereo-chemistry of a molecule, i.e. the analysis of the angles between the bonds and how they affect the possibility of vibrations.
However, for those still not convinced, where my approach is superior to Suppe's and his followers' is the possibility to separate the mathematics of data analysis from the mathematics of the theoreticians in the theoretical part. The example of infrared spectroscopy does not show that very good, because its data is composed by single numbers which can be directly used for conformation. But in cases where data points must be statistically evaluated, the set of observations of a theory can be extended by a statistical theory. Its derivations can then be compared to the correspondence rules.
Moreover, my separation of the numbers appearing in the correspondence rules and the mathematics of the theoretical part allows a theoretical evaluation of many possible mathematisations according to their adequacy to the gathered data. Something the mere claim, that mathematics and theoretical sciences cannot be sharply distinct, seems not capable of.

3.7 Evolutionary Biology - A Respond to Otsuka

Very recently Jun Otsuka published a book which is closely related to my topic here: *The Role of Mathematics in Evolutionary Biology.* (See [50]) In his book Otsuka asks very similar questions as I do.

> Why can such mathematical theorems tell anything at all about actual and concrete evolutionary processes? In part, this is an echo of an old philosophical conundrum dubbed by Eugene Wigner (1960) as "the unreasonable effectiveness of mathematics in the natural sciences". [50, p. 9]

This book is not only interesting, because Otsuka trys to solve the same problems as I in the narrower context of evolutionary biology. But also, because he directly attacks (something very close to) the received view. He does this as a proponent of Suppes' tradition. Moreover, he conflates mathematisation and formalisation in exactly that manner which I was criticising Suppes for at the beginning of Chapter 3.

Otsuka describes two competing tradition in the philosophy of evolutionary biology: one he calls received view and the other statisticalism. What he calls the received view is of course narrower in scope than the more general approach of Carnap. But claiming that it is a derivative seems justifiable. As Otsuka explains, the view tries to show that “survival of the fittest” is not a tautology, but an empirical statement. The received view's approach is semantical in nature; its proponents argue that “fit” must be understood as a propensity coming from a design analysis of an animal's anatomy.

> Ornithologists conjecture that colorful male song birds attract more females than less-colorful ones, and functional morphologists evaluate various forms of fins in their capacity to produce underwater propulsion. By so doing, they estimate the fitness based on the morphological properties of an organism. Such design analysis, therefore,[...] estimate[s] the propensity fitness.
> [50, p. 19]

This propensity is then coded into numbers which enter probability theory as parameters for more general laws of probability theory. Therefore, they particularise these general laws to empirical statements about a species. Hence, the general laws of probability theory lose their generality by substitution of particulars and, therefore, became empirical statements. They can then be true or false depending to the actual population of this species out in the fields. Otsuka is very justified indeed to call this process a formation of a correspondence rule, as he does in [50, p. 35]. This justifies the name *received view*. However, it is criticised by the so called Statisticalists.

> The claim of Matthen and Ariew (2002) can therefore be understood as stating that the outcome of design analysis and the fit-

> ness parameter are on different scales, and that this fundamental gap disqualifies design analysis [...]. [50, p. 27]

Statisticalists as Matthen and Ariew view evolutionary theory not as a causal theory but as statistically in nature. However, from a logical perspective they only use a different type of parameters in their correspondence rules and give, therefore, fitness a different partial interpretation. So Otsuka is again very justified to put them in the same box as a version of the received view.[23] (See [50, Sec.4]) Then Otsuka is launching his attack on both in three ways. He evokes the Quineian criticism on the analytic-synthetic distinction, he criticises the semantical character of the two approaches and he is quoting Suppes:

> There is no theoretical way of drawing a sharp distinction between a piece of pure mathematics and a piece of theoretical sciences. [67, p. 33]

Otsuka's own approach, which is presented in [50, Sec. 5], is then a description of a process how evolutionary theory becomes gradually more mathematically advanced. In his view, this follows Quine's net of believes, where the core of mathematical statements is also only gradually distinct from those of the particular sciences. Then Otsuka presents his own solution to the tautology problem of "survival of the fittest"; the problem which triggered the solutions of the received view originally. He applies probabilistic graph-theory to his description of evolutionary theory. Then he argues successfully that probabilistic graph-theory is in fact a mathematical theory of causation and not just pure mathematics.

I am not following him in this respect. As my own view on formalisation shows, the analytic-synthetic distinction is not playing a major role here,

[23] Of course metaphysically and epistemologically these positions are very different and lead to different outcomes.

> Hence, if statistical properties of a population are all there is to know, inductive reasoning about evolution becomes a metaphysical impossibility. [50, p. 32]

But methodologically they are in the same tradition, just with different stances. But I do not know whether this is a novelty to statisticalists or just to Otsuka.

because the notion of semantics can be taken very weak when analysing a scientific theory.[24]

I also consider Suppes' quote as misapplied here. In evolutionary theory there is no issue in separating theoretical science and pure mathematics, because all we have is statistics and probability theory. Otsuka seems to believe, that these branches of mathematics are somehow pure mathematics. This is very doubtable, especially in the light of his own argument with probabilistic graph-theory. I cannot see why statistics and probability theory should not be a very advanced theory of counting as probabilistic graph-theory is of causation. Because combinatorics is the branch of mathematics whose proofs are notoriously uninteresting, while the theorems are not. If there is a debate whether formal proofs are of any significance whatsoever, than it takes place in combinatorics. Because in many cases it is just boring induction. There is a theoretical reason for this: pigeon-hole principles are proof-theoretical reducible to schemas of induction. (See [33, p. 42, Theorem 1.41]) Combinatorics takes place in weak arithmetical theories at most. Hence, its techniques are just fancy ways of pairing things with numbers and counting them interestingly. Therefore, there is no need for the kind of mathematics Wigner was puzzled about and of which Maddy studied the development of, i.e. there are no structures. As a result, I already doubt that the application problem of mathematics appears, when evolutionary biology uses laws of probability theory. These are straight forwards applications of the mathematics used to build a data model for a theory. As explained in Section 1.2.5, this is covered by what Carnap called the extended observational language. Their place is not in the theoretical part of a scientific theory. Therefore, what I see at stake here is not the empirical part of evolutionary theory, but the

[24]In fact, Quine says himself that his criticism targets a general distinction which is seen as independent of the particularities of a certain language.

> The notion of analyticity about which we are worrying is a purported relation between statements and languages: a statement S is said to be analytic for a language L, and the problem is to make sense of this relation generally, that is, for variables 'S' and 'L'. [56, p. 33]

However, he grants the triviality that a distinction of analytic and synthetic is always possible in a special case. And the language of mathematised evolutionary biology is for sure a very special case of a language.

theoretical part. Is there a theoretical part of evolutionary theory that is worth a mathematisation? Probably not, which is good for the metaphysical and epistemological grounds of this theory. Don't make up problems!

Summary

I am not summarising the book here, because its overall structure was already briefly explained in the Outline. But I want to summarise what I have done in Chapter 3 in order to solve the Application Problem of Mathematics.

I started with a description of the developments of mathematics since antiquity, which was taken from Maddy [47]. I used that to support two claims: first, that there is no need to separate applied and pure mathematics in this context and, second, that we should take mathematics as providing structures ready to be applied in the sciences.

As a second step I introduced the Application Problem in the same spirit, as the question how these structures, which are studied independently from applications in the sciences, can be nevertheless applied in the sciences with great success.

Then I introduced my notion of mathematisation as a case of reference change and contrasted it to the notion of formalisation and the use of number terms; I called the later quantification.

I continued with studying a special case as an explanatory example. I applied ramseyfication to the theory of infrared spectroscopy to reach the structural content which this theory is presupposing. Then I applied my notion of mathematisation to the Ramsey-sentence of infrared spectroscopy to provide a particular mathematical structure which can act as a new interpretation of the theory. Moreover, this mathematical structure enjoys more structural properties than the original interpretation.

I closed by describing how my solution explains the applicability of mathematics and its success in the sciences. The first was explained through the possibility to do this reference change and the second was explained through the richer structure which was made accessible for theoretical purposes by these means.

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