THE INTENDED INTERPRETATION OF THE INTUITIONISTIC FIRST-ORDER LOGICAL OPERATORS

PhD Dissertation by

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ABSTRACT

The present thesis is an investigation on an open problem in mathematical logic: the problem of devising an explanation of the meaning of the intuitionistic first-order logical operators, which is both mathematically rigorous and faithful to the interpretation intended by the intuitionistic mathematicians who invented and have been using them. This problem has been outstanding since the early thirties, when it was formulated and addressed for the first time.

The thesis includes a historical, expository part, which focuses on the contributions of Kolmogorov, Heyting, Gentzen and Kreisel, and a long and detailed discussion of the various interpretations which have been proposed by these and other authors. Special attention is paid to the decidability of the proof relation and the introduction of Kreisel's extra-clauses, to the various notions of 'canonical proof' and to the attempt to reformulate the semantic definition in terms of proofs from premises.

In this thesis I include a conclusive argument to the effect that if one wants to withdraw the extra-clauses then one

cannot maintain the concept of 'proof' as the basic concept of the definition; instead, I describe an alternative interpretation based on the concept of a construction 'performing' the operations indicated by a given sentence, and I show that it is not equivalent to the verificationist interpretation.

I point out a redundancy in the internal -pseudo-inductive- structure of Kreisel's interpretation and I propose a way to resolve it. Finally, I develop the interpretation in terms of proofs from premises and show that a precise formulation of it must also make use of non-inductive clauses, not for the definition of the conditional but -surprisingly enough- for the definitions of disjunction and of the existential quantifier.

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In May 1995 I gave a talk on the topic of this thesis at the Staff/Graduate Seminar of the Philosophy Departments of LSE and King's College London, where I benefited from the questions and comments of several people in the audience.

Finally, Dr Andrew Powell generously offered me full assistance while he was still a graduate student -albeit one senior to me, and infinitely much wiser. He read a good deal of my written work, and patiently discussed with me many of the topics of the thesis; when I finished the final draft, he reviewed it carefully and made comments on almost every page. His observations, his criticisms and his illuminating expositions have no doubt made this thesis much better than it would have been otherwise. Moreover, from an early stage he also honoured me with a sincere friendship which I have enjoyed ever since.

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CHAPTER 0

INTRODUCTION

§0.1. The topic of this thesis and its significance

The present thesis is an investigation into an open problem in mathematical logic: the problem of devising an explanation of the meaning of the intuitionistic first-order logical operators, which is both mathematically rigorous and faithful to the interpretation intended by the intuitionistic mathematicians who invented and have been using them.

This problem has been outstanding since the early thirties, when it was formulated and addressed for the first time; since then, the amount of literature dealing with it has never stopped growing, but no satisfactory solution has as yet been attained. As we shall see throughout the thesis, the difficulties in solving this problem are largely of a conceptual or philosophical nature.

The explanations of the intuitionistic logical operators could be used to define a genuine semantics for the intuitionistic predicate calculus. At present, none of the models which are being used for its metamathematical study, in spite of being formally adequate, can be properly motivated as *the* intended interpretation of that calculus.

Moreover, a positive solution to this problem would constitute a very big step towards the clarification of the whole intuitionistic project for the foundations of mathematics, and of the notion of 'constructive proof' in particular. In turn, if it could be shown that such an explanation is not possible this would be a strong support for the view of some, that behind intuitionistic mathematics there is not a coherent conception at all.

Finally, the work of Michael Dummett in recent times has connected intuitionistic logic with a number of philosophical issues. In particular, the intuitionistic informal explanations of the logical operators has been pointed out as the prototype of a verificationist theory of meaning for a natural language, which Dummett has been trying to reformulate. Hence the feasibility of such a theory -one of the few theories of meaning around- seems to depend on the possibility of making those explanations more rigorous.

A final remark to make on the topic of the thesis is the following. As is only natural, the various attempts which have been made to define the intended interpretation of the intuitionistic logical constants make ample use of the notion of 'mathematical construction', which is, within constructive mathematics, evidently the most basic notion of all.

From the sixties onwards various 'theories of constructions' have been attempted to provide an explicit characterization of this notion -none of them having yet reached a satisfactory stage. The investigation of those theories lies, however, beyond the scope of this thesis, and I shall only refer to them at some points where the definition of the logical operators would crucially depend on the particular way in which the notion of 'construction' were to be defined.

§0.2. Summary of contents

0.2.1. Chapter 1. This thesis has three chapters, apart from the present, introductory one.

In the first chapter I examine the principal attempts which have been made to resolve the problem of this mainly with an expository purpose. I start with Brouwer, because although he never set out to give an explicit definition of the intuitionistic logical operators, he was the main person who invented them, and the first to use them systematically for doing mathematics. Also, in this first section I outline the basic ideas of the verificationist interpretation, and introduce the interpretation which I shall call 'operational', linking both of them directly to Brouwer's writings.

Then I devote separate sections to the two first authors who, in the early thirties, clearly posed the question of the thesis and tried to give an answer to it -Kolmogorov and Heyting-; and a short section to examine Gentzen's important contribution about the same time.

Finally, the fifth and last section of Chapter 1 is dedicated to the study of Kreisel's approach in the sixties, and in particular to the introduction of what has become known as the 'extra-clauses'.

As I go along I examine some of the more obvious problems of all these proposals -in particular, for instance, the objections raised against the extra-clauses-, although most of the critical discussion is located in the second chapter.

0.2.2. Chapter 2. This chapter opens with a discussion about the decidability of the proof relation (i.e. whether we should consider that, in general, '*c proves A*' is decidable), and its relation to the interpretation of the logical constants.

Then I devote a section to developing the operationalist interpretation and to emphasizing the differences between this and the verificationist interpretation, which have so often been neglected. There follows another section in which I re-examine Kreisel's interpretation; I point out a curious redundancy or imprecision in the internal -pseudo-inductive- structure of the definition, and I propose a way of correcting it.

Afterwards there are two sections on canonical proofs, in which I examine the various proposals that have been made to define such a special notion of proof and to give the definitions of the logical operators in terms of this notion. I study the distinct motivations which lie behind these proposals and the difficulties, in each case, of elaborating a precise definition which satisfies them.

Finally, there is a section on proofs from premises and another on proofs with free variables. In these cases the idea is to define a broader notion than that of 'straight' or 'categorical proof', and to give the interpretation of the logical operators using it.

0.2.3. Chapter 3. The third and final chapter has two sections only. In the first one I discuss the current interpretations of intuitionistic logic and why none of them, despite being so successful from the formal point of view, can be truly said to encapsulate the intended interpretation of the logical operators.

In the second section of this chapter I touch upon the efforts that have been made by some philosophers to show that it is *impossible* to give a precise explanation of the intuitionistic logical constants because there are inherent inconsistencies in the very conception of these constants as distinct from the classical ones. This is a topic which would require a much longer treatment, since intuitionism has been traditionally criticized for the vagueness of its philosophical motivation, and the arguments formulated in this respect, and in particular against the intuitionistic conception of the logical constants, are very numerous; however, I shall restrict myself to the consideration of a few of the most recent ones only, more as a sample than as a comprehensive survey.

0.2.4. The Conclusion. Finally, the last part of the thesis -before the Bibliography- is the Conclusion, in which I make a tentative diagnosis on the present state of the debate over our problem, the intended interpretation of the intuitionistic first-order logical operators.

§0.3. Original content of the thesis

In short, the originality of this thesis lies in the following points:

(a) I argue that if one wants to withdraw Kreisel's extra-clauses then one cannot maintain the concept of 'proof' as the basic concept of the semantic definition of the logical constants (§2.1).

This concerns one of the two most frequent presentations of the verificationist interpretation, favoured for example in textbooks such as Troelstra and van Dalen [1988] (p. 9). In particular, I point out a number of distinct paradoxes which arise from this type of version, and which show quite patently that it is untenable.

On the other hand, it is to be noted that the role that these explanations play in those textbooks is essentially heuristic and not technical, so that there is no fear that an error in the explanations might lead to any other problem.

(b) In relation to this, I describe and discuss an alternative interpretation which does not make use of the extra-clauses, based on the concept of a construction 'performing' the operations indicated by a given sentence (§2.2). I call this interpretation 'operational', I show that the differences between it and the verificationist are not at all trivial, and I illustrate how the operational interpretation is largely supported by Brouwer's and Heyting's ideas -despite the fact that, for example already in Heyting's writings, the difference between the two interpretations is explicitly neglected.

Moreover, this new interpretation would also constitute a prototype for an anti-realist theory of meaning for a natural language, which could perhaps be developed in a parallel way to the verificationist theory, constituting a possible alternative to it.

(c) I point out a curious redundancy or imprecision in the internal -pseudo-inductive- structure of Kreisel's interpretation -i.e. with the extra-clauses (§2.3). In light of this redundancy the current version of Kreisel's interpretation appears to be either incorrect or at least very inelegant; however, the only way to resolve it seems to require that we have previously defined an interpretation akin to the operational one, and this has further consequences for the viability of each interpretation and the relation between them.

(d) The interpretation in terms of proofs from premises and, relatedly, in terms of free variables are subjected to a thorough discussion most of which is totally unprecedented -in fact Dummett is virtually the only author who has been careful to distinguish between these types of interpretations and the other ones. In particular, I show how the notion of 'proof from premises', which prima facie constitutes a solution for the inductive definition of the conditional, after a consistent development turns out to originate a very similar problem in the definitions of disjunction and the existential quantifier -a most surprising result (§2.6).

(e) Finally, the whole structure of the thesis has been conceived in an original and independent way. In comparison with other survey articles on this topic such as van Dalen [1979] or Sundholm [1983], the thesis is not only -obviously- longer, but it attempts to cover the most recent literature; it focuses on careful historical distinctions which have been traditionally ignored -e.g. the difference between Heyting's and Kolmogorov's interpretation (1.3.5, §2.6)-; it contains clear expositions of topics such as canonical proofs -singling out *four* distinct and independent reasons which have been given for introducing them (2.4.1, 2.4.4, 2.5.2 and 2.5.5)-; and finally, it also includes a discussion of some hostile arguments *against* the feasibility of the whole project of formulating a precise semantic explanation (§3.2).

§0.4. The intuitionistic 'ideology' and the study of intuitionism today

> "Here constructivism is to be understood in the wide sense (...). The ending '-ism' has ideological overtones: 'constructive mathematics is the (only) right mathematics'; we hasten, however, to declare that we do not subscribe to this ideology, and that we do not intend to present our material on such a basis." (Anne Sjerp Troelstra and Dirk van Dalen [1988], p. vii).

These words, written at the beginning of one of the major handbooks on the subject, express the declared attitude of the majority of mathematicians and philosophers who work today in the field of constructivism. There are still some strict intuitionists among the great figures (like Dummett), but they are few.

I find myself in agreement with this trend, and do not adhere either to the intuitionistic or to any constructive ideology. In particular, I do not subscribe to the view that intuitionism (or constructivism) is the only legitimate direction in the foundations of mathematics -philosophy of mathematics, set theory or mathematical logic. To use a terminology coined by Kreisel, I subscribe to the *positive* thesis of intuitionism -namely, that intuitionism is a coherent, legitimate and interesting way of doing mathematics- without endorsing the *negative* one -i.e., that intuitionism is *the only* such way, and that classical non-constructive mathematics must be rejected.

In any case, intuitionism remains one of the most serious and promising alternatives in the area of foundations -still a very uncertain field- being the only one among the three main traditional schools at the beginning of the century which has survived mostly in its original form, while not having been shaken by adverse results. In addition to this, its applications to independent areas of philosophy (such as verificationist theories of meaning for natural languages or anti-realist metaphysics and epistemology), to classical mathematics (e.g. in topos theory and numerical mathematics), to physics and to computer science, make intuitionism today a clearly interesting subject.

§0.5. The other main schools of constructive mathematics

0.5.1. The logic of constructive mathematics. Although intuitionistic logic was originally created and developed as a codification of intuitionistic mathematical reasoning only, today it is often considered as representative also of the two other main trends of constructive mathematics -the Russian school of recursive mathematics and Bishop's constructivism. Thus, in general handbooks on constructivism such as Bridges and Richman [1987] or Troelstra and van Dalen [1988] it is at least tacitly assumed that intuitionistic logic is *the* logic of constructive mathematics (cf. p. 11 and p. 35 respectively).

0.5.2. The status of Markov's principle. In fact intuitionistic logic is probably the logic of Bishop's constructivism, but not -it appears to me- that of Russian recursive mathematics. Indeed, my point is that Markov's principle -the main difference between the Russian school and the other two- is expressible in intuitionistic first-order logic, for example by the following sentence (for a unary relation symbol F):

$$\forall x(F(x)\vee\neg F(x))\rightarrow(\neg\neg\exists xF(x)\rightarrow\exists xF(x))$$

(cf. e.g. Dummett [1977], p. 22 or Troelstra and van Dalen [1988], p. 203).

Hence it seems that it is a logical principle, in which case a correct axiomatization of Russian constructive logic should incorporate as axioms all sentences of the language in question which take that form. The resulting logic would be intermediate between intuitionistic and classical logic, and the usual explanations of the intuitionistic logical constants would obviously not be adequate to it.

In any case, once it has been shown that the principle is expressible in a pure first-order logical vocabulary it could seem artificial to continue to regard it as a mathematical and not a logical principle -as has sometimes been defended, e.g. by McCarthy [1994] (p. 105). In particular, it would seem incorrect not to take Markov's principle into account for a precise explanation of the use of the logical operators -and in particular, of the quantifiers- in Russian constructivism.

Of course the principle is not expressible in classical logic, where the decidability of the property corresponding to F does not reduce to the assertion $\forall x(F(x) \vee \neg F(x))$; but that is a completely different matter -surely the logic of Russian recursive mathematics is not classical logic either.

0.5.3. More on Markov's principle. In connection with this point we should notice the following: constructive mathematics has been repeatedly criticized on the grounds that it is not powerful enough for the needs of our most successful physical theories (e.g. Putnam [1975], p. 75). Whether this is strictly true or not, it does not pose a problem for constructivism as such, which relates to pure, rather than applied mathematics.

In particular, an observant intuitionistic mathematician, for example, could perhaps agree on the use of classical principles as part of a physical theory, as long as it is employed to obtain results about the physical reality only. There is no reason in principle, for instance, why he could not use the law of excluded middle when applied to real existing objects even if he cannot determine which of the two options holds; and in doing this he would be effectively treating the law of excluded middle as a physical law, in spite of being expressible in pure logical terms.

The situation, however, is different from that of the use of Markov's principle in Russian recursive mathematics, since in that case the principle in question is held by Markov and his followers as valid in every domain, and not only in a restricted area of phenomena. Hence a distinction between it and other logical principles that they accept would not have any practical consequences at all.

0.5.4. Conclusion. It is important to notice then, that in this thesis we shall be concerned with the intuitionistic logical operators only -that is, the logical operators as used by intuitionistic mathematicians. We can take them to be broadly representative of constructive mathematical reasoning as a whole, but, as we have seen, they do not have to coincide exactly with those used in any school of constructivism other than the intuitionistic.

§0.6. Notational conventions and preliminary notions

0.6.1. First-order languages. First-order languages are assumed to have the four connectives ($\wedge$, $\vee$, $\rightarrow$ and $\neg$) and the two quantifiers ($\exists$ and $\forall$) that are needed for intuitionistic purposes. For simplicity I shall restrict myself to languages without equality (and hence without function symbols other than constants); as is well-known, intuitionistic equality is, in general, a defined non-primitive relation.

Most of the interpretations that we shall study in this thesis require that first-order languages be enriched with a new logical symbol $\bot$, to which they will assign, roughly speaking, a basic absurdity. Syntactically this symbol is to behave exactly as a new atomic sentence.

When dealing with a fixed first-order language $\mathscr{L}$ I shall sometimes write 'variables', 'formulas', etc, meaning -respectively- 'variables of $\mathscr{L}$', 'formulas of $\mathscr{L}$', etc.

0.6.2. Constructive sets. Following Troelstra and van Dalen [1988] I use the term 'set' for what was traditionally called 'species'. Hence a set will be a definite condition determining a certain collection: a condition such that, for any previously accepted object, we know what to count as a proof that the object satisfies the condition -cf. Dummett [1977], p. 38. In other words, a set is given by a decision procedure that can be applied to any arbitrary pair of constructions $\langle c,d \rangle$ to determine whether or not c is a proof that d belongs to it.

Moreover, a set will be *non-empty* (traditionally, 'inhabited') when we know how to produce a particular object which satisfies it; and a set is a *subset* of another set when we can prove that all elements of the first are elements of the second.

Finally, '$c \in U$' is read as 'we can prove that the construction c belongs to the set U', and '$c \notin U$' as 'we can prove that c does not belong to U'. The symbols '$\cap$' and '$\cup$' are used for intersection and union of sets respectively, and '$-$' for the complement of a set in another; '$\times$' for the Cartesian product of two or more sets, and 'U^n' for the Cartesian product of the set U with itself n times.

0.6.3. Constructive interpretations. I now sketch the definition of a constructive interpretation for a first-order language $\mathscr{L}$. This definition would have to be completed in a number of details which I leave here simply outlined. The subject of the thesis is precisely the discussion of those details.

I first consider the case where the domain of the interpretation is a decidable set, and later I explain how to adapt the definition to the case where it is not -a possibility which is also accepted by many intuitionists.

Let $\mathscr{L}$ be a first-order language. Then a *constructive interpretation* $\mathfrak{I}$ for $\mathscr{L}$ consists of the following ingredients.

(a) A set $\mathscr{D}$ called the *domain* of $\mathfrak{I}$, plus a decision procedure whereby we can decide whether or not an arbitrary construction c is a member of $\mathscr{D}$.

(b) A construction that can be applied to any constant t of $\mathscr{L}$ to yield a member $\mathfrak{I}(t)$ of $\mathscr{D}$.

(c) For each m-ary predicate symbol F of $\mathscr{L}$, a subset $\mathfrak{I}(F)$ of $\mathscr{D}^n$. According to the definition of an intuitionistic set this must be given by a decision procedure such that for any m-tuple $\langle b_1,...,b_m\rangle$ of members of $\mathscr{D}$ and construction c, it determines whether or not c is a proof of $\langle b_1,...,b_m\rangle\in\mathfrak{I}(F)$.

Now let $x_1,...,x_n$ be distinct variables of $\mathscr{L}$, and φ a formula whose free variables are among $x_1,...,x_n$. Then, for any n-tuple $\langle a_1,...,a_n\rangle$ of members of $\mathscr{D}$, the semantic definition will associate to φ a *statement* $\mathfrak{I}(\varphi(a_1,...,a_n))$, which, intuitively, will be the statement made by the formula φ under the interpretation $\mathfrak{I}$ when $a_1,...,a_n$ are taken as the values of $x_1,...,x_n$ respectively.

0.6.4. More on constructive interpretations. The basic semantic definition is given then by induction according to the following schema. Let φ be as before, a formula whose free variables are among $x_1,...,x_n$.

If φ is an atomic sentence $Ft_1,...,t_m$ for some m-ary predicate symbol F and terms $t_1,...,t_m$, then we let $\mathfrak{I}(\varphi(a_1,...,a_n))$ be the atomic statement $\langle b_1,...,b_m\rangle\in\mathfrak{I}(F)$, where each b_i (for $1\leq i\leq m$) is either $\mathfrak{I}(t_i)$, if t_i is a constant, or a_j (for $1\leq j\leq n$) if t_i is a variable x_j (in which case, by definition, it has to be among $x_1,...,x_n$).

Then, for the specification of the meaning of $\mathfrak{I}(\varphi(a_1,...,a_n))$ there is little choice, since the only information that we have about $\mathfrak{I}(F)$ is the decision procedure to check whether or not a given construction c is a proof of $(b_1,...,b_m)\in\mathfrak{I}(F)$. Hence in this case the meaning is given by the proof-conditions: the conditions under which c would be a proof of $\mathfrak{I}(\varphi(a_1,...,a_n))$.

Next, the definition should specify the meaning of the statement $\neg\varphi(a_1,...,a_n)$ in terms of the meaning of $\varphi(a_1,...,a_n)$, and the meaning of the statements $(\varphi\wedge\psi)(a_1,...,a_n)$, $(\varphi\vee\psi)(a_1,...,a_n)$, and $(\varphi\rightarrow\psi)(a_1,...,a_n)$ in terms of the meanings of $\varphi(a_1,...,a_n)$ and $\psi(a_1,...,a_n)$. As we shall see in the course of the thesis, here there is a greater choice in the way that these meanings are recursively given.

Finally, the definition should specify as well the meaning of the statements $\forall x\varphi$ and $\exists x\varphi$ in terms of statements of the form

$$\varphi(a_1,...,a_{i-1},a,a_{i+1},...,a_n),$$

where x is the variable x_i (for $1\leq i\leq n$) and a is also a member of $\mathscr{D}$; and here again there is a wide range of options.

The discussion of the thesis is focused, then, in how to complete this description with the appropriate explanations. For example, according to one of them, the meaning of the statement $(\varphi\wedge\psi)(a_1,\ldots,a_n)$ is given by indicating that a proof of this statement is a proof of $\varphi(a_1,\ldots,a_n)$ plus a proof of $\psi(a_1,\ldots,a_n)$.

To make the discussion more lively, however, I shall often adopt a more informal style, in which we discuss directly the interpretation of intuitionistic statements without specifying a formal language and so on. In these cases both the informal notation and the way to apply it to the consideration of $\mathfrak{I}$ will be obvious. For example, a claim that 'the meaning of a statement $A\wedge B$ is given by specifying that a proof of it consists of a proof of A plus a proof of B' will be equivalent to the specification mentioned in the previous paragraph.

I shall not come back to description of $\mathfrak{I}$, simply because it would take too long to go over all the details each time that I discuss a new approach. This adaptation, however, will always be straightforward.

0.6.5. Constructive interpretations with a non-decidable domain. Most authors accept non-decidable domains as intuitionistically meaningful. In these cases the definition of $\mathfrak{I}$ will be modified as follows (cf. e.g. Dummett [1977], p. 24-25, or Troelstra and van Dalen [1988], p. 9).

First, the domain $\mathscr{D}$ is given simply as an intuitionistic set, that is, by means of a procedure to decide, for any arbitrary pair of constructions $\langle c,d \rangle$, whether or not c proves $d \in \mathscr{D}$. However, we do not require a decision procedure to determine whether or not a given arbitrary construction belongs to $\mathscr{D}$; in general, we will not be able to decide this.

Next, the interpretation of each m-ary predicate symbol F will be a decision procedure which acts on any m-tuple $\langle b_1,...,b_m \rangle$ of constructions *given another construction* c which proves that all b_i (for $1 \leq i \leq m$) belong to $\mathscr{D}$. Similarly, if φ is a formula whose free variables are among $x_1,...,x_n$, the semantic definition will associate a statement to each n-tuple of constructions $\langle a_1,...,a_n \rangle$ *given another construction* c which proves that all a_i (for $1 \leq i \leq n$) belong to $\mathscr{D}$.

Finally, the clauses corresponding to the quantifiers will also have to refer to a proof that the critical object a in question belongs to $\mathscr{D}$. For example -using informal terms- if a proof of $\exists x A(x)$ is to be defined in the case of a decidable domain as a construction c plus a proof of $A(c)$, then in the case that $\mathscr{D}$ is not decidable we should require in addition to this a further proof that $c \in \mathscr{D}$.

0.6.6. Conventions regarding quotations. I use double quotation marks for all quotations. I do not change the underlining or italics of the original text unless otherwise stated; however, *I have changed the original notation in most quotations*, to make them fit with one another and with the notation which I use in the thesis, in order to facilitate the reading.

The pages referred to will be those of the edition which is mentioned in the 'Bibliography' in the first place; that edition does not always coincide with the original -earliest- one, but is often the most easily available today.

Finally, when I quote from a paper which has not been translated into English I use my own translation, but I reproduce the original text in footnotes.

CHAPTER 1

THE SEARCH FOR THE INTENDED INTERPRETATION: EXPOSITION

§1.1. Brouwer's use of the logical constants

1.1.1. Introduction. Any search for the intended meaning of the intuitionistic logical operators must in one way or other start with Brouwer, the founder of intuitionism, the main person who invented them and the first who used them systematically for doing mathematics. Naturally, these operators have later been used by other intuitionistic mathematicians apart from Brouwer, but apparently with very few changes.

As is well-known, Brouwer did not have a great interest in mathematical logic, and he never committed himself to giving a rigorous explanation of the logical operators; the actual use that he made of them in his proofs of intuitionistic theorems, plus a number of observations accompanying those proofs, were enough for him.

The task of turning this use into an explicit characterization starts with Kolmogorov and Heyting. In the present section I shall briefly discuss a few of Brouwer's original writings, mainly as a reference for the rest of the chapter -a fairly recent and thorough study of Brouwer's writings and life is van Stigt [1990].

1.1.2. Verificationist interpretations: meaning as provability. As we shall see throughout this chapter, most attempts to define the intended interpretation of the intuitionistic logical operators take as the basic key concept the notion of 'proof'.

The idea is to equate the meaning of a mathematical statement with its provability conditions. That is: to give the interpretation of a statement by means of a definition of what is to be a proof of it.

This seems more adequate for intuitionistic semantics given that, in contrast with the structural (platonistic) point of view, intuitionistic mathematics focuses primarily on the *subject* (the creative mathematician) and his ability to perform certain mathematical operations by applying his previously designed constructions (*knowing how*). Hence a notion such as 'proof', which refers to the successful completion of a human action, appears to be more suitable than that of 'truth'.

On the other hand, classical mathematics focuses essentially on the *object*: eternal pre-existing mathematical structures (*knowing that*); and for this reason, the notion of 'truth', with its prominent descriptive untensed character, is more appropriate. Of course we can also use the predicate 'true' with its intuitionistic sense -e.g. as 'having been proved', as many intuitionistic mathematicians do; but that could be misleading to a classical mathematician if we do not make our intention explicit, especially in the context of a semantic definition.

The idea that intuitionistic meaning should be equated with the proof-conditions is implicit in many of Brouwer's writings, although he never states it directly. The following quotation is a relatively clear illustration. It is taken from the *Cambridge Lectures*, but the same idea is expressed in many others of his papers, sometimes with almost the same words (cf. e.g. [1955], pp. 551-552).

> "Classical logic presupposed that independently of human thought there is a *truth*, part of which is expressible by means of sentences called 'true assertions', mainly assigning certain

properties to certain objects or stating that objects possessing certain properties exist or that certain phenomena behave according to certain laws.

(...)

"Only after mathematics had been recognized as an autonomous interior constructional activity (...) the criterion of truth and falsehood of a mathematical assertion was confined to mathematical activity itself, without appeal to logic or to hypothetical omniscient beings. An immediate consequence was that for a mathematical assertion A the two cases of truth and falsehood, formerly exclusively admitted, were replaced by the following three:

(1) A has been proved to be true;

(2) A has been proved to be absurd;

(3) A has neither been proved to be true nor to be absurd, nor do we know a finite algorithm leading to the statement either that A is true or that A is absurd." ([1981], pp. 90-92).

In a footnote he adds: "the case that A has neither been proved to be true nor to be absurd, but that we know a finite algorithm leading to the statement either that A is true, or that A is absurd, obviously is reducible to the first and second cases" (p. 92).

1.1.3. Verificationist interpretations: meaning as problem-solving. A variant on this view is to consider that every mathematical statement is the statement of a problem, to be solved either positively or negatively. This is the idea exploited in Kolmogorov's interpretation, and is also suggested more or less directly by Brouwer's writings in some places.

For example in [1928] he urges the formalists to accept

> "(...) the identification of the principle of excluded middle with the principle of the solvability of every mathematical problem." (p. 491).

This identification means that the principle of excluded middle holds if and only if each mathematical problem is solvable. In the direction from right to left the implication is obvious: if every mathematical problem were solvable then the principle of excluded middle could not fail to hold. However, the implication in the other direction, that is, from excluded middle to the principle of solvability, seems to entail that the meaning of each statement is the formulation of a mathematical problem. Otherwise we could not understand why the fact that either a statement or its negation holds entails that the respective problem is solvable.

Of course in classical mathematics we can also assign to each statement a corresponding problem -the problem of proving that what the statement affirms is true; but that would only be an oblique interpretation with respect to the primary meaning of the statement -which, e.g. under the platonistic view, would be the statement of a mathematical 'fact'.

1.1.4. The operational interpretation. Finally, in Brouwer's writings we also find support for a more basic explanation of the meaning of mathematical statements, in terms of elementary manipulations with mathematical constructions. I shall call this interpretation 'operational', here and in the rest of the thesis -the term was suggested to me by Professor Machover; it appeared in Prawitz [1973] (p. 231) in reference to the verificationist interpretation, but it has not been used again since then.

The idea of the operational interpretation is that a mathematical statement expresses an expectation that the result of performing a particular construction will satisfy certain properties, or better, that it will *agree* with the constructions corresponding to those properties, if they are also completely effected. This idea is very well-known, and indeed essential to intuitionism and to Brouwer's thinking. Here are a couple of quotes, from Brouwer's PhD Dissertation and from [1923]:

"Often it is quite simple to construct inside such a structure, independently of how it originated, new structures, as the elements of which we take elements of the original structure or systems of these, arranged in a new way, but bearing in mind their original arrangement. The so-called 'properties' of a system express the possibility of constructing such new systems having a certain connection with the given system.

"And it is exactly this *imbedding* of new systems in a given system that plays an important part in building up mathematics (...)." ([1907], p. 52).

"Within the limits of a definite finite main system one can always test, that is prove or reduce to absurdity the properties of the system, i.e. test whether a system can be fitted into another according to prescribed incidence of elements since the fitting-in as determined by the property can in every case be executed in only a finite number of ways, which each in turn can be undertaken and pursued either until it is successfully completed or until it gets stuck." ([1923], p. 235; this translation, however, is from van Stigt [1990], p. 243).

In the case of a numerical equality over natural numbers, for example, this would mean that the two completed constructions fit perfectly well into each other; for example '$12^2=144$' would mean that the result of effecting both sides of the equality comes to the same final construction.

In the case of an atomic statement other than an equality, this would mean that the constructions corresponding to the objects involved satisfy the construction corresponding to the relation in question; for example the meaning of '$2^{11213}-1$ is prime' would be that the result of effecting the operation $2^{11213}-1$ would satisfy positively the constructive procedure to test the property of 'being prime'.

On the other hand, in the case of more complex statements, the expectation would be that some simpler constructions can be connected according to the main logical operator, which itself would be a constructive procedure of some kind.

1.1.5. The verificationist versus the operational interpretation. The operational explanation is fairly close to the provability or verificationist interpretation, and has been assimilated to it by most authors. The point would be that the proof of a statement consists precisely in producing the mathematical construction which the statement demands.

Brouwer himself seems to support this identification when he writes, for example, "the words of your mathematical demonstration merely accompany a mathematical *construction* that is effected without words" ([1907], p. 73).

However, as I shall stress again and again throughout the thesis, this identification is correct only in the cases where it is obvious that the construction which has been produced has the required properties -in particular, for instance, with proofs of atomic statements. In general however, the proof of the statement in question will have to include not only the required construction, but also an argument that it is in effect such a construction; and in some cases this argument may necessarily be very complicated.

This is not the time, however, to carry on this discussion. It is enough to notice that the two interpretations may not coincide, and that we should not equate them beforehand.

1.1.6. The meaning of negation. As Brouwer made clear many times, for him to negate a mathematical statement was to claim the absurdity or impossibility of what the statement says. Apparently, Brouwer was not the first person to conceive negation in this way; it seems that similar definitions had been given before at least by Husserl and Oskar Becker (cf. Heyting [1931], p. 59).

Indeed, Brouwer often writes 'is absurd' and similar phrases instead of 'not' or 'is false'; for example, he formulates the law of excluded middle as the principle that every property is either correct or *impossible* ([1923], p. 335). In [1949] he says: "by non-equivalence we understand absurdity of equivalence, just as by noncontradictority we understand absurdity of contradictority" (p. 95, footnote 2); and sometimes he even writes "false i.e. absurd" (e.g. [1955], p. 552).

In classical mathematics to say that a statement is 'absurd' means that it is obviously false, something which has to do more with the psychological perception of the statement than with the statement itself. 'Absurdity' in this sense, is not a proper technical term of classical mathematics.

In intuitionistic mathematics, on the contrary, 'absurdity' is the most interesting way of expressing negation. The absurdity of a hypothetical construction means that not only is it difficult to effect it -because it requires great ingenuity or hard work- but that it is intrinsically impossible, so that we shall no longer bother to attempt it.

Sometimes the absurdity is plain to see and does not need any demonstration (e.g. '1=2': it is obvious that the two constructions do not 'agree' or fit into each other). Some other times the absurdity is not obvious but we can find a way of reducing the constructions in question to a point where the impossibility becomes clear.

It is for this reason that some later authors define a negation $\neg A$ as the conditional statement $A \to \bot$, where $\bot$ is a fixed absurdity. In fact $\bot$ is often called a 'contradiction', but that must not be interpreted in the classical sense (e.g. as any statement of the form $B \wedge \neg B$), because then it would be obviously vacuous.

Instead, we can take $\bot$ as a basic absurdity -such as '1=2'-, whose only role is to make absolutely evident that the construction which has been reduced to it is impossible: "at the point where you enounce the contradiction, I simply perceive that the construction no longer *goes*, that the required structure cannot be imbedded in the given basic structure." ([1907], p. 73).

Later, Dummett [1977] has suggested that given a decidable atomic statement B we could identify $\bot$ with $B \wedge \neg B$, where the meaning of $\neg B$ would be given directly by the decision procedure attached to it. Then, anyone who understands the decision procedure will recognize that it is impossible for it to give two opposing results, and hence that whichever construction that has been reduced to such a statement is also impossible.

Other authors, in a vaguely similar way to Kripke semantics, have defined a proof of $\neg A$ directly as 'a proof that there cannot be a proof of A' (Bell and Machover [1977], p. 406, and Dummett himself in [1976], p. 110). However, it is difficult to make constructive sense of this idea independently of the reduction to a basic absurdity. For, in general, the impossibility of a complicated construction will not be plain to see, and will have to be shown by means of a reduction of this construction to another, elementary one, whose absurdity is obvious.

On the other hand, the claim is not simply that *as a matter of fact* we shall never be able to perform A -e.g. because A is too complicated-, but that A is intrinsically impossible. However, we cannot admit *a priori* this type of impossibility because that would imply a certain reification over the universe of constructions: constructions are not assumed to exist or not (to be possible or impossible) independently of us -independently of our ability to prove it so.

Hence the only way of making sense of this idea is, again, by means of a reduction of A to a basic impossibility.

1.1.7. Negation and hypothetical constructions. As for hypothetical constructions, there is a passage in Brouwer's PhD Dissertation which has misled some into believing that he rejected them:

> "In one particular case the chain of syllogisms is of a somewhat different kind, which seems to come nearer to the usual logical figures and which actually seems to presuppose the hypothetical judgement from logic. This occurs when a structure is defined by some relation in another structure, while it is not immediately clear how to effect its construction. Here it seems that the construction is *supposed* to be effected, and that starting from this hypothesis a chain of hypothetical judgements is deduced. But this is no more than apparent; what actually happens is the following: one starts by setting up a structure which fulfills part of the required relations, thereupon one tries to deduce from these relations, by means of tautologies, other relations, in such a way that these new relations, combined with those that have not yet been used, yield a system of conditions, suitable as a starting-point for the construction of the required structure. Only by this construction will it be proved that the original conditions can be fulfilled." ([1907], p. 72).

However, he is not condemning the appeal to hypothetical constructions in general, but only the assumption that a mathematical construction can exist without us having first *proved* that it could be effected and how. Moreover, he himself often referred to hypothetical constructions in his proof of negation and conditional statements (e.g. in his proof of the law of triple negation, [1981], p. 12).

Later Freudenthal [1937] and Griss [1946] criticized the use of hypothetical constructions especially in the cases where the supposed construction turns out to be impossible, as happens in a proof of a negation, if the proof is successful. This led Griss to the extreme position of trying to develop intuitionistic mathematics without using negation at all [1946], [1955].

Heyting [1937], [1961], on the contrary, defended the use of hypothetical constructions in mathematical reasoning:

> "The following simple example shows that the problem $A \rightarrow B$ in certain cases can be solved *without* a solution for the problem A being known. For A I take the problem 'find in the sequence of decimals of π a sequence 0123456789', for B the problem 'find in the sequence of decimals of π a sequence 012345678'. Clearly B can be reduced to A by a very simple construction." ([1937], p. 117; the translation is from Troelstra and van Dalen [1988], p. 31).

As we shall see later in the thesis, the interpretations of the intuitionistic logical operators fall into two groups: those which make essential use of the notion of hypothetical construction and those which do not. In the former case the characterization of hypothetical constructions has to be taken seriously, in the sense that, for example, we must indicate recursively for each logical type of statement (conjunctions, disjunctions, etc) what is a proof of that statement which uses a hypothetical proof of a given premise.

In other words, that we should give a general characterization of the notion of 'proof from premises'. As we shall see at the time, the task of making this characterization is not entirely trivial.

Thus, by reducing the negation to an absurdity operator, and more in general, to the construction which shows that absurdity, the intuitionistic mathematician manages to assign a *positive* meaning to each negation statement, in accordance with the constructive philosophy of mathematics. An intuitionistic negation is strictly speaking a positive claim -that which reduces the hypothetical construction to a basic impossibility such as '1=2'-, but it carries with it an implicit denial -the denial that we shall ever be able to perform the construction corresponding to the statement negated. Moreover, this will be obvious to anyone who understands the absurdity of the basic impossibility in question -e.g. the absurdity of '1=2'.

1.1.8. The meaning of the conditional. Brouwer's conception of the conditional in its strongest sense appears in his attempted proof of the bar theorem (e.g. in [1927], pp. 459-462; a neat exposition and discussion is Dummett [1977], pp. 94-104). There, Brouwer considers a conditional statement (bar induction), classifies all possible proofs of the antecedent into three types, and tries to show that each of these proofs can be converted into a proof of the consequent.

This suggests that an intuitionistic proof of a conditional statement $A \rightarrow B$ is a method of transforming every proof of A into a proof of B.

An obvious question, however, is how can we know in advance which form any arbitrary proof of the antecedent should take, so that we ensure that our method will transform all of them into proofs of the consequent. This question turns out to be a deep one.

We must notice that, in particular, as it happens Brouwer's attempted proof of the bar theorem is incorrect, and no way has been found to correct it while preserving its original form (see again Dummett [1977], pp. 101-102).

In practice, most intuitionistic proofs of conditional statements appeal to only one obvious property that every proof of the antecedent must satisfy: *to be a proof of the antecedent* -that is, to have the antecedent as the final line or conclusion of the proof. The method then does not enter to transform the proofs of A internally, but simply *extends* them to obtain proofs of B.

Brouwer himself, in other proofs of conditional statements returns to this simple procedure. For example, in his proof of the law of *triple* negation (e.g. [1981], p. 12), he first assumes $\neg\neg\neg A$ (that is, $\neg\neg A \rightarrow \bot$) and then shows how we can transform that construction into a proof of $\neg A$ (that is, into a proof of $A \rightarrow \bot$), independently of any *actual* proof of the former. Moreover, every notable intuitionistic proof of a conditional statement has proceeded in a similar way as well (Dummett [1977], pp. 15 and 104).

1.1.9. Disjunction, conjunction and $\leftrightarrow$. From his discussion of the law of excluded middle we can see that in order to accept a disjunction Brouwer requires that one of the disjuncts is known to hold -or at least that a decision procedure is known which could be used to determine which one. This is something on which he insists many times. The following quote provides an illustration:

> "Now consider the principium *tertii exclusi*: it claims that every supposition is either true or false; in mathematics this means that for every supposed imbedding of a system into another, satisfying certain given conditions, we can either accomplish such an imbedding by a construction, or we can arrive by a construction at the arrestment of the process which would lead to the imbedding. It follows that the question of the validity of the principium tertii exclusi is equivalent to the question *whether unsolvable mathematical problems can exist*. There is not a shred of a proof for the conviction, which has sometimes been put forward that there exist no unsolvable mathematical problems.
>
> "Insofar as only finite discrete systems are introduced, the investigation whether an imbedding is possible or not, can always be carried out and admits a definite result, so in this case the principium tertii exclusi is reliable as a principle of reasoning." ([1908], pp. 109).

However "in infinite systems the principium tertii exclusi is as yet not reliable" (p. 110).

In this way Brouwer succeeds in attaching a constructive meaning to disjunction statements: to assert a disjunction, the subject must be able to perform the constructions corresponding to one of the disjuncts.

Conjunction shall not detain us long, neither here nor in the rest of the thesis. This is indeed, among the five logical operators, the only one which essentially does not change its meaning, except for the fact that it is now embedded in an intuitionistic language, and the other logical operators to which it relates are different from those of classical mathematics.

For that matter, the biconditional is also defined as in classical logic -e.g. '$A \leftrightarrow B$' is an abbreviation of '$(A \rightarrow B) \wedge (B \rightarrow A)$'. This is not to say that intuitionistically '$A \leftrightarrow B$' *means the same* as in classical logic, because again both '$A \rightarrow B$' and '$B \rightarrow A$' have changed their meaning with respect to classical logic.

Similarly, '$A \wedge B$' does not mean the same, because A and B will have also changed their meaning with respect to their classical counterparts.

1.1.10. The quantifiers. Brouwer's conception of the existential quantifier is probably the most characteristic of all the logical operators. Intuitionistically mathematical objects are not assumed to exist by themselves, but only as a result of a generation or construction process. To prove that a certain entity satisfying a given condition exists, it is not enough, for example, to reduce the hypothesis that it did not exist to a contradiction: we must actually produce one, or at least show how it could be produced: "(...) in intuitionist mathematics a mathematical entity is not necessarily predeterminate" ([1955], p. 552). This means that $\exists$ can no longer be read as 'there is' in the classical sense -i.e. there exists independently of us-, but rather, as 'we can construct'.

The following quote is an illustration:

> "(...) now let us pass to infinite systems and ask for instance if there exists a natural number n such that in the decimal expansion of π the nth, $(n+1)$th, ..., $(n+8)$th, and $(n+9)$th digits form a sequence 0123456789. This question (...) can be answered neither affirmatively nor negatively. But then, from the intuitionist point of view, because outside human thought there *are* no mathematical truths, the assertion that in the decimal expansion of π a sequence 0123456789 either does or does not occur is devoid of sense." ([1981], p. 6).

Finally, as for the universal quantifier, Brouwer's requirement for a proof of a universal statement $\forall x A(x)$ was that a method had been produced to establish $A(c)$ for each element c in the domain. In particular it would not be enough, as before, to derive a contradiction from the hypothesis that an object d such that $\neg A(d)$ exists; that derivation would not be enough in general to prove $A(c)$ of every individual c. Instead, he required an effective method -a construction- for doing exactly this.

§1.2. Kolmogorov's interpretation

1.2.1. An interpretation in terms of mathematical problems. Kolmogorov [1932] made the first attempt to give an explicit and systematic account of all the intuitionistic logical operators.

In this paper Kolmogorov outlines an interpretation which is patently verificationist. He argues that it would be a mistake to try to give an interpretation of intuitionistic logic based on the notion of 'truth'; instead, he proposes the notions of 'problem' and 'solution to a problem':

> "In addition to theoretical logic, which systematizes a proof schemata for theoretical truths, one can systematize a proof schemata for solutions to problems (...).
>
> "(...) In the second section, assuming the basic intuitionistic principles, intuitionistic logic is subjected to a critical study; it is thus shown that it must be replaced by the calculus of problems, since its objects in reality are problems, rather than theoretical propositions." (p. 58*).

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* My translation. "Neben der theoretischen Logik, welche die Beweisschemata der theoretischen Wahrheiten systematisiert, kann man die Schemata der Lösungen von Aufgaben (...) systematisieren.

"(...) Im zweiten Paragraphen wird, unter Anerkennung der allgemeinen intuitionistischen Voraussetzungen, die intuitionistische Logik kritisch untersucht; es wird dabei gezeigt, daß sie durch die Aufgabenrechnung ersetzt werden sollte, denn ihre Objekte sind in Wirklichkeit keine theoretischen Aussagen, sondern vielmehr Auf-

As we shall see immediately, Kolmogorov defines what is to be the solution of a complex problem in terms of solutions of its logical components, depending on what the main logical operator is. By doing this he establishes the general form of the verificationist interpretation: an inductive compositional definition of the notion of 'solution to a problem'; later versions shall use the concept of 'proof' rather than that of 'solutions to problems', but this is only a terminological difference. Also, in so doing, Kolmogorov is giving the first general definition of the concept of 'constructive proof', -although, again, he does not present it under this title.

1.2.2. Kolmogorov and Heyting. Kolmogorov was anticipated in several respects by Heyting [1930] and [1931]. In particular, in those papers Heyting also outlines the essence of the verificationist interpretation and uses it to explain the intuitionistic use of negation and disjunction.

However, Kolmogorov's work was independent. Indeed, at the end of the paper he includes a footnote, added at the proof-reading stage, in which he credits the similarity

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gaben".

between his interpretation and that of Heyting [1931], which had appeared recently: "this interpretation of intuitionistic logic is intimately related to the ideas that Mr Heyting has advanced in the latest volume of *Erkenntnis* (...)" (p. 65, footnote 17*).

Heyting, on the other hand, in [1934] acknowledges the independence -as well as the similarity- of Kolmogorov's interpretation in terms of problems, and adopts it himself to explain the meaning of the intuitionistic logical constants and to give semantic motivation to several logical theorems (pp. 17-23).

1.2.3. The interpretation of the connectives. Kolmogorov's interpretation of the connectives is as follows. Let A and B be mathematical problems, then:

(a) $A \wedge B$ is "the problem of solving both A and B";

(b) $A \vee B$ is "the problem of solving at least one of A and B";

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* "Diese Interpretation der intuitionistischen Logik hängt eng zusammen mit den Ideen, welche Herr Heyting im letzten Bande der 'Erkenntnis' (...)".

(c) $A \to B$ is "the problem of solving B supposing that the solution to A is given"; and finally

(d) $\neg A$ is "the problem of obtaining a contradiction supposing that the solution to A is given" (pp. 59-60*).

With respect to the conditional, Kolmogorov explains: "or, what amounts to the same, 'to carry the solution of B back to the solution of A'" (p. 59**). That is, what he has in mind is a partial solution or solution-schema of B, which would become a full solution if complemented with a solution of A. In other words: a solution of B *with premise* A.

This implies the appeal to a hypothetical proof of A. On the other hand, negation stands exactly in the same situation. In fact, a negation statement $\neg A$ appears as a special kind of conditional statement, $A \to B$, in a case where B is a contradict-

* "Wenn a und b zwei Aufgaben sind, bezeichnet $a \wedge b$ die Aufgabe 'beide Aufgaben a und b lösen', wärhend $a \vee b$ die Aufgabe bezeichnet 'mindestens eine der Aufgaben a und b lösen'. Weiter ist $a \supset b$ die Aufgabe 'vorausgesetzt, daß die Lösung von a gegeben ist, b lösen' (...).

"(...) Dementsprechend bezeichnet $\neg a$ die Aufgabe 'vorausgesetzt, daß die Lösung von a gegeben ist, einen Widerspruch erhalten'".

** "(...) oder, was dasselbe bedeutet, 'die Lösung von b auf die Lösung von a zurückzuführen'".

ion. Kolmogorov does not explain, however, what he understands by 'contradiction' (*Widerspruch*).

1.2.4. The interpretation of the quantifiers. Kolmogorov seems more concerned with the interpretation of intuitionistic propositional logic than with predicate logic -in fact, the 'calculus of problems' he gives contains propositional axioms only (pp. 61-62). However, after explaining the connectives he extends his interpretation in terms of problems to the universal quantifier:

> "Generally speaking, if x is a variable (of the type desired) and $A(x)$ is a problem whose meaning depends on the variable x, $\forall xA(x)$ is the problem 'to indicate a general method for the solution of $A(x)$ for each particular value of x'. This should be understood like this: to solve the problem $\forall xA(x)$ means to be in a position to solve the problem $A(c)$ for each given value c of x, after a series of steps given in advance (before the choice of c)." (p. 60*).

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* "Im allgemeinen bedeutet, wenn x eine Variable (von beliebiger Art) ist und $a(x)$ eine Aufgabe bezeichnet, deren Sinn von dem Werte von x abhängt, $(x)a(x)$ die Aufgabe 'eine allgemeine Methode für die Lösung von $a(x)$ bei jedem einzelnen Wert von x anzugeben'. Mann soll dies so verstehen: Die Aufgabe $(x)a(x)$ zu lösen, bedeutet, imstande sein, für jeden gegebenen Einzelwert x_0 von x die Aufgabe $a(x_0)$ nach einer endlichen Reihe von im voraus (schon vor der Wahl won x_0) bekannten Schritten zu lösen."

This does not need any comment -at least for the time being.

After giving the interpretation of $\forall$, Kolmogorov does not also give the interpretation of the existential quantifier, as we would expect; but elsewhere in the paper he gives ample explanations on the meaning of existential claims in intuitionistic mathematics -and in particular, to the central point concerning them: that the person who makes the claim must be able to indicate a particular instance of it.

In any case it is very easy to apply the preceding definition to the intuitionistic $\exists$, thus -with $A(x)$ as before:

the solution to $\exists x A(x)$ is the indication of a particular object c plus a solution to $A(c)$.

Heyting, for example, in his exposition of Kolmogorov's interpretation, includes basically this definition of $\exists$ as a straightforward extension of the definition ([1934], p. 21).

§1.3. Heyting's interpretation

1.3.1. Introduction. Heyting's interpretation in its standard form does not appear until [1956]. It is in this book that we first find his own systematic explanation of all the intuitionistic logical operators, entirely based on the notions of 'proof' and 'assertability conditions'.

The basics of this definition, however, are already clear in [1930], [1931] and [1934]. In those works Heyting openly defends the verificationist point of view, uses it to define several connectives, and comments positively on the variant of Kolmogorov.

1.3.2. The verificationist point of view. Heyting's defence of verificationism departs from the constructive standpoint:

> "Here is thus an important result of the intuitionistic critique: *the idea of an existence of the mathematical entities outside our mind should not enter into the demonstrations.* I think that even the realists, while continuing to believe in the transcendent existence of mathematical entities, should recognize the importance of knowing in what way mathematics can be built without using this idea.
>
> "For the intuitionists mathematics constitutes a magnificent edifice built by human reason. Perhaps they would do better to avoid entirely the word 'to exist'; if they continue to use it

nevertheless, it could not have, for them, any other sense than this of 'having been built by reason'." ([1930], p. 958*).

Consequently, mathematical statements have to be interpreted in a non-realist way:

"A mathematical proposition expresses a certain expectation. For example, the proposition, 'Euler's constant E is rational', expresses the expectation that we could find two integers n and m such that $E=n/m$. Perhaps the word 'intention', coined by the phenomenologists, expresses even better what is meant here." ([1931], p. 58).

"There is a criterion by which we are able to recognize mathematical assertions as such. Every mathematical assertion can be expressed in the form: 'I have effected the construction A in my mind'." ([1956], pp. 18-19).

These explanations seem to support the operational interpretation. However, Heyting makes it quite clear that,

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* My translation. "Voici donc un résultat important de la critique intuitionniste: *L'idée d'une existence hors de notre esprit des entités mathématiques ne doit pas entrer dans le démostrations.* Je crois que même les réalistes, tout en continuant de croire à l'existence transcendante des entités mathématiques, doivent reconnaître l'importance de la question de savoir comment les mathématiques s'édifient sans l'usage de cette idée.

Pour les intuitionnistes les mathématiques constituent un édifice grandiose construit par la raison humaine. Peut-être feraient-ils mieux d'éviter tout à fait le mot «exister»; s'ils continuent néanmoins à l'employer, il ne saurait avoir pour eux d'autre sens que celui d' «être construit par la raison»".

for him, to effect the construction required by a mathematical statement and to give a proof of it are one and the same thing:

> "The demonstration of a proposition consists in the realization of the construction that it requires." ([1934], p. 17*).
>
> "(...) a mathematical proposition A always demands a mathematical construction with certain given properties; it can be asserted as soon as such a construction has been carried out. We say in this case that the construction *proves* the proposition A and call it a *proof* of A." ([1956], p. 98).
>
> "(...) every mathematical theorem is the expression of a result of a successful construction. The proof of the theorem consists in this construction itself, and the steps of the proof are the same as the steps of the mathematical construction." ([1958], p. 107).

As I have said before this identification is probably incorrect, but I shall not give a detailed argument until later.

The way in which he defines the same connective at different places confirms this identification too; for example: "$A \vee B$ signifies that intention which is fulfilled if and only if at least one of the intentions A and B is fulfilled", and "$A \vee B$

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* I translate this text from the French 1955 expanded edition, which is the only one I could find in London. However, this and the following quotes belonged to the original German 1934 edition -the 1955 additions to the original text are clearly marked in the French version.

"La démonstration d'une proposition consiste dans la réalisation de la construction qu'elle exige."

can be asserted if and only if at least one of the propositions A and B can be asserted" ([1931], p. 59 and [1956], p. 97 respectively).

1.3.3. Heyting and Kolmogorov. On the other hand, as we know Heyting also acknowledges the similarity between the proof interpretation and Kolmogorov's. As I said before, in [1934] he uses consistently Kolmogorov's interpretation to motivate the intuitionistic rejection of various classical logical principles and the acceptance of others. Before doing that he writes:

> "Kolmogorov (...) has proposed a similar conception (...). He interprets this calculus as a calculus of problems. (...) he does not explicate this concept, which we could interpret as the request to effect a mathematical construction which satisfies certain conditions." ([1934], p. 17*).

Indeed, earlier in [1930] he had written:

* "Kolmogoroff (...) a proposé une conception voisine (...). Il interprète ce calcul comme un calcul de problèmes. (...) il n'explicite pas ce concept, qu'on peut interpréter comme la demande d'effectuer une construction mathématique qui satisfasse à certaines conditions."

> "A proposition (...) expresses a problem, or even better a certain expectation (...)". (p. 958*).

Later in [1958] he will insist:

> "The older interpretations by Kolmogorov (as a calculus of problems) and Heyting (as a calculus of intended constructions) were substantially equivalent." (p. 107).

1.3.4. The interpretation of the connectives. In any case, in [1956] Heyting takes the concept of 'provability' (or 'assertability conditions') as the basic notion of the whole definition as so will do most authors afterwards:

> "It will be necessary to fix, as firmly as possible, the meaning of the logical connectives; I do this by giving necessary and sufficient conditions under which a complex expression can be asserted." ([1956], p. 97).

It is in these terms then that he gives the interpretation of the connectives, as follows:

(a) "$A \wedge B$ can be asserted if and only if both A and B can be asserted";

(b) "$A \vee B$ can be asserted if and only if at least one of the propositions A and B can be asserted";

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* "Une proposition (...) exprime un problème, ou mieux encore une certaine attente (...)."

(c) "$\neg A$ can be asserted if and only if we possess a construction which, from the supposition that a construction A were carried out, leads to a contradiction"; and

(d) "$A \rightarrow B$ can be asserted, if and only if we possess a construction c, which, joined to any construction proving A (supposing that the latter be effected), would automatically effect a construction proving B" ([1956], pp. 97-98).

The definitions of $\wedge$ and $\vee$ do not deserve any special comment. In the definition of $\rightarrow$ it is to be noticed that Heyting writes "which, joined to...". That is: he is not considering -at least apparently- the possibility that the construction in question operates internal transformations on the proofs of A. He rather refers to a simple juxtaposition; and his other formulations of the same clause are sometimes less specific, but similar -e.g. "$A \rightarrow B$ then represents the intention of a construction which, from each demonstration of A, leads to a demonstration of B" ([1934], p. 17*).

* "$a \supset b$ représente alors l'intention d'une construction qui, de chaque démonstration pour a, conduit à une démonstration pour b".

1.3.5. The conditional and negation. On the other hand, this clause (d) refers to a construction which *as a matter of fact*, if joined to a construction proving the antecedent (*A*) would effect the consequent (*B*). We could paraphrase it as 'a construction *c* such that, for any construction *d*, if *d* proves *A* then *c*(*d*) proves *B*'. Hence the reference to hypothetical constructions is not essential -we shall see along §2.6 that this makes a non-trivial difference-; and the formulation of [1934] just mentioned also agrees with clause (d) in 1.3.4.

On the other hand, this definition puts no bound to the proofs of *A* referred to -e.g. to their complexity or otherwise. This means that among the proofs considered there might be some which have been built up from *c* itself. In other words: this clause is impredicative -self-reflexive.

In contrast, the definition of ¬ appeals explicitly to a proof of a contradiction from premise *A*, that is: a hypothetical proof of a contradiction which would use as a premise the existence, hypothetical as well, of a construction proving *A*. In addition, other definitions of negation that Heyting gives in different places are also of this form -e.g. "the proposition '*E* is not rational', (...) signifies the expectation that one can derive a contradiction from the assumption that E is rational" ([1931], p. 59).

This means that there is a difference between Heyting's definitions of $\rightarrow$ and $\neg$; a subtle difference but an important one, as I have pointed out before and I shall explain in detail later. The definition of $\neg$ uses the notion of 'proof from premises' -and hence the notion of hypothetical proof- and in this sense is similar to Kolmogorov's. The definition of $\rightarrow$, on the other hand, merely requires a construction by means of which it is possible to produce an actual proof of the consequent provided that we possess a proof of the antecedent.

Finally, Heyting spells out his idea of a contradiction briefly:

> "I think that contradiction must be taken as a primitive notion. It seems very difficult to reduce it to simpler notions, and it is always easy to recognize a contradiction as such. In practically all cases it can be brought into the form 1=2." ([1956], p. 98).

1.3.6. The interpretation of the quantifiers. Heyting's interpretation of the quantifiers is as follows:

(a) "$\vdash \forall x A(x)$ means that $A(x)$ is true for every x in $\mathscr{D}$ [the domain]; in other words, we possess a general method of construction which, if any element c of $\mathscr{D}$ is chosen, yields by specialization the construction $A(c)$"; and

(b) "$\exists x A(x)$ will be true if and only if an element c of $\mathscr{D}$ for which $A(c)$ is true has actually been constructed" ([1956], p. 102).

It is remarkable that in the latter clause Heyting does only require that an instance of $A(x)$ is produced, but not that it is *shown* to be such an instance -in general this will not be evident, and will ask for a separate proof.

§1.4. Gentzen's natural deduction rules

1.4.1. Introduction. When Gentzen presented his natural deduction calculus, and in particular the intuitionistic version, he wrote that "the introductions [the introduction rules] represent, as it were, the 'definitions' of the symbols concerned, and the eliminations are no more, in the final analysis, than the consequences of these definitions" ([1935], p. 80). Indeed, he had intended to create a formal system which came as close as possible to actual mathematical reasoning (p. 74); hence the way in which the rules governing each logical constant were given -and in particular, the introduction rules- would have to be immediately connected with its intuitive meanings.

1.4.2. Gentzen's introduction rules. Gentzen's rules are well-known. According to them:

(a) a proof of $A \wedge B$ is given by a proof of A plus a proof of B;

(b) a proof of $A \vee B$ is given either by a proof of A or by a proof of B;

(c) a proof of $A \rightarrow B$ is a proof of B from premise A;

(d) a proof of $\neg A$ is a proof of $A \rightarrow \bot$, where $\bot$ is any false statement;

(e) a proof of $\forall x A(x)$ is a proof of $A(y)$ for a critical variable y which does not occur in $\forall x A(x)$ or in any non-discharged premise;

(f) a proof of $\exists x A(x)$ is a proof of $A(t)$ for some term t ([1935], pp. 77-79).

1.4.3. Discussion. Gentzen's contribution is important even if his main concern was not that of giving a semantic explanation, because he makes a clear and explicit use of the notion of 'proof from premises' to define both negation and the conditional, and introduces a definition of the universal quantifier somehow connected to it: the definition in terms of 'proofs with free variables', which has later been adopted by a number of authors, and which I shall thoroughly examine later in the thesis.

§1.5. Kreisel's interpretation

1.5.1. Introduction. Kreisel [1962] made the first attempt to make the previous informal explanations of the logical constants fully rigorous. Kreisel acknowledges that Heyting's interpretation is basically sufficient to convey the meaning of the logical constants, but he argues that from the technical point of view it would be desirable to make the definition more precise (p. 199).

Kreisel completes the definition with the specification of an atomic case, introduces a uniform notation for all the clauses, and modifies the definitions of $\neg$, $\rightarrow$ and $\forall$ in a way which is going to be slightly controversial.

Kreisel's interpretation is clearly verificationist:

> "The Intuitionistic Position (General Statement):
>
> "The *sense* of a mathematical assertion denoted by a linguistic object A is intuitionistically determined (or understood) if we have laid down what constructions constitute a *proof* of A." ([1962], p. 201).

Indeed, the project is closely connected with the formulation of an 'abstract theory of constructions', in which the two most basic notions of intuitionistic mathematics -'construction' and 'constructive proof'- would receive a systematic treatment (p.

198); as I said at the beginning I shall not be concerned here with the details of this theory or of its later developments, which are numerous.

1.5.2. Kreisel's definition. For the interpretation of an atomic statement $P(c_1,...,c_2)$ over a universe $\mathscr{D}$ it is enough to indicate a set -a species- of n-tuples of $\mathscr{D}$ plus a series of objects $a_1,...,a_n \in \mathscr{D}$. As I remarked in 0.6.2, intuitionistically a set has to be presented by means of a definite condition of which we know how to recognize a proof that it applies to a given object.

Hence for an atomic statement to be intuitionistically acceptable its proof-conditions have to be laid down in advance, and by doing that we also fix its constructive meaning.

Then Kreisel gives separate clauses for each complex statement depending on which is its main logical operator. He uses a very compact notation -taken from the theory of constructions-, which I shall translate to more informal terms as usual:

(a) c is a proof of $A \wedge B$ when it is a pair $\langle c_1, c_2 \rangle$ such that c_1 is a proof of A and c_2 is a proof of B;

(b) c is a proof $A \vee B$ when it is either a proof of A or of B;

(c) c is a proof of $A \rightarrow B$ when it is a pair $\langle c_1, c_2 \rangle$ such that c_1 proves that for any construction d, if d proves A, then $c_2(d)$ proves B;

(d) c is a proof of $\neg A$ when it is a pair $\langle c_1, c_2 \rangle$ such that c_1 proves that for any construction d, if d proves A, then $c_2(d)$ proves $1=0$;

(e) c is a proof of $\exists x A(x)$ when it is a pair $\langle c_1, c_2 \rangle$ such that c_1 proves $A(c_2)$;

(f) c is a proof of $\forall x A(x)$ when it is a pair $\langle c_1, c_2 \rangle$ such that c_1 proves that for any construction d, $c_2(d)$ proves $A(d)$ ([1962], p. 205).

1.5.3. Discussion. The constructions in the definition can be objects (functions of zero arguments), or genuine functions, which operate on other objects and functions. No type distinction is made explicit between them (p. 202). The application of one construction to another is understood in the usual way except that in the cases where it does not make sense it is given an artificial value so as to ensure that it is always defined: in those cases $c(d)$ is taken to be c itself.

In the case of the construction c_2 of clause (c), its role is to transform *any possible* construction which is a proof of A into a proof of B. Hence the clause involves a quantification over all constructions and, in particular, over all possible proofs of A, so it is impredicative as happened with Heyting's clause; and exactly the same is true here for clause (d).

In this case, however, internal transformations of the proofs of A are allowed.

1.5.4. The decidability of the proof relation. In contrast with Heyting's interpretation, the constructions corresponding to $\rightarrow$, $\neg$ and $\forall$ are here *pairs* of constructions. In the case of a proof of $A \rightarrow B$, for example, c_2 plays the role of transforming all proofs of A into proofs of B; but in general it will not be evident whether or not it does this, and so c_1 is to provide an argument which proves that c_2 indeed works as required.

The idea of these 'extra-clauses' was first suggested by Kreisel in [1961], footnote 4, p. 107. Later he shall call the corresponding construction c_1 in each clause a 'judgement proof', since its purpose is to 'judge' that the other construction works as expected ([1971], p. 129 and note 11, p. 146). In turn, we could call the second construction the 'working proof', since it is this that performs the essential task of the proof in question.

As a result the proof-relation induced by the definition is a decidable relation: if it is not obvious that c_2 works as required then c_1 will prove it so. The decidability of the proof-relation is important for Kreisel:

> "(...) we are adopting the basic intuitionistic idealization that we can recognize a proof when we see one, and so r_A [the proof-predicate for a statement A] is decidable." ([1962], p. 202).

Later he shall call this assumption a 'fundamental principle' ([1965], p. 124), and writes:

> "This principle is embodied in the usual formal systems where, for any particular (representation of a proof by a) sequence of symbols, it can be decided whether it proves (the assertion expressed by) any given formula. In addition, formal systems require the decision (i) to be *mechanical*, and (ii) for arbitrary formulae (not only universal ones)." (p. 124).

Also Dummett [1977], for example, adheres to this idea:

> "The explanation of each constant must be faithful to the principle that, for any construction that is presented to us, we shall always be able to recognize effectively whether or not it is a proof of any given statement." (p. 12)

Finally, it is to be noticed that the clause for $\vee$ has been later modified by several authors (starting with Kreisel himself in [1965], p. 129), as to include an indication of which disjunct is the one being proved; for example, by requiring that a proof of $A \vee B$ be a pair $\langle c_1, c_2 \rangle$ such that either c_1 proves A and then $c_2=0$, or c_1 proves B and then $c_2=1$.

This modification, however, is unnecessary if, as it happens here, the proof relation is ensured to be decidable with respect to the other clauses. In particular, if c_1 proves A or c_1 proves B then we already know how to check which one is the case, and we do not need an indicator of it. This point is made by Dummett [1977] (p. 320) -Hellman [1984] also mentions a lecture by Scott Weinstein in 1977 in which he makes this observation too.

1.5.5. The debate on the extra-clauses. The addition of the extra-clauses has the effect of destroying the inductive structure of the definition. Indeed, if the definition is to be inductive, then, as I explained at the beginning, the ascription of meaning to a compound statement would have to be given in terms of the ascription of meaning to statements of smaller complexity.

In this case, the definition of proof of a complex statement would have to be done in terms of proofs of statements logically simpler than it; but this requirement is broken by Kreisel's extra-clause, since it appeals to a proof of a very general fact: that for any construction *d*, $c_2(d)$ proves the corresponding statement. Naturally this does not fall under the scope of the inductive definition.

During the years following Kreisel's introduction of the extra-clauses they were naturally adopted by most authors, as a plain improvement over the preceding interpretations. Examples are -apart from Kreisel himself- Troelstra ([1969], [1977]), Nicolas Goodman ([1970]), van Dalen ([1973], [1979]), Dummett ([1977], p. 399) and Bell and Machover ([1977], pp. 406-407).

Troelstra [1977] and [1981] introduced the abbreviation 'BHK' for 'Brouwer-Heyting-Kreisel' to denote the intended explanations of the logical constants.

However, others remained reluctant to include the extra-clauses in the definition. Scott [1970] argues that we should distinguish between *constructions* and *proofs*:

> "We have *no* abstract proofs only constructions and species of constructions. When the author finally obtained his formalism the proofs-as-objects vanished." (p. 241)
>
> "Assuming for simplicity that no hypothesis of declarations are required, what must be done in order to establish $A \rightarrow B$? One must produce a *construction* together with a *proof* that this construction transforms *every* construction that could establish A into a construction for B.
>
> "The construction is an object *of* the theory [the theory of constructions] while the proof is an elementary argument *about* the theory. Kreisel calls such proofs 'judgements' and asks for an abstract theory of them. We have not provided this because we did not see why such a theory was needed." (pp. 261-262).

Then, to convey the meaning, for instance, of the conditional, one would need to refer to constructions only. However, in a note at the end of the paper he declares having been convinced by Kreisel and Gödel in conversation of the need for decidability and abstract proofs, and almost withdraws the theory (Postscript, p. 272).

Prawitz [1977] considered that the addition of Kreisel's extra-clauses is untenable:

> "In the cases when A is an implication or a universal sentence (...) we must require not only a construction or a description of an

> appropriate procedure but also an understanding of this procedure. The knowledge required in this case is thus of a considerably more involved character. One may ask whether this knowledge should not consist of a description of the procedure together with a proof that this procedure has the property required, as suggested originally by Kreisel. But this would lead to an infinite regress and would defeat the whole project of a theory of meaning as discussed here." (p. 27).

Prawitz is wrong -I think- that Kreisel's extra-clauses lead to an infinite regress, because the judgement construction, being a proof, must include everything that is needed to ensure that the other construction works as required; it cannot be that a second judgement proof to 'judge' c_1 is needed, because if c_1 is not enough to show that c_2 works as required then this will mean that c_1 is not adequate as a judgement proof.

However, he is probably right about the impossibility of basing a theory of meaning on a non-inductive definition.

1.5.6. More on the debate. Later other papers such as Sundholm [1983] and Weinstein [1983] appeared exploring the costs of adopting the extra-clauses. For example Weinstein writes:

> "It is evident that the new clauses [Kreisel's extra-clauses] for the conditional and universal quantifier taken together with the

old clauses for the atomic formulas and the remaining connectives can no longer be viewed as an inductive definition (...).

"This means of securing the decidability of the proof conditions for formulas of arithmetic is not without cost." (p. 264).

Sundholm [1983] (p. 161) quotes one of Heyting's later publications, [1974], p. 87, and notices that Heyting did not adopt the extra-clauses in his own explanations of $\rightarrow$ and $\forall$. To this we could add that Heyting maintained the original formulations in the further revised editions of [1956], in 1966 and 1971. More particularly, Sundholm says that Heyting mentioned the extra-clauses in [1968] (p. 318), but only in the process of a survey of recent work within intuitionism in which he describes Kreisel's contributions of [1962] and [1965]. Finally, Sundholm also refers to the fact that Troelstra, in conversation, stressed to him that it would not be fair to assume that Heyting was against the introduction of the extra-clauses from the fact that they never appeared in his work (footnote 13, p. 169).

Later we shall see more about Sundholm and this debate, but the thing is that as a result of it, some of the authors who had happily welcomed the extra-clauses became sceptical about them. Van Dalen [1983] (p. 166), [1986] (p. 231), gives definitions without them, and writes:

> "It must be pointed out however that the decidability of the proof-relations has been criticized and the 'extra clauses' are not universally accepted." ([1986], p. 232).

In Troelstra and van Dalen [1988], they give an explanation of the logical constants without the extra-clauses (p. 9), and write: "Kreisel proposed this version [the one with judgement proofs] in the hope of obtaining interesting new models for intuitionistic systems, but this hope was not fulfilled" (p. 32). Ironically, here they again use the abbreviation 'BHK' but this time standing for 'Brouwer-Heyting-Kolmogorov'; later, other authors continued to use this abbreviation with the first sense, e.g. Ruitenburg [1991] (p. 156) or Hellman [1989] (p. 50) -although it is not clear that the latter is well informed about Kreisel's extra-clauses since in his discussion of the decidability of the proof relation on pp. 57-59 he ignores them completely.

1.5.7. The naive verificationist interpretation. The interpretation which I shall call here 'the naive interpretation' is what results from Kreisel's interpretation after we remove the extra-clauses. With minor changes, it can be found in van Dalen [1983] (p. 166), [1986] (p. 231) and Troelstra and van Dalen [1988] (p. 9), although there might be earlier versions. It does not coincide, however, with Kolmogorov's, Heyting's or Gentzen's.

It goes as follows:

(a) a proof of $A \wedge B$ is a proof of A plus a proof of B;

(b) a proof of $A \vee B$ is either a proof of A or a proof of B;

(c) a proof of $A \rightarrow B$ is a construction which transforms every proof of A into a proof of B;

(d) a proof of $\neg A$ is a construction which transforms every proof of A into a proof of some absurd statement $\bot$;

(e) a proof of $\exists x A(x)$ is a construction c plus a proof of $A(c)$;

(f) a proof of $\forall x A(x)$ is a construction which transforms every construction c in the domain into a proof of $A(c)$.

This definition does not coincide with Heyting's for several reasons: the definition of $\rightarrow$ appeals to *transformations* of the proofs of A in general, and not simply to *juxtaposition* of other constructions to them as Heyting did. Hence the full power of Brouwer's conditional is here recaptured. Moreover, the definition of $\neg$ is different, since, as we saw, Heyting appeals to proofs from premises. Finally, the definition of ∃ is also different, since Heyting only required a construction *c* which satisfies the condition -that is, a construction *c* such that *A*(*c*) holds- but not a proof that *c* is such a construction.

The difference with Kolmogorov's and Gentzen's interpretation is even more obvious, because the appeal to proofs from premises here is totally absent -as we shall see in §2.6 this makes a non-trivial difference.

This interpretation is untenable unless we replace its central concept -'proof'- by a different one, and that is why I have called it 'naive'; but this is something the discussion of which I shall postpone for the next chapter. In any case, this could not constitute a problem for the works in which the definitions appears, since in those works it does not play a technical role proper, but stands mainly as a heuristic guide.

1.5.8. Other considerations. Another consequence of the decidability of the proof relation achieved with the addition of the extra-clauses is that the meta-connectives that are *used* in the definition can be taken to be the classical ones -that is, the truth-functions-, since in the context of decidable statements the intuitionistic and classical propositional connectives behave exactly in the same way.

This is very important for Kreisel:

> "If the logical operations, in terms of which the usual assertions are built up, are not primitive but explained, then the *basic* proofs must be proofs of special assertions in which the (problematic) logical operations are not involved." ([1965], p. 123).

The same point is also made by Nicolas Goodman:

> "If the definition is not to be circular, then the 'if ..., then' in the definition must be essentially simpler than the intuitionistic implication being defined. This is achieved by requiring that the proof predicates (...) be decidable, so that, even from an intuitionistic point of view, we can make unproblematic use of the truth-functional connectives." ([1970], p. 105).

However, this point loses its force if we consider that a similar reduction is *not possible* in the case of the quantifiers -whether we take the proof relation to be decidable or not, the meta-quantifiers used in the clauses must be the intuitionistic ones.

The fact that the semantic definition of the intuitionistic logical operators has to use these very operators in the metalanguage need not be more worrisome here than in the classical case, where exactly the same thing occurs. As Prawitz writes:

> "(...) in one sense we already know what is to count as a proof and what it is to grasp the meaning of an expression. That is, in practice, we are able to tell whether something is a proof and whether somebody has grasped the meaning of a given expression. What semantics and logic have to do is to explain this practice by giving a systematic account of it, and by doing this, our implicit knowledge may be improved and become explicit to some extent (although it seems that the explanation will usually still have to presuppose some implicit knowledge of the same kind)." ([1979], pp. 26-27).

This systematic account consists precisely in giving the interpretation of each logical constant by specifying what the meaning of each compound statement in terms of the meaning of its constituents is.

Finally, another point which deserves to be mentioned is that, as Goodman [1970] pointed out, if the proof relation is assumed to be decidable then the impredicativity of $\rightarrow$ can lead to a paradox in the theory of constructions.

The paradox arises from the production of a sentence which basically asserts of itself that it is unprovable; then, using the decidability of the proof relation it is possible to construct a proof of that sentence, leading to a paradox. Goodman acknowledges in his paper (p. 109) that the paradox had also been derived independently by Kreisel, and so it has been called the ‘Kreisel-Goodman paradox’ (e.g. in Weinstein [1983], p. 264). I shall not analyze this paradox here, as it does not have any direct relation to our problem. However, it is one of the motivations for the stratification of the universe of constructions into levels, that we shall see later.

CHAPTER 2

THE SEARCH FOR THE INTENDED INTERPRETATION: DISCUSSION

§2.1. The decidability of the proof relation

2.1.1. A fundamental dichotomy in intuitionistic mathematics. At the heart of intuitionistic mathematics, there lies a fundamental dichotomy: the difference between having a mathematical construction which performs a certain task and *knowing* that it does perform such a task. Very often it is obvious that our construction does the work required, but not always. Sometimes it is difficult to verify, and sometimes -perhaps- even impossible.

This dichotomy leads to two different ways of interpreting intuitionistic logic and mathematics depending on which of the two following assumptions is adopted:

(a) the meaning of a statement is simply that we can perform a certain construction; or

(b) the meaning of a statement is that we can perform a certain construction *and* prove that it has the desired properties -i.e. that it works as required.

It is easy to see that they lead, respectively, to the operational and the verificationist interpretation of the logical constants, the latter *with* Kreisel's judgement proofs, of course.

The second interpretation is obviously 'more constructive' than the first, although they seem to induce exactly the same system of predicate logic -Heyting's- and hence also the same relation of logical consequence. This is not too surprising if we consider that if one construction implies another -or can be easily transformed into another- then a proof that it works as required will normally imply that the other construction also works as required -that is, the first proof should be easy to transform into the second.

I shall now give a few examples which illustrate to what extent it might not be obvious that a given construction works as expected.

2.1.2. A trivial case. A case where the verification is trivial is the following.

Theorem (Euclid). *There are infinitely many prime numbers.*

Proof. Constructively, we must produce a construction which, when applied to each $n \in N$ yields a number p which is prime and bigger than n.

Construction. Let n be any natural number. Calculate $n!+1$ and take the smallest divisor p of that number which is greater than 1 (if there is no other, $n!+1$ itself).

Verification that our construction works as required. Obvious: p must be prime since otherwise it would have smaller divisors which would also divide $n!+1$; and it must be bigger than n, since otherwise $n!/p$ would be a whole number and hence $1/p$ would have to be a whole number too.

2.1.3. A less trivial case. A case where the verification is less trivial results after a small specialization on the preceding result.

Theorem (Dirichlet). *There are infinitely many prime numbers of the form* $4m-1$ *for some* $m \in N$.

For $m>0$, $4m-1 = 4(m-1)+3$. Hence it follows at once that there are infinitely many primes of the form $4m+3$. A special case of Dirichlet's theorem.

Proof. To establish this classically it would be enough to assume that the prime numbers of the form $4m-1$ are finite and then derive a contradiction (e.g. Long [1987], p. 73). Constructively we must, however -as before-, produce a construction which transforms every natural number n into another p, which is bigger and of the required form.

Construction. Let n be any natural number. Calculate $4n!-1$ and then take the smallest divisor of that number which is of the form $4m-1$ (if there is no other, $4n!-1$ itself). Let p be the resulting number.

Verification that our construction works as required. p is obviously of the form $4m-1$. Also, since p divides $4n!-1$ exactly it is plain that $p>n$; otherwise $4n!/p$ would be a whole number and $1/p$ would have to be whole too; but p is of the form $4m-1$, so it cannot be 1.

To see that p is prime, suppose that it were composite and let $p_1, ..., p_k$ be its prime factors. Then we reason as follows.

Every natural number is uniquely of one the forms:

$4m$,

$4m+1$,

$4m+2$, or

$4m+3$

(m being the quotient and 0, 1, 2 and 3 respectively, the remainder of the division of that number by 4).

Furthermore, $4m+3 = 4(m+1)-1$. So each p_i (for $1 \leq i \leq k$), being smaller than p, must be of one of the first three forms.

However, the product of two numbers of these forms never gives a number of the form $4m-1$. This is a matter of routine checking:

$$
\begin{aligned}
4m \cdot 4m' &= 4(4mm') \\
4m \cdot (4m'+1) &= 4(4mm'+m) \\
4m \cdot (4m'+2) &= 4(4mm'+2m) \\
(4m+1) \cdot (4m'+1) &= 4(4mm'+m+m')+1 \\
(4m+1) \cdot (4m'+2) &= 4(4mm'+m+2m')+2 \\
(4m+2) \cdot (4m'+2) &= 4(4mm'+2m+2m'+1).
\end{aligned}
$$

Hence p cannot be obtained by a product of $p_1, \ldots, p_k$ (for $k>1$) and it must itself be prime.

2.1.4. A difficult case. A case where the verification is genuinely difficult -perhaps even impossible- is this.

Conjecture (Goldbach). *Every even number is the sum of two primes.*

Construction. Obvious. Let n be any even number. Consider every prime number m for $1 \leq m \leq n/2$. Then let p be the first of these numbers such that $n-p$ is also prime, or, if there are not any put $p=n$, for definiteness.

Verification that our construction works as required. (That is: verification that we never have $p=n$, and hence for every even n, both p and $n-p$ are prime). Not known; if it is possible it must be very difficult.

2.1.5. Discussion. According to the operational interpretation the constructive content of Goldbach's conjecture (G) could have been realised already in the construction that I have just given, assuming that the conjecture is correct.

We cannot *assert* G because we do not have a proof of it, and we cannot assert $\neg G$ either because we do not have a refutation -hence we can neither assert $G \vee \neg G$. However, under this interpretation we might already possess the construction indicated by G -the construction *meant* by this statement according to the operational interpretation.

The same situation does not apply to all meaningful statements by any means. If we take the twin prime conjecture, for example -the conjecture that *there are infinitely many twin prime numbers*-, we simply do not have any candidate for a construction which determines, for any natural number n, a pair of twin primes bigger than n.

We can search systematically for one -given a particular n-, but this would not be a well-defined intuitionistic construction since there is no guarantee that this operation will always terminate. This type of procedure would be acceptable in the Russian school of recursive mathematics (because the relation of 'being twin primes' is decidable), but that is a different matter -e.g. the indirect classical proofs of 2.1.2 and 2.1.3 would also be acceptable within this school.

Similarly, we do not have a construction for finding 7 consecutive occurrences of a natural number n in the decimal expansion of π, or for finding a perfect number bigger than a given number n.

2.1.6. The decidability of the proof relation. We saw at the time that Heyting did not distinguish essentially between the verificationist interpretation and the kind of interpretation which I have called 'operational'. This was so because he was making the assumption that the proof of a statement and the realization of the construction that the statement demands are one and the same thing.

However, we can now conclude that this is wrong. Sometimes it is not at all obvious that the construction described works in the way it is required to; and in these cases it would be very misleading to call that construction alone a 'proof' of the statement in question.

As Professor Machover once stressed to me, a proof is a *convincing argument*; if it does not convince us then it is not a proof. We might need some time to understand all the concepts that appear in the proof, and its internal structure, or to work out the trivial details that have not been made explicit by the author, but, assuming we are able to do all this, once we have done it, if it still does not convince us then it is not a proof.

Tymozcko [1979] (p. 59) lists as the first of three essential requirements on proofs that they must be *convincing.* We could quote other sources (Wittgenstein [1956] (p. 171), Goad [1980] (p. 39)), but as Sundholm [1993] points out (pp. 48-49 and 53), it is enough to look the word up in a dictionary.

In relation to this Nicolas Goodman wrote:

> "(...) we often think we have a proof of an assertion when, as a matter of fact, the argument we have in mind is still confused. We [Goodman] take this fact as evidence not of the undecidability of the proof predicate, but rather of a lack of clarity in the way the putative proof is presented. (...) Thus we assume that a clearly given construction always either is or is not a proof of a given assertion" ([1970], p. 107).

2.1.7. An interesting metaphor by Sundholm. Sundholm [1986] has advanced an interesting metaphor on this matter. He compares the understanding of a proof with the understanding of a sentence of the language, and writes:

> "One can compare the situation with understanding a meaningful sentence: we understand a meaningful sentence when we see (or hear!) one but if we don't understand that does not necessarily mean that there is nothing there to be understood. Failure to understand a meaningful sentence seems parallel to failure to follow, or grasp, a proof." (p. 493).

This metaphor is a good one to illustrate our own position (indeed, it would have to be if *meaning of sentences* is to be explained in terms of *proofs*). Let us develop it a little. There are various ways in which we can fail to understand a sentence. One is when we do not know the meaning of some words, or the grammatical construction is new to us. That is: when we still do not have a full proficiency of the language in question, and the given sentence lies beyond our knowledge.

This situation can be compared with the case when we fail to understand a proof because we are not familiar with the notation used, or with the terminology, or with the corpus of basic facts that the author of the proof takes for granted (prerequisites). This case is not problematic. All we have to do is to study the language further, and in the case of a proof, to study further the background knowledge.

Another case would be when the sentence is so cumbersome that it exceeds our human capacity; for example, if it is very long (think of the sentence made up by putting together by using conjunctions, the sequence of all sentences which appear in Shakespeare's works; or a sentence with an alternate chain of 2.000 universal and existential quantifiers).

In these cases we are faced with memory and processing limitations.

This can also happen in a proof: the typical example would be the celebrated proof of the 4-colour theorem. However, I do not think that these cases pose a serious problem for the decidability of the proof relation: the point is that if the proof is given with all its details (with everything made explicit), then it would have to be possible for any person of standard intelligence to check *any single step* of the proof even if he could never check *all of them*, and therefore he could never grasp the proof as a whole.

2.1.8. More on the decidability. In any case, if the problem of understanding a proof is that there is a missing step in it which requires true ingenuity for it to be bridged, then we would say that the proof is incomplete and hence that properly speaking it was not a proof.

For example, we would not say that the construction for decomposing an even number into a sum of primes that I gave in 2.1.4 is a *proof* of Goldbach's conjecture, because it does not prove it at all. Similarly, we should not say that the construction I gave for finding infinitely many primes of the form $4m-1$ (in 2.1.3) is a *proof*, because it is only a *part* of the proof, and it alone would not convince us that there are infinitely many such primes -unless we have figured out already the argument which comes later.

Only in the most trivial cases -such as 2.1.2- can we identify the basic construction in question and the proof of the corresponding statement; what happens in these cases is that the mere possession of the construction allows us to assert the corresponding statement.

We can now see that the dispute over the decidability of the proof relation was partly based on a misconception. The "basic intuitionistic idealization that we can recognize a proof when we see one" (Kreisel [1962], p. 202) could not be in doubt: any precise definition of the concept of 'proof' -the intuitionistic or any other- should render it a decidable relation.

The real point was that if someone wants to avoid the appeal to Kreisel's extra-clauses then he would have to avoid the concept of 'proof' as well, and base the definition of the logical constants on an entirely different concept. A concept which intuitively would correspond to a non-decidable relation and whose definition, thus, would not require judgement clauses of any kind.

This shows that the naive interpretation of 1.5.7 is as it stands untenable.

2.1.9. Sundholm on proofs and constructions. Sundholm [1983] contains an interesting point which is that we should distinguish between *constructions* and *proofs* because the two notions are conceptually very far apart from each other. He gives the example of a proof of $(A \wedge B) \to A$ (pp. 165-166); a proof in a natural deduction style would proceed by showing that A can be obtained from a proof of $A \wedge B$ simply by taking the first component of such a proof -that is, by applying the function $\lambda xy.x$ (the combinator K).

Then, Sundholm argues that we should distinguish between the construction or function $\lambda xy.x$ itself, which is an *object*, and the *process* by means of which this object has been constructed; according to Sundholm the latter is at the same time the *proof* that the object works as required:

> "That this object (this function, this construction) has such and such properties is guaranteed by the way it is constructed. (...) In order to prove *any* proposition one always has to exhibit a construction (object) (...), which must satisfy certain properties. These are guaranteed to hold by means of the construction (process) (...)." (p. 166).

Later (e.g. in [1993]) he insists on these and similar distinctions.

This is related to what I called at the beginning of this chapter (in 2.1.1) the 'fundamental dichotomy', although it is not exactly similar; in fact, as it turns out, the fundamental dichotomy means that Sundholm is wrong. Here the distinction is mainly conceptual.

I think that Sundholm may have been misled by taking too simple a case, whereas the need for the extra-clauses certainly did not arise from these type of cases. Indeed, Diller and Troelstra [1984] point out that:

> "To what extent does a proof-object determine a proof? In other words, presented with a proof-object, can we construct a proof, that is, can we mentally follow a (proof-)process that results in the given proof object?
>
> "In simple situations this is certainly possible (...). In general, this is not plausible any more. Consider for example the following situation: if t, t' are closed numerical terms, $t=t'$ is *formally* proved by evaluating t, t' (...). However, a proof of $t=t'$ as a mental construction (-process) with a corresponding proof-object

> concerns the *objects* n, m denoted by the terms t, t'. Comparison of these objects is immediate and carries no mathematical information beyond the truth of the equation; so we amy as well denote such a trivial canonical proof by an arbitrary fixed object, say 0. A proof of $\forall x(t(x)=0)$, f a numerical term with parameter x, convinces us of the fact that $\lambda x.0$ is a function which assigns to each $x \in N$ a proof-object of $t(x)=0$, hence $\lambda x.0$ is a proof-object for $\forall x(t(x)=0)$, from which we cannot reconstruct the possibly quite complicated argument showing that $\forall x(t(x)=0)$." (pp. 258-259).

Hence they conclude that "the divergence between proof-objects and the informal arguments corresponding to formal proofs of the usual kind is at first sight unsatisfactory" (pp. 259-260).

A case of this is the construction that I described before for finding, for each natural number n, a bigger prime of the form $4m-1$. It was not at all obvious by looking at the construction that it should work as required. Also, it was not clear that the *process of finding* the function can be identified with the proof -as a matter of fact, I first thought of such a construction as one intuitively plausible, then I tested it with some random cases of small numbers, and only after having convinced myself that it could work in general did I start to look for a proof. The judgement proof in this case, as we saw at the time, is itself a complicated argument which requires the consideration of new constructions as objects (e.g. the division algorithm to divide a natural number by 4); and the same will happen in similar cases.

2.1.10. More on Sundholm's conceptual distinction. However, I do think that Sundholm has an interesting point in that conceptually there is a great difference between a construction (an object, a constructive function) and a proof (an argument); and that from this point of view the two should be separated, and the difference between the working component of Kreisel's clauses and the judgement proof should be stressed. In particular, a proof has an essential *epistemic* character (it is a proof *of something*, it has an *intentional* character), which a simple construction has not.

For example a proof of A would serve in principle equally well as a proof of A and as a proof of $A \vee B$ for *any other* statement B. Hence it is simultaneously a proof of infinitely many statements. Because of this Troesltra and van Dalen [1988] specify that a proof of $A \vee B$ is either a proof of A or a proof of B "plus the stipulation that we want to regard the proof presented as evidence for $A \vee B$" (p. 9). This would be like requiring that the proof includes a 'label' which indicates which statement we want to regard it as a proof of.

Also Prawitz has insisted on this point: for instance, "it is not enough that we have just constructed these two canonical proofs separately to be in the position to assert $A \wedge B$ -they entitle us only to assert A and to assert B. To assert $A \wedge B$ we must also be aware of the fact that these two proofs form a sufficient ground to go one step further and assert $A \wedge B$" ([1977], pp. 25-26).

Later he insists:

> "(...) it is not enough that the steps of a proof happen to follow from the preceding ones, it must also be *seen* that they follow; and it is this last requirement that must be attacked in any real analysis of the notion of proof." ([1978], p. 26).
>
> "To be in possession of a proof of a sentence, it is of course not sufficient to have constructed an argument for the sentence in the sense of something that has just the form of a proof, i.e. a structure of sentences some of which are said to follow from others. The argument must at least be supplemented, for each step, by some alleged ground for the claim that the step follows from certain preceding ones. (...) it [the alleged ground] must consist in knowledge of a procedure for how to find a canonical proof of the conclusion given canonical proofs of the premisses." ([1987], p. 160).

To sum up, this conceptual difference is of great interest.

§2.2. The operational interpretation

2.2.1. The basis of the operational interpretation. The operational interpretation is based on the concept of a construction 'performing' the operations indicated by a given statement. This is different from 'proving' the statement (or from 'solving the problem corresponding to that statement'), in that it does not contain an implicit judgement that we have to know that the construction does that; but it is similar to the other phrases that Heyting used in his earlier papers.

The idea thus would be that the meaning of an intuitionistic statement be given by the conditions under which a given construction performs the operations indicated by that statement. That is: it would be an interpretation based on 'performing conditions'.

Something similar is suggested by Bishop [1967] with respect to the conditional. According to him $A \rightarrow B$ means that "the validity of the computational facts implicit in the statement A must insure the validity of the computational facts implicit in the statement B" (p. 7). It is also in the line of Kleene's realizability (see 3.1.3 later).

This interpretation will have a certain non-constructive character, but one which might not be very significant.

Indeed, on the contrary, to subscribe this interpretation we would have to accept the idea that a construction might perform certain operations independently of us having verified it so or being in a position to verify it -otherwise the difference with the verificationist interpretation would disappear; and this is contrary to the strictest intuitionistic doctrine, under which it is impossible that a construction might have a property that we are not in a position to verify:

> "They [mathematical objects] exist only in virtue of our mathematical activity, which consists in mental operations, and have only those properties which they can be recognized by us as having." (Dummett [1977], p. 7).

On the other hand, this deviation may be a very small one. In fact this will only apply to some statements, relatively few: those for which we have already a 'candidate' -a construction which could satisfy the statement-, but we lack the proof that it always works as required. Even in these cases we will not be able to *assert* the statement in question (say A), since we are not certain of our construction; and similarly we will not be able to assert $\neg A$ or even to assert $A \vee \neg A$. Dummett himself in a later paper comes near to accepting this idea:

> "Of course, we cannot have a procedure without knowing that we have it: but we may have it without knowing what its outcome will be. This depends upon its outcome's being determinate, even though we do not know it; but I think we can go a certain distance along the road of admitting such determinacy without our theory's collapsing into realism from another direction.
>
> "These issues are difficult. I am far from sure how to resolve them (...)." ([1987], pp. 285-286).

2.2.2. The operational interpretation in action: the connectives. I shall now give an example of the form that an operational interpretation of the logical constants could take. The discussion which comes later in the thesis could be used to modify it -and perhaps to improve it- at various points, but the following version will be helpful for future reference, as a prototype.

A similar interpretation has never been tried before in a systematic and completely explicit way, because, as we know, even those authors who intentionally eliminated the extra-clauses from their definitions continued anyhow to give them in terms of proofs.

The treatment of atomic statements is not different from that of the verificationist interpretation, since, as it is easy to see, the dichotomy between the construction that the statement requires and a proof of it cannot arise in the case of an atomic statement. This is simply because according to the definition of an intuitionistic set -*species*- we must be given a decision procedure to determine whether a construction is or is not a proof that a particular object (or a sequence) belongs to it. Hence the proof relation with respect to atomic statements is always decidable, and no essential distinction can be made between a proof of the statement and a construction fulfilling it.

The definitions of conjunction and disjunction will not change its structure with respect to the verificationist interpretation, but only the basic terms in which it is formulated:

(a) a construction *performs* $A \wedge B$ when it *performs* A and *performs* B;

(b) a construction *performs* $A \vee B$ when either it *performs* A or *performs* B.

The definitions of $\to$ and $\neg$ could proceed as in the naive interpretation but with the new terminology:

(c) a construction c *performs* $A \to B$ when for any construction d, if d *performs* A then $c(d)$ *performs* B;

and then let $\perp$ be a fixed atomic construction which is obviously impossible,

(d) a construction c *performs* $\neg A$ when for any construction d, if d *performs* A then $c(d)$ *performs* $\perp$.

2.2.3. The operational interpretation of the quantifiers.

(a) a construction c *performs* $\forall x A(x)$ when, for any construction d in the domain, $c(d)$ *performs* $A(d)$;

and the clause for the existential quantifier:

(b) a construction c *performs* $\exists x A(x)$ when it is a construction such that $A(c)$.

In the formulation of (b) I do not require the corresponding proof that c satisfies this condition, or, rather, I do not require the construction which would perform the statement $A(c)$, because this seems more in line with the spirit of the present interpretation. In fact it coincides with Heyting's definition of [1956], although all other authors do require this second construction.

I am not too sure about this point (see also 2.3.5 later). In any case we could add that requirement without any worry, since it will not destroy the inductive structure of the definition.

2.2.4. The naive interpretation revisited. We can now come back to what was wrong with the naive interpretation (1.5.7). In particular, according to this interpretation we recall that

> a *proof* of $\forall x A(x)$ is a construction which transforms every construction of an object c in the domain into a *proof* of $A(c)$.

However, as we have seen here again and again this clause is very misleading, because it is simply not true that a procedure to transform each object c into a proof of $A(c)$ is necessarily itself a proof of $\forall x A(x)$ (cf. 2.1.3 and 2.1.4 for example).

The same happens with his definition of the conditional:

> a construction c *proves* $A \rightarrow B$ when for any construction d, if d *proves* A then $c(d)$ *proves* B.

Indeed, suppose for example that $A(x)$ and $B(x)$ are two numerical conditions, and that we have found a calculation to transform any number satisfying $A(x)$ into one satisfying $B(x)$. Then we can use this method to transform any proof of $\exists xA(x)$ into a proof of $\exists xB(x)$, simply by applying these calculations to the number provided in the proof of $\exists xA(x)$, and then checking that the result is a number satisfying $B(x)$ -assuming that the latter is decidable.

However, this procedure will *not* be a proof of

$$\exists xA(x) \rightarrow \exists xB(x)$$

unless we have a separate argument that it will always work as expected, something which, again, might be very far from obvious.

2.2.5. More on the naive definition of $\rightarrow$. Moreover, the preceding definition of the conditional faces an additional problem: in the case where the antecedent A is false and hence there can be no proofs of it, the definition is vacuous and accordingly *anything* would be a proof of $A \rightarrow B$ -because there will be no proofs of A to transform.

Heyting had already noticed this problem:

"You remember that $A \to B$ can be asserted if and only if we possess a construction which, joined to the construction A, would prove B. Now suppose that $\vdash \neg A$, that is, we have deduced a contradiction from the supposition that A were carried out. Then, in a sense, this can be considered as a construction, which, joined to a proof of A (which cannot exist) leads to a proof of B. I shall interpret the implication in this wider sense." ([1956], p. 102).

However, once we have established that A is false it will remain the case that *anything* is a proof of $A \to B$, according to the main clause.

In a case where both the direct proof of $A \to B$ and -in particular- the proof of $\neg A$ were truly difficult we could end up with the paradoxical situation that after having finally found a proof of $\neg A$ we would have to conclude that anything was a proof of $A \to B$ anyway.

2.2.6. The naive definition of $\neg$. To make matters even worse the naive definition of $\neg$ is also defective:

a construction c is a *proof* of $\neg A$ when for any construction d, if d *proves* A then $c(d)$ *proves* a contradiction $\perp$.

Then, if A is actually false there will never be a proof of it and hence, as before, *any* arbitrary construction will vacuously satisfy the clause.

This means that once a construction is accepted as a proof of a negation statement then any other construction would have to be regarded as a proof of it too.

Of course, none of these paradoxes arise under the presence of Kreisel's extra-clauses, which require not only one construction that transforms all proofs of A into whatever else -which it may do in a vacuous way-, but also a second construction that 'verifies' this; and obviously not any arbitrary construction will do that.

2.2.7. Hypothetical constructions. The operational interpretation is also affected by this problem although in a more indirect way. Indeed, according to this interpretation it is also the case that if A is false then any construction would vacuously 'perform' $\neg A$ or $A \rightarrow B$ -for any B (see 1.5.7). This is somehow less paradoxical because the concept of 'performing' is more flexible, and it does not have to correspond to an intuitive use as in the case of 'proof'.

In other words: the only purpose of the operational definition is to give an adequate explanation of the logical operators, and the concept which is placed at the centre of it only has to satisfy a technical role. In contrast, the verificationist interpretation carries with it the inductive definition of the notion of 'proof', and hence it is constrained by the inherent properties of this notion.

In any case, a way out of these paradoxes without appealing to Kreisel's extra-clauses -and which could be used for both the verificationist and the operational interpretation- is the use of conditional proofs or 'proofs from premises'. This is essentially what Kolmogorov did in his definitions of $\neg$ and $\rightarrow$, and Heyting in his definition of $\neg$. I shall discuss it in detail later in this chapter.

2.2.8. The operational interpretation and the theory of meaning. As is well-known, the work of Michael Dummett in recent times has connected intuitionism with a number of philosophical issues. In particular, he has pointed to the explanations of the intuitionistic logical constants as the general pattern of a verificationist theory of meaning for a natural language, which he has been trying to reformulate:

> "The intuitionistic explanations of the logical constants provide a prototype for a theory of meaning in which truth and falsity are not the central notions.

(...)

"Such a theory generalizes readily to the non-mathematical case." ([1976], p. 110).

The interest of Dummett in intuitionism is part of a general inclination towards anti-realist positions in several philosophical fields. In fact he was the first to notice that some of the most characteristic features of intuitionism -such as the rejection of bivalence, the adoption of intuitionistic logic or the inadequacy of a theory of meaning based on truth-conditions- were also common to all other versions of philosophical anti-realism:

"In a variety of different areas there arises a philosophical dispute of the same general character: the dispute for or against realism concerning statements about a certain type of subject-matter, or, better, statements of a certain general type." ([1969], p. 358).

"It is difficult to avoid noticing that a common characteristic of realist doctrines is an insistence on the principle of bivalence -that every proposition, of the kind under dispute, is determinately either true or false. (...) What anti-realists were slow to grasp was that, conversely, they had in the most typical cases equally compelling grounds to *reject* bivalence and, with it, the law of excluded middle. (...)

"Those who first clearly grasped that rejecting realism entailed rejecting classical logic were the intuitionists (...)." ([1991], p. 9).

Among the list of areas in which the dispute between realism and anti-realism arises, Dummett includes -apart from mathematics- subjunctive conditionals ([1976], p. 81), material objects, theoretical entities of science, mental states, events and processes, statements about the past or the future ([1978], pp. 147-148), or ethical judgements ([1991], p. 6).

The interest of a verificationist theory of meaning for a fragment of the language which we wish to interpret in a non-realist way is very simple: to give meaning conditions which do not transcend any possible recognition:

> "According to this [the anti-realist account], the meanings of statements of the class in question are given to us, not in terms of the conditions under which these statements are true or false, conceived of as conditions which obtain or do not obtain independently of our knowledge or capacity for knowledge, but in terms of the conditions which we recognise as establishing the truth or falsity of statements of that class." ([1969], pp. 358-359).

As it happens, Dummett has identified these other conditions with the assertability (or proof) conditions, as has almost everybody else, to the point that the phrases 'intuitionistic theory of meaning' and 'verificationist theory of meaning' are often equated (e.g. Martin-Löf [1987], pp. 409, 413, Dalla Pozza and Garola [1995], p. 101).

However, if my argument is correct then, in the case that Kreisel's extra-clauses were to be rejected, the central concept of the definition would no longer be that of 'proof', and hence the whole project of a verificationist theory of meaning for a natural language would have to be modified into an 'operational' theory of meaning or something of the like.

I shall not pursue here the idea of the adaptation of this meaning theory for a natural language, but perhaps it is adequate to notice that if this interpretation is suitable for intuitionism then there is no reason why it should not be suitable for other forms of anti-realism too. In particular, although there is an ingredient of 'non-constructiveness' -or recognition transcendence- in the concept of 'performing', if it is intuitionistically acceptable, then it could also be acceptable in anti-realism in general.

2.2.9. Recapitulation. I shall not attempt to resolve the question of which general form -the verificationist or the operationalist- an adequate interpretation of the intuitionistic logical constants should take. The point that I have tried to stress is that the result of eliminating Kreisel's extra-clauses from the definition, is an interpretation of the operational type, where the central concept is not that of 'proof' and which will necessarily have a certain non-constructive character.

§2.3. Kreisel's interpretation revisited

2.3.1. Introduction. A close analysis of Kreisel's interpretation shows that the role of judgement proofs in the definition is partly redundant and can be made more precise. When we try to do this, however, we discover a surprising relation between Kreisel's interpretation and the operational.

2.3.2. A suggestion by Kreisel. The inspiration here came from a footnote by Kreisel himself, where he says:

> "There is an additional distinction which has so far not been formally necessary, but which is probably important, for example in the explanation of implication (or universal quantification). When we think of the pair $\langle c_1, c_2 \rangle$
>
> c_1 proves the identity: for variable d, if d proves A then $c_2(d)$ proves B,
>
> c_2 is a genuine function or operation, while c_1 recognizes that c_2 satisfies the condition stated; thus c_1 is a judgement. But similarly, since in general both the arguments d and the values $c_2(d)$ of c_2 are such pairs, say $d=\langle d_1,d_2\rangle$ and $c_2(d)=\langle a_1,a_2\rangle$, should the function a_2 depend both on d_2 and d_1 (or only on d_2)?" (Kreisel [1970], footnote 11, pp. 145-146).

Kreisel does not answer his own question, and surprisingly enough he has not developed this point since -at least not to my knowledge.

The natural answer is 'only on d_2'. Indeed, let us suppose for example that c proves a statement $\forall xA(x) \to \forall xB(x)$. This means that we will have

(a) c_1 proves: for any d, if d proves $\forall xA(x)$ then $c_2(d)$ proves $\forall xB(x)$.

Now assume that $d = \langle d_1, d_2 \rangle$ is an actual proof of $\forall xA(x)$, that is:

d_1 proves: for any b, $d_2(b)$ proves $A(b)$.

Hence $c_2(d)$ will prove $\forall xB(x)$; and if we put

$c_2(d) = a = \langle a_1, a_2 \rangle$

then we obtain

a_1 proves: for any b, $a_2(b)$ proves $B(b)$.

The question is: should a_2 depend on d_1?; that is to say: should it depend not only on the 'working' proof of the antecedent (d_2), but also on its corresponding judgement proof (d_1)? The answer seems clear to me: 'only on d_2'.

As a matter of fact, in general there will be various different ways in which to establish that d_2 does the work required by $\forall xA(x)$. Each of them will constitute a good candidate for a judgement proof d_1 of the corresponding construction d; but there is no reason why the manner in which d_2 is transformed into a construction a_2 -which does the work required by $\forall xB(x)$- should vary according to the judgement proof which accompanies d_2.

Of course the corresponding judgement proof for a_2 -that is, a_1- could depend on it, but that is a completely different matter; in fact, as we shall see immediately, a_1 is quite irrelevant in the presence of c_1.

2.3.3. Discussion. Now let us suppose that we are actually in possession of the proof c, and in particular, of its first component -the judgement- c_1. This means that we can prove that c_2 will work as required, that is: we can prove that c_2 will transform all proofs of $\forall xA(x)$ into proofs of $\forall xB(x)$.

Then suppose that we are given not a full proof of $\forall xA(x)$, but simply a method which transforms any construction b in the domain into a proof of $A(b)$ -that is: the working component d_2 of a full proof d.

We already know that the result of applying c_2 to this method -that is, $c_2(d_2)$- will result in the production of a method corresponding to $\forall xB(x)$: a method which transforms any construction b in the domain into a proof of $B(b)$. This is so precisely because a_2, the working component of the proof of $\forall x B(x)$, could not depend in any essential way on whatever judgement proof was given accompanying the construction d_2.

Moreover, our general judgement c_1 must ensure that this is so: that $c_2(d_2)$ is a method corresponding to $\forall x B(x)$. Indeed, we have, on the one hand, that c_1 proves that c_2 transforms all proofs of $\forall x A(x)$ into proofs of $\forall x B(x)$. Hence it must, in particular, transform the working component of the proof of $\forall x A(x)$ into a working component for a proof of $\forall x B(x)$. In this transformation the nature of the respective judgement proofs of $\forall x A(x)$ and $\forall x B(x)$ does not make a difference: c_1 cannot use them in any essential way.

Therefore we conclude that c_1 proves, in particular, that $c_2(d_2)$ is a method corresponding to $\forall x B(x)$.

However, in that case, why should we make any further reference to the judgement proofs of $\forall x A(x)$ or $\forall x B(x)$? If I am supplied with an actual proof of $\forall x A(x)$, say $d = \langle d_1, d_2 \rangle$, then I proceed to feed d_2 into c_2 to get the working component of a proof of $\forall x B(x)$. By c_1 I know that if d_2 was a method according to $\forall x A(x)$ then the result $c_2(d_2)$ will be a method according to $\forall x B(x)$; and by d_1 I know that d_2 was indeed a method according to $\forall x A(x)$.

Hence I can immediately construct an argument to the effect that $c_2(d_2)$ is a method for $\forall x B(x)$, that is: a judgement proof to accompany $c_2(d_2)$.

This means that Kreisel's definition as it stands is too complicated, and unnecessarily so.

2.3.4. The operational definition reappears. The obvious way to resolve this redundancy is to give two inductive definitions, one right after the other. The first would not include judgement proofs at all, and hence it would coincide exactly with the operational definition of meaning; and the second one would be based on the first, but adding the requirement of judgement proofs wherever they are needed.

The result will be that in analysing what is the proof of a given statement according to the definition only *one* judgement proof will be required -the last one.

2.3.5. Kreisel's definition polished. We assume that we have an operational definition for the notion of a construction 'performing' the operations required by a given statement. On the basis of that concept we define this version of the verificationist interpretation. The cases of atomic, conjunction and disjunction statements -which do not require judgement proofs- are treated as usual (1.5.2).

Then:

(a) c is a *proof* of $A \rightarrow B$ if c is a pair $\langle c_1, c_2 \rangle$ such that c_1 *proves* that for every construction d in the domain, if d *performs* A then $c(d)$ *performs* B.

(b) c is a *proof* of $\neg A$ if c is a pair $\langle c_1, c_2 \rangle$ such that c_1 *proves* that for every construction d in the domain, if d *performs* A then $c(d)$ *performs* the impossible construction $\perp$.

(c) c is a *proof* of $\forall x A(x)$ if c is a pair $\langle c_1, c_2 \rangle$ such that c_1 *proves* that for every construction d in the domain, $c(d)$ *performs* $A(d)$.

(d) c is a *proof* of $\exists x A(x)$ if c is a pair $\langle c_1, c_2 \rangle$ such that c_1 *proves* $A(c_2)$.

It is interesting to notice that in the definition of $\exists$ the first component c_1 of the construction c is clearly a judgement proof as well. For instance, a proof of $\exists x A(x) \rightarrow \exists x B(x)$ will require a method of transforming every particular instance of

$A(x)$ into one of $B(x)$ -that is, c_2-, plus the judgement proof that the method does this -c_1. However, we do not need to invoke a hypothetical judgement proof for $\exists x A(x)$, nor the way to transform it into one for $\exists x B(x)$. c_1 will be enough to do this, as it happened in the case discussed in 2.3.3.

This suggests that the status of the first component of a proof of an existential statement (c_1 in d above) is essentially that of a judgement proof, despite the fact that it does not break the inductive structure of the clause.

2.3.6. Discussion. The conclusion of my argument here is that the operational definition has an importance which is independent of whether we consider it as the basic semantic definition or not, since -if I am correct- it is the basis of Kreisel's definition anyhow. Hence the concept of 'performing' which is induced by the operational definition would have an interest in itself, and would deserve to be studied, whichever position we adopt on the debate over the extra-clauses.

Moreover, if we accept that the notion of *performing* has an independent sense in itself (and if we are to revise Kreisel's interpretation as suggested here we *must* do so) then we must also accept that it is *possible* (intuitionistically meaningful) to interpret mathematical statements in these terms, and then the most natural thing is to do so. In other words: the operational interpretation is simpler and more natural, and therefore if it is not the correct one it must be because it does not make sense constructively; and if it does make sense constructively, then, it appears, it must be the correct one.

§2.4. Canonical proofs

2.4.1. Introduction. The need to draw a distinction between canonical and non-canonical proofs has been defended in response to various different motivations. The simplest one -which I shall discuss first- is the existence of indirect proofs in which the elements required by the corresponding clauses are not actually provided, but only an effective procedure for finding them.

This is particularly obvious in the case of disjunction and existential statements when, instead of producing a proof of one of the disjuncts, or producing an element of the domain which satisfies the existential claim, a mere procedure for finding one is indicated. Brouwer had already noticed this quite clearly:

> "The case that A has neither been proved to be true nor to be absurd, but that we know a finite algorithm leading to the statement either that A is true, or that A is absurd, obviously is reducible to the first and second cases." ([1981], p. 92, footnote).

Something similar happens sometimes with atomic statements. For example a proof of

$$10^{10} \cdot 10^{20} = 10^{30}$$

may proceed not by performing the actual calculations -which would be impossible in practice- but by showing that the equality $x^y \cdot x^z = x^{y+z}$ holds in general -by induction-; that is: by giving a general method which, if effected, would produce a proof for any triple of natural numbers $\langle x,y,z \rangle$.

Finally, a similar problem may occur with informal proofs of conjunction statements, although in a derivative way: when either of the conjuncts is of one of the above forms. However this problem cannot arise with conditional, negation or universal statements -at least under Kreisel's clauses- because the definition of them already refers to an effective method.

2.4.2. Canonical proofs versus demonstrations. In informal intuitionistic mathematics the use of this type of proofs is not only normal -starting with Brouwer himself- but sometimes, as we have seen, the only kind of proof available to us for practical reasons.

The structure of these proofs is similar to that of the proofs of conditionals, negations and universal quantifications in that the fundamental dichotomy that I discussed in 2.1.1

reappears: once the proof contains an effective method to do something, there is an immediate need for the corresponding *judgement* proof that the method works as required -something which might not be obvious.

There are essentially two ways in which to tackle this problem (cf. Dummett [1977], p. 20). One is to reformulate the clauses for atomic, disjunction and existential statements so as to allow that, for example, a proof of a disjunction be simply an effective method to find a proof of one of the disjuncts, and not necessarily the actual proof itself. This would call for the requirement of judgement proofs within these clauses, thereby changing the meaning attributed to these logical constants -and to atomic statements.

The other way -which is the one recommended by Dummett- is to leave the definition unchanged, but to stipulate that the assertion of a mathematical statement need not be understood as a claim that we have a proof of it, but only that we have a method, in principle, for obtaining one -plus a proof that the method does this (cf. as well Prawitz [1977], p. 27). In other words, to distinguish between *canonical proofs*, which would be the ones defined by the usual clauses, and any general argument in which a procedure for finding a canonical proof is described. Dummett calls this second type of argument a 'demonstration' ([1975], p. 122 and [1977], p. 392).

2.4.3. Canonical and normal form proofs. Prawitz has related the canonical proofs in the preceding sense with the proofs in normal form of a system of natural deduction. Roughly speaking, a normal form proof is one without roundabouts or 'cuts' -local peaks of logical complexity.

In fact the first time that the phrase 'canonical proof' was used in this context was precisely to compare them with normal form proofs (Prawitz [1974], p. 71; see also [1973], pp. 232-233, where the same idea is already present, and the remark in [1985], Note 1, p. 171).

Since we have been so far, considering categorical proofs only -that is, proofs without premises- the normal proofs in question would be those which do not use *at all* the elimination rules. Naturally, this kind of proofs would always be canonical in the preceding sense -e.g. a normal proof of a disjunction $A \vee B$ cannot proceed in any other way but from a previous proof of A or of B.

It is to be noticed, however, that the converse is not strictly true: a proof might be canonical according to the definition without being in normal form -e.g. a proof of $A \vee B$ might contain an unnecessary detour but still constitute an actual proof of A or of B.

2.4.4. The impredicativity of $\rightarrow$ and $\neg$. A second reason to introduce some notion of canonical proof concerns the definition of the conditional. As we saw at the time, this definition -within both Kreisel's and Heyting's interpretations- is highly impredicative, in that it refers to the *totality* of proofs of the antecedent A, a totality which presumably would include the proof of $A \rightarrow B$ itself, as well as proofs which could have been built up from it in some way; and a similar thing happens with the definition of $\neg$.

This impredicativity would be ameliorated if we could refer in the clause not to arbitrary proofs of the antecedent in general, but only to some restricted, particularly simple, type of proofs. In other words: if the basis of the proof of $A \rightarrow B$ was a construction which transformed all *canonical* proofs of A into proofs of B. However, if we did that, then we would have to make sure that any arbitrary proof of A can be reduced to one of the canonical type, or otherwise we would not be justified in the unrestricted use of modus ponens once a proof of A -of any kind- has actually being found.

This would presuppose a *reducibility hypothesis*, that for any given statement there is an *a priori* limit on the complexity that a proof of it needs to have. This hypothesis was first explicitly formulated in Kreisel [1965] (pp. 126-127) -as a hypothesis, not as a claim.

Later, Nicolas Goodman, for example, has adhered to it firmly:

> "It seems to us essential to the intuitionistic position that given a fixed assertion A about a well-defined domain, there is always an *a priori* upper bound to the complexity of possible proofs of A. In case A is an implication, this principle already guarantees the existence of some sort of reducibility operator." ([1970], p. 111).

2.4.5. Dummett on the need for canonical proofs. Dummett has gone farther to argue that without a distinction between canonical and non-canonical proofs the definition of $\rightarrow$ would become vacuous. In particular Dummett worries that

> "We could admit anything we liked as constituting a proof of $A\rightarrow B$, and it would remain the case that, given such a proof, we had an effective method of converting any proof of A into a proof of B, namely by adding the proof of $A\rightarrow B$ and performing a single inference by modus ponens. Obviously, this is not what is intended". ([1975], p. 123).

Hence, he concludes

> "(...) if the intuitionistic explanation of implication is to escape, not merely circularity, but total vacuousness, there must be a restricted type of proof -canonical proof- in terms of which the explanation is given, and which does not admit modus ponens save in subordinate deductions". ([1975], p. 123).

Naturally, as it is very easy to see, a similar argument can be applied to $\forall$: an arbitrary construction c could also be used allegedly to construct a vacuous 'proof' of $\forall x A(x)$, simply by considering that c is already a proof of $\forall x A(x)$, and then using it to eliminate $A(n)$ for any given n; and a similar argument would apply to $\neg$.

I think, however, that Dummett is wrong. Indeed, suppose that we admitted an arbitrary construction as a proof of $A\rightarrow B$ and immediately after we used it to obtain B from A using modus ponens; then, would the resulting construction constitute a proof B according to the inductive definition? Not necessarily. For example, if B is an existential statement $\exists x C(x)$, then the resulting proof of B should indicate an object a plus a proof of $C(a)$, or else a procedure for finding them; but the vacuous construction of Dummett's that I have just described will not in general do this.

2.4.6. More on Dummett's argument. In [1977] Dummett refined his argument slightly, by considering as the basis of the vacuous proof, not arbitrary constructions in general but only those previous proofs of $A \rightarrow B$ which are intuitively valid from the intuitionistic point of view:

> "(...) whatever we chose to accept as being a proof of $A \rightarrow B$, it would, provided that it itself conformed to the canons of ordinary informal proof, supply us with an effective means of transforming any proof of A into a proof of B, namely by annexing to the proof of A the given proof of $A \rightarrow B$ and then appending a single application of modus ponens". (p. 393).

Then he concludes, accordingly:

> "The constraints on what constituted a proof of statements of these kinds would then all come from whatever intuitive prior notion of an informal proof we were appealing to (...).
>
> "Obviously, however, this is not what is intended when these explanations of the logical constants are given." (p. 393).

However he again misses the point that, as before, what matters is that the procedure yields proofs of B which agree with the rest of the definition; and that is something independent of any prior informal notion of validity.

To see this clearly, let us consider the following example. Let c be a proof of $A \rightarrow B$ and let us assume that c is canonical and perfectly valid by all standards. Then we can apply Dummett's construction to c in the obvious way: for any given proof of A, join it with c and obtain B by modus ponens. Let d be this new construction.

In the presence of a proof of A we have that d will certainly be enough to convince us of the truth of B, at least as much as c is enough to convince us of $A \rightarrow B$. However, surprisingly enough d will *not* in general be a proof of B according to the inductive definition.

Indeed, let us consider for example the conditional $\exists x A(x) \rightarrow \exists x B(x)$, where $\exists x A(x)$ and $\exists x B(x)$ are numerical statements, and suppose that c provides a way of transforming any given number with the property $A(x)$ into another one with the property $B(x)$. For the sake of argument we may also assume that c also includes a proof that the construction works as required, a decision procedure for $B(x)$, and whatever else is needed.

Then, given a proof of $\exists xA(x)$ we could apply c to it to obtain an object a and a proof of $B(a)$, or at least a procedure for finding them -if the proof of $\exists xA(x)$ does not give an object explicitly.

On the other hand, by its own construction d will only yield an argument to the effect that $\exists xB(x)$ holds; an argument which will be good enough to gain conviction that it holds and can be proven constructively, but not a proof according to the inductive definition. Indeed, in the case where the proof of $\exists xA(x)$ provides a definite object a such that $A(a)$, the result of applying d to it will not yield a corresponding number satisfying $B(x)$; and in the case when the proof of $\exists xA(x)$ consists in providing a procedure for finding a, then d will not point to the corresponding procedure which would consist in obtaining a method for finding an instance of $B(x)$, by appending c to it. Instead, all d points to in both cases is a certain inference by modus ponens.

Therefore, we have to conclude that the result of applying *d* to a proof of $\exists x A(x)$ will be a good argument for $\exists x B(x)$, but not yet a proof of it.

The real proof -*c*- is somehow contained within *d*; but *d* does not exhibit it in the appropriate way, and because of this it does not qualify.

As we can see, the situation is not at all one in which the constraints on what constituted a proof would come from an intuitive notion of informal proof, as Dummett feared, but rather, the opposite.

2.4.7. Proofs in normal form again. In the case of the non-canonical proofs of 2.4.2 the reducibility hypothesis is obvious, since they are defined precisely as any effective procedure for obtaining a canonical proof. However, the restriction to canonical proofs in this sense -i.e. proofs which agree exactly in the clauses for $\vee$, $\exists$ and atomic statements- would not reduce the impredicativity of $\rightarrow$ and $\neg$ in any significant way: among those proofs of the antecedent A which are canonical in this sense, there might very well be some that have been built up from the proof of $A \rightarrow B$, which is the proof that is being defined. The range of 'canonical proofs' of this type is huge.

A good candidate, however, for such a reduction would be the concept of 'proof in normal form'. A proof of A in normal form, being free of roundabouts, would build up gradually and could not use a statement more complex than A itself -hence could not be based on $A \rightarrow B$. If the definition of $\rightarrow$ referred exclusively to normal proofs, the impredicativity would disappear.

However, in the case of normal form proofs the reducibility hypothesis is not so obvious. The normal form proof theorems for intuitionistic systems of sequents (Gentzen [1934], the *Hauptsatz*) and of natural deduction (Prawitz [1965]) establish that within these formal systems every proof can be reduced to one in normal form. However, it is not so clear whether an analogous result applies outside formal mathematics.

Prawitz [1977] observes:

> "(...) the presence of $\rightarrow$ and $\forall$ have the effect that in general the conditions for asserting a sentence cannot be exhausted by any formal system; (...) there is no formal system generating all the procedures that transform canonical proofs of A to canonical

proofs of B, and it is left open what more complicated sentences can be involved in such procedures. For instance, such a procedure may be definable in an extension of a certain language without being definable in the language itself, and hence, in this respect, the extension of a language obtained by introducing new logical constants may not be a conservative extension of the original language. Consequently, while the operations of forming canonical proofs run parallel to the introduction rules of Gentzen's system of natural deduction, it is clear that the rules for asserting a sentence do not amount to inference rules of any formal system." (p. 29).

In [1987] he mentions Gödel's first incompleteness theorem to argue again that a statement might be provable, but not if we restrict the proof to particularly simple methods:

"From Gödel's incompleteness theorem, we know indeed that, unlike the situation in first order predicate logic, a sentence $\forall x A(x)$ or $\forall x(A(x) \rightarrow B(x))$ with $A(x)$ and $B(x)$ recursive, although unprovable in elementary arithmetic, may be provable by introducing new concepts outside elementary arithmetic and principles for them, and that, on the whole, we cannot at all put any formal constraints on how such a sentence can be proved." (p. 159).

Dummett has also made a similar point:

"(...) it does not follow, from the normalization theorem for first-order logic, that a similar theorem will hold good for any specific formalized first-order theory for some part of intuitionistic mathematics. What our present considerations show is that it is both necessary and plausible that a normalization property should hold good of those canonical intuitive proofs which constitute the fundamental type of mental constructions in terms of which any intuitionistic theory is given meaning." ([1977], pp. 396-397).

2.4.8. Dummett on the stability of proofs. In more general terms, Dummett has doubted that the methods of mathematical proof can be surveyed in advance, and hence that we can put any limit on the minimum complexity that a convincing argument for a given statement needs to have. In [1977] he also mentions Gödel's theorem and concludes that "the totality of methods of proof, within a given mathematical theory, is likely to be an indefinitely extensible one" (p. 401).

This leads him to the surprising conclusion that a mathematical theorem such as $A \rightarrow B$ could be fallible. Dummett's argument is as follows. Suppose that we have a construction c such that, for any proof of A which has been elaborated with our present proof methods, c will transform it into a proof of B. According to Dummett this should be enough to accept c as a proof of $A \rightarrow B$; but then it could happen that later we discover an entirely new way of proving A, and that the application of c to this new proof does not result in a proof of B:

"When this happens, some proof, involving a conditional $A \rightarrow B$, that had formerly seemed acceptable, may be invalidated. Hence, because of the peculiarities of the intuitionistic interpretation $\rightarrow$, provability is not a stable property (...); mathematics becomes a subject whose results are fallible and liable to revision, like those of other sciences." ([1977], pp. 401-402).

However, the acceptance of such an assumption would constitute a change in meaning of $\rightarrow$ far more dramatic than the problem of its impredicativity, which we were trying to solve in the first place.

Prawitz finds this last conclusion of Dummett's rather extreme:

"These consequences are indeed very strange, I think. That the development of mathematics by the emergence of new forms of reasoning should put in doubt all previous proofs of implications and force us to reconsider them seems to be contrary to our historical experience." ([1987], p. 158).

To which Dummett replies:

"I feel as unhappy as he [Prawitz] does with the conclusion that mathematical proof, and hence mathematical truth, is unstable; whether he has found the way to avoid this conclusion would take too long to discuss." ([1987], p. 285).

Finally, we must observe that if the meaning of a statement A is clearly understood, there will be nothing in a hypothetical proof of A which could bring about a change in this meaning. We know what *any* possible proof of A must and will show: the possibility of the transformations claimed by A.

§2.5. Canonical proofs (continued)

2.5.1. Goodman's levels. A different attempt to resolve the impredicativity of the definition of $\rightarrow$ is Nicolas Goodman's stratification of the universe of proofs and constructions into a cumulative hierarchy of levels, according not to their internal complexity, but to their subject matter (Goodman [1970]). In particular, there would be a first level containing the basic constructions of the universe (in the typical case, the natural numbers), plus all constructive functions operating on them. The second level would include the whole of the first level, plus those proofs which operate on all the constructions of the first level, e.g. the proofs of a statement $A \rightarrow B$ where A belongs to the first level; and so on.

More formally, Goodman assigns to each statement A a *depth*, according to the "nesting" of conditionals and universal quantifiers in it -he treats negations as a special case of conditional statements. Inductively, if A is an atomic statement then its *depth* $\mathscr{d}(A)$ is 0; if $A=B\vee C$ or $A=B\wedge C$ then $\mathscr{d}(A)=\max\{\mathscr{d}(B), \mathscr{d}(C)\}$; if $A=\exists xB(x)$ then $\mathscr{d}(A)=\mathscr{d}(B(x))$; and finally if $A=B\rightarrow C$ then $\mathscr{d}(A)=1+\max\{\mathscr{d}(B), \mathscr{d}(C)\}$, and if $A=\forall xB(x)$ then $\mathscr{d}(A)=1+\mathscr{d}(B(x))$. In short, if we consider the tree-process of formation of the statement according to the syntactic rules of the language, the depth is the biggest number of conditional and universal quantifiers that have been added in a single branch.

Although the definition of $\forall$ is not as clearly impredicative as that of $\rightarrow$, Goodman treats the occurrences of these two logical operators equally (e.g. in his definition of *depth*). Perhaps the point is that although in the definition of a proof c of a statement $\forall x A(x)$ not all arbitrary proofs are referred to, (only all constructions *within* the domain, which are usually simple objects, and not proofs), nevertheless c must transform any one of them, say d, into a proof of $A(d)$, and it is for this latter proof that c itself could be somehow invoked.

Then Goodman defines a proof of a conditional statement $A \rightarrow B$ as a construction which transforms all proofs of A of level $\mathscr{d}(A \rightarrow B)$ into proofs of B. Since the proofs of level $\mathscr{d}(A \rightarrow B)$ cannot yet refer to $A \rightarrow B$ itself, the impredicativity is avoided.

However, as before, the problem with Goodman's stratification is the weak intuitive validity of the corresponding reducibility hypothesis. As a result, few people have accepted this division into levels. The following quote by Weinstein contains the basic objection:

> "Goodman's response to this problem created by stratifying the universe of constructions is to suppose that any proof of a statement which involves quantification only over constructions of a given level may be replaced by a proof of the next highest level. (...) But the justification for this assumption is not very clear. Let us consider, for example, the statement for every natural number, *n*, $f(n)=0$. It may be that any proof which we in fact have of this statement is an argument the premisses of which involve quantification over constructions of a high level. If, as Goodman assumes, constructions of natural numbers lie at the lowest level then the above assumption implies that we must be able to extract from such a proof another proof of the statement which does not make use of quantification over high levels of the constructive universe. That we have a proof of the statement in question implies that there is a constructive function which assigns to each natural number, *n*, a proof that $f(n)=0$. But I see no reason to think that we have grounds to assert that this constructive function has the property in question which do not make use of insights about higher levels of the constructive universe." ([1983], pp. 265-266).

2.5.2. Brouwer's 'fully analyzed proofs'. A third reason for wanting to draw a distinction between proofs in general and proofs of a predetermined, canonical form, is the desire to make a full exploitation of Brouwer's conditional. This was Brouwer's motivation behind the introduction of his own notion of canonical proof -that which Dummett [1975] (p. 183) called 'fully analyzed proof'-, in the attempted proof of the bar theorem (see e.g. Brouwer [1927]).

I shall not discuss that proof in detail here (I refer again to the analysis of Dummett [1977], pp. 94-104). However, the idea seems to be that a fully analyzed proof does not contain logically complex statements, but operates directly with the atomic statements that the complex statements would correspond to. In particular, instead of universal quantifications, the fully analyzed proof would contain all the statements which constitute its instances -usually infinitely many:

> "Now, if the relations employed in any given proof can be decomposed into basic relations, its 'canonical' form (that is, the one decomposed into elementary inferences) employs only basic relations.
>
> (...)
>
> "These *mental* mathematical proofs that in general contain infinitely many terms must not be confused with their linguistic accompaniments, which are finite and necessarily inadequate, hence do not belong to mathematics." (Brouwer [1927], p. 460 and Note 8 on the same page).

2.5.3. Infinite proofs. Dummett has pointed out that:

> "(...) on the intuitionistic understanding of infinity, the only way in which we can draw an inference from infinitely many premisses

is by recognizing *that* each of these premisses can be proved; and that, in turn, can be accomplished only by recognizing, of some general procedure, that it will yield a proof of each of the premises. Thus the only way of understanding the idea of an inference from denumerably many premises $A(0), A(1), \ldots$ which is consistent with a constructivist outlook proves to coincide exactly with the intuitionistic interpretation of an inference from $\forall n A(n)$." ([1977], pp. 96-97).

However, this seems to eliminate the difference between Brouwer's fully analyzed proofs and real ones. Indeed, in order to admit the actual existence of an infinite fully analyzed proof we would have to take a strong non-constructive standpoint; and otherwise, it seems, our idea of such a proof is exactly that of the finite procedure which would generate it in principle.

It is therefore surprising that Dummett ends up granting Brouwer's contention:

> "An intuitionistic proof involving inferences from universally quantified statements really is, therefore, what Brouwer maintains, a representation of a more fully analyzed proof containing inferences from infinitely many premisses." (Dummett [1977], p. 97).

To me it seems, on the contrary, that what Dummett has shown is that the notion of 'fully analyzed proof' cannot be made sense of, constructively, as something essentially different from an ordinary proof. In other words: that a proof concerning an infinite domain can never be 'fully analyzed', because the only way of making sense of this constructively is through its finite representation.

2.5.4. The interpretation of → again. In any case, the most serious difficulty in the definition of this notion of 'fully analyzed proof' concerns the interpretation of →. Indeed, as Dummett remarks it is easy to imagine how, within a fully analyzed proof, the different logical constants -except → and ¬- would be eliminated in favour of more elementary statements.

In particular, a statement $A \vee B$ would be replaced by either of the disjuncts, depending on which of them is really established in the proof; a statement $A \wedge B$ would be replaced by the two conjuncts; a statement $\exists x A(x)$ would be replaced by a particular $A(c)$ for some construction c in the domain; and a statement $\forall x A(x)$ would be replaced, as we have seen, by all its -possibly infinitely many- instances. Then the resulting statements would be likewise replaced by more elementary ones by the same procedure; and this operation would be repeated until we had obtained in all cases the corresponding atomic statements, which could not be analyzed further.

However, it is not at all clear how we could reduce a statement $A \to B$ in an analogous way:

> "Given a proof of $A \to B$, this would involve generating in turn each putative proof of A, and either demonstrating it not to be a proof of A or applying to it the transformation which will convert it into a proof of B. However, it appears quite contrary to the intuitionistic insistence on the impossibility of surveying possible proofs of a given mathematical statement to suppose that we could, in any such way, systematically generate a class of constructions which should include all proofs of a given statement A." (Dummett [1977], p. 102).

This is related to Dummett's remarks concerning the reducibility hypothesis with respect to normal form proofs that we quoted in 2.4.8.

He concludes that it will not be possible to exploit fully the meaning of Brouwer's conditional until we have found a solution to this problem, something which seems very difficult ([1977], pp. 103-104).

I agree; but I also wonder whether it is really so pressing to make such an exploitation, given the fact that until now the only serious attempt to do so -according to Dummett himself- was Brouwer's proof of the bar theorem, which, as I said at the time, is incorrect.

Instead, we could content ourselves with the use of a more modest conditional, in which the only property of the proofs of the antecedent that could be used was precisely that of *proving the antecedent*, that is: that of establishing the intuitionistic claim attached to that statement. If we did that, a construction proving $A \rightarrow B$ would not be allowed t operate on the internal structure of the given proofs of A, but simply to *extend* these proofs as to obtain proofs of B; and in that case our subsequent analysis of $\rightarrow$ would be significantly simplified.

2.5.5. Negation. Dummett has noticed yet one more reason to appeal to canonical proofs, which concerns the definition of $\neg$; according to him: "to say that 0=1 is unprovable is to make a very large claim, namely that intuitionistic mathematics as a whole is consistent" ([1977], p. 397). Hence, each time we assert the denial of a statement we are relying on the fact that constructive mathematics is consistent.

However, Dummett argues, the reduction to canonical proofs would dissolve this problem: "numerical equations demand, for their proof or disproof, the simplest imaginable type of construction, and it is evident that there is not, among these, one that proves 0=1" ([1977], p. 397). Therefore, according to Dummett we could define a proof of $\neg A$, not as a construction which transforms any proof of A into an *arbitrary* proof of '0=1', but one which transforms any proof of A into a *canonical* proof of '0=1'. I do not think, however, that this is too big a problem, given that, in any case, there is little doubt about the consistency of constructive mathematic. Dummett says that the consistency of intuitionistic mathematics is as trivial for an intuitionist as is that of classical mathematics for a platonist ([1977], pp. 397-398); but this is not entirely fair, since most *classical* mathematicians are sensitive to the greater intuitive 'feeling' of consistency of constructive results, while, conversely, a strict intuitionist has no grounds in principle to believe that a classical non-constructive theory should be consistent.

2.5.6. The operational interpretation again. In [1975] Dummett had written:

"The notion of canonical proof thus lies in some obscurity; and this state of affairs is not indefinitely tolerable, because, unless it is possible to find a coherent and relatively sharp explanation of the notion, the viability of the intuitionistic explanations of the logical constants must remain in doubt." (p. 124).

In [1977] he insists:

"(...) no one can at present give a detailed account of canonical proofs even of statements of first-order arithmetic." (p. 400).

However he also says:

"It would be a mistake to be stampeded by these considerations into a state of despair about the chances of showing the intuitionistic theory of meaning to be viable." (p. 399).

In any case, most of what we have seen in these two sections is somehow added 'evidence' for the suggestion that we should distinguish between the construction which carries the meaning of a given statement and the infinite variety of ways that we could use to convince ourselves that such a construction is within our reach -that is: that we could, at least in principle, produce it if we wanted to. In other words, that we should distinguish between *meaning* and *proof.*

In particular, if we adopted the operational interpretation (and hence referred simply to constructions, and not to proofs) then the distinction between canonical and non-canonical constructions in the sense of 2.4.1 would be clearly not needed: for example, a construction which performs $A \vee B$ *must* perform either A or B, and there is no way it can do this indirectly -it is a *construction*, not a *proof*.

In fact, the distinction between canonical and non-canonical proofs, and the stipulation that a non-canonical proof is an argument to the effect that we know how to obtain in principle a canonical proof, suggests that we go a step further and distinguish directly between *constructions* and *proofs*; where proofs would be defined as arguments to the effect that we know how to obtain in principle the required construction.

§2.6. Definitions in terms of 'proofs from premises'

2.6.1. Introduction. One final attempt to overcome the difficulties in the definition of $\rightarrow$ and $\neg$ which deserves to be examined is the appeal to the notion of 'proof from premises'. The idea is to define a proof of $A \rightarrow B$ as a proof of B from premise -or hypothesis- A; and similarly, to define a proof of $\neg A$ as a proof of a contradiction $\perp$ from premise A.

This idea corresponds to Kolmogorov's definition of these two connectives in terms of mathematical problems, as we saw at the time, and also to with Heyting's definition of $\neg$. It has since been adopted by Martin-Löf (e.g. [1987]), Sundholm [1986] and Bridges and Richman [1987] (p. 11).

Most authors assimilate this definition of the conditional to Heyting's, as if there were no essential difference between them -e.g. Martin-Löf [1987], p. 412. However, as we shall see immediately there are many non-trivial differences between them.

2.6.2. The appeal of this strategy. The appeal of this strategy is that the need for the judgement proof disappears: we should always be able to tell whether something is or not a proof of B from premise A; to be able to do that the only thing we need to know is the meaning of A -its constructive meaning, that is- and see whether the argument convinces us that assuming that A is correct then B should be correct too.

Indeed, as a primitive notion, the idea of a 'proof from premises' is quite natural -at least when the number of premises is finite-, provided that we already know the meaning of both the premises and the conclusion. It is a richer and more general notion than that of 'categorical' proof, which constitutes a special case of it: the case where the number of premises is 0; and if only for this reason it would be a perfectly interesting notion to be examined from the intuitionistic point of view.

Naturally we will always be able to transform any possible proof of A into a proof of B, simply by appending to it our conditional proof of B from premise A; but the transformation will be *uniform* for all those proofs, and *external* to them.

This means that if we take this definition we should give up the idea of making a full exploitation of Brouwer's conditional.

Moreover, the fact that a proof of B from premise A might be used to transform all proofs of A into proofs of B is only a *consequence* of the definition, and not part of the definition itself. If A turns out to be false, for example, then our conditional proof will *not* transform proofs of A into anything, because those proofs will never exist; but this does not in any way make the concept of a proof from premise A less clear -the situation can be compared with that of Heyting's definition, discussed in 2.2.5.

Hence, by what appears to be a simple change in wording, we seem to eliminate at once both the impredicativity and the need for the extra-proof which makes Kreisel's interpretation non-inductive.

On the other hand, the adoption of this definition also means the renunciation of Brouwer's conditional in its strongest sense.

2.6.3. $\rightarrow$ *and* $\forall$. The strategy of defining $\rightarrow$ and $\neg$ in terms of proofs from premises is somehow connected to another one designed for $\forall$, which consists in defining a proof of $\forall x A(x)$ as a proof of $A(x)$ with free variable x. As before, this leads to a uniform procedure to transform each object c into a proof of $A(c)$, but only as a *consequence* of the definition.

Also as before, the need for a judgement proof seems to disappear; and most of the difficulties that occur with the definition of $\rightarrow$ in terms of conditional proofs also arise with this definition of $\forall$ in terms of proofs with free variables.

However, I shall devote a separate section -the next one- to discussing this other strategy.

2.6.4. Two elementary difficulties. One obvious fault in the definitions as given by all the authors mentioned in 2.6.1 is that they invoke the notion of proof from premises in the clauses for $\rightarrow$ and $\neg$ (in the case of Heyting, only for the latter), but they do not mention it in the other clauses. The definitions that result are bound to be ill-constructed, as I shall now show.

For example, a proof of $A \rightarrow B$ is defined -as we have seen- as a proof of B from premise A. If we suppose now that B is a conjunction $C \wedge D$, we have that a proof of $A \rightarrow B$ will be a proof of $C \wedge D$ from premise A. However, the clause which defines proofs of conjunctions does not refer to premises: it simply says, applied to this case, that 'a proof of $C \wedge D$ is a proof of C plus a proof of D'. Hence the concept of 'proof of $C \wedge D$ from premise A' is left undefined.

In other words, this approach amounts to taking seriously the notion of 'hypothetical proof' (that is, a proof the possibility of which depends on certain premises); and in so doing we have to define what a hypothetical proof of a conjunction, of a disjunction, etc is. This is, by the way, the only serious way to treat negation in such a way that the clause is not vacuous in the relevant case, where the statement negated is intuitionistically false.

A related difficulty is that the definitions mention only one premise, when in fact it is very easy to see that often more premises will be necessary. For example, a proof of $A \rightarrow (B \rightarrow C)$ would be a proof of $B \rightarrow C$ from premise A, that is: a proof of C from premises A, B. It is quite obvious, however, that the number of premises will always be finite, since it cannot exceed the number of occurrences of $\rightarrow$ and $\neg$ in the statement in question.

These two problems seem to have an immediate solution: to re-define all the clauses appealing always to some finite set of premises. However, when we try to do that we find further difficulties.

2.6.5. Premises which are themselves proofs from premises. Given a certain proof from premises, it might happen that some of these premises are themselves conditional or negation statements, and so they will also be analyzed as proofs from premises. Hence these 'premises' will be, essentially, other proofs from premises.

The resulting structure is familiar from natural deduction systems. For example the rule of 'introduction of the conditional' -sometimes called precisely the 'deduction theorem'- allows us to infer $A \rightarrow B$ whenever, having A as an assumption, we have been able to deduce B:

$$\frac{\left[\begin{array}{c} A \\ \vdots \\ B \end{array}\right.}{A \rightarrow B}$$

Similarly, the 'elimination of disjunction' -or 'proof by cases'- allows us to infer C from $A \vee B$ whenever we have separate deductions of C, from A and from B:

$$\frac{A \vee B \qquad \left[\begin{matrix} A \\ \vdots \\ C \end{matrix}\right. \qquad \left[\begin{matrix} B \\ \vdots \\ C \end{matrix}\right.}{C}$$

These examples -which are valid in both classical and intuitionistic logic- may help us to make sense of the notion of a proof which uses as premises the existence of other proofs from premises previously constructed.

However, it would be a mistake to rely on a particular deductive system and then to define proofs from premises as proofs within that system, since in that case we would need an independent semantic justification for that system.

Dummett discusses this possibility briefly:

> "It may be thought that (...) such a notion (...) of a proof of B from A as hypothesis depends essentially upon the context of a particular formal system in which the proofs are carried out, and so would be inappropriate where we are concerned with intuitive proofs, not restricted to any formal system. Such a claim may be correct; but it is not evidently so, and reliance on it would therefore be imprudent." Dummett [1977], p. 15.

Hence the notion of 'proof from premises' should be taken as primitive, or otherwise we would vitiate the whole project.

2.6.6. The inductive structure of the definition of the notion of 'proof from premises'. If the definition is to be inductive we shall have to take into account the logical complexity of the premises too, since the understanding of an argument from premises will heavily rely on the meaning of those premises.

Accordingly, we could measure (for the purpose at hand) the complexity of a proof from -finitely many- premises as the sum of the total number of occurrences of logical constants in the premises plus those in the conclusion. Thus we would have to bear in mind that, in each of the clauses, the definition is given in terms of proofs from premises which are on the whole logically simpler -in this sense- than that of the proof being defined.

2.6.7. Atomic statements, $\wedge$, $\rightarrow$ and $\neg$. In particular, the case of atomic statements is treated as follows. Let A be an atomic statement and $\mathscr{P}$ a finite set of premises. If $\mathscr{P}$ is empty then the proof of A from premises $\mathscr{P}$ reduces to a straight or 'categorical' proof of A, and we can apply our usual treatment.

If $\mathscr{P}$ is not empty then by the induction hypothesis of the definition we already know the meaning of all the statements in $\mathscr{P}$ -as well as that of A of course-, since we already know what is a proof (from no premises) of each of these statements.

The proof of the atomic statement A from premises $\mathscr{P}$, then, will simply be an argument to the effect that we can assert A assuming that we can assert all the statements in $\mathscr{P}$. These statements may very well be false and provably so by constructive methods: but all we require is an argument based on the assumption that they were assertable.

Next we proceed to give the clauses for $\wedge$ and of course for $\rightarrow$ and $\neg$, for which this whole strategy is intended. Let $\mathscr{P}$ be again a finite set of premises, and $\perp$, as usual, a fixed contradiction:

(a) a proof of $A \wedge B$ from premises $\mathscr{P}$ is a proof of A from premises $\mathscr{P}$ plus a proof of B from premises $\mathscr{P}$.

(b) a proof of $A \rightarrow B$ from premises $\mathscr{P}$ is a proof of B from premises $\mathscr{P} \cup \{A\}$.

(c) a proof of $\neg A$ from premises $\mathscr{P}$ is a proof of $\perp$ from premises $\mathscr{P} \cup \{A\}$.

The clause for $\vee$, however, raises an additional problem which also appears in the corresponding clause for $\exists$. I shall discuss the definition of $\exists$ first.

2.6.8. The definition of $\exists$. There seem to be two options for the definition of a proof of $\exists x B(x)$ from a finite set of premises $\mathscr{P}$. They are:

(a) a proof from premises $\mathscr{P}$ that an object c can be constructed and proved to satisfy the condition $B(c)$;

and

(b) the construction of an object c, plus a proof of $B(c)$ from premises $\mathscr{P}$.

Dummett has shown quite clearly that the definition (b) is not feasible. In the following passage he seems to favour (b) as the natural definition of 'proof of $\exists x B(x)$ from premises'; at the same time, he shows that this definition does not work. It is all somehow implicit, since he does not consider openly the choice between the two available options:

> "Suppose we have a proof of B from the hypothesis A: i.e. something that is like a proof of B save that A is cited as a premiss without justification. Then we have a method of transforming any proof of A into a proof of B: namely, by appending the proof of B from A to the proof of A. Such an operation (...) is a *uniform* operation: it does not depend upon the structure of the proof of A. Again, a proof of $A \rightarrow B$ does not have to take this simple form; it may be that we can recognize some operation which involves internal transformation of any given proof of A as nevertheless always yielding a proof of B. If this were not so, then we could not admit an inference from
>
> $\forall x(A(x) \rightarrow B(x))$
>
> to
>
> $\exists x A(x) \rightarrow \exists x B(x)$
>
> as intuitionistically valid, since it would be impossible to derive a constructive proof of $\exists x B(x)$ by merely appending something to a proof of $\exists x A(x)$; we should need to know for which particular natural number n the proof of $\exists x A(x)$ yielded a proof of $A(n)$." [1977], pp. 14-15.

It seems from this quotation that according to Dummett's conception, a proof from premises of a statement $\exists x B(x)$ is subject to condition (b); otherwise the precise n which satisfies the premises would not be needed.

Indeed, suppose that the properties $A(x)$ and $B(x)$ were actually related, in the sense that an instance of $B(x)$ can be obtained -constructively- by some transformations on an instance of $A(x)$ -in the trivial case, for example, when $B(x)$ is $A(x)$ itself. If $A(x)$ and $B(x)$ are not related in some sense then a proof of $\exists xB(x)$ from premise $\exists xA(x)$ could not use that premise in any relevant way.

Then, in order to obtain a proof of $\exists xB(x)$ from premise $\exists xA(x)$ according to the stipulation (b), we would need an instance of $A(x)$. That is: the method for transforming instances of $A(x)$ into instances of $B(x)$ would not be enough. Since this method clearly constitutes a proof of $\forall x(A(x) \rightarrow B(x))$, the inference to $\exists xA(x) \rightarrow \exists xB(x)$ would fail.

It is plain that this does not happen under definition (a), and so Dummett is quite correct on the substance of his argument: the adoption of definition (b) will have as a result that certain intuitionistic inferences could not be validated. As Dummett considers that (b) is the only possibility, he then disregards the definition in terms of proofs from premises altogether, as we have seen from the quote -"a proof of $A \rightarrow B$ does not have to take this simple form".

In fact Dummett has independent reasons for rejecting the present definition of $\rightarrow$ since, as we already know, he is a resolute defender of Brouwer's conditional in its strongest sense.

2.6.9. More on the definition of $\exists$. In fact, it is clause 2.6.8(a) which seems the most natural to me, and probably -I guess- the one that corresponds to what Kolmogorov, Martin-Löf and Sundholm had in mind, or would have preferred if confronted with this dilemma.

Indeed, this clause still contains the vital information for the constructive meaning of $\exists$: that any proof of $\exists x B(x)$ should include an indication of how to find a particular instance. Being merely a proof from premises, it only requires that this indication help us to find such an instance, *supposing* that we already have proofs of each of the premises, and therefore that we are in possession of all the information provided therein.

However, it has a prominent limitation which the other does not: it ruins the inductive structure of the definition. Indeed, according to this clause a proof from premises $\mathscr{P}$ of a statement $\exists x B(x)$ is a proof from premises $\mathscr{P}$ that 'an object c can be constructed and proved to satisfy condition $B(x)$'.

However, the latter statement is a straight translation of $\exists x B(x)$ into the metalanguage, and has exactly the same logical complexity. Moreover, it seems impossible to reformulate this clause so as to make it fit into the inductive structure without obtaining clause 2.6.8(b), which we already know is inadequate.

2.6.10. The definition of $\vee$. The definition of $\vee$ faces an identical difficulty. Suppose we were to put -with $\mathscr{P}$ as before:

(a) a proof of $A \vee B$ from premises $\mathscr{P}$ is a proof of A from premises $\mathscr{P}$ or a proof of B from premises $\mathscr{P}$.

In that case we will have to admit for instance the inference

$$C \rightarrow (A \vee B) \;\vdash\; (C \rightarrow A) \vee (C \rightarrow B),$$

which is not intuitionistically valid -for example, it is not deducible in the intuitionistic propositional calculus.

Hence the definition would have to read:

(b) a proof of $A \vee B$ from premises $\mathscr{P}$ is a proof from premises $\mathscr{P}$ that either a proof of A or a proof of B can be constructed.

However, as before, this definition is a direct translation into the metalanguage of the proof defined, and not an explanation in terms of proofs logically simpler than such a proof.

Ironically enough, it turns out that this strategy, whose main virtue was to avoid the difficulties in the definitions of $\rightarrow$ and $\neg$, now manifests the most prominent limitation of Kreisel's interpretation -the collapse of the inductive structure of the definition- but with respect to two of the 'opposite' operators: $\vee$ and $\exists$.

2.6.11. The definition of $\forall$. The interpretation of $\forall$ in terms of proofs from premises does not face the same problem. Indeed, in this case the choice is between these two options -$\mathscr{P}$ again as before:

(a) a proof of $\forall x B(x)$ from premises $\mathscr{P}$ is a proof from premises $\mathscr{P}$ that we can transform any construction of an object c in the domain into a proof of $B(c)$;

(b) a proof of $\forall x B(x)$ from premises $\mathscr{P}$ is an effective method which transforms any construction of an object c in the domain into a proof of $B(c)$ from premises $\mathscr{P}$.

In this case, however, the two are clearly equivalent in meaning. In particular, from a proof from premises $\mathscr{P}$ that we can transform each *c* into a proof of $B(c)$, it is very easy to obtain a method which transforms each *c* into a proof of $B(c)$ from premises $\mathscr{P}$: all we will have to do is to apply the previous proof to each given *c*; and the implication in the other direction -from (b) to (a)- is even more obvious.

Nevertheless we do not solve in this way the inherent difficulties of the definition of ∀. Indeed, in contrast with what happened in all the other clauses, (b) is not a 'decidable' clause -it is not conservative over the decidability of the proof relation induced. There might well be a construction which *as a matter of fact* could transform every object *c* in the domain into a proof of $B(c)$, but, this not being evident, it requires a separate proof.

Then, if we want the definition to induce a decidable relation we could supplement (b) with a judgement proof -which would break its inductive structure anyhow. However, there is an alternative which also deserves to be explored: the definition of ∀ in terms of free-variable proofs, which attempts to do for ∀ exactly the same as the definition in terms of proofs from premises does for $\rightarrow$ and $\neg$. I will discuss this possibility in detail in the next section.

2.6.12. Discussion. The definition in terms of proofs from premises is a most interesting attempt to overcome the difficulties in the interpretation of the conditional and negation. Although it does somehow fail, in that it cannot preserve the inductive structure of the entire definition, it remains a serious alternative to Kreisel's interpretation for those who are sceptical about the full exploitation of Brouwer's conditional.

Unfortunately, this definition has not been sufficiently discussed in the published literature. In fact the only place where it is distinguished from Heyting's interpretation of the conditional is Dummett [1977], pp. 14-15. Sundholm [1983], p. 159 or Martin-Löf [1987], p. 410, for example, still assimilate the two (also e.g. Troelstra and van Dalen [1988], p. 9); and none of the difficulties that I have treated in the sections 2.6.4, 2.6.9 and 2.6.10 had been investigated before.

§2.7. Definitions in terms of 'proofs with free variables'

2.7.1. Introduction. The approach discussed in the last section runs parallel to the attempt to overcome the difficulties in the explanation of $\forall$ by appealing to the notion of *proof with free variables*, according to which a proof of $\forall x A(x)$ would be a proof of $A(x)$ with free variable x.

This strategy has been followed by Martin-Löf (e.g. [1987]) and Sundholm [1986], together with the definition of $\rightarrow$ and $\neg$ in terms of proofs from premises. It also coincides with Gentzen's. Moreover, Martin-Löf [1987] (p. 412) attributes this definition to Kolmogorov, but as we saw at the time, Kolmogorov's interpretation of $\forall$ appeals to a *general method* to establish $A(x)$ -or to solve it- for each x. As before, there is a non-trivial difference between these two formulations, which will be apparent immediately.

2.7.2. The idea of a free variable proof. As in the case of conditional proofs, the appeal of this strategy is that the need for a judgement proof disappears. We should always be able to tell whether something is or is not a proof of $A(x)$ with free variable x, that is: an argument that $A(x)$ holds for a fixed but otherwise arbitrary x in the domain.

Also as before, the notion of a free-variable proof -or equivalently, the proof of a formula which does not need to be a statement-, is quite natural, and a richer and more general notion than that of a 'proof of a statement', which would constitute a special case of it. In fact, if only for this reason it would still be an interesting notion to be studied from the intuitionistic point of view.

The basic idea of a *proof with a free variable* is a proof which establishes that a condition holds for an object of the domain without specifying at all which object it is. However, the notion of a free-variable proof must be preserved as a primitive notion, or otherwise it would lose its independent interest. In particular, if we have a free-variable proof of $A(x)$ it will follow at once that we have a method to prove $A(c)$ of every object c in the domain. However, this should be considered as a *consequence* of the notion of 'free-variable proof' and never as a definition of it -if it were, the need for a judgement proof would appear immediately.

Naturally this notion should not be confused with that of a proof which uses free variables temporarily -e.g. to instantiate a given quantification and to extract consequences- but whose final conclusion is a statement.

2.7.3. Two elementary difficulties. The definitions by Martin-Löf and Sundholm suffer from two obvious shortcomings, exactly analogous to those discussed in 2.6.4 in relation to →.

On the one hand, the 'proof with free variables' is only invoked in the clause for ∀; but then the terms in which this clause is given -e.g. 'a proof of $A(x) \wedge B(x)$ with free variable x'- will not in general be covered by the definition, and so the induction does not work.

On the other hand, the clause only mentions one free variable, while it is clear that often more free variables will be necessary.

As before, these two problems seem to have an immediate solution: to redefine all the clauses appealing always to some finite set of free variables.

2.7.4. The definition. The atomic case here is that of *formulas* in general, since they might contain free variables. However, it is very easy to adapt our usual treatment of atomic statements to those atomic formulas which contain free variables.

Indeed, if A is an atomic formula all whose free variables are among $y_1, y_2, ..., y_n$, then a proof with free variables $y_1, y_2, ..., y_n$ of the formula A is an argument, with free variables $y_1, y_2, ..., y_n$, to the effect that the application of the corresponding decision procedure would confirm A; that is: an argument that the decision procedure would confirm A for any values of $y_1, y_2, ..., y_n$ in the domain.

Next, all the propositional cases are obvious. In particular, the treatment of $\rightarrow$ (and so of $\neg$) can be easily obtained from the usual definitions: for example, supposing that all the free variables of A and B are among $y_1, y_2, ..., y_n$, a proof with free variables $y_1, y_2, ..., y_n$ of the formula $A \rightarrow B$ would be:

a method which transforms proofs with free variables $y_1, y_2, ..., y_n$ of the formula A into proofs with free variables $y_1, y_2, ..., y_n$ of the formula B.

However, it can also be given in terms of proofs from premises, as Martin-Löf and Sundholm do; in this case we should need to reformulate the whole definition in terms of the complex notion of 'proofs with free variables from a finite set of premises' (the adaptation is also straightforward).

Now for the quantifier cases. The case of the existential quantifier is quite clear; let $\{y_1, y_2, ..., y_n\}$ contain all free variables of $\exists xB(x)$, then a proof with free variables $y_1, y_2, ..., y_n$ of the formula $\exists xB(x)$ will be

> the construction of an object c in the domain plus a proof with free variables $y_1, y_2, ..., y_n$ of the formula $B(c)$.

Notice that in this case there would be no point in requiring, instead, that a proof of $\exists xB(x)$ with free variables $y_1, y_2, ..., y_n$ be 'a hypothetical proof with free variables $y_1, y_2, ..., y_n$ that c can be constructed', since the free variables $y_1, y_2, ..., y_n$ can play no essential role in the plain construction of an object. Hence we can refer directly to the construction of c and thus preserve the inductive setting of the clause.

Finally, the crucial case whose treatment has motivated this definition: let $B(x)$ be as before; then, a proof with free variables $y_1, y_2, ..., y_n$ of the formula $\forall xB(x)$ is a proof of $B(x)$ with free variables $y_1, y_2, ..., y_n, x$.

This clause needs no special comment.

2.7.5. The nature of proofs with free variables. The point of the present definition is that we keep the notion of proof with free variables as primitive. In particular, we should not try to explain it as 'any proof schema which, when supplemented with any choice of particular objects $n_1, n_2, ..., n_m$, yields a proof of the corresponding statement referring to those objects'.

If we do that then the definition will immediately collapse into one of the usual ones.

The conceptual difference between

(a) a proof of $B(x)$ with free variable x

and

(b) a method which transforms any construction of an object n in the domain into a proof of $A(n)$

is that (a) designates a decidable property while (b) does not: if something does not convince us of the constructive 'open' statement $A(x)$ -the statement that the condition $A(x)$ holds for any construction of an object in the domain-, then it is not a proof of it. However, something might well be a method which transforms every object n into a proof of $A(n)$ without being obviously so. Hence, if we want to ensure the decidability of the resulting relation we will need a Kreisel's judgement proof, and this would break the inductive structure.

Thus, the point of this definition is that the notion of proof with free variables is taken as primitive (particularly in the case of atomic statements, on the basis of which the whole definition is given). In this respect the present definition also behaves like the definition in terms of proofs from premises. Similarly, the concept of 'proofs with free variables' is also natural and appealing as an intuitive notion. It is obviously richer than that of a proof of statement (i.e. a formula with 0 free variables) and, as before, if it were only for this reason it would be a perfectly interesting notion to be examined from the constructive viewpoint.

2.7.6. Inadequacy of this definition. With the present definition of $\forall$ we seem to get the best of both worlds: it is inductive, and the resulting definition is decidable. However, the bad news is that the definition does not work.

This time we have a very easy example of an intuitionistic proof of a universal statement $\forall x A(x)$ which is not a proof of $A(x)$ with free variable x. The example is provided again by Dummett, and consists in a proof of such a statement by induction:

> "Suppose that we have a proof of $A(0)$ and a proof of $\forall x(A(x)\rightarrow A(x+1))$, which we may suppose for simplicity to have been obtained by means of a free-variable proof of $A(x)\rightarrow A(x+1)$. Then, for each n, we can find a proof of $A(n)$. When $n=1$, we apply modus ponens to $A(0)$ and $A(0)\rightarrow A(1)$; when $n=2$, we first obtain $A(1)$ by the preceding modus ponens step, and then apply modus ponens again to $A(1)$ and $A(1)\rightarrow A(2)$; and so on." Dummett [1977], p. 14.

In this case we do have a *method* which transforms each number n into a proof of $A(n)$; but the result is not a 'uniform' schema, that only needs to be completed with the number in question, just as an application form is filled in with the name of the applicant. A proof with free variables may be divided into various options (e.g. whether the variable x is 0 or not), but in this case the number of these options is infinite. This is why we cannot talk of a simple 'proof with free variables'.

Indeed, the resulting proofs will be of different lengths according to the magnitude of n. As Dummett says: “there is no uniform proof-skeleton (except one which allows explicit appeal to induction)” ([1977], p. 14).

Such a method cannot be considered a simple ‘proof with free variable’ in any genuine understanding of this notion.

2.7.7. Conclusion. The argument presented in the preceding subsection seems strong enough to dismiss this attempt altogether, although recognizing that it was an interesting idea to be pursued.

CHAPTER 3

OTHER TOPICS

§3.1. The 'unintended' interpretations

3.1.1. Introduction. Besides the search for the intended interpretation of the logical operators, intuitionistic logic has been subjected to various other semantical investigations, which have led to a number of mathematical models for the intuitionistic predicate calculus. These models have an interest in their own right, among other things as efficient procedures for obtaining underivability results.

In the case of classical logic, for example, we can take the Beth-Smullyan method of *semantic* tableaux, which is highly effective for establishing the consistency of a finite set of sentences although it does not assign them -at least in principle- any plausible interpretation at all.

In any case, until the intended interpretation of the intuitionistic logical constants can be made fully rigorous, these models will remain the only available semantics that we can use in relation to the deductive systems, for example, for establishing soundness and completeness theorems.

3.1.2. Translations. In addition to this, we have a number of translations from intuitionistic logic into various other logical systems. Some of the most interesting ones are the translation into modal predicate logic (by Gödel [1933], developed in McKinsey and Tarski [1948], Fitting [1969] and Shapiro [1985]), and the translation into the logic of dialogues (by Lorenzen [1960]; for further developments and a survey see Felscher [1986]). A recent and particularly interesting one is the pragmatic interpretation of intuitionistic propositional logic by Dalla Pozza (in Dalla Pozza and Garola [1995]).

Some authors, adopting a classical point of view, have tried to use these translations for defending the idea that the conflict between classical and intuitionistic mathematics dissolves into a mere problem of interpretation. However, they dismiss the intuitionistic critique to classical mathematics, and have had little impact on intuitionistic mathematicians and current contributors to intuitionism.

3.1.3. Kleene's realizability. Kleene's realizability, which first appeared in Kleene [1945], deserves special mention. This notion was directly inspired by Hilbert and Bernays's analysis of the constructive import of an existential statement $\exists x A(x)$ as a partial communication of a more complete statement in which a particular number n with property A is given, or at least a method for finding such n (Hilbert and Bernays [1934], p. 32). Kleene generalized this idea to all logical operators (e.g. 'what *completes* $A \to B$ is an effective method by means of which we can transform whichever *completes* A into that which *completes* B') (Kleene [1945], pp. 109-110). This idea is very natural and very much in line with the operational interpretation, and with Heyting's explanations that a constructive statement demands that a construction is made. I shall not give full details of Kleene's definition because, as we are going to see, very few people regard it nowadays as carrying the intended meanings of the intuitionistic logical constants.

Kleene identified the notion of constructive function with that of partial recursive function. Then he took the Gödel numbers of these functions and constructed the definition as a numerical interpretation (to each statement we assign the Gödel number of the partial recursive function which realizes it, if there is one). As it turns out, the basic relation (*c* realizes *A*) is not decidable; in this it coincides with the operational interpretation; the two differ, however, in the treatment of $\exists$, since the number which realizes $\exists x A(x)$ 'includes' information not only on the *n* such that $A(n)$ -as it happens in the operational interpretation-, but also on the number which realizes $A(n)$).

Dummett has criticized the fact that the realization is not decidable, and, on those grounds, dismissed it as a semantical interpretation:

> "We cannot, of course, effectively decide, for a given number *n*, whether or not it is the Gödel number of a general recursive function, and so certainly cannot in general decide whether or not n **r** $\forall x A(x)$ [*n realizes* $\forall x A(x)$]. For this reason, the notion of realizability diverges very considerably from the intended meanings of the intuitionistic logical constants." (Dummett [1977], p. 320).

In addition to this, if we adopt a classical standpoint with respect to realizability, then all classically valid sentences appear to be realizable (in particular, it is very easy to show, for example, that, for example, $A \vee \neg A$ or $\neg\neg A \rightarrow A$ are realizable). This had already been noticed by Kleene [1945] (p. 114) and Griss [1953] (p. 11), and later reaffirmed with respect to later versions of this notion by Kleene in Kleene and Vesley [1965]:

> "Early in the investigations of 1945-realizability (since 1941), formulas were encountered whose realizability was only proved classically (...). In such a case, the realizability interpretation fails to exclude the formula's being provable intuitionistically, but on the other hand, we lack adequate grounds for affirming that it should hold intuitionistically (...). The situation is the same with the present notion of realizability." (p. 119).

Another problem is the identification of constructive functions with partial recursive functions, as pointed out here by van Dalen:

> "Realizability differs in nature from semantic interpretations on several accounts. (...) it heavily relies on Church's Thesis: 'each algorithm is a partial recursive function' (at least in motivation), which puts its stamp on the class of realizable sentences. This appears, e.g., from the fact that the first order version of Church's Thesis is realizable. Hence the realizability interpretation is not faithful." ([1979], pp. 137-138).

3.1.4. The range of interpretations of intuitionistic logic. The great variety of mathematical models for intuitionistic logic which are currently found in the literature can be classified into four main groups: topological models, algebraic models, Beth models and Kripke models.

Although most of them were originally proposed by classical mathematicians, and completeness -with respect to the intuitionistic predicate calculus- was proven by classical means, other versions and proofs have since been found using intuitionistic metamathematics only. Intuitionistic completeness results usually seem weaker if we compare them directly with their classical counterparts, but this is only natural if we consider the dramatic changes in the meaning of the results as well as in the proof methods.

Both Kripke models and Beth models allow a heuristic motivation very much in accordance with the intuitionistic conception of mathematical knowledge -and even with the verificationist interpretation. The interpretations of the other two groups are more properly called 'unintended': whether they could serve as a genuine semantics is out of the question.

In the present section I shall give a detailed definition of a Kripke model and I shall analyze to what extent it succeeds and to what extent it fails to capture the intended interpretation of the intuitionistic predicate calculus. Beth models -which are very similar- will be discussed more briefly.

This is a well-established point, so no originality will be required for our task -cf. in particular Dummett [1977], pp. 403-418.

3.1.5. Kripke models. Kripke models were first defined in Kripke [1965]. The material is very standard now. The following definition of a Kripke model contains a progression of stages, representing the continuous growth of mathematical knowledge. Each stage will be characterized by a basic background of mathematical facts known at that stage. Then, the growth of knowledge from one stage to the following will be strictly cumulative: all discoveries made at a particular stage will be preserved henceforth, that is, will be automatically incorporated into the basic knowledge of all later stages.

At each stage different possibilities of progress will be allowed according to the various mathematical discoveries that we can achieve at that stage. However, there will be no obligation to pass from one stage to another if new discoveries have not been made.

Let $\mathscr{L}$ be a first-order language without equality -as described in 0.6.1. A *Kripke model* $\mathfrak{K}$ for $\mathscr{L}$ consists of the following ingredients:

(a) A non-empty set U called the *universe* of $\mathfrak{K}$.

(b) A mapping that assigns to every constant c of $\mathscr{L}$ an object $\mathfrak{K}(c)$ of U.

(c) A non-empty set S whose members are called the *stages* of $\mathfrak{K}$.

(d) A (reflexive) partial order $\leq$ on S. If $s,s'\in S$ are different and such that $s\leq s'$ then we shall say that s' is a *later* stage than s; moreover, if there is no $r\in S$ such that $s\leq r\leq s'$ then we shall say that s' is an *immediate successor* of s. Finally, we let $s'\geq s$ mean $s\leq s'$.

(e) A mapping that assigns to each n-ary predicate symbol F and stage s, a subset $\mathfrak{K}_s(F)$ of U^n -i.e. a set of n-tuples of members of U- in such a way that for any two $s,s'\in S$ if $s\leq s'$ then $\mathfrak{K}_s(F)\subseteq\mathfrak{K}_{s'}(F)$.

Intuitively speaking the elements of $\mathfrak{K}_s(F)$ are those members of U^n which are, at stage s, *known* to be in the relation corresponding to F. This knowledge will be preserved in all later stages, since for any $s' \geq s$, $\mathfrak{K}_s(F) \subseteq \mathfrak{K}_{s'}(F)$.

At s the other members of U^n might be *known not* to be in that relation or might be *undecided*; but the difference between these two groups will not be reflected in the model.

A *Kripke valuation* $\mathscr{K}$ based on $\mathfrak{K}$ is a mapping that assigns to each variable x of $\mathscr{L}$ a member $\mathscr{K}(x)$ of U. Also, if c is any constant we put $\mathscr{K}(c)=\mathfrak{K}(c)$.

Moreover, if x is any variable and $u \in U$, we let $\mathscr{K}[x/u]$ be the valuation which assigns the object u to the variable x and is otherwise identical to $\mathscr{K}$. In any case, notice that since U is non-empty and therefore we know how to produce an element u of U, we also know how to construct at least one valuation based on $\mathfrak{K}$; namely, that in which all variables receive the value u.

3.1.6. Forcing. We now define the relation of a Kripke valuation $\mathscr{K}$ *forcing* a formula φ at a stage $s \in S$ (briefly $s \Vdash_{\mathscr{K}} \varphi$).

For atomic formulas, let F be an n-ary predicate symbol of $\mathscr{L}$ and $t_1,\ldots,t_n$ terms of $\mathscr{L}$:

$$s \Vdash_{\mathscr{X}} F(t_1,t_2,\ldots,t_n) \text{ if } \langle \mathscr{X}(t_1),\mathscr{X}(t_2),\ldots,\mathscr{X}(t_n)\rangle \in \mathfrak{K}_s(F).$$

Hence an atomic formula will be forced by a valuation $\mathscr{X}$ at a stage s when at that stage the objects that $\mathscr{X}$ assigns to the terms t_1, t_2, ..., t_n are known to be -in that order- in the relation corresponding to F.

Next, let φ and ψ be any formulas of $\mathscr{L}$, x any variable and s' also belong to S:

(a) $s \Vdash_{\mathscr{X}} \neg\varphi$ if $s' \nVdash_{\mathscr{X}} \varphi$ whenever $s' \geq s$;

(b) $s \Vdash_{\mathscr{X}} \varphi \rightarrow \psi$ if $s' \Vdash_{\mathscr{X}} \psi$ whenever $s' \geq s$ and $s' \Vdash_{\mathscr{X}} \varphi$;

(c) $s \Vdash_{\mathscr{X}} \varphi \vee \psi$ if $s \Vdash_{\mathscr{X}} \varphi$ or $s \Vdash_{\mathscr{X}} \psi$;

(d) $s \Vdash_{\mathscr{X}} \varphi \wedge \psi$ if $s \Vdash_{\mathscr{X}} \varphi$ and $s \Vdash_{\mathscr{X}} \psi$;

(e) $s \Vdash_{\mathscr{X}} \exists x\varphi$ if there is a $u \in U$ such that $s \Vdash_{\mathscr{X}[x/u]} \varphi$;

(f) $s \Vdash_{\mathscr{X}} \forall x\varphi$ if $s' \Vdash_{\mathscr{X}[x/u]} \varphi$ whenever $s' \geq s$ and $u \in U$.

If α is a sentence it is easy to see that its value at a given stage is invariant with respect to the valuation, and thus we let $s \Vdash_{\mathfrak{K}} \alpha$ (that is: $\mathfrak{K}$ *forces* the sentence α at stage s) be the case when $s \Vdash_{\mathscr{X}} \alpha$, where $\mathscr{X}$ is any valuation based on $\mathfrak{K}$.

Finally, we say that a formula φ of $\mathscr{L}$ is *enforceable* when there is a Kripke model $\mathfrak{K}$ for $\mathscr{L}$, a stage s and a valuation $\mathscr{K}$ based on $\mathfrak{K}$ such that $s \Vdash_{\mathscr{K}} \varphi$.

3.1.7. Kripke models in action: an example. The best way to illustrate how Kripke models work is with an example. Let $\mathscr{L}$ contain a unary predicate symbol F, and consider the sentence $\neg\forall x(F(x)\vee\neg F(x))$. This sentence is inconsistent classically, but not intuitionistically (that is: it might be added as an axiom to the intuitionistic predicate calculus without destroying its consistency).

Accordingly, there is a Kripke model for it: a Kripke model in which, at a given stage, $\neg\forall x(F(x)\vee\neg F(x))$ is forced. In fact it is very easy to find such a model.

We define a model $\mathfrak{K}$ whose universe is the set N of natural numbers and whose stages form an infinite linear sequence $S=\{s_0,s_1,s_2,...\}$.

Next we put for each $s_n \in S$, $\mathfrak{K}_{s_n}(F)=\{0,1,2,...,n\}$. Hence, at stage s_n we know that all numbers from 0 to n have the property corresponding to F; and at the following stage s_{n+1} we will discover that the number $n+1$ also has the property.

Finally, we let $\{x_0,x_1,x_2,...\}$ be variables of $\mathscr{L}$ and $\mathscr{K}$ be the valuation based on $\mathfrak{K}$ such that for each x_n we have $\mathscr{K}(x_n)=n$.

Then we reason as follows. At each stage s_n, the formula $F(x_{n+1})$ is not yet forced by $\mathscr{K}$, so $s_n \not\Vdash_{\mathscr{K}} F(x_{n+1})$. However, that formula will be known to be true at the next stage, and hence forced by $\mathscr{K}$ at that stage. Therefore -by 3.1.4(a)- $s_n \not\Vdash_{\mathscr{K}} \neg F(x_{n+1})$. Hence by 3.1.4(c) $s_n \not\Vdash_{\mathscr{K}} F(x_{n+1}) \vee \neg F(x_{n+1})$; and it follows at once -by 3.1.4(f)- that $s_n \not\Vdash_{\mathscr{K}} \forall x(F(x) \vee \neg F(x))$.

However, this happens *for all* s_n, so it will also be the case, in particular, for all later stages s_m for $m \geq n$. Hence by 3.1.4(a) again, $s_n \Vdash_{\mathscr{K}} \neg\forall x(F(x) \vee \neg F(x))$. Moreover, since $\neg\forall x(F(x) \vee \neg F(x))$ is a sentence,

$$s_n \Vdash_{\mathfrak{K}} \neg\forall x(F(x) \vee \neg F(x))$$

We have thus found that in fact *any* stage in this Kripke model forces $\neg\forall x(F(x) \vee \neg F(x))$.

3.1.8. Discussion. As we can see, Kripke models have, in common with the verificationist interpretation, that they adopt an epistemic point of view. It is not *how things are* which matters as much as *what the subject knows* (or *can* *prove*) about them. This is in plain accordance with the constructivist philosophy of mathematics.

Indeed, the clauses for $\wedge$, $\vee$ and $\exists$ which appear in this definition are exactly similar to those of Heyting's interpretation -if only we read $s \Vdash_{\mathfrak{K}} \alpha$ as 'we can prove α at s'. The major difference lies in the clauses for the 'difficult' operators, $\neg$, $\rightarrow$ and $\forall$.

This is clear from our example in the previous subsection. On the one hand the fact that $\neg\forall x(F(x) \vee \neg F(x))$ is so easily enforceable in a Kripke model illustrates the technical qualities of these kind of models for obtaining consistency results; and on the other, the nature of the model provided also illustrates what is left out in this type of semantics.

For after describing $\mathfrak{K}$ we still do not have a way of understanding $\neg\forall x(F(x) \vee \neg F(x))$ that permits us to assert it, simply because the model above does not ascribe any real meaning to the predicate symbol F. We end up without knowing any concrete number-theoretical predicate for which we can assert $\neg\forall x(F(x) \vee \neg F(x))$.

Thus, we end up not knowing the meaning of the whole sentence either.

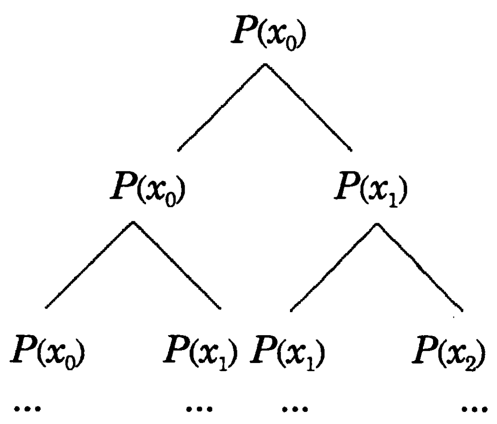
As it happens, it does not even follow from the model that there must exist such a predicate. What 𝔎 displays is a condition typical of a predicate for which we could assert the sentence in question -a condition which establishes what could be our cognitive relation with such predicate. However, this condition is not yet a *content* of the predicate.

Similarly, the clause corresponding to $\neg$ for instance, seems to be establishing a *consequence* of the meaning of $\neg$ rather than its content. This meaning makes it inevitable that if we get to know a sentence $\neg\alpha$ one day, we shall never discover α later; but clearly what is *meant* by uttering '$\neg\alpha$' must be something different -and it is. As Kreisel has written:

> "Kripke's interpretation is not regarded as an explanation or 'reduction' but as asserting properties of these logical operations obtained by reflection upon their meaning." ([1971], p. 146).

3.1.9. Beth models. Beth models are in exactly the same situation as Kripke models. In fact, the two of them are very similar even in the technical details; the main difference, roughly speaking, being that in a Beth model we are regularly forced to pass from one stage to another, whether we have discovered new facts or not. That is why in general Beth models look slightly more complicated than Kripke models.

For example, to account for our sentence $\neg\forall x(F(x)\vee\neg F(x))$ we would have to add after each stage an alternative route with no new discoveries, in case we are forced to move on before the next number has been checked out. Then, representing each stage by the last sentence decided, we would have



That is, we start by discovering that $F(x_0)$ holds -always under our valuation $\mathscr{K}$- and from then on we either discover that the next number also has the property, or reach a new stage without more information.

In any case the discussion above applies equally well also to these models.

§3.2. Some accusations of incoherence

3.2.1. Introduction. Part of the traditional defence of classical mathematics from the intuitionistic critique has rested on arguing that the intuitionists rely on too vague assumptions, and that its basic notions such as 'construction', or the intended meanings of the logical constants, are very obscure.

As I commented at the beginning of the thesis, some critics have gone further and claimed that the intuitionistic meanings of the logical constants are actually incoherent. That is: that they are not only obscure and vague, but that they are necessarily so, because behind them there is no coherent picture at all.

In fact, part of the motivation for addressing the problem of this thesis lies precisely in trying to determine whether these people are right or wrong; therefore, it is only fair that I devote a last section to discuss some of the attempts which have been produced in recent times to show that the intuitionistic logical operators are essentially incoherent.

I must stress that I shall only consider those criticisms of intuitionism which have tried to show that the meanings of the intuitionistic logical particles cannot be made precise (so that I remain faithful to the consideration of the topic of the thesis), but I shall not discuss general criticisms against intuitionism, of any other kind. Moreover, I shall only consider a few recent arguments, with no attempt to cover all of them, but rather, with the purpose of giving a taste of which lines these arguments usually take -I have chosen those which seemed most interesting to me. Also, I shall concern myself only with the intuitionistic logical constants as primarily intended, that is, as the logical operators of any other fragment of a natural language other than the mathematical one.

3.2.2. Hellman on the communication problem. Hellman [1989] has argued that the intuitionistic logical constants are insufficient to express certain basic facts about themselves (about their own status and behaviour) which are essential for the motivation of intuitionistic against classical mathematics. In particular, he focused on the impossibility of expressing intuitionistically the idea of absolute undecidability. He acknowledges that this is a fact well-known to the intuitionists (he refers on p. 60 to a quote from

Dummett [1977], p. 17 that a proof that a statement is absolutely unprovable amounts intuitionistically to a refutation of it).

Hellman stresses that without this notion the usual motivation for intuitionistic logic is not feasible:

> "Take for example intuitionism's stance on 'the law of excluded middle'. How does it motivate its refusal to accept this law (admittedly without having to claim to refute it)? Not by the paltry observation that there are propositions which we now can neither prove nor refute. (...) Now, I take it, what gives real force to intuitionism's stance is that this *may never* happen (...)." (p. 62).

However, he writes, this possibility is not expressible intuitionistically: indeed, the corresponding universal quantification 'for every stage of knowledge, there is at least one unsolved problem' would have to be read classically, because intuitionistically it would mean that we have a method to generate a new unsolved problem in every situation, which is obviously false and *not* what was intended.

He quotes Dummett [1977] to illustrate the fact that intuitionists, in their critique to classical reasoning, adopt such notions:

> "Since we can be virtually certain that the supply of such unsolved problems will never dry up, we can conclude with equal certainty that the general statement will never be intuitionistically

> provable. Such a recognition that a universally quantified statement is unprovable does not amount to a proof of its negation (...)." (p. 45 of Dummett [1977], quoted on pp. 63-64 of Hellman [1989]).

However, as Hellman remarks, the 'never' in this statement has to be read classically, or the paragraph would not make sense. In fact the second part of the last sentence of this quotation -which Hellman omits- is also very interesting:

> "Such a recognition (...) does not amount to a proof of its negation, since the proposition is not, as it stands, a theorem or even a mathematical proposition at all." (Dummett [1977], p. 45).

This means that Dummett realizes immediately that the proposition in question -that there will always be unsolved problems- implies a certain reification and that strictly speaking is not intuitionistically meaningful.

3.2.3. Discussion. Hellman's point is an interesting one, which can be found in other authors in somewhat different forms. Two things should be noticed here. First, that the type of motivation for intuitionism that Hellman criticizes is only needed in the context of a structuralist dominated ideology, but it is not essential for the *actual practice* of intuitionistic mathematics (Hellman would probably agree on this).

Secondly, I think that it is quite possible to reconstruct these motivations using strict intuitionistic language. In particular, the justification of the rejection of the law of excluded middle would have to be based *only* on the existence of particular instances of unsolved mathematical problems, and not on the plausibility that there will *always* be some, because this possibility implies a reification which makes sense classically, but not intuitionistically. Indeed, each unsolved mathematical problem is a counterexample to the law of excluded middle as intuitionistically interpreted.

Hellman is right in my opinion in pointing out that intuitionists set a bad example by using classical language to motivate the rejection of classical mathematics, and that they should be more careful; but this critique is not so lethal.

3.2.4. Hossack on the meaning of negation. In [1990] Hossack presents an argument against the intuitionistic definition of negation which is somewhat related to the argument by Hellman that we have just seen. Hossack's main contention in this paper is that, unlike the classical case, an intuitionistic user of negation has to have thoughts which are not expressible in the corresponding intuitionistic object language.

There are essentially two of these thoughts. The first one is the thought that the assertability conditions for a given statement at a particular time do not obtain, that is: that we do not have a proof of it. Hossack relies heavily on the decidability of the proof relation, an assumption which he takes from Dummett. Hossack reasons as follows:

> "Therefore they [the intuitionists] have to know for each *A* what it would be for a construction to be a proof of *A*. But (...) they cannot know what it would be for a construction to be a proof of *A*, unless they also know what it would be for a construction not to be a proof of *A*." (Hossack [1990], p. 215).

If this happens with each particular construction we can generalize:

> "Thus they cannot know what it is for the assertability conditions to be fulfilled, unless they also know what it is for the assertability conditions to fail to be fulfilled." (p. 215).

Then:

> "The thought that the assertability conditions do obtain can in a sense be expressed by the proposition itself. But there is no sentence to express the thought that they do not obtain. The only possible candidate for such a sentence is the negation of the proposition, and that is already reserved for a different use.

> "This shows that there are thoughts that a competent user of L needs to be able to have to use the language correctly, but which cannot themselves be expressed in L [where L is a mathematical language to be interpreted in terms of assertability conditions]." (p. 215).

I partly agree with Hossack here. In fact this observation is not new. Heyting [1956] had already noticed it:

> "Every mathematical assertion can be expressed in the form: 'I have effected the construction A in my mind'. The mathematical negation of this assertion can be expressed as 'I have effected in my mind a construction B, which deduces a contradiction from the supposition that the construction A were brought to an end', which is again of the same form. On the contrary, the factual negation of the first assertion is: 'I have not effected the construction A in my mind'; this statement has not the form of a mathematical assertion." (p. 19).

Indeed, the point here is that the statement that the assertability conditions do not obtain is not a mathematical statement proper. This does not mean that we cannot express it at all; on the contrary, the statement, for example, 'I do not have a proof of A' is perfectly acceptable from the constructive standpoint (provided that we agree on the decidability of the proof relation, which is one of Hossack's premises). However, it is not a mathematical statement proper, and hence there is no reason why we should want to have a *logical* or *mathematical* operator which corresponds to it.

According to Hossack:

> "Note the sharp distinction between intuitionist and classical negation here. The user of the classical negation also needs to be able to have the thought that the truth conditions for A do not obtain. But this thought is always expressible in the classical language itself, if it is equipped with a sign for classical negation. Thus the user of the classical language, unlike the user of L, does not need to have any thoughts that cannot be expressed in the language." (p. 216).

However, if we consider, in the classical case, the respective thought that 'I have not proved A' or 'I do not know whether A is the case', we see that the speaker needs to be able to have these thoughts to be able to use A correctly (in particular, to assert it adequately or refrain from asserting it), but they cannot be expressed with a mathematical symbol.

Of course these type of thoughts will not be needed in the mere process of understanding an utterance of a given sentence, neither in the classical nor in the intuitionistic case; but if we consider the mastery of the language as a whole, then these type of thoughts are probably needed in both cases.

3.2.5. More about Hossack on negation. The second thought to which Hossack refers is that the statement '0=1' -which appears often in the definition of ¬- is an unprovable statement; and relatedly, the thought that it is impossible to prove both a statement and its negation (assuming $\neg A$ is defined as $A \rightarrow 0=1$):

> "But we cannot express in L our knowledge that it is absurd to suppose that 0=1 can be proved [where L is as before]. The best we can do within L is to say *not*(0=1), which is just $0=1 \rightarrow 0=1$. (...) Thus the knowledge that A and $\neg A$ are incompatible is not something that one can learn just from the rules of L by reasoning within L.
>
> "(...) We can imagine someone who knows only the assertability conditions believing themselves to have proved both A and $\neg A$, if they thought they had constructed a proof of each proposition. In thinking this, they would be breaking none of the assertability rules of L (...) This is again in sharp contrast with the classical case. To be credited with a grasp of the truth conditions of negation, the user of the classical negation must treat a proposition and its negation as incompatible. If someone seriously asserted both A and $\neg A$ we would simply conclude that they had not grasped the truth conditions concerned." (p. 217).

However, this is not fair, because it is impossible to know the constructive meaning of 0=1 (that is: in terms of basic calculations) without realizing that it is false -i.e. that there can be no proof of it.

Hossack is right in that this is equivalent to claiming that the whole of intuitionistic mathematics is consistent, but this is something quite obvious in the case of intuitionistic mathematics. On the other hand, he is also right in that a statement of absolute unprovability (as we have seen before) implies a classical claim and a certain reification, and hence if we try to solve it by a semantic ascent to a metalanguage, sooner or later we will have to find classical negation at some level (which would destroy the whole project) (p. 218).

However, in that case the fact that intuitionistically this thought is not expressible is more a virtue than a defect.

My conclusion is: Hossack is right that there are thoughts classically *obvious* that constructivists simply cannot make, because they reject reification; but they are not strictly necessary for their practice of mathematics.

3.2.6. Hossack on the meaning of the quantifiers. In a second paper [1992] Hossack articulates similar arguments against the intuitionistic quantifiers. In particular, he claims that given the clause for the intuitionistic $\forall$, the speaker should necessarily have an explicit knowledge of the metalanguage.

Hossack focuses on the status of the extra-clauses, and how the speaker is to be attributed knowledge of the relevant condition:

> "The constructivist theory (...) demands that object language speakers who grasp the meaning of 'all n are P' should *recognize* of a construction *that for all n* it yields a proof of $P(n)$. (...)
>
> "What has to be recognized here is the obtaining of a certain state of affairs described by a sentence of the metalanguage." (p. 85).

Then, Hossack argues that if this knowledge is to be explicit knowledge at the level of the metalanguage, another metalanguage would be required for the metalanguage, and we would end up in an infinite regress. However, he writes:

> "It may be replied that constructivism does not need the assumption that the ability to recognize that a construction is a proof should take the form of explicitly thinking this thought in an appropriate language." (p. 86).

In fact, previously he himself had referred to a theory of propositional attitudes to dissolve the circularity of a classical theory and complement it:

> "The success of a classicist theory of meaning will then turn on its containing a second component, a theory of propositional attitudes, which explains what it is for a person to grasp an abstract object of this sort." (p. 85).

However, this, alleges Hossack, is not available in the case of the constructivist quantifiers:

> "Thus the explanatory force of the constructivist truth theory can turn on its appeal to recognitional capacities possessed by speakers quite independently of any linguistic competence.
>
> "This defence is plausible in the case of the connectives of sentential logic, which is decidable both classically and constructively. (...) But there can be no hope of a similar account in the case of quantification, for no mechanical device can capture our conception of generality." (p. 86).

However, here Hossack makes a fallacy of equivocation. Because the 'fact' to be recognized here is not the universal fact itself, but the fact that a given construction *proves* the universal fact. These are two very different things. Then he writes:

> "Suppose we had a device with a detector which checked items for some property, and another detector causally sensitive to items that are still unexamined. The device could tell us if $\forall x P(x)$ were false. If $\forall x P(x)$ were true it could tell us so if there were only finitely many cases for it to examine. But our judgements about the truth of $\forall x P(x)$ go beyond the deliverances of the device. For we will say $\forall x P(x)$ is true even if the device itself never returns an answer. For if it never will, that can only be because $\forall x P(x)$ is true. It is therefore clear that what the device says does not capture our conception of generality. Nor is there a superior device which could fill this role, as the recursive unsolvability of the halting problem shows." (p. 87).

However, the point was not detecting $\forall x P(x)$, but detecting whether something was a proof of it; and for that reason Hossack has not shown that we cannot have a theory of propositional attitudes which correspond to it and explain it.

In the rest of the paper Hossack argues that the natural language quantifiers are not constructive, something which could be true, and does not pose a problem to us.

CONCLUSION

The problem which I have addressed in this thesis (the intended interpretation of the intuitionistic first-order logical operators) is an extremely difficult one. After more than sixty years of research, and the contributions of some very eminent philosophers and mathematicians, no solution to it has been found which is wholly satisfactory.

In this thesis I have argued in favour of the interpretation which I have called the 'operational' interpretation. This interpretation is essentially a reformulation of Kolmogorov's and Heyting's interpretations, where the notions of 'proof' and of 'solution to a problem' have been replaced by that of 'performing', and the appeal to proofs from premises has been completely eliminated.

I have explained that the operational interpretation is in a better position than any of its competitors. In particular, I have argued that if one wants to dispense with Kreisel's extra-clauses, the one cannot maintain the concept of 'proof' as the basic concept of the semantic definition of the logical constants; therefore Heyting's interpretation is untenable, unless we replace in it the concept of 'proof' by a different notion. Moreover, I have also shown that Kreisel's interpretation as it stands is redundant, and that in order to eliminate its redundancy we must reformulate this interpretation in a way which makes it rely on the operational interpretation; I have therefore concluded that the operational interpretation is more basic, and for this reason it is more adequate as a fundamental semantical explanation of the logical constants. Furthermore, I have argued that the interpretations in terms of 'proofs from premises' that have been put forward so far, are as they stand defective, and that we cannot resolve their deficiencies while preserving at the same time the inductive structure of the definition; and finally, I have shown -following Dummett- that the interpretation in terms of 'proofs with free variables', which is closely related to the interpretation in terms of 'proofs from premises', is inadequate as well.

Despite all this, however, I think that it would be premature to conclude that the operational interpretation is the final solution to our problem. Indeed, this interpretation also carries some problems with it; in particular, the fact that its basic semantic relation -that of 'performing'- is not decidable, confers a certain non-constructive character to the operational interpretation. I have suggested that this non-constructive character is so small that the interpretation could still be acceptable from the intuitionistic point of view, but this a difficult issue, and I am far from being certain.

The debate remains, thus, essentially open, for new arguments to come -or perhaps, new modifications on the existing interpretations. The difficulties of the project might throw a certain pessimism upon us, but we have to bear in mind that all the attempts that have been made to show that the problem is intrinsically unsolvable have also failed, as I have explained in the last section of this thesis.

Finally, I would like to remark that if a problem such as the present one remains alive today, is because it is not only one of extreme difficulty, but also one of extreme importance. Indeed, the whole philosophical basis of intuitionism crucially depends on the possibility of clarifying its most fundamental notions, and among them, its logical concepts. If this clarification is finally attained, intuitionism -or constructivism- would constitute no doubt a much more serious alternative to the other philosophies of mathematics.

BIBLIOGRAPHY *

Agazzi, E. (ed.)
[1981] *Modern Logic. A Survey*, Reidel, Dordrecht.

Artemov, S.
[1990] Kolmogorov's logic of problems and a provability interpretation of intuitionistic logic, in Parikh [1990], pp. 257-272.

Baayen, P.C., D. van Dulst and J. Oosterhoff (eds.)
[1979] *Proceedings of the Bicentenial Congress Wiskundig Genootschap*, Mathematisch Centrum, Amsterdam.

Barwise, K.J. (ed.)
[1977] *Handbook of Mathematical Logic*, North-Holland, Amsterdam.

Barwise, K.J., H.J. Keisler and K. Kunen (eds.)
[1980] *The Kleene Symposium*, North-Holland, Amsterdam.

* I first mention the edition that I have mainly used, and to which any quotation in the text would be referred. In the case where this is not the original edition, I mention the latter afterwards; unless otherwise stated, it is to this original -earliest- publication that the date between square brackets always corresponds.

Beeson, M.J.

[1981] Formalizing constructive mathematics: why and how?, in Richman [1981], pp. 146-190.

[1982] Problematic principles in constructive mathematics, in van Dalen, Lascar and Smiley [1982], pp. 11-55.

[1985] *Foundations of Constructive Mathematics*, Springer, Berlin.

[1988] Computerizing mathematics: logic and computation, in Herken [1988], pp. 191-225.

[1990] Some theories conservative over intuitionistic arithmetic, in Sieg [1990], pp. 1-15.

[1993] Constructivity in the nineties, in Czermack [1993], pp. 127-144.

Bell, J.L., and M. Machover

[1977] *A Course in Mathematical Logic*, North-Holland, Amsterdam.

Benacerraf, P., and H. Putnam (eds).

[1983] *Philosophy of Mathematics. Selected Readings* (2nd rev. ed.), Cambridge University Press, New York. First ed. published by Prentice-Hall, Englewood Cliffs, New Jersey, NJ, in 1964.

Bernardi, C. and P. Pagli (eds.)

[1985] *Atti degli Incontri in Lògica Matemàtica*, vol. **2**, Università di Siena, Dipartimento di Matemàtica, Scuola di Specializzazione in Lògica Matemàtica, Siena (Italy).

Beth, E.W.
[1959] *The Foundations of Mathematics*, North-Holland, Amsterdam. 2nd ed. published in 1965.

Beth, E.W., H.J. Pos and H.J.A. Hollack (eds.)
[1949] *Library of the 10th International Congress of Philosophy*, vol. **2**, L. J. Veen, Amsterdam.

Bibel, W. (ed.)
[1980] *5th Conference on Automated Deduction*, Springer, Berlin.

Bishop, E.A.
[1967] *Foundations of Constructive Analysis*, McGraw-Hill, New York. 2nd rewritten ed. is Bishop and Bridges [1985].
[1970] Mathematics as a numerical language, in Myhill, Kino and Vesley [1970], pp. 53-71.

Bishop, E.A., and D.S. Bridges
[1985] *Constructive Analysis*, Springer, Berlin. 2nd rewritten ed. of Bishop [1967].

Boffa, M., D. van Dalen and K. McAloon (eds.)

[1979] *Proceedings of the Logic Colloquium '78*, North-Holland, Amsterdam.

Bridges, D.S., and F. Richman

[1987] *Varieties of Constructive Mathematics*, Cambridge University Press, Cambridge.

Brouwer, L.E.J.

[1907] On the foundations of mathematics (PhD dissertation), in Brouwer [1975], pp. 11-101. First published in Dutch as *Over de Grondslagen der Wiskunde*, by Maas and van Suchtelen, Amsterdam.

[1908] The unreliability of logical principles, in Brouwer [1975], pp. 107-111. First published in Dutch in *Tijdsch Voor Wijsbegeerte* **2**, pp. 152-158.

[1923] On the significance of the principle of excluded middle in mathematics, especially in function theory, in van Heijenoort [1967], pp. 335-345. First published in Dutch, in *Wis- en Natuurkundig Tijdschrift* **2**, pp. 1-7.

[1925] Zur Begründung der intuitionistischen Mathematik (I), in Brouwer [1975], pp. 301-314. First published in *Mathematische Annalen* **93**, pp. 244-257.

[1927] On the domains of definition of functions, in van Heijenoort [1967], pp. 457-463. Extract of a longer version first published in German, in *Mathematische Annalen* **97**, pp. 60-75.

[1928] Intuitionistic reflections on formalism, in van Heijenoort [1967], pp. 490-492. Extract of a longer version first published in German, in *Koninklijke Nederlandse Akademie van Wetenschappen. Proceedings of the Section of Sciences* **31**, pp. 374-379.

[1949] Conciousness, philosophy and mathematics, in Benacerraf and Putnam [1983], pp. 90-96. First published in Beth, Pos and Hollack [1949], pp. 1235-1249.

[1950] Remarques sur la notion d'ordre, in Brouwer [1975], pp. 499-500. First published in French, in *Comptes Rendus Hebdomadaire des séances de l'Académie des Sciences, Paris* **230**, pp. 263-265.

[1955] The effect of intuitionism on classical algebra and logic, in Brouwer [1975], pp. 551-554. First published in *Proceedings of the Royal Irish Academy* **A57**, pp. 113-116.

[1975] *Collected Works*, vol. **1** (*Philosophy and Foundations of Mathematics*), ed. by A Heyting, North-Holland, Amsterdam.

[1981] *Brouwer's Cambridge Lectures on Intuitionism*, ed. by D. van Dalen, Cambridge University Press, Cambridge.

Cohen, L.J., J. Łos, H. Pfeiffer and K.-P. Podewski (eds.)

[1982] *Logic, Methodology and Philosophy of Science*, vol. **6**, North-Holland, Amsterdam.

Colloque International de Logique, CNRS, Paris, 1977.

Cozzo, C.

[1994] What can we learn from the paradox of knowability?, *Topoi* **13**, pp. 71-78.

Crossley, J., and M.A.E. Dummett (eds.)

[1965] *Formal Systems and Recursive Functions*, North-Holland, Amsterdam.

Czermak, J. (ed.)

[1993] *Philosophy of Mathematics. Proceedings of the 15th International Wittgenstein-Symposium*, vol. **1**, Hölder-Pichler-Tempsky, Vienna.

Dalla Pozza, C. and C. Garola

[1995] A pragmatic interpretation of intuitionistic propositional logic, *Erkenntnis* **43**, pp. 81-109.

Dalen, D. van

[1973] Lectures on intuitionism, in Mathias and Rogers [1973], pp. 1-94.

[1979] Interpreting intuitionistic logic, in Baayen [1979], pp. 133-148.

[1983] *Logic and Structure* (2nd rev. ed.), Springer, Berlin.

[1986] Intuitionistic logic, in Gabbay and Guenthner [1986], pp. 225-339.

Dalen, D. van, D. Lascar and T.J. Smiley (eds.)

[1982] *Proceedings of the Logic Colloquium '80*, North-Holland, Amsterdam.

Diller, J., and A.S. Troelstra

[1984] Realizability and intuitionistic logic, *Synthese* **60**, pp. 253-282.

Drucker, Th. (ed.)

[1991] *Perspectives on the History of Mathematical Logic*, Birkhäuser, Boston.

Dummett, M.A.E.

[1959] Wittgenstein's philosophy of mathematics, in Dummett [1978], pp. 166-185. First published in *Philosophical Review* **68**, pp. 324-348.

[1963] The philosophical significance of Gödel's theorem, in Dummett [1978], pp. 186-201. First published in *Ratio* **5**, pp. 140-155.

[1969] The reality of the past, in Dummett [1978], pp. 358-374. First published in *Proceedings of the Aristotelian Society* **69**, pp. 239-258.

[1973] The justification of deduction, in Dummett [1978], pp. 290-318. First published as a pamphlet by the British Academy, London.

[1975] The philosophical basis of intuitionistic logic, in Benacerraf and Putnam [1983], pp. 97-129. First published in Rose and Shepeherdson [1975], pp. 5-40.

[1976] What is a theory of meaning? (II), in Evans and McDowell [1976], pp. 67-137.

[1977] *Elements of Intuitionism*, Clarendon Press, Oxford.

[1978] *Truth and Other Enigmas*, Duckworth, London.

[1980] Comments on professor Prawitz's paper, in von Wright [1980], pp. 11-18.

[1982] Realism, *Synthese* **52**, pp. 55-112.

[1987] Reply to Prawitz, in Taylor [1987], pp. 281-286.

[1991] *The Logical Basis of Metaphysics*, Duckworth, London.

Edwards, J.

[1990] Atomic realism, intuitionistic logic and Tarskian truth, *Philosophical Quarterly* **40**, pp. 13-26.

Evans, G., and J. McDowell (eds.)

[1976] *Truth and Meaning*, Clarendon Press, Oxford.

Feferman, S.

[1979] Constructive theories of functions and classes, in Boffa, van Dalen and McAloon [1979], pp. 159-224.

Felscher, W.

[1986] Dialogues as a foundation for intuitionistic logic, in Gabbay and Guenthner [1986], pp. 341-372.

Fenstad, J.E. (ed.)

[1971] *Proceedings of the Second Scandinavian Logic Symposium*, North-Holland, Amsterdam.

Fitting, M.C

[1969] *Intuitionistic Logic, Model Theory and Forcing*, North-Holland, Amsterdam.

Fraenkel, A.A., Y. Bar-Hillel and A. Levy

[1973] *Foundations of Set Theory* (2nd rev. ed.), North-Holland, Amsterdam. First ed. published in 1958.

Franchella, M.

[1994] Brouwer and Griss on intuitionistic negation, *Modern Logic* 4, pp. 256-265.

Freudental, H.

[1937] Zur intuitionistischen Deutung logischen Formeln, *Compositio Mathematica* 4, pp. 112-116.

Gabbay, D.

[1981] *Semantical Investigations in Heyting's Intuitionistic Logic*, Reidel, Dordrecht.

Gabbay, D., and F. Guenthner (eds.)

[1986] *Handbook of Philosophical Logic*, vol **3** (*Alternatives to Classical Logic*), Reidel, Dordrecht.

Gentzen, G.

[1935] Investigations into logical deduction (I and II), in Gentzen [1969], pp. 69-131. First published in German, in *Mathematische Zeitschrift* **39**, pp. 176-210 and 405-431.

[1969] *Collected Papers*, ed. by M.E. Szabo, North-Holland, Amsterdam.

George, A.

[1994] Intuitionism and the poverty of the inference argument, *Topoi* **13**, pp. 79-82.

Ghose, A.

[1978] Different notions of constructivity -their ontology, *Dialectica* **32**, pp. 245-253.

Goad, C.A.

[1980] Proofs as descriptions of computation, in Bibel [1980], pp. 39-52.

Gödel, K.

[1933] Eine Interpretation des intuitionistischen Aussagenkalküls, *Ergebnisse eines mathematischen Kolloquiums* 4, pp. 39-40.

Goodman, N.D.

[1970] A theory of constructions equivalent to arithmetic, in Myhill, Kino and Vesley [1970], pp. 101-120.

[1973] The arithmetic theory of constructions, in Mathias and Rogers [1973], pp. 274-298.

Griss, G.F.C.

[1946] Negationless intuitionistic mathematics, *Indagationes mathematicae* **8**, pp. 675-681.

[1955] La mathématique intuitionniste sans negation, *Nieuw Archiev voor Wiskunde (3)*, **3**, pp. 134-142.

Haack, S.

[1974] *Deviant Logics*, Cambridge University Press, Cambridge.

Hand, M.

[1992] Classical and intuitionistic negation, *Southwest Philosophy Review* **8**, pp. 157-164.

[1993] Negations in conflict, *Erkenntnis* **38**, pp. 115-129.

Heijenoort, J. van (ed.)

[1967] *From Frege to Gödel. A Source Book in Mathematical Logic 1879-1931*, Harvard University Press, Cambridge, MA.

Hellman, G.

[1989] Never say "never"! On the communication problem between intuitionism and classicism, *Philosophical Topics* **17**, pp. 47-67.

[1989a] *Mathematics without Numbers*, Oxford University Press, Oxford.

Herken, R. (ed.)

[1988] *The Universal Turing Machine: a half-century Survey*, Clarendon Press, Oxford.

Heyting, A.

[1930] Sur la logique intuitionniste, *Académie Royale de Belgique. Bulletin de la Classe des Sciences (5)* **16**, pp. 957-963.

[1931] The intuitionistic foundations of mathematics, in Benacerraf and Putnam [1983], pp. 52-65. First published in German, in *Erkenntnis* **2**, pp. 196-115.

[1934] *Le fondements des Mathématiques. Intuitionisme. Théorie de la Démonstration*, Gauthier-Villars, Paris, 1955. Expanded edition of *Mathematische Grundlagenforschung. Intuitionism. Beweistheorie*, Springer, Berlin.

[1937] Bermerkungen zu dem Aufsatz von herrn Freudental "Zur intuitionistischen Deutung logische Formeln", *Compositio Mathematica* 4, pp. 117-118.

[1956] *Intuitionism. An Introduction*, North-Holland, Amsterdam. 2nd rev. ed. published in 1966, and 3rd rev. ed. in 1971.

[1958] Intuitionism in mathematics, in Klibansky [1958], pp. 101-115.

[1959] (ed.) *Constructivism in Mathematics*, North-Holland, Amsterdam.

[1961] Remarques sur le constructivism, *Logique et Analyse* **4**.

[1968] Intuitionism in mathematics, in Klibansky [1968], pp. 316-323.

[1974] Intuitionistic view on the nature of mathematics, *Synthese* **27**, pp. 79-91.

Hilbert, D. and P. Bernays

[1934] *Grundlagen der Mathematik*, vol. **1**, Springer, Berlin.

Hintikka, J., I. Niiniluoto and E. Saarinen (eds.)

[1979] *Essays on Mathematical and Philosophical Logic*, Reidel, Dordrecht.

Hossack, K.G.

[1990] A problem about the meaning of intuitionistic Negation, *Mind* **99**, pp. 207-219.

[1992] Constructivism and quantifiers, *Aristotelian Society Supplements* **66**, pp. 79-97.

Hyland, J.M.E.

[1982] Applications of constructivity, in Cohen, Łos, Pfeiffer and Podewski [1982], pp. 145-152.

Infinitistic Methods. Proceedings of the Symposium of Foundations of mathematics, Warsaw 2-9 September 1959, Pergamon Press, Oxford, 1961.

Kleene, S.C.

[1945] On the interpretation of intuitionistic number theory, *Journal of Symbolic Logic* **10**, pp. 109-124.

[1957] Realizability, in Heyting [1959], pp. 285-289. First published in *Summaries of Talks presented at the Summer Institute for Symbolic Logic. July 1957, Ithaca, New York, NY*, pp. 100-104.

[1973] Realizability: a retrospective survey, in Mathias and Rogers [1973], pp. 95-112.

Kleene, S.C., and R.E. Vesley

[1965] *The Foundations of Intuitionistic Mathematics. Especially in Relation to Recursive Functions*, North-Holland, Amsterdam.

Klibansky, R. (ed.)

[1958] *Philosophy in the Mid-Century. A Survey*, vol. **1** (*Logic and Philosophy of Science*), La Nuova Italia, Florence (Italy).

[1968] *Contemporary Philosophy*, vol. **1** *(Logic and Foundations of Mathematics)*, La Nuova Italia, Florence (Italy).

Kolmogorov, A.N.

[1932] Zur Deutung der intuitionistischen Logik, *Mathematische Zeitschrift* **35**, pp. 58-65.

Kreisel, G.

[1961] Set-theoretical problems suggested by the notion of potential totality, in *Infinitistic Methods*, pp. 103-140.

[1962] Foundations of intuitionistic logic, in Nagel, Suppes and Tarski [1962], pp. 198-210.

[1965] Mathematical logic, in Saaty [1965], pp. 95-195.

[1970] Church's thesis: a kind of reducibility axiom for constructive mathematics, in Myhill, Kino and Vesley [1970], pp. 121-150.

[1971] A survey of proof theory (II), in Fenstad [1971], pp. 109-170.

[1979] Some facts from the theory of proofs and some fictions from general proof theory, in Hintikka, Niiniluoto and Scarinen [1979], pp. 3-23.

Kripke, S.A.

[1965] Semantical analysis of intuitionistic logic, in Crossley and Dummett [1965], pp. 92-130.

Laudet, M., D. Lancombe, D. Nolin and M. Schüttzenberger (eds.)

[1970] *Symposium on Automatic Demonstration. December 1970. Rocquencourt*, Springer, Berlin.

Lenzen, W.

[1991] What is (or at least it appears to be) wrong with intuitionistic logic?, in Schurz [1991].

Lévy, P.

[1926] Critique de la logique empirique, *Revue de Métaphysique et de Morale* **33**, pp. 545-551.

Lolli, G. G. Longo and G. Marcja (eds.)

[1984] *Proceedings of the Logic Colloquium '82*, North-Holland, Amsterdam.

Long, C.T.

[1987] *Elementary Introduction to Number Theory* (3rd ed.), Prentice-Hall, Englewood Cliffs, New Jersey, NJ.

López-Escobar, E.G.K.

[1981] Equivalence between semantics for intuitionism (I), *Journal of Symbolic Logic* **46**, pp. 773-780.

Lorenzen, P.

[1960] Logik und Agon, in Lorenzen and Lorenz [1978], pp. 187-194. First published in *Atti del XII Congresso Internazionale di Filosofía* vol. **4**, Sansoni, Florence (Italy), 1960, pp. 187-194.

Lorenzen, P. and K. Lorenz (eds.)

[1978] *Dialogische Logik,* Wissenschaftliche Buchgesellschaft, Darmstad.

Luo, Z.

[1994] *Computation and Reasoning,* Clarendon Press, Oxford.

Machover, M.

[1983] Towards a new philosophy of mathematics, *British Journal for the Philosophy of Science* **34**, pp. 1-11.

Macintyre, A., L. Pacholski and J. Paris (eds.)

[1978] *Proceedings of the Logic Colloquium '77,* North-Holland, Amsterdam.

Marcus, R.B., G.J.W. Dorn and P. Weingartner (eds.)

[1986] *Logic, Methodology and Philosophy of Science VII,* North-Holland, Amsterdam.

Markov, A.A.

[1970] On the logic of constructive mathematics, *Moscow University Mathematical Bulletin* **25/2**, pp. 45-60. Original version published the same year in Russian, in *Vestnik Moskovskogo Universiteta (I). Matematika Mekhanika* **25/2**, pp. 7-29.

Martin-Löf, P.

[1975] An intuitionistic theory of types: predicative part, in Rose and Shepherdson [1975], pp. 73-118.

[1982] Constructive mathematics and computer programming, in Cohen, Łos, Pfeiffer and Podewski [1982], pp. 153-175.

[1984] *Intuitionistic Type Theory* (Notes ed. by Giovanni Sambin), Bibliopolis, Naples (Italy).

[1985] On the meaning of the logical constants and the justification of the logical laws, in Bernardi and Pagli [1985], pp. 203-281.

[1987] Truth of a proposition, evidence of a judgement, validity of a proof, *Synthese* **73**, pp. 407-420.

Martino, E., and Usberti, G.

[1994] Temporal and atemporal truth in intuitionistic mathematics, *Topoi* **13**, pp. 83-92.

Mathias, A.R.D., and H. Rogers (eds.)

[1973] *Cambridge Summer School in Mathematical Logic*, Springer, Berlin.

Mc Carthy, D.C.

[1994] On theorems of Gödel and Kreisel: completeness and Markov's principle, *Notre Dame Journal of Formal Logic*, **35**, pp. 99-107.

McGuiness, B., and G. Oliveri (eds.)

[1994] *The Philosophy of Michael Dummett*, Kluwer Academic, Dordrecht.

Mc Kinsey, J.C.C. and A. Tarski

[1948] Some theorems on the sentential calculi of Lewis and Heyting, *Journal of Symbolic Logic* **13**, pp. 1-15.

Müller, G.H. (ed., in collaboration with W. Lenski)

[1987] *Ω-Bibliography of Mathematical Logic*, vol. **6** (*Proof Theory and Constructive Mathematics*), vol. especially ed. by D. van Dalen, J.E. Kister and A.S. Troelstra, Springer, Berlin.

Myhill, J.

[1966] Notes towards an axiomatization of intuitionistic analysis, *Logique et Analyse* **35**, pp. 280-297.

[1968] The formalization of intuitionism, in Klibansky [1968], pp. 324-341.

[1975] Constructive set theory, *Journal of Symbolic Logic* **40**, pp. 347-382.

[1985] Intensional set theory, in Shapiro [1985a], pp. 47-61.

Myhill, J., A. Kino and R.E. Vesley (eds.)

[1970] *Intuitionism and Proof Theory*, North-Holland, Amsterdam.

Nagel, E., P. Suppes and A. Tarski (eds.)

[1962] *Logic, Methodology and Philosophy of Science*, vol. **1**, Stanford University Press, Stanford, CA.

Pagin, P.

[1994] Knowledge of proofs, *Topoi* **13**, pp. 93-100.

Parikh, R.J.

[1990] (ed.) *Theoretical Aspects of Reasoning about Knowledge*, Morgan Kaufmann, San Mateo, CA.

Petkov, P.P. (ed.)

[1990] *Mathematical logic. Proceedings of the Heyting '88 Summer School*, Plenum Press, New York.

Prawitz, D.

[1965] *Natural Deduction. A Proof-theoretical Study*, Almqvist and Wiksell, Stockholm.

[1971] Ideas and results in proof theory, in Fenstad [1971], pp. 237-309.

[1973] Towards a foundation of a general proof theory, in Suppes, Henkin, Joja and Moisil [1973], pp. 225-250.

[1974] On the idea of a general proof theory, *Synthese* **27**, pp. 63-77.

[1977] Meaning and proofs: on the conflict between classical and intuitionistic logic, *Theoria* **43**, pp. 1-40.

[1979] Proofs and the meaning and completeness of the logical constants, in Hintikka, Niiniluoto and Saarinen [1979], pp. 25-40.

[1980] Intuitionistic logic: a philosophical challenge, in von Wright [1980], pp. 1-11.

[1985] Remarks on some approaches to the concept of logical consequences, *Synthese* **62**, pp. 153-171.

[1985a] Normalization of proofs in set theory, in Bernardi and Pagli [1985], pp. 357-371.

[1987] Dummett on a theory of meaning and its impact on logic, in Taylor [1987], pp. 117-165.

[1987a] Some remarks on verificationist theories of meaning, *Synthese* **73**, pp. 471-477.

[1994] Meaning theory and anti-realism, in McGuiness and Olivery [1994], pp. 292-298.

Putnam, H.

[1975] *Mathematics, Matter and Method (Philosophical Papers*, vol. **1**), Cambridge University Press, Cambridge.

Quine, W.v.O.

[1970] *Philosophy of Logic*, Prentice-Hall, Englewood-Cliffs, New Jersey, NJ.

Richman, F.

[1981] (ed.) *Constructive Mathematics. Proceedings of the New Mexico State Conference*, Springer, Berlin.

Rose, G.F.

[1953] Propositional calculus and realizability, *Transactions of the American Mathematical Society* **75**, pp. 1-19.

Rose, H.E., and J.C. Shepherdson (eds.)

[1975] *Proceedings of the Logic Colloquium '73*, North-Holland, Amsterdam.

Ruitenburg, W.

[1991] The unintended interpretations of intuitionistic logic, in Drucker [1991], pp. 134-160.

Saaty, T.L. (ed.)

[1965] *Lectures on Modern Mathematics*, vol. **3**, Wiley and Sons, New York, pp. 95-195.

Schurz, G. (ed.)

[1991] *Advances in Scientific Philosophy*, Rodopi, Amsterdam.

Scott, D.S.

[1970] Constructive validity, in Laudet, Lacombe, Nolin and Schützenberger [1970], pp. 237-275.

Shanin, N.A.

[1958] On the constructive interpretation of mathematical judgments, *American Mathematical Society Translations (II)* **23**, pp. 109-189 (1963). First published in

Russian, in *Trudy Ordena lenina Matematcheskogo Instituta imeni V.A. Steklova, Academiya Nauk SSSR* **52**, pp. 226-311.

Shapiro, S. (ed.)

[1985] Epistemic and intuitionistic arithmetic, in Shapiro [1985a], pp. 11-46.

[1985a] *Intensional Mathematics*, North-Holland, Amsterdam.

Sieg, W. (ed.)

[1990] *Logic and Computation*, American Mathematical Society, Providence, RI.

Stigt, W.P. van

[1990] *Brouwer's Intuitionism*, North-Holland, Amsterdam.

Summaries of Talks presented at the Summer Institute for Symbolic Logic. July 1957, Ithaca, New York, NY, Institute for Defense Analyses, Communications Research Division, 1957.

Sundholm, G.

[1983] Constructions, proofs and the meaning of the logical constants, *Journal of Philosophical Logic* **12**, pp. 151-172.

[1986] Proof theory and meaning, in Gabbay and Guenthner [1986], pp. 471-506.

[1993] Questions of proof, *Manuscrito* **16**, pp. 47-70.

[1994] Existence, proof and truth-making. A perspective on the intuitionistic conception of truth, *Topoi* **13**, pp. 117-126.

[1994a] Vestiges of realism, in McGuiness and Olivery [1994], pp. 318-328.

Suppes, P., L. Henkin, A. Joja, Gr.C. Moisil (eds.)

[1973] *Logic, Methodology and Philosophy of Science*, vol. **4**, North-Holland, Amsterdam.

Swart, H.C.M. de

[1974] An intuitionistically plausible interpretation of intuitionistic logic, ch. 1 of [1974a].

[1974a] *Intuitionistic Logic in an intuitionistic Metalanguage*, Mathematisch Instituut, Katholike Universiteit, Nijmegen (Netherlands).

[1976] Another intuitionistic completeness proof, *Journal of Symbolic Logic* **41**, pp. 644-662.

[1977] An intuitionistically plausible interpretation of intuitionistic logic, *Journal of Symbolic Logic* **42**, pp. 564-578.

Taylor, B.M. (ed.)

[1987] *Michael Dummett*, Martinus Nijhoff, Dordrecht.

Tennant, N.

[1987] *Anti-realism and logic*, Clarendon Press, Oxford.

Troelstra, A.S.

[1969] *Principles of Intuitionism*, Springer, Berlin.

[1973] *Metamathematical Investigation of Intuitionistic Arithmetic and Analysis*, Springer, Berlin.

[1977] Aspects of constructive mathematics, in Barwise [1977], pp. 973-1052.

[1977a] Completeness and validity for intuitionistic logic, in *Colloque International de Logique*, pp. 39-58.

[1981] The interplay between logic and mathematics: intuitionism, in Agazzi [1981], pp. 197-221.

[1981a] Arend Heyting and his contribution to intuitionism, *Nieuw Archief voor Wiskunde (3)* **29**, pp. 1-23.

[1988] A short course in intuitionistic metamathematics, in Zanardo [1988], pp. 9-14.

[1988a] Validity and completeness for intuitionistic predicate logic, in Zanardo [1988], pp. 15-21.

[1990] On the early history of intuitionistic logic, in Petkov [1990], pp. 3-17.

Troelstra A.S., and D. van Dalen

[1982] (eds.) *The L.E.J. Brouwer Centenary Symposium*, North-Holland, Amsterdam.

[1988] *Constructivism in Mathematics. An Introduction*, vols. **1** and **2** (page numbers run continuously), North-Holland, Amsterdam.

Tymozcko, T.

[1979] The four-colour theorem and its philosophical significance, *Journal of Philosophy* **76**, pp. 57-82.

Weinstein, S.

[1983] The intended interpretation of intuitionistic logic, *Journal of Philosophical Logic* **12**, pp. 261-270.

Wittgenstein, L.

[1956] *Remarks on the Foundations of Mathematics*, ed. by G.H. von Wright, R. Rhees and G.E.M. Anscombe, Blackwell, Oxford. 2nd. ed. published in 1967 and 3rd. rev. ed. in 1978.

Wright, G.H. von (ed.)

[1980] *Logic and Philosophy*, Martinus Nijhoff, The Hague.

Zanardo, A. (ed.)

[1988] *Atti degli Incontri di Lògica Matemàtica*, vol. **4**, Università di Siena, Dipartimento di Matemàtica, Scuola di Specializzazione in Lògica Matemàtica, Siena (Italy).