THE TWO INTERPRETATIONS OF NATURAL DEDUCTION: HOW DO THEY FIT TOGETHER?

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This is almost exactly half a year off Dag's thesis defence, as we saw yesterday, on 31 May in 1965, but it is only a week or so off our first meeting in December 1965, which means that I have a kind of double celebration here, unlike the rest of you. I got a copy of *Natural Deduction* from Dag, as far as I remember, two years after it was defended, in 1967, in connection with his work on cut-elimination for full second-order logic that was published in *Theoria* later that year—but it may have been already in 1966, I cannot tell for sure. It was the first logic that I was exposed to in my capacity as a mathematician at that time, after having studied Uspensky's lectures on computable functions and Kleene's *Introduction to Metamathematics*. It was also very interesting for me, as a mathematician, to see what theoretical philosophers were doing: I had not met a theoretical philosopher before I met Dag. And it came to incite me to extend natural deduction and normalization to stronger systems than the systems considered by Dag himself in his thesis. This was in a series of four papers just over one year, from September 1969 to September 1970.[1] During this period we worked very closely together, but this came to an end when I started to work on type theory in October 1970. Since then, the best simile that I can think of for our professional relation is that we have somehow been following parallel paths, rather than cooperating, as we did in 1969–70. But these parallel paths are so parallel that, although 50 years have elapsed, we are still intermingled somehow, I mean, thinking about the same problems. That is quite an unusual professional relationship. Usually it is more like this: you come to meet someone, and some new ideas develop, perhaps you write some paper together, and then 10 years later, there are other people who you come to interact with in this way. But in this case, here we are, 50 years after we first met.

The title of my talk proper is *The two interpretations of natural deduction: how do they fit together?* Natural deduction is of course Gentzen's Kalkül des

[1] "Infinite terms and a system of natural deduction" in *Compositio Mathematica* 24: 93–103; "Hauptsatz for the intuitionistic theory of iterated inductive definitions" in J. E. Fenstad (editor), *Proceedings of the Second Scandinavian Logic Symposium* (Amsterdam: North-Holland), pages 179–216; "Hauptsatz for the intuitionistic theory of species" in loc. cit., pages 217–233; "A construction of the provable well-orderings of the theory of species" in A. A. Anderson and M. Zeleny (editors), *Logic, Meaning and Computation* (Dordrecht: Kluwer), pages 343–352.

natürlichen Schließens, and as Peter Schroeder-Heister said yesterday, there are two novelties in it compared with the logical systems that were prevalent at the time. The first feature is that it was deduction from assumptions rather than axioms. The second feature, to my mind maybe the most important one, is the duality between the introduction and elimination rules, where the concepts themselves—introduction and elimination rule—came from Gentzen. The first feature, as we now know very well because of Corcoran and Smiley's work in the 1970s, was initiated by Aristotle in *Analytica priora*. It is absolutely clear if one knows anything about deduction from assumptions that Aristotle's theory is a theory of deduction from hypotheses, or assumptions. So what is startling is not that: what is startling is that one had not noticed this in the last century, somehow, before Corcoran and Smiley pointed it out. Concerning the second feature, the duality between introduction and elimination rules, Gentzen had some source of inspiration, although not much. It was adumbrated somehow by Hilbert's formulation of the axioms for conjunction and disjunction in his second Hamburg lecture in 1927, printed in '28: those are just the natural axiomatic versions of the standard natural deduction rules for conjunction and disjunction. We must also remember Herbrand, who proved the deduction theorem in 1930: the deduction theorem is essentially the rule of implication introduction.

Dag was in this fortunate situation of being able to continue directly from where Gentzen had left off in dealing with natural deduction. As Peter also said yesterday, there was of course a direct following of Gentzen by Schütte and Lorenzen, but they did not take up natural deduction, hence it was a fresh ground for Dag. He there develops what were some minor indications in the text: in the second section, in § 5.1.3, Gentzen somehow indicates the introduction-elimination pattern and the reduction rule in the case of implication—but it is not written out schematically in the way Dag did. I tend to think that this second chapter, where Dag formulates the reduction rules for natural deduction, is the most important part, actually, in the whole thesis, the discovery that natural deductions admit of these reduction rules, which are completely natural, though if one has not seen them before, it is a big challenge, I mean, what does this really mean? It is connected with identity of proofs, as Peter said yesterday, but, of course, this is a somewhat biased view: from a proof-theoretic view, there is of course much, much more in the thesis, but from a conceptual point of view, it seems to me that this second chapter—for which Dag was generous enough to use Lorenzen's term "inversion principle"—is the most important one. That introduced then the whole topic of reduction to normal form, or normalization, as Kreisel said, that some of us have spent so much energy on bringing into good form.

Let this be enough as an introduction. Two interpretations of natural deduction: what are these two interpretations? Well, you can describe them in three different ways, to my mind, but it is the same dichotomy: an ancient interpretation versus a modern interpretation; and the ancient interpretation is an epistemological interpretation, whereas the modern one is a purely ontological one; or you may describe it as the difference between the interpretation of a natural deduction as a demonstration versus as a proof, in the precise sense of proof object.

So now I want to say something about terminology here. Dummett introduced, in 1973, the dichotomy

canonical proof – demonstration,

using, in a good way, that we have two words, both proof and demonstration, in English and in most other close languages as well. So, if one wants to make a terminological distinction here, one has the option of using proof and demonstration, not synonymously, but in different ways. Dummett did it in this way, and I am going to do it in another way, namely to distinguish, first of all, demonstration from proof, and when I want to stress that I mean proof, not in the sense of demonstration, but proof in the distinct sense, then it is natural to put object there, following Diller and Troelstra, who introduced the term proof object in a paper from 1984. Proofs, on the other hand, can be either canonical or non-canonical. So here you have Dummett's distinction, but in a new terminology, which means that I get demonstration free:

proof object — demonstration

canonical / non-canonical

It is good to remember from where the terminology comes. I mean, it is clear that Dummett got it from Brouwer: in his proof of the Bar Theorem in 1924 and 1927, Brouwer spoke of Beweisführungen, and they were infinite trees, so clearly they were not proofs in the normal sense of proof, because they are not infinite—they were some kind of mathematical entities that looked like proofs. And, most interestingly, he spoke about canonizing them, kanonisieren, and that is how the term canonical proof has come about.

Concerning the notion of proof object, where does it come from, I mean, who introduced it first? To my mind it is Hilbert in his first Hamburg lecture in 1921, printed in 1922, the programmatic paper where the whole idea of his proof theory is developed. There he has this completely new idea of considering the combinatorial sign configurations, I mean the proof figures, and think of them as inductively generated mathematical objects that can be studied by mathematical means, whence it becomes possible mathematically to prove consistency, which no-one had ever thought about before. So I think it is actually Hilbert that has the idea of considering proofs as mathematical objects for the first time, and then, I take it that two years later, Brouwer with his very original mind got inspired by that idea, just that one should not think of them as combinatorial objects. Brouwer was against essentially everything that had to do with language and formalism, so he considered them as abstract objects, in his case certain infinite well-founded trees that he studied mathematically. The concept thus got into intuitionism, and then Heyting made very, very good use of it in the meaning explanations of intuitionistic logic. So it has come about as some kind of cooperation between Hilbert and Brouwer, although they were, of course, not directly cooperating at all—on the contrary—at the time.

Now to the ancient interpretation of natural deduction. Let me symbolize a natural deduction in the usual way as some hypotheses $A_1, \ldots, A_n$ and a conclusion at the bottom, like this:

$$\begin{array}{c} A_1 \ldots A_n \\ \ddots \,\cdot\, \dot{} \\ A \end{array}$$

Then, of course, in metamathematics, in particular in Dag's thesis, these are all formulas. But now I am going to think contentually in the rest of this lecture, and then we should think of $A_1, \ldots, A_n$ and A as propositions rather than formulas. I take it then that a proposition is defined by its truth conditions, but these truth conditions are elaborated into canonical-proof conditions by the constructivist. So when we give the truth condition for implication, for instance, it takes the form: if B is true provided that A is true, then $A \supset B$ is true:

$$\frac{\begin{array}{c} (A \text{ true}) \\ \vdots \\ B \text{ true} \end{array}}{A \supset B \text{ true}}$$

This is the truth condition for implication. In terms of underlying proof objects, it becomes instead: if b is a proof of B depending on the arbitrary proof x of A, then we get the proof which we may denote by $\lambda x.b$ of $A \supset B$, discharging the assumption:

$$\frac{\begin{array}{c} (x : A) \\ \vdots \\ b : B \end{array}}{\lambda x.b : A \supset B}$$

So there is no contradiction at all between proof conditions and truth conditions: proof conditions are simply truth conditions made with more information in them, or conversely, truth conditions come from underlying proof conditions by suppression of the proof objects that you have on the left-hand side here.

In the ancient interpretation, I want to write out that the assumptions are really to the effect that A_1 is true,..., A_n is true:

$$\begin{array}{c} A_1 \text{ true} \ldots A_n \text{ true} \\ \ddots \,\cdot\, \dot{} \\ A \text{ true} \end{array}$$

That is clear, I think, immediately, that, What is it that you assume when you assume a proposition A? Well, you assume it to be true. You cannot assume a truth condition: you assume a truth condition to be fulfilled, and if you want to make that explicit, then you can do it, for instance, in this way. Then I am introducing this new form,

$$A \text{ true}$$

and, by some kind of miracle, it is in Aristotle. So, Aristotle had this form

$$A \text{ is}$$

ἔστιν ἄνϑρoπως is the typical example. Aristotle was very explicit that the sense of 'is' there is the same as 'is true'. Of the many meanings that the copula can have, one is the one that is referred to usually as τὸ ὂν ὡς ἀληϑής, being as truth. As Aristotle says in chapter 10 of *De interpretatione*, this is the case where the copula is added as a second component to the A to form the whole content—*secundum adiacens*, as the Scholastic term was—as compared with

$$B \text{ is } A$$

where it is added as a third component in addition to A and B here, *tertium adiacens*.

Once I have taken the step of making the truth particle explicit, then there is a simple second step to make, namely of forming the context Γ of all these A_1 true, …, A_n true, and then subjecting everything here to Γ:

$$\begin{array}{c} \Gamma \vdash A_1 \text{ true} \ldots \Gamma \vdash A_n \text{ true} \\ \ddots \ \vdots \ \cdot^{\cdot^{\cdot}} \\ \Gamma \vdash A \text{ true} \end{array}$$

The purpose of this move is precisely not to have hypotheses any longer. Now we have simply a demonstration, from axioms, because all of these,

$$\Gamma \vdash A_1 \text{ true}, \ldots, \Gamma \vdash A_n \text{ true}$$

have become axioms.

This, to my mind, is quite clearly how Aristotle must have thought of the interpretation of his syllogisms. You can put it this way: if you try to think of a syllogism with hypotheses as a demonstration, well, then you have to follow the demonstration and see, What is it that you know at each stage in the demonstration? And now it is clear. You know that A_1 is true provided all of $A_1, \ldots, A_n$ are true, and you know that A_n is true provided all of $A_1, \ldots, A_n$ are true, that is clear, and then we follow the rules downwards and arrive at the knowledge of $\Gamma \vdash A$ true. This is a demonstration that gives knowledge, absolute knowledge that is not relative to any hypotheses. What we know as a result is that if $A_1, \ldots, A_n$ are all true, then A is true: what we have demonstrated is a consequence, but, of course, that is something Aristotle could not say, because he had not introduced the notion of consequence, this particular form of assertion, or judgement. That is why he has this very intricate formulation—his definition of syllogism is quite intricate, but it is his way of saying what I have just tried to explain here. This shows immediately something that Dag is taking an interest in presently, namely that the necessity that this has is apodictic necessity, necessity as a result of having been demonstrated. Apodictic necessity is the same as that for which Dag has revived the term necessity of thought, Denknotwendigkeit, a terminology that belongs to the 1800s, a very fine notion that is identical to apodictic necessity.

I said I had three classifying adjectives. It is ancient, because it is exactly Aristotle's interpretation. It is epistemological, because a demonstration yields knowledge of its conclusion. And you may call it the demonstrative interpretation of a natural deduction, since you interpret it as a demonstration.

Let this be enough about the ancient interpretation. What do I have in mind with the modern interpretation? I think the most driving force towards this modern interpretation has been the Curry–Howard interpretation, so I will approach it this way. Notationally, I will write

$$\begin{array}{c}\Pi\\ A\end{array}$$

In Curry–Howard notation, A appears as the type of the proof Π, and then Howard uses the standard way from Church, at least, of displaying an object with its type as a superscript or sometimes a subscript:

$$\Pi^A \text{ or } \Pi_A$$

I will also use the type-theoretic notation:

$$\Pi : A$$

It is the same amount of information that is fiddled around with here, but now one new thing has come in, namely the copula. I am using De Bruijn's notation for the copula. I thought myself long ago that one should use Peano's notation,

$$\Pi \in A$$

to honour him, but it was thought that this was so closely related to ZF-style set theory that it was better to have a new notation there, and De Bruijn had introduced such a new notation, and it has won completely.

What do I mean by the modern interpretation? Well, let me take some rule, for instance again implication introduction. In the standard natural deduction notation it looks something like this:

$$\dfrac{\begin{array}{c}\overset{x}{A}\\ \Pi\\ B\end{array}}{A \supset B}\ \supset\text{I}, x$$

We have a label, x, for which Gentzen used numbers, as we know, 1, 2, 3, etc., which means that, in a sense, Gentzen used De Bruijn indices many decades before De Bruijn. In particular, when you need such a label, the first time you need it, you take 1, and then the second time you need one, proceeding from above going below, the second time you use 2, the third time you use 3, etc., then it is exactly the De Bruijn indices that you get in the typed proof term. So this is the notation of natural deduction, with the rule being put here together with information about what is being discharged.

In the Curry–Howard interpretation we know that this is rewritten as implication introduction operating on Π^B, with A being discharged—written as a functional abstraction, and in Howard's notation you write the type of this whole thing, $A \supset B$, as a superscript:

$$\supset\text{I}\big([x^A]\Pi^B\big)^{A\supset B}$$

I think it is enough to write out one introduction rule and one elimination rule, because this is so well known:

$$\dfrac{\begin{array}{cc}\Pi & \Sigma\\ A \supset B & A\end{array}}{B}\;\supset\!\mathrm{E}$$

In Howard's notation we get instead

$$\supset\!\mathrm{E}(\Pi^{A\supset B}, \Sigma^{A})^{B}$$

In the type-theoretic notation, you fiddle around with these signs. It is the same signs, but you rearrange:

$$\supset\!\mathrm{I}(A, B, [x]\Pi) : A \supset B$$

You see it is much less heavily typed than the Howard notation, because there every sub-term is explicitly typed all the time. It is a notation that is practically impossible to use, I would say, but you are at least sure that all the typing information is there, because there is so much there that you cannot fail to have it, whereas in type theory, enough information is there, but you have to be careful to see how you get it. And, in the elimination case it is

$$\supset\!\mathrm{E}(A, B, \Pi, \Sigma) : B$$

I suppose you are familiar with this now in the case of all the other rules also.

Let me just then give one example of a little derivation:

$$\dfrac{\dfrac{\dfrac{\overset{x}{P} \qquad \overset{y}{Q}}{P \wedge Q}\;\wedge\mathrm{I}}{Q \supset (P \wedge Q)}\;\supset\!\mathrm{I},\, y}{P \supset (Q \supset (P \wedge Q))}\;\supset\!\mathrm{I},\, x$$

Here is an example of a natural deduction, which is now even assumption free. This is a certain symbolic configuration of the kind that we call a natural deduction, and from a semantic point of view, what is it, what kind of object is this? Well, the answer is that it is a proof of $P \supset (Q \supset (P \wedge Q))$, depending on the parameters, which is the same as the free variables,

$$P, Q : \mathrm{prop}$$

Since a dependent object is the same as a function with that object as value, you could equally well say that what this denotes is a function of the two arguments P and Q which as value takes a proof of this proposition, $P \supset (Q \supset (P \wedge Q))$.

So this is the modern interpretation, which we may call the proof interpretation, or more explicitly, proof-object interpretation, or you may also say that it is the functional interpretation, if by functional interpretation you do not mean Funktionalinterpretation in Gödel's sense, you mean simply its interpretation as a function of these two arguments.

Now I want to put some more light on this. First of all, when we are in this situation,

$$\begin{array}{c}\overset{x_1}{A_1}\ldots\overset{x_n}{A_n}\\ \Pi\\ A\end{array}$$

then the derivation comes out as a certain object, that is what I have just explained, dependent proof object. But if you look at it in the type-theoretic formulation, it will be formulated as: Π is a proof of A, an object of type $\mathrm{pr}(A)$, and then we have to list the variables which it depends on, which are $x_1, \ldots, x_n$, where they label the assumptions:

$$\Pi : \mathrm{pr}(A) \quad (x_1 : \mathrm{pr}(A_1), \ldots, x_n : \mathrm{pr}(A_n))$$

This is how it comes out type-theoretically. And now you see a gain that has been achieved by the insertion of the copula here, because if you start with a natural deduction and think about Frege, then where are you going to put the assertion sign? Well, you cannot put the assertion sign in front of $A_1, \ldots, A_n, A$ for the obvious reason, because they are not asserted. So where on earth should you put the assertion sign? It seems the only place you can put it is somehow outside everything here, in front of the whole proof object:

$$\vdash \begin{array}{c}\overset{x_1}{A_1}\ldots\overset{x_n}{A_n}\\ \vdots\\ A\end{array}$$

But can you put the assertion sign in front of an object? If you think, say, of 0 written in Church's notation, $0^{\mathbf{N}}$, the natural number 0: can you put an assertion sign in front of that? No, that does not seem to make good sense, because an object is not an assertoric content, as it has to be if you are going to put the assertion sign in front of it. However, if you instead move down the type information to write it in this way,

$$0 : \mathbf{N}$$

then, of course, it makes perfectly good sense, namely it is the assertion that 0 is a natural number:

$$\vdash 0 : \mathbf{N}$$

What has had the effect here is the insertion of the copula. Similarly in this case:

$$\Pi : \mathrm{pr}(A) \quad (x_1 : \mathrm{pr}(A_1), \ldots, x_n : \mathrm{pr}(A_n))$$

This makes perfectly good sense as an assertoric content, reading the colon as the copula, hence it makes good sense to put the assertion sign in front of it:

$$\vdash \Pi : \mathrm{pr}(A) \quad (x_1 : \mathrm{pr}(A_1), \ldots, x_n : \mathrm{pr}(A_n))$$

This is how you do it type-theoretically.

A further generalization is necessary here, which you saw in the example derivation, where you have the parameters P and Q. You have to take care of the fact that, not only do you have assumptions like

$$\overset{x}{P} \qquad \overset{y}{Q}$$

but the whole proof object may depend on parameters—in that example, the P and the Q that were both propositions. We need to generalize this further to the parametrical case. Let us call the parameters x_1 of type α_1, etc., x_m, say, of type α_m, and then you have the assumptions A_{m+1} labelled by x_{m+1}, say, up to A_n labelled by x_n, and then you prove A:

$$\begin{array}{c} x_1^{\alpha_1} \ldots x_m^{\alpha_m} \overset{x_{m+1}}{A_{m+1}} \ldots \overset{x_n}{A_n} \\ \Pi \\ A \end{array}$$

Now I have already taken for granted something, namely that in natural deduction, when you have parameters, or free variables, then when that free variable is introduced, you are making an assumption, another kind of assumption, but still an assumption, just as $A_{m+1}, \ldots, A_n$ are to be counted as assumptions. When this now is linearized in type-theoretical notation, then we get

$$\Pi : \mathrm{pr}(A) \quad (x_1 : \alpha_1, \ldots, x_m : \alpha_m, x_{m+1} : \mathrm{pr}(A_{m+1}), \ldots, x_n : \mathrm{pr}(A_n))$$

The generalization is clear, of course, since we now have the possibility of having parameters here.

This we recognize as the general form of a formal, or logical, consequence—formal consequence was the Scholastic terminology, and Tarski changed it, I mean, he retained that terminology and added logical consequence—so A is a logical consequence of $A_{m+1}, \ldots, A_n$.

As probably all of you know it is Dag who has brought up the question of how logical consequence is to be defined within constructive semantics, for the first time in a paper which was read at a meeting in 1972, published in '74, and there is a later paper in Synthese in '85, where the meeting was in '81, I think, and there are further papers, and I am sorry I have not had access to them in preparing this lecture. The question is now, How is this related to Dag's analysis of logical consequence?

The intuitive idea behind Dag's analysis is an extremely clear and convincing one, namely that if you have

$$\Gamma \vdash A$$

then what could that mean except that A is derivable from Γ, that is, that there exists a derivation, or deduction, of A from Γ? This idea is present already in Bolzano's term for consequence: Bolzano did not call it consequence, he called it Ableitbarkeit, derivability, but then nothing about derivations afterwards—he defines it in the modern way, essentially semantically. This is now very clearly the intuitive idea behind Dag's treatment of the notion, and the reason why it comes out differently from what I have said here is that the notion of proof object was not in place at that time, in '72, so Dag instead used his own notion of valid argument, and defined consequence as the existence of a valid argument for A from Γ. This notion of valid argument, however, which has been central to Dag's work from the paper that has given the name to this meeting, *General Proof Theory*, in '71, is distinct from the intuitionistic notion of proof, or proof object.

This is perhaps the best way of explaining my simile with parallel paths: Dag has based a lot of his work during the following decades on this notion of valid argument, in particular his definition of logical consequence is based on it, whereas I thought, already at that time—though I had a lot of more urgent things to think about in '71—I thought that this notion of valid argument is not the intended interpretation of the notion of proof, intuitionistic proof: it is more like a realizability interpretation, that is, a non-intended interpretation that validates all the formal laws, but it is not, nevertheless, the intended interpretation. I therefore put all my energy into getting the proper explanation of the notion of proof object, whereas Dag has, at least for a very long time, stayed with his notion of valid argument. But, mind you, if you read carefully in Dag's contribution to the Wansing volume, which is only a few years ago, then now something has happened: there is a section on valid arguments, a relatively short one, but then there is a whole chapter on grounds, what he calls grounds. That terminology comes, actually, from Myhill in 1966 and '68:[2] Myhill introduced the term ground for asserting a proposition, which is the same as proof of a proposition, so that was just meant as another terminology for the intuitionistic notion of proof object, and Dag has preferred that term. But it means that in this chapter on grounds, Dag is now actually using what I call proof objects, and he calls grounds, and which to my mind are the same thing.

Now it is high time that I finish, so some final remarks. What is so striking about these developments that I have spoken about is that they affect the very architecture of logic, and that has not happened often. A standard way of structuring some of the most basic notions is this:

proposition
truth
consequence
formal/logical consequence
proof/deduction

This has been the structure. Certainly, we all know, in modern logic, that it comes in this order, and even in Bolzano it comes in this order, so deduction is dealt with after formal consequence. Now the striking thing is that the notion of truth is put down one step in the analysis that I have spoken about, because truth is defined as existence of proof, so proof has to come first:

proposition
proof/deduction
truth
consequence
formal/logical consequence

[2]"Notes towards an axiomatization of intuitionistic analysis" in *Logique et Analyse* 9: 280–297; "The formalization of intuitionism" in R. Klibansky (editor), *Contemporary Philosophy I: Logic and Foundations of Mathematics* (Florence: La Nuova Italia Editrice), pages 324–341.

So proof is more fundamental than truth and consequence and formal consequence. This is clearly behind a lot that Dag has written on this topic, I mean, to make proof more fundamental than the model-theoretic notion of formal, or logical, consequence. I certainly agree on this point.

Then there is the point that I have already mentioned, the syllogistic necessity in Aristotle, that that syllogistic necessity in modern logic has been interpreted as the variational necessity, that is: for all values of the non-logical constants, something holds. That notion of variational necessity, to my mind, is not to be found at all in Aristotle. We have to wait until the consequence treatises of the 14th century—Buridan and others—to find that notion, and then later in Bolzano and Tarski. On that point, therefore, we also seem to agree completely.

Now a third point, about these proof objects. Well, they form a new type of logical objects—logische Gegenstände in Bolzano's and Frege's and Wittgenstein's terminology—that have to be dealt with in a new chapter of logical ontology, and that chapter has to come precisely at this point [proof/deduction in the table]. The question is, How on earth does one succeed in introducing a new chapter of ontology? Well, we have to learn to see, or to grasp, these new ontological objects sufficiently precisely, and the way we are doing this is to introduce a precise notation for them—that is certainly one half, but one also has to explain carefully what those notations mean, so a careful semantics. I tend to call it the syntactic-semantic method of clarification of the ontological objects in question, and that you can put, if you want, as a kind of slogan or equation:

syntax + semantics = the way of achieving/reaching an ontology

And then my final remark, that, to my mind, is the overcoming of the split between intuitionism and proof theory that has led to the remarkable developments in mathematical logic and foundations of mathematics during the last 50 years that I have been trying to sketch. This means that the antagonism of the Hilbert–Brouwer controversy that was so pungent in the 1920s has been replaced by a spirit of joining forces, first manifested in the title of Heyting's book from 1934: *Mathematische Grundlagenforschung*, if you take that as the main title, and then comes the subtitle *Intuitionismus. Beweistheorie*, and then much more forcefully 34 years later at the Buffalo conference which was entitled precisely *Intuitionism and Proof Theory*, and which Dag and I both attended back in 1968, so that is also almost 50 years ago now. So if you want it in a slogan: it is much more fruitful to join forces rather than to continue the positional warfare that you had in the '20s between Hilbert and Brouwer.