PHILOSOPHIA SCIENTIÆ

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Philosophia Scientiæ, tome 3, nº 4 (1998-1999), p. 279-302
<http://www.numdam.org/item?id=PHSC_1998-1999__3_4_279_0>

Beth's Nonclassical Valuations

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Abstract. We describe E. W. Beth's use of nonclassical valuations (in his own terminology *pseudo-valuations*) in propositional logics. Three periods are distinguished. In the first period (1954) he develops the idea of pseudo-valuation intending to apply it to obtain a subformula theorem for arbitrary propositional logics. When this fails, he obtains in the second period (1958-1961) some simple but elegant applications of the idea, mainly with regard to proofs of independence of axioms systems. The third period (1961-1964) is the application of the idea towards the introduction of a semantics (his second one) for intuitionistic logic. We will show that it is highly likely that Beth discovered this version of 'possible worlds semantics' for intuitionistic and some modal logics essentially independently from Kripke. The history of the concept of semantic tableaux, but we will touch one the latter subject only in so far as is necessary for our coniderations.

Résumé. Nous décrivons l'usage fait par E. W. Beth de valuations non-classiques (pseudo-valuations dans sa propre terminologie) en logiques des propositions. Trois périodes sont distinguées. Pendant la première (1954), il développe l'idée de pseudo-valuations dans le but de l'utiliser pour obtenir un théorème des sous-formules pour logiques des propositions quelconques. Après un échec de ce projet, il obtient pendant une seconde période (1958-1961) plusieurs applications simples mais élégantes de cette

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idée, surtout en ce qui concerne des preuves d'indépendance de systèmes axiomatiques. La troisème période (1961-1964) est consacrée à l'application de l'idée de pseudo-valuations à l'introduction d'une sémantique (sa seconde) pour la logique intuitioniste. Nous montrerons qu'il est fort probable que Beth ait découvert cette version d'une 'sémantique des mondes possibles' pour la logique intuitioniste et certaines logiques modales indépendamment de Kripke. L'histoire du concept de pseudo-valuation est fortement liée à la naissance du concept de tableaux sémantiques, mais nous n'aborderons ce dernier sujet que dans la mesure où cela sera nécessaire pour nos considérations.

This paper is about E.W. Beth's use of nonclassical valuations to various purposes, almost all in propositional logic. In Beth's own terminology these nonclassical valuations are called *pseudo-valuations*. One can actually distinguish at least three periods for his use of pseudo-valuations. In the first period (1954) he has a grandiose idea, which unfortunately does not turn out to work as he would have liked it, the second period (1958-1961) consists of some simple but elegant applications of the idea, and the third period (1961-1964) is the application of the idea in a direction in which one might say that present day logic still uses them. This concerns the introduction of a semantics for intuitionistic logic, his second, based on nonclassical valuations, in this case called I-valuations. Moreover, we will show that it is highly likely that Beth discovered this version of 'possible worlds semantics' for intuitionistic and some modal logics essentially independently from Kripke. The reader will see that like all truly told stories on the development of science it is a story of failures as well as of successes. This little history is intertwined with the birth of the concept of semantic tableaux. We will touch on the latter subject only in so far as is necessary for our considerations, but the reader can find considerably more detail in Guillaume's contribution to this volume.

Already before 1954 Beth used nonclassical valuations in the predicate calculus in his completeness proof [Beth 1951]. However, in that case the 'nonclassicalness' of the valuations is only with regard to the universal quantifier, and does not constitute an essential deviation of the two standard truth values; and it is the latter that is our main interest in this paper. So, even though it is probably true that Beth's pseudo-valuations arose from the ideas used in that paper it did not seem useful to take up the connection in our considerations here.

1. The first period

The first mention of 'pseudo-valuation' we can find is in an unpublished paper [Beth 1954a] accompanying a letter to Alfred Tarski of June 30, 1954, but let us first explain the idea of pseudo-valuations in the context of pure implicational logic, Beth's favorite system to demonstrate his ideas on.

A *valuation* is a function v from, in this case propositional, formulas to 0 and 1 such that:

$$v(A \to B)=1 \text{ iff } v(A)=0 \text{ or } v(B)=1,$$

or in other words:

$$v(A \to B)=0 \text{ iff } v(A)=1 \text{ and } v(B)=0.$$

(Actually Beth usually used 0 and 2 instead of 0 and 1. According to a personal oral communication to D. de Jongh of around 1962, the purpose of that was to make room for a third truth value, like undefined, in between, but we will write the more standard 0 and 1, even changing direct quotes accordingly.) This, of course, gives one standard classical logic and Beth wanted to apply it more generally. He was thinking of subsystems of classical logic axiomatized by some axiom schemes **Ax** with the rule of *modus ponens*:

$$A,\ A \to B / B.$$

This means in terms of the valuations that

if $v(A)=1$ and $v(A \to B)=1$, also $v(B)=1$, or more perspicuously,

if $v(A)=1$ and $v(B)=0$, then $v(A \to B)=0$,

just half of the equivalence above. This is Beth's definition in [Beth 1954a] (on this photo and the following ones we of course kept the 2 instead of the 1 for *true*):

Defintion 2. We call pseudo-valuation, any function w which assigns to each formula U a value w(U), in accordance with the following conditions :

(i) w(U) = 0 or w(U) = 2 (one may interpret 'o' as False and '2' as True ; but it is, perhaps, more suitable to construe '2' as Asserted, and '0' as Non-asserted):

(ij) If w(U) = 2, then w($\bar{U}$) = 0;

(iij) If w(U) = 2 and w(V) = 0, then w(U-→V) = 0.

Lemma 3. Every valuation (in the usual sense) is a pseudo-valuation.
Of course, the converse is not true ;

(ii) Beth realized that this was precisely what is needed to describe the situation of arbitrary subschemes of classical logic and even more generally (lemma 9 of [Beth 1954a], lemma 7 of [Beth 1954b]):

> If **Ax** is a set of axiom schemes of implicational logic one can give the value 1 to all the formulas C that are derivable from **Ax** by *modus ponens*, and value 0 to all other formulas, and one indeed has obtained such a pseudo-valuation. This pseudo-valuation simply makes everything true that is derivable from **Ax** and makes everything else false.

Of course, this is a kind of general *completeness theorem* for propositional logics. Beth's grandiose idea, that actually did not work out, was that this could be made to work in such a way that not only could one get completeness, but also *decidability*, which would solve a problem that Tarski stated in a lecture in Princeton in 1946 [Tarski 1947]:

> To be able to decide when a set of formulas is an *adequate* axiom system, a system from which all tautologies are derivable.

Namely, to decide whether an axiom system **Ax** is adequate, one would only need to ascertain whether one of the well-known axiom systems would be derivable from **Ax** (and this would be decidable), after verifying of course that **Ax** consists of only of tautologies.

Naturally, Beth consulted his friend Tarski on this. As we stated above, Beth included in his letter to Tarski a manuscript. There is no hard proof, but we may assume that it is a paper "On a subformula theorem for the sentential calculus and ..." intended to be dedicated to Feys, two versions of which can be found in the archives ([Beth 1954a], [Beth 1954b]). Presumably it is [Beth 1954a] that accompanied Beth's letter to Tarski, he refers to Corollary 3 as solving Tarski's problem, and this is in accordance with [Beth 1954a], but not with [Beth 1954b]. In fact, [Beth 1954b] seems to be a revised version of [Beth 1954a] announced in the latter to Tarski which he must have been working on the next few weeks. There also exists a copy of an abstract for the meeting of the Association of Symbolic Logic in Amsterdam that year with the same content [Beth 1954c]. Beth himself organized this conference (the first one in Europe) and, was in addition to organizing the conference planning to talk about this subject, but later decided not to, and the abstract was not published. Also the paper dedicated to Feys was replaced by a different one [Beth 1955a]. The reason for all of this, we will come to in a minute.

How incredibly active Beth was in this period can be demonstrated by his time table in August-September. The conference of the ASL that he organized started on September 1. On August 31 he gave a lecture on Nieuwentyt, a Dutch philosopher of Science, for the Dutch Logic Society [Beth 1954/55]. On September 11, just a few days after the logic conference, he gave a lecture in the Hague on the philosophy of Henri Poincaré [Beth 1955c]. In the meantime he was involved in the organization of the International Congress of Mathematicians in Amsterdam, September 2-9.

The reason that the above mentioned papers remained unpublished is clarified by Tarski's answer, dated July 13, 1954:

> I haven't had time to study your paper, but there is one remark which I have to make at once. Your Corollary 3 (which gives an affirmative answer to a problem formulated in my Princeton talk) is in direct contradiction to a result stated by Lineal and Post in the Bulletin of the Amer. Math. Soc. vol. 55, 1949, p. 50.

He refers to an abstract of Post and Lineal [Lineal & Post 1949] which claims that that there are finite sets of tautologies for which it is undecidable what is derivable from it. Actually Beth's answer to Tarski on July 22 shows that this did not really surprise Beth very much, because in the meantime he had realized, when he tried to write down a full proof of his ideas, that seemingly small gaps were unexpectedly difficult to fill. In fact, a letter to Tarski of July 14 crossing Tarski's return letter to him already mentions these problems:

> I have been unable to stop thinking about the matter . . . . I found several gaps . . . . It seems that I will have to resort to several other tricks besides the introduction of pseudo-valuations.

And after Tarski's reply, in his letter of July 22, Beth writes:

> ... it explains why I could gradually improve my argument but not finish it.

So, he accepted this setback immediately. From the same letter:

> ... there is no reason to doubt its truth ...

But he did not lose his sense of humor in the process:

> So I have changed the title of my paper to: "A subformula theorem for the sentential calculus?".

He tried to contact Post, on July 13, 1954, for the proof of the results which contradicted his, because the reference given by Tarski was an abstract without proofs. Beth got an answer, dated September 21, 1954, from Post's wife that Post had died in May. A little later he got a letter from Post's co-author, dated October 18, 1954. The name Samuel Lineal turned out to be a pseudonym of Samuel Gulden. Although Gulden did not provide him with the proofs, Beth clearly accepted the results. Actually, Gulden's own publications afterwards were not in logic, but in topology, and the first full published proofs of the Post-Lineal results saw the light much later in [Yntema 1964], after a sketch in [Davis 1958].

It is also good to stand still for a moment and consider how Beth intended to prove decidability here. Just that year he was not only concerned with pseudo-valuations and all these other activities, but also one of his lasting contributions to logic, his semantic tableaux, were getting their shape in his mind. We find a kind of *prototableaux*, both in the lectures on "l'existence en mathématiques" in 1954 March-April in Paris [Beth 1956a], and in the two versions of the unpublished paper in honor of Feys that we mentioned above. We call them prototableaux because as Beth says in a letter to Hasenjaeger of February 8 of the next year in these

> ... es fehlte jedoch noch etwas wesentliches,

(he had not invented the splittings of the tableaux)

> bis ich in Dezember 1954 auf die Zweiteilung der Spalten kam. Jezt sieht es alles so einfach aus, dass man kaum versteht wozu all diese Mühe nötig war.

In [Beth 1956a] he still obtained B on the true left side of the tableau from A and A→B on the left side (*modus ponens*) directly without first splitting the tableau. It is extremely interesting to note that from the very start his tableaux, and even his prototableaux, were used for radically distinct purposes. In [Beth 1956a] it was used to construct a what we now call closed tableau for a valid sequence to show its validity. But in [Beth 1954a], as we see below, he uses a tableau to show that a certain tautology, p→(q→(p→q)), can be falsified only by a pseudo-valuation.

In

My proofs, I apply a certain generalisation of the well-known truth-table method, which in itself already offers some of the advantages of Gentzen's method. Let us consider the formula :

$$p \rightarrow [\, q \rightarrow (\, p \rightarrow q\,)] \qquad (1)$$

and let us try to find a valuation by which it obtains the value False , the result of our attempt may be summed up in the following diagram :

True	False
	(1)
p	$q \rightarrow (p \rightarrow q)$
q	$p \rightarrow q$

from the diagram, it appears that, in order to make formula (1) false, we must assign to p, q, and p → q certain truth values which are not in accordance with the familiar valuation rules.

Note that the formula p→(q→(p→q)) illustrates Beth's point much better than the more standard tautology q→(p→q) would. In [Beth 1954b] he has replaced the example p→(q→(p→q)) by the formula ¬p→(p→q) and he considers also pseudo-valuations for formulas with negation (¬).

His idea was to use the tableau-like methods to show that only certain combinations of the subformulas of the schemes to be investigated need to be substituted in the schemes, i.e., a finite number of formulas, so that decidability follows. A final word on this first period. Beth was not directly successful in applying his pseudo-valuations in the manner he envisaged, but we are reasonably sure that something can be done with his ideas even nowadays, they have not been fully exploited.

2. The second period.

In the second period Beth used the idea of pseudo-valuations to prove the independence of axiom systems in propositional, and even predicate logic. Of course, an adequate set of axiom schemes **Ax** is independent, if, for all schemes S in **Ax**, **Ax** minus S (**Ax**–S) is not adequate. And naturally, to determine whether **Ax**–S is adequate, it suffices to check whether S is derivable from **Ax**–S. To show that it is not, it suffices to give a pseudo-valuation that makes **Ax**–S true and S false. Even though this is in general apparently not decidable, in practice it may of course be successful. We have always found this technique highly original and elegant. The normal method would be to give some many-valued matrix. Let us simply give some examples; you do not find this in the regular logic text books. A first example is found in a letter to Church on July 12, 1958. Beth does use Church's axiom system **P2** from [Church 1956]. Stated as axiom schemes as is usual nowadays:

202. $A \to (B \to A)$

203. $(A \to (B \to X)) \to ((A \to B) \to (A \to X))$

204. $(\neg B \to \neg A) \to (A \to B)$

Beth writes to Church:

> We can stick to two-valued truth-tables, provided we admit non-regular formulas, whose vales are not consistent with these truth-tables.

For instance, in order to prove the independence of your axiom 203, I single out three atomic formulas a, b, and c, and I consider the following valuation :

(i) $w(a) = 2$, $w(p) = 0$ for every atomic formula p different from a ;

(ij) $w(\overline{X})$ as usual ;

(iij) $w(a \to b) = 2$, otherwise $w(X \to Y)$ as usual.

Now $w(U) = 2$ for every formula on the basis of 202 and 204 alone, whereas $w([a \to (b \to c)] \to [a \to b) \to (a \to c]) = 0$.

It does not follow, from $w(U) = w(U \to V) = 2$, that $w(V) = 2$. But this does follow, if U and $U \to V$ are provable in the full system, and this is sufficient for establishing independence. As to substitution, I rely on "Zurück-verlegung der Einsetzungen".

(Zurückverlegung der Einsetzungen, i.e. Rückverlegung der Einsetzungen, see [Hilbert-Bernays 1934], p. 225-228.) The example was later published in essentially the same form in [Beth 1960a], the proceedings of the international congress of mathematicians in Edinburgh of 1958. Beth continues the letter by showing that the method is also applicable to show independence of the axioms of predicate logic, which is not to our direct interest here. Then Beth follows with:

> However, it turns out to be difficult to show by this method that Peirce's law cannot be derived from 202 and 203 alone.

The role of S (as above) is played here by Peirce's law: $((A\to B)\to A)\to A$. Beth used during his last ten years this formula as a test for the (non)classicalness of a logical axiom system **Ax**. Actually, Beth had less than two weeks before, June 30, 1958, written to Tarski about a proposed solution to exactly the problem concerning Peirce's law. This solution was mistaken however, and apparently he had realized that very quickly. Only in [Beth 1960b] did he succeed in solving the problem by a relatively complicated valuation, which is based on an earlier Euratom report [Beth 1961b] (see below) and reproduced in [Beth 1962]. The solution of [Beth 1960b], which was received by the JSL on October 24, 1961 (the publication dates of the JSL in this period are utterly confusing!), is the following:

> The independence of Peirce's law $((A\to B)\to A)\to A$ with respect to the positive implication calculus can be proved by means of a pseudo-valuation w which associates with each formula U a truth value $w(U) = 0$ (false) or $= 1$ (true), as follows.
>
> We have $w(A) = w(B) = 0$, $w(M) = 1$ for each atom M different from A and B, $w(U\to V)$ as usual, unless U and V are formulas $U_1\to(U_2\to(\dots \to(U_m\to A) \dots))$ and $V_1\to(V_2\to(\dots \to(V_n\to B) \dots))$, $w(U_j) = w(V_k) = 1$ $[0 \leq j \leq m,\ 0 \leq k \leq n]$, in which case $w(U\to V) = 0$.

It will be easy for the reader to check that this works. But here is a simpler example of a pseudo-valuation that gives the result that Beth wanted: Let us consider the regular valuation w:

> $w(A)=1$, $w(B)=0$, for some specific atoms A, B,

and consider the pseudo-valuation v defined by

> $v(A) = 0$, $v(B) = 0$,
>
> $v(U\to V) = 0$ iff ($v(U) = 1$ and $v(V) = 0$) or ($w(U) = 1$ and $w(V) = 0$), for all formulas U, V.

Again it is easy to check that v is a pseudo-valuation and that it satisfies schemes 202 and 203, but that $v(((A\to B)\to A)\to A) = 0$.

However, the reader may justly feel cheated: to obtain this simple example we have used semantic tableaux for intuitionistic logic and the method of possible worlds models, and this belongs in the third period that we are going to describe. But, on the other hand, the public start of that third period is this very abstract [Beth 1960b]. He continues after the previous excerpt with

> Further analysis of the idea suggests the following semantic construction of a logical system which is, at least for a classical point of view, identical with intuitionistic logic.

and describes the *I-valuations* as he coined the pseudo-valuations which exactly fit intuitionistic logic.

> An I-valuation is an ordered triple $[W, \leq, w^0]$ composed of a set W of functions w, a partial ordering $\leq$ of W, and the largest element w^0 in W. The functions w associate truth values $w(U) = 0$ or $= 1$ with formulas U in accordance with the following semantic rules:
>
> (S1) If A is an atom, $w' \leq w$ and $w(A) = 1$, then $w'(A) = 1$;
>
> (S2) If, for every $w' \leq w$, $w'(U) = 0$ or $w'(V) = 1$, then $w(U \rightarrow V) = 1$; otherwise $w(U \rightarrow V) = 0$.
>
> Theorem: the following conditions are equivalent:
>
> (i) $w^0(U) = 1$ for every I-valuation $[W, \leq w^0]$;
>
> (ij) the deductive tableau for the sequent $\varnothing / U$ is closed;
>
> (iij) U is a theorem of the inferential (= positive) implication calculus.
>
> — Presumably the construction is similar to one previously announced by Kripke.

One can connect the following intuitive idea with this semantics:

W can be seen as a set of possible stages of knowledge (knowledge being what has been proven).

$w' \leq w$ means the that w' is a possible future stage of knowledge as seen from the stage w.

$U \rightarrow V$ is true in a stage, if it is known, proven, at that stage, which means precisely that at all future possible stages, if U is known, then so is V. However good this is to work with, it is only indirectly connected to the interpretation the intuitionists themselves give to the connectives (compare the next subsection).

It is interesting to note that Beth must have been able to give the solution to his problem with Peirce's law in the form that we gave it. However, he didn't, and never did. Why not? We can only speculate on this. Did he prefer the historical order in which he found his particular solution and the general concept (the Euratom reports [Beth 1961b] and [Beth 1961c] came out May 15 and October 15, 1961, respectively), did he think his solution simpler, or more according to the rules of the game, because it refers to no other valuations, or because it uses two-valued truth tables except for non-regular formulas, did he just want to make this solution—for which he had looked for 3 years—known, or did he overestimate the complexity of the pseudo-valuation based on the general concept of I-valuation that we gave above?

The third period.

We now get to the third period, and will switch to the word *nonclassical* valuations and use the word pseudo-valuation only for the very special valuations of the second (and first) period. The history is rather complicated. On the one hand, it forms part of Beth's involvement with intuitionistic logic, and his first approaches to this logic advanced from very different ideas; on the other hand, Saul Kripke's work on nonclassical valuations for intuitionistic logic was slightly earlier and to a large extent independent of Beth's work. Kripke's work on modal logic clearly precedes this, but as we stated in the introduction, we think that Beth's knowledge of Kripke's work was very limited.

Beth's earlier investigations in intuitionistic logic. Before we get to the third period proper, let us first treat Beth's first formal approach to intuitionistic logic which was really independent of his ideas on pseudo-valuations. As an introduction, we return to the semantic tableaux. The year after his struggle with the subformula property for propositional logics, not only had he completed his concept of semantic tableau, he had within that year published the definitive article [Beth 1955b] on it. But not even that is all, in September of that year, 1955, he had already lectured in Paris on the form of semantic tableaux for *intuitionistic* logic [Beth 1958a], another proof of the fact that from the very start semantic tableaux were not taken to be the expression of an orthodox classical view on truth values.

However, these tableaux were not by Beth in the ensuing papers connected to nonclassical valuations, but to Brouwer's choice sequences. His justly famous paper "A semantic construction of intuitionistic logic" appeared already in 1956 [Beth 1956b]. Of course choice sequences in that paper are—much more than pseudo-valuations ever could be—connected to the intended meaning of the intuitionistic connectives; Beth was one of the very few people, not adhering to the intuitionistic point of view, but deliberately working in agreement with this point of view, and his highly technical paper was the start of some very complicated research by Dyson and Kreisel, and later Veldman, Friedman and Troelstra on the completeness of intuitionistic logic from an intuitionistic point of view ([Dyson-Kreisel 1961], [Veldman 1976], [Friedman 1975], [Troelstra 1977]).

His completeness result was heavily criticized as inadequate by Kreisel (e.g. [Kreisel 1956]). D. de Jongh remembers that Beth in one of his later conversations with him expressed the opinion that the difficulties that Kreisel pointed out would be overcome. Indeed, he had briefly indicated what was needed in his magnum opus [Beth 1959]; his idea was rediscovered by Veldman and Friedman; a contribution was also made by [Dummett 1977]. Nevertheless, the point of view of choice sequences blocked the way for him to the discovery of the models later known as Kripke models or possible worlds models. At least from the present day view possible worlds models for intuitionistic logic are just the combination of Beth's original models with his idea of pseudo-valuations. Beth had both ideas, but with pseudo-valuations one looks at intuitionistic logic more as an outsider, and he wasn't prepared to do that at that moment. In [Beth 1956b], p. 386, for example, he mentions:

> Already in 1938 a completeness proof for intuitionistic logic was given by Tarski. They [deJvU: Tarski and Rasiowa] start from Heyting's formulation of intuitionistic logic and, for this formal system, they establish a certain interpretation which is entirely based upon the structural properties of the system and has hardly any connection with intuitionistic mathematics itself.

and when he finally does get to do more purely formal work on intuitionistic logic, in [Beth 1961c], he still feels the need to cover himself up, and calls it 'logique inférentielle' (or 'derivative Logik' [Beth 1965]), saying:

> Dans notre Rapport No. 1 (du 1 mai 1961) [deJvU: [Beth 1961a]], la logique inférentielle fut appelée "<u>logique</u> <u>intuitioniste</u>"; nous avons choisi un terme plutôt neutre pour éviter toute discussion philosophique.

Kripke arrives on the stage. The story of the pseudo-valuations continues in January, February of 1957, the year after the main publication on intuitionism. Beth's friend H.B. Curry who later, after Beth's death, had his chair for a number of years, writes to him, January 24, 1957,

> I have recently been in communication with a young man in Omaha, Nebraska, named Saul Kripke. ... This young man is a mere boy of 16 years; yet he has read and mastered my Notre Dame Lectures and writes me letters which would do credit to many a professional logician. I have

> suggested to him that he write you for preprints of your papers which I have already mentioned. These, of course, will be very difficult for him, but he appears to be a person of extraordinary brilliance, and I have no doubt something will come of it. If you can possibly send the preprints or have them sent to him, I suggest that you do it.

Indeed, Kripke's letter with the expected request for Beth's papers follows on February 1 of 1957, and, on the 7th of February, Beth sends Kripke his two main papers on semantic tableaux for classical and intuitionistic logic, i.e. [Beth 1955b] and [Beth 1956b]. It is pretty obvious that these have had a considerable influence on Kripke. His two famous papers of the period after this, in 1959 and 1963, containing his new models with possible worlds, first for modal logic [Kripke 1959a] and then for intuitionistic logic [Kripke 1965], had a backbone of semantic tableaux. However, the exact order in which Kripke's ideas developed is somewhat unclear.

On August 25, 1958, Kripke's first article [Kripke 1959a] was received by the JSL; the publication date was November 26, 1959. In the article the completeness of the modal-logical system S5 with respect to Kripke's new possible worlds semantics is proved. In footnote 4 on page 12, Kripke says:

> In earlier work I carried this alternative proof out in detail, before acquaintance with Beth's paper led me to generalize the truth tables to semantic tableaux and a completeness theorem.

This footnote refers to a proof of the main theorem using truth tables instead of semantic tableaux, which is sketched by Kripke in his paper. Note that the JSL received his paper one and a half year after Kripke had received [Beth 1955b]. Note also that the completeness proof for S5 does in a sense contain Kripke's possible worlds, but not the accessibility relation that figures in Kripke's models, because in the S5 models it plays no role. [Kripke 1959a] was read in Beth's privatissimum in May 1960 (see [Nieland 1960]). On October 21, 1959, the JSL received Kripke's abstract [Kripke 1959b]. In it he announces:

> Semantic completeness theorems are now available for various systems of modal logic, using an appropriate model-theory to define com-plete-ness for each system, and using Beth's semantic tableaux to facilitate the proof.

It concerned a whole sequence of modal logics, among which the modal-logical system S4. Using truth tables these completeness theorems would be cumbersome to prove, to say the least. Semantic tableaux remained the only practical method of proving completeness until around 1970 Henkin type completeness proofs became popular (see [Bull-Segerberg 1984]). Kripke also mentions in his abstract that:

> The methods for S4 yield a semantical apparatus for Heyting's system which simplifies that of Beth.

It is important to note that this abstract gives little information. For example, it does not mention the notion of an accessibility relation. Also relevant is that the abstract [Kripke 1959b] only actually appeared on March 24, 1961 (note again here the confusing publication dates of the JSL in this period!) and that full publication of the results was delayed until 1963. Beth got more precise information than mentioned above presumably only in a lecture that Kripke held in Amsterdam that year; it is conceivable that Kripke sent him something before, but, if so, we have no found no trace. But more about Kripke's contributions to modal and intuitionistic logic and the contact in 1963 in a moment.

Beth's investigations under the Euratom contract. Beth did not sit out the considerable time that Kripke's results took in appearing. He had succeeded in obtaining a research contract with the European Community for Atomic Energy (Euratom). The start of the project was December 14, 1960. Two important points in the program of the project agreed on under this contract were (a) semantic tableaux and pseudo-valuations, (b) the further development of modal logic. He himself and his co-workers Nieland and de Jongh, the second of which arrived as a student in September 1961 and immediately joined the project, fulfilled these promises quickly. First, the above mentioned report to Euratom [Beth 1961c] and the above mentioned abstract in the JSL [Beth 1960b] appeared, later [Beth 1965] (received by the Archiv, January 6, 1962), all the time for pure implicational intuitionistic logic. Note that the relation ≤ (see the quote from [Beth 1960b] above) between the functions is an accessibility relation. In [deJongh 1962] de Jongh added the other connectives, and even discussed the general notion of intuitionistic connective in this context. De Jongh kept recurring to Beth's semantics all through his career, lately e.g. in [deJongh-Chagrova 1995]. Simultaneously with the work on intuitionistic logic, Beth and Nieland worked out the semantics for S4 in [Nieland-Beth 1961, Beth-Nieland 1965].

Let us recapitulate. In December 1960, Beth signed a contract with Euratom to work on pseudo-valuations and on modal logic. Clearly he had been thinking about this before. In March 1961, Kripke's abstract came out in the JSL. The abstract gave no indications of proofs. Kripke's paper on S5 that had appeared in 1959 contained the concept of possible world but not the concept of accessibility relation. It seems clear that one may assume that the essential ideas for this type of semantics were discovered independently by Beth during 1961. When Kripke lectured on the subject in Europe in the summer of 1963 (see below) Beth's results were well established. On the other hand, as we will see presently in the discussion on Kripke's results, there were other people on their way to the same concepts. Finally, one must remember that Beth's health was slowly giving out at this time, and that his illness took him away from his work for longer and longer periods until his death on March 12, 1964.

Kripke's work on modal and intuitionistic logic and Beth's relationship to it. Kripke lectured on his semantics for modal and intuitionistic logics on July 8, 1963 in the 8th Logic Colloquium in Oxford. A month later, on August 1963, he was a guest at the colloquium of the Euratom group in Amsterdam, and talked about intuitionistic logic (see Notulenboek [Minutes] Euratom 1961/1963 on August 20, 1963, and a letter from Beth to the Presidium of the University of Amsterdam of August 23, 1963). His results on modal logic appeared first in [Kripke 1963a] (propositional logic) and [Kripke 1963b] (predicate logic); his results on intuitionistic logic in [Kripke 1965], which are actually the proceedings of the earlier mentioned Colloquium in Oxford. In [Kripke 1963a] and [Kripke 1963b] he gives a list of people who had had similar ideas on modal logic: [Hintikka 1961], T.J. Smiley and McKinsey (based on [McKinsey 1945]), [Guillaume 1958], [Kanger 1957], [Jónsson-Tarski 1951]. Apparently he did not know of the Beth-Nieland work. In [Kripke 1965] he mentions [Beth 1960b] and [Dummett-Lemon 1958] with regard to semantics for intuitionistic logic. Kripke's work was clearly the most advanced and encompassing of all of these. He treated all systems in full generality and with all details properly filled out. He immediately progressed to applications to the logics in question.

The difference between Beth's original semantics for intuitionistic logic and Kripke's and Beth's second semantics is that in Beth's semantics, if a formula is false at a certain stage (world, point in time), it is always so that there is a future stage at which it is also false. Models will essentially always be infinite in this manner, whereas in Kripke's style this is unnecessary, at least in the propositional calculus. This has its influence on the semantic tableaux which are constructed in line with the semantics and are supposed to give the apparatus for proving completeness. In the original Beth tableaux the formulas on the right side of the tableau are cycled so that they reappear all the time in a new stage of the tableau. That a change in the tableau rules was necessary was not yet fully understood by Beth and de Jongh in 1961/1962. For the results on pure implicational logic in Beth's papers this actually does not make an essential difference. But [de Jongh 1962a] (as does [Beth 1965]) adds to the above quoted rules (S1) and (S2) for I-valuations ([Beth 1960b]) the rule (S3) stating that if A is an atom, w(A) = 0 and there is a world w′ < w, then there is a world w″ < w with w″(A) = 0. Theorem 2 of [deJongh 1962a] stating that this property transfers to all formulas is actually false. In subsequent work by de Jongh under Beth's supervision this was rectified. In [deJongh 1962b] and in de Jongh's master's thesis [deJongh 1964], which are again mainly concerned with the general concept of intuitionistic connective, the rule (S3) has been deleted again. That is also the case in the work of de Jongh and his co-student Kamp on programming intuitionistic propositional logic in 1963/64 ([Kamp-deJongh 1964]). Of course, there the rules of the semantic tableaux were by necessity made very precise for the sake of implementation. Naturally, this research had been stimulated by Beth's interest in 'thinking machines'. Already in [Beth 1955a] he discussed his 'traffic lights machine'. All his work on this ([Beth 1957, 1958, 1961d]) was theoretical (Beth in a letter to H.A. Simon of September 20, 1959: "I am not primarily interested in technological matters."). The work on programming intuitionistic and other nonclassical logics also extends into the present day with for example the Ph.D. thesis of de Jongh's student Hendriks [Hendriks 1996].

Intuitionistic logic and S4. As a last point it is worthwhile to spend some time on the relationship between S4 and intuitionistic logic. We already quoted [Kripke 1959b] on the subject. In [Kripke 1963b], which is mostly on modal logic he ends up remarking:

> Finally, we remark that, using the usual mapping of intuitionistic logic into S4, we can get a model theory for the intuitionistic predicate calculus.

He does not work this out, but it is clear that he is referring to Gödel's translation ([Gödel 1933], first fully proved in [McKinsey-Tarski 1948]) where, for example, $A \rightarrow B$ is translated as $N(NA^* \rightarrow NB^*)$ [N for necessity], if A and B are translated as A^* and B^* respectively. By this translation, in a possible worlds model of S4 with a reflexive and transitive accessibility relation, a Kripke-model of intuitionistic logic can be obtained. This will then have a reflexive and transitive accessibility relation, which is not necessarily anti-symmetric. But persistence (from [Beth 1960b]: (S1) if $w' \leq w$ and $w(A) = 1$, then $w'(A) = 1$) obviously holds. For that reason, worlds which are accessible from each other in that model will force the same formulas and can be identified with each other, after which a standard model with $\leq$ a partial ordering results. Also in his [Kripke 1965], p. 92, he mentions:

> The semantics for modal logic which we announced in [Kripke 1959b] and developed in [Kripke 1959a], [Kripke 1963b], together with the known mappings of intuitionistic logic into the modal system S4, inspired the present semantics for intuitionistic logic. It would in fact be possible to derive the completeness of Heyting's predicate logic in our semantics by using the mappings into S4 together with the results of [Kripke 1959a], [Kripke 1963b]. We prefer, however, to develop the semantics of intuitionistic logic independently of that of S4; this procedure will enable us, we believe, to obtain somewhat more information about intuitionistic logic, including the mapping into S4 as a consequence thereof.

[Nieland-Beth 1961] also states the existence of the mapping of intuitionistic logic into S4 as a theorem, and could have proved it from the semantics as Kripke did. However, their proof consists of one line only: "Il suffit de comparer les tableaux respectifs pour deux séquences quelconques K/L et K*/L*." This means a syntactic proof, and a syntactic proof is definitely possible and interesting, in fact it had been done for a sequent calculus by [Maehara 1954] (for the history of the result and some more information, see [Troelstra-Schwichtenberg 1996], p. 256), but it is surprisingly complicated. Probably the authors did not think it worthwhile to work it out completely, because in [Beth-Nieland 1965] the subject has been deleted from the paper.

If it is clear that Kripke obtained his inspirations about intuitionistic logic from modal logic, for Beth it was obviously the other way around. As we have seen he developed his new semantics for intuitionistic logic and the one for S4 simultaneously, and the new semantics for intuitionistic logic was very close to his original one.

Acknowledgements.

We thank H. Visser, A. Troelstra and J. van Benthem for pointing out important references and for other suggestions. The first author was very happy at the opportunity to talk at the conference "Evert Willem Beth and his philosophical friends" in Nancy-Méréville, April 22 to 24, 1998, in honor of his teacher, and in fact to talk about the origin of ideas he has kept working on since his education. We thank the Evert Willem Beth Foundation for giving this opportunity.

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