Tarski's Claim

THIRTY YEARS LATER

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Tarski's Claim Thirty Years Later (2010)

voor YLA – *weer jarig*

Abstract

Tarski's Claim (TC = Theorem 8 in [13]) follows from simple considerations in (type-free) lambda-calculus. The present note records essentially a proof of Lemma 1.1 in [16], i.e. TCL = the type-free lambda-calculus variant of TC, as well as a few historical comments appearing there. Additional remarks are meant to insure the fact that TCL can be transferred verbatim to typed lambda-calculus [TCLT]. (TCLT is just a notational variant of the derivation of TC in ordinary Łukasiewicz / traditional style.) The *Addendum* contains a transcription, in type-free lambda-calculus terms, of a Łukasiewicz / traditional style derivation of TC (notationally equivalent to TCLT), due to John Halleck [6] (September, 2010).

Tarski's Claim. Alfred Tarski claimed in 1925, without proof, the following (meta-) statement [hereafter TC]:

> Let L be a propositional logic in a propositional language containing at least implication (→). If L is finitely axiomatizable with modus ponens for → (and substitution) then L is also axiomatizable with a single axiom, and modus ponens (and substitution), provided it contains
>
> [**K**] p→(q→p) – [irrelevance]
>
> [**D**] p→(q→((p→(q→r))→r)) – [pairing]
>
> as theorems ('theses').

Apparently, the Claim above was first mentioned in print in 1929, by Stanisław Leśniewski, Tarski's PhD advisor, in [10], §§1–11[1]. Tarski published his Claim slightly later, still without a proof, as Theorem 8, in [13]. Beyond Leśniewski, the *original proof* of TC was known to other Polish logicians, during the early twenties (as, e.g., to Jan Łukasiewicz), as well as to some other people, mathematicians and / or philosophers, at a later time. Among them one could mention, for instance, Carew A. Meredith (who attended Łukasiewicz's lectures held at the Royal Irish Academy from 1947 on, cf. [14], page 514) and his occasional collaborator, Arthur N. Prior (cf., e.g., [15], §10, page 181). In retrospect, it is also likely that one could have included Bołeslaw Sobociński in the list. On the other hand, by the end of the seventies, any information as regards the 'original method' of proof seems to have been irretrievably lost. So, David Meredith (Carew's cousin), in correspondence:

[1] Cf. pp. 58–59, in the German original; the full paper, containing also §12, dated 1938, appears, in English translation, in his **Collected Works** [11], II, pp. 410–605; cf., specifically, [11], II, §10, page 467.

> "The oft talked about but rarely if ever documented 'methods of Tarski' for finding single axioms have been a longstanding sore point with me: by the time I realized they were not in every book on logic, it was too late for me to ask either of the people – Łukasiewicz or Carew A. Meredith – who could have explained them to me. So I've been doubly irritated with myself." (Letter of December 28, 1979 to me; a reference from [16], fn. 12.)

A Historical Aside. Alfred Tarski was concerned with axiomatizability problems while working on his PhD dissertation, [22], under Leśniewski[2]. Specifically, during the early twenties, he was involved, among other things, in studying the so-called 'Protothetic', a 'logistic' system proposed by his *Doktovater*. In modern terms, Leśniewski's Protothetic was meant to be the 'pure' logical segment of a larger, more ambitious foundational enterprise[3]. Tarski's doctoral thesis provided actually the starting point for Leśniewski's Protothetic. Incidentally, this explains Leśniewski's frequent references to the work of his PhD student[4]. Technically, the main result of [22] consists of the observation that the propositional connectives of the two-valued logic can be obtained from 'material' equivalence and the [propositional] universal quantifier. Whence, in the end, in view of the fact that the system contained appropriate type-distinctions, Leśniewski's 'General Theory of Sets' could have been based on three primitives, only – using equivalence, a general quantifier and something similar to the membership relation –, by using a single axiom, together with several 'directives', i.e., in our terms, appropriate rules of inference. For more details about Tarski's 'Polish period' see, e.g., the biography [2].

A Personal Aside (excerpted from [16], §0, and fn. 12, etc.). I came across TC – and the problem of reconstructing a would-be proof of it – some time around 1975, in Bucharest, when a Romanian mathematician interested in logic matters noticed, not without some irritation, that "Tarski did not usually publish proofs" (this is only in part true). TC was mentioned, conversationally, as a case in point.[5] As a matter of fact, this 'practice' concerns other Polish logicians active within the Lvov-Warsaw Logic Seminar, as well, and it has a quite reasonable explanation: during the twenties and the early thirties, the members of the Seminar produced a large mass of results in a relatively new discipline, by then, so that they were forced to state even important results without insisting in ultimate details on the specific methods of proof therein involved, and this

[2] Published first in Polish in [23], English translation in [27], pp. 1–23, etc. Much later, he came back to such problems (in algebra, mainly group theory), cf. e.g., [26], [28], in [29], II, IV.

[3] A 'General Theory of Sets'; cf. the title of [8], in [11], I, pp. 128–173 and [9], in [11], I, pp. 174–382.

[4] For a complete list of references to Tarski, see, e.g., the Index of [11], II, page 794.

[5] Another example could have been even the so-called Deduction Theorem [DT]. Although usually credited to Jacques Herbrand (circa 1934), the Frenchman comes much later into the picture on this: Tarski was aware of DT already in 1921 or 1922.

just in order to make available / understandable more involved ones. On the other hand, even though intriguing at a first look, TC is not a 'result' that could have been seen as an 'important' one, in itself. – As I was already familiar by then – i.e., during the mid-seventies –, with the so-called 'formulas as types' approach (or else with the 'functionality theory' of H. B. Curry, popularised by Curry himself, as well as by Carew A. Meredith, during the 1960's[6], I strongly suspected one could eventually prove TC in (typed) lambda-calculus. As nothing came out after a one moment's reflection (and I was, anyway, involved in some other – more interesting – things), a would-be proof of TC along such lines was duly post-poned. However, I came back later to it, in Geneva, around 1978–1979, while working for a PhD on (subsystems of) lambda-calculus under Dirk van Dalen (Utrecht). The actual thesis supervisor was Henk Barendregt, so, among other things, I was supposed to become familiar with what came to be known, later on, as 'The Bible' of the discipline ('type-free lambda-calculus': [1], first edition [1981], then still in manuscript). Barendregt had plenty of entertaining Exercises in the Bible, and many more in his trans-finite personal archive. In particular, the problem of the 'single point bases' for the (pure) 'type-free' lambda-calculus appears in [1], Chapter 8, pp. 161–162, as well as in Exercise 8.5.1 (containing two examples from J. B. Rosser: correspondence with Barendregt of 1971) and 8.5.15 (with four more examples from Carew A. Meredith, Barendregt himself, Corrado Böhm and J. B. Rosser; other examples of the kind – due to Carew A. Meredith, Ivo Thomas, J. B. Rosser, Corrado Böhm, W. L. van der Poel, J. W. van Briemen, etc., most of them unpublished – are also mentioned in [16]). As Barendregt's book concerned mainly the 'type-free' lambda-calculus, there was no special reason to mention the 'typed' case separately there (although Meredith's construction in Exercise 8.5.15, based on [15], involved implicitely a 'typed' case, motivated by logic reasons alone). A fortiori, there was no special reason to mention TC, in the Bible, either. – Nevertheless, I 'translated' TC in Biblical [type-free lambda-calculus] terms and came, after a (short) while, with a rather simple solution (fitting on less than a page in print). This is the main object of the present note and comes next.

The Singleton Basis Claim [TCL, idest Tarski's Claim 'translated' in type-free lambda-calculus (Lemma 1.1. in [16]).

> Let $\mathfrak{A}$ be a set of (closed) lambda-terms [combinators], such that [1] $\mathfrak{A}$ is closed under application and $\beta - reduction$, and [2] $\mathfrak{A}$ has a finite basis. Then $\mathfrak{A}$ has also a singleton basis, provided it contains the combinators $\mathbf{K} := \lambda$xy.x and $\mathbf{D} := \lambda$xyz.zxy.

Proof. Some convenient notation first. Set <X,Y> := λz.zXY (x not free in X, Y). So $\vdash$ **D**XY = <X,Y>.[7] Iterate this '[by accumulating] to the left', in the obvious way, taking care of the limit case (and counting from 1):

[6] The label comes actually from [7], a manuscript circulated from 1969 on, whence the current terminology in the literature on lambda-calculus: the 'Curry-Howard correspondence', cf. [20], etc. The first full documentation of 'Curry-Howard' in print appears in [21].

[7] This notation for pairs comes from Frege's *Grundgesetze der Arithmetik*, [5], I, 1893. In

$F_1 = {<}X_1] := X_1$,

$F_2 = {<}X_1,X_2] := {<}X_1,X_2{>}$,

$F_3 = {<}X_1,X_2,X_3] := {<}F_2,X_3{>} = {<}{<}X_1,X_2{>}, X_3{>}$,

and so on. In other words, the inductive step is:

$F_{k+1} = {<}X_1,\ldots, X_{k+1}] := {<}F_k,X_{k+1}{>}$ (up to $k < n - 1$).

That is to say, for $n > 0$, $\vdash \mathbf{D}\ldots\mathbf{D}X_1\ldots X_n = F_n = {<}X_1,\ldots, X_n]$ ($n-1$ times $\mathbf{D}$). (Here one can read β-equality = β-conversion as β-reduction, as well.) Let $\{X_1,\ldots, X_n\}$ be the finite basis of the hypothesis. For $n = 1$ there is nothing to show. If $n > 1$ set

$F := F_n = {<}X_1, \ldots, X_n] = \mathbf{D}\ldots\mathbf{D}X_1\ldots X_n$, as above, and

$G := {<}\mathbf{K},\mathbf{KK},F] = {<}\mathbf{K},\mathbf{KK},{<}X_1, \ldots, X_n]]$.

Obviously, G is in the set $\mathfrak{A}$ (since so are $\mathbf{K}$, $\mathbf{D}$ and therefore F, as $\mathfrak{A}$ is supposed to be closed under β-reduction). On the other hand, one checks easily (just a matter of milliseconds with a lambda-calculus reduction machine) that

$\vdash GG = \mathbf{K}$,

$\vdash G(\mathbf{KK})\mathbf{K} = F = {<}X_1, \ldots, X_n]$,

whereas extracting the X_i's ($0 < i < n + 1$) from $F := F_n$ is straightforward, even by hand:

$\vdash F\mathbf{K}\ldots\mathbf{K} = {<}X_1, \ldots, X_n]\mathbf{K}\ldots\mathbf{K} = {<}X_1, \ldots, X_k]$

($n-k$ times $\mathbf{K}$ postponed, $k \in \{1, \ldots, n-1\}$, so, in particular,

$\vdash F\mathbf{K} = {<}X_1, \ldots, X_n]\mathbf{K} = X_1$ (for $k = n - 1$, i.e., $n - k = 1$), and

$\vdash F_k(\mathbf{KK})\mathbf{K} = {<}X_1, \ldots,X_k](\mathbf{KK})\mathbf{K} = X_k$, for all $k \in \{2, \ldots, n\}$. QED.

[As promised, the argument fits on less than a page in print.]

Remark 1. Certainly, one could have 'accumulated' Frege-Church pairs 'to the right' in F, as well (Barendregt's own preferred way of gathering in his [1]), but the latter choice would have required different 'projections' (and so 'extraction patterns'), beyond the mere pretty old $\mathbf{K}$. [In practice, the underlying 'extraction patterns' were meant to be short and 'easy', first of all.]

this respect, Church's initial paper on lambda-calculus ([3], 1932, [4], 1933) contains just an independent rediscovery. (From private correspondence with Alonzo Church, it turns out he managed to read Frege's *Grundgesetze* – as well as [19], in fact –, only later.) Whence also the terminology used here: 'Frege-Church pairs'.

Remark 2. Obviously, the assumption that $\mathfrak{A}$ contains **D** can be weakened [down] to: $\mathfrak{A}$ is closed under the 'rule' (**D**): $\mathrm{X},\mathrm{Y} \in \mathfrak{A} \Rightarrow <\mathrm{X},\mathrm{Y}> \in \mathfrak{A}$. (Yet, Tarski, as well as his former Polish colleagues – Stanisław Jaśkowski and Mordchaj Wajsberg excepted, perhaps – was less concerned with rules, however; in any case, he vastly preferred a 'Satzlogik' instead of a 'Regellogik', so to speak: an algebraist's habit more or less.)

Remark 3. [TCLT = the 'typed' variant of TCL]. TCL exemplified in the above can be repeated in typed lambda-calculus. (This step is particularly boring[8], but it is, ultimately, trivial for the case in point. In general, such a step is *necessary*, however, because we can have singleton bases for sets of closed terms in type-free calculus for which the 'extraction pattern' has no 'typed' counterpart. As reported by Arthur N. Prior in 1963, an example of the latter kind was produced first by Carew A. Meredith, some time around 1956. Cf. [15], §9, '*A Combinatory Base Without C-Positive Analogue*' and / or the Appendix [18] of [17], '*On a Singleton Basis for the Set of Closed Lambda-terms*'. Meredith's 1956-example was $\mathrm{G} := \lambda \mathrm{xyz.y}(\lambda \mathrm{u.z})(\mathrm{xz})$, with $\mathrm{GGG} := \lambda \mathrm{xy.y}(\mathrm{xx})$ [untypable, of course], the latter term being used in the 'extraction' of $\mathbf{C}_*$ [= **CI**] = G(GGG)G, as well as **I**, **K′** [= **CK**] and, ultimately, **K**. – Actually, Carew A. Meredith claimed the latter fact without a proof, and it took us quite a while – several people on two continents, not just me, appropriate software included –, before I was able to realize how it was meant to be done!)

Remark 4. The proof of TC appearing in [17] is a generalization of the above (in a variant of the 'typed' lambda-calculus, using [Carew A.] Meredith's 'condensed detachment' and the like). As the paper is available in print, there is no point in pausing on it again, once more.

A Final Remark and a Moral. As a matter of fact, there is no need to repeat the previous construction in 'typed' lambda-calculus, because, *in this case*, TCLT ['typed'] follows from TCL ['type-free'] and the observation that F and G are head normal forms (hnf's) of *a very special kind*, since we have also the following rather obvious Metatheorem:

> Let $\mathrm{H} := \lambda \mathrm{x}_1 \ldots \mathrm{x}_m.\mathrm{x}_j \mathrm{X}_1 \ldots \mathrm{X}_n$ be a closed term in hnf, such that the X_i's are closed terms $(0 < i < n+1, 0 < j < m+1)$. If all the X_i's are stratifiable (= typable = have a principal type [scheme]) then so is H (and a principal type [scheme] of H can be derived effectively from the principal types of its 'components' X_i).

Indeed, our G was a Frege-Church pair

$$\mathrm{G} := <\mathbf{K},\mathbf{KK},<\mathrm{X}_1, \ldots, \mathrm{X}_n]] = <<\mathbf{K},\mathbf{KK}>,\mathrm{F}> = \lambda \mathrm{x.x}(\lambda \mathrm{y.y}\mathbf{K}(\mathbf{KK}))\mathrm{F},$$

[8] Especially so, if you don't already have at hand a convenient Robinson-like soft-engine, to do 'unifications', and so to find 'most general unifiers', for you, in milliseconds.

i.e. of the form H above, as well as <**K**,**KK**>, with **K** and **KK** typable; so G must be typable if so is F. But F is a Frege-Church pair, too, and by (repeating inside-out) the same argument, F is typable because so were supposed to be its 'ultimate' subterms X_i $(0 < i < n + 1)$. As an[other] aside, a similar argument applies – *mutatis mutandis* – to Church n-tuples in general $(n > 2)$, i.e., terms of the form $\lambda x.xX_1 \ldots X_n$, with all X_i's typable closed terms $(0 < i < n+1)$. Since most examples of singleton bases for the full (pure) lambda-calculus mentioned in passing earlier (as, e.g., those provided by J. B. Rosser, W. L. van der Poel, J. W. van Briemen, etc.) were just Church n-tuples of typable combinators, with $n > 2$, there was no need to check the typability of the derivations (the 'extraction patterns') in detail in order to establish the fact that their (principal) types were also single axioms for Heyting's pure implication. (An example, the shortest of the kind, is Rosser's G = <**K**,**S**,**K**> = λx.x**KSK**, with ⊢ GGG = **K** and ⊢ G(GG) = **S**; cf. [1], Proposition 8.1.4.) The Moral is that – in such (rather specific) conditions – we can always forget about *what we prove* (i.e. the formulas / types themselves) and pay attention only to the *form of the proofs* (i.e. to the corresponding lambda-terms).

Addendum. Based, apparently, on an approach due to Dolph Ulrich (2004–2005), John Halleck provided by e-mail [6], dated September, 23, 2010, the following variant of the proof above (a fully 'typed' version of it, with detailed 'traditional'/ Łukasiewicz-style derivations in propositional logic: a remarkable achievement, as well as a unusual example of patience, perhaps, quite rare by post-modern standards, these days). On notational reasons, we start, this time, counting from 0. So the finite basis of the hypothesis is going to be now $\{X_0, \ldots, X_n\}$, n natural (with, for $n = 0$, nothing to show, as above). Set also **K**(X) := λx.X (x not free in X), for arbitrary X. We iterate the Frege-Church pairs as before, but with **K**(X_j) – instead of X_j (parentheses are just to improve readability here) –, after the first one:

$F_0 = X_0$,

F_{k+1} := <F_k,**K**(X_{k+1})>,

so that

F := F_n = <X_0,**K**(X_1), …, **K**(X_n)]

(equally compact and pretty readable). Now, once we have **K**, we can get X_0 and **K**(X_j) := $\lambda x.X_j$ $(0 < j < n+1)$, as before, whence also the X_j's $(0 < j < n+1)$, as well, since ⊢ **K**XY = X, for arbitrary X, Y. (Halleck used actually a different 'extraction pattern', a trifle more involved.) Of course, in view of the above, the use of **K**'s in F is not necessary (but, as already announced, I'm just recording the content of Halleck's [6] in type-free lambda-terms). Finally, set

H := <F,**KK**> and

G := **K**(<H,**KK**>) = **K**(<<F,**KK**>,**KK**>).

Halleck first noticed that

$$\vdash \mathrm{GGG} = \mathbf{K}(\mathbf{KK}),$$

and obtained **K** as

$$\vdash \mathrm{GG}(\mathrm{GGG}) = \mathbf{K}.$$

Actually, we have, in detail,

$\vdash \mathrm{GX} = <\mathrm{H},\mathbf{KK}>$, for arbitrary X (so, in particular, choose X := G), while

$\vdash \mathrm{GX}\mathbf{K} = \mathrm{H} = <\mathrm{F},\mathbf{KK}>$, for arbitrary X (choose X := G, again),

wherefrom one can get also $\mathrm{F} := \mathrm{F}_n$, by

$$\vdash \mathrm{H}\mathbf{K} = \mathrm{F} = \mathrm{F}_n.$$

In particular, Halleck's G is a special hnf of the kind discussed earlier (a term of the form **K**X is typable if so is X, G is typable if so is $<\mathrm{H},\mathbf{KK}>$, the latter pair is typable if so is H, while H is a Frege-Church pair with typable 'ultimate' components, etc.). So, in view of the Metatheorem referred to in the above, there is no need to check in detail the 'typed' version of the argument, either.

A Last Comment (on the non-technical topic of 'conceptual means'). To be fair, Halleck's point in his note [6] was meant to show that Tarski *could have* produced a proof of TC 'with the conceptual means of his time', that is: without using (typed) lambda-calculus (the type-free lambda-calculus appeared in 1932–1933 in print while the 'typed' variant came out even later, around 1937–1938. Halleck's argument – 'full-spelling' in Old Polish – was meant to invalidate a previous (non-technical) claim of mine, which, roughly, consisted of saying that Tarski must have known / anticipated some lambda-calculs and / or combinatory logic (at least an 'applied' form of it, as Henk Barendregt used to think about such things in his early 'type-free' life), and that, as (I said) "there is, essentially, no other way of proving TC" (an approximate self-quote). Halleck's argument showing TCLT in the spirit of the early twenties (Łukasiewicz style derivations in propositional logic) does not 'prove', however, the fact that my non-technical claim is wrong. After all, I could have provided myself a full, 'typed' variant of the proof of TC above, as well (by writing down due derivations in propositional logic à la Łukasiewicz, with substitutions displayed explicitely, and so on): the outcome would have been even somewhat simpler / shorter than the alternative one, as displayed in [6]. (As already noted above, there was no need to use cancellators, **K**'s, in Halleck's F.) Whereby, my proof of TC (TCLT, in fact) would have used 'the conceptual means of the early twenties', as well! Both arguments (Halleck's, as well as mine, in its TCLT variant) are ultimately 'based on typed lambda-calculus' – actually on 'typed combinatory logic', because we can also rephrase the whole story, once more, in terms of the Schönfinkel-Curry 'combinatoruy logic', anyway. Yet, in order to produce the argument for TC, one must be aware of the abstract (reduction / conversion) behavior of **K** and **D**, at least (resp. of **K** and <X,Y>), or else, one must be aware of 'equivalences' like **K**XY = X, **D**XYZ = <X,Y>Z = ZXY, whatever the actual notation, and the 'intended meanings'. Historically speaking, such things *could have been* available to Tarski at some time-point before 1925, I suppose. In fact, Moses Schönfinkel did read his paper on combinators [19] in Göttingen, at a local mathematical conference, on December 7, 1920, and this pioneering paper (actually prepared for publication – from, the author's talk –, by Heinrich Behmann) was already in print by 1924. Moreover, Leśniewski referred to [19] explicitely (as well as to John von Neumann [1927], who used a related form of 'combinatory logic' in his foundational papers), although the Pole claimed somewhat later he was not 'acquainted with' Schönfinkel's paper while preparing [10]. (See the Index to [11], for exact references.) In view of this, whatever the *actual historical detail*, it is sensible to suppose that Tarski *could have* used, around 1925, some form of the so-called 'typed combinatory logic' and / or some form of the 'formulas as types' approach (kind of a crude variant of the Curry-Howard correspondence), at least for the purpose of proving TC. (A minor result, in the end, as already noticed in the above.) The fact that neither Tarski (nor Leśniewski, Łukasiewicz and, later, Jaśkowski, for that matter), did exploit explicitely the [Curry-Howard] 'correpondence' in order to recover a would-be meta-theory (of proofs / derivations) can be explained in various ways, and is irrelevant in this discussion. We can speculate, at best. Yet, I'd rather post-pone such speculations, for a would-be less 'technical' talk.

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[9] The revision concerns, essentially, the typography.

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