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**Virtues and Vices of Interpreteted
'Classical' Formalisms: Some Impertinent
Questions for Pavel Materna on the
occasion of his 70th Birthday**

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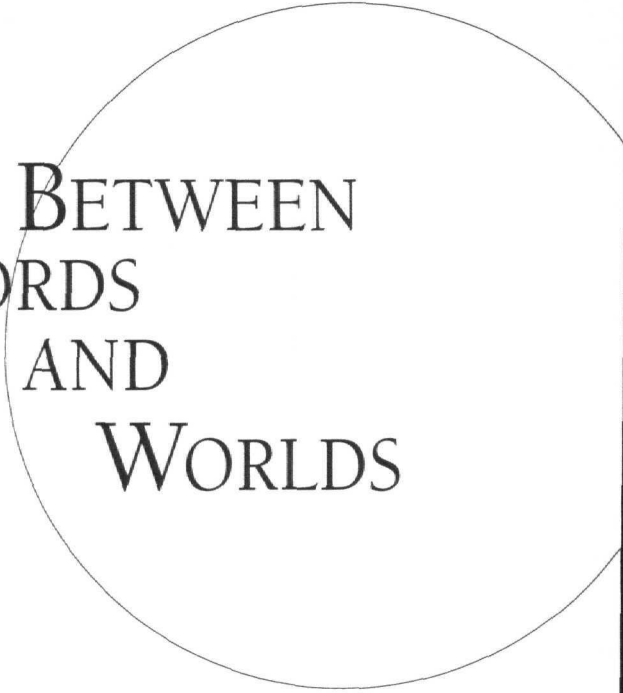
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BETWEEN
WORDS
AND
WORLDS



A FESTSCHRIFT FOR PAVEL MATERNA

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Virtues and Vices of Interpreted *Classical* Formalisms:

Some Impertinent Questions for Pavel Materna
on the Occasion of His 70th Birthday

GÖRAN SUNDHOLM

1. The Background to the Charge

My acquaintance with the work of the late Pavel Tichý stems from the Wittgenstein Centenary Symposium at Kirchberg in August 1989. His book *The Foundations of Frege's Logic* had just been published at forbidding cost by de Gruyter in Berlin and was on offer at a discount. From a subjective point of view it was immediately clear that the work was highly interesting and probably important: even a cursory inspection showed that Tichý shared many of my logical prejudices and preferences. That I bought the book was a foregone conclusion.

On further acquaintance with the work it was manifest that my first impression had been right: Tichý was a powerful writer with a highly interesting story and strong words with which to tell it. I could not but regret that his chosen title and mode of exposition carry the suggestion of his being primarily interested in Frege-exegesis: first and foremost, the book is a provocative presentation of Tichý's own position and not that of Frege. Possible because of this discrepancy between title and content, Tichý's work has been undeservedly neglected.

The work attracts through terse and pointed statements, for instance:

Twentieth-century logicians turned away from Frege not because they refuted him but because they decided to ignore him. ... A new paradigm arose; and paradigms, of course, do not assert themselves through rational argument but through intellectual stampede.

I beg to be excused from joining the stampede called symbolic logic. Turning logic into the study of an artificial language (which nobody speaks) does not strike me as the height of wisdom.¹

Also the fine Chapter 13 on Inference is highly congenial, with its stress on the meaningfulness of the underlying formal logic and its trenchant ar-

¹ Pavel Tichý, *The Foundations of Frege's Logic*, Berlin, de Gruyter, 1988, Preface, p. viii.

guments against the unreflective use of mere assumptions in contentual derivations of the Natural-deduction style. However, it proved quite difficult to go beyond such first flares of recognition. The book is not written in an accessible key and did not provide a compelling reason for putting in the (not inconsiderable) intellectual effort that is necessary in order to master the Transparent Intensional Logic, Tichý's foremost formal creation.

I gained deeper understanding only in the late 1990's when I became a regular participant in the annual LOGICA conferences that are organised at Castle Liblice by the Department of Logic at the Institute of Philosophy of the Academy of Sciences of the Czech Republic. As so often it was not further reading, but personal contact, that provided the necessary insight. In my case the catalyst was the passionate impromptu expositions of Pavel Materna (rather than his formal conference talks). These discourses were delivered either in the congenial surrounding of the castle garden at Liblice, or Socratically in his favourite Prague wine bar, and served to rekindle my interest in Tichý's work, and TIL in particular. I also realised that I was not the only one to be intrigued (or *tured?*) by Pavel's insistent advocacy, a fact to which the present volume bears ample witness; it is gratifying to see how Materna's untiring work in promoting interest in Tichý's work has begun to bear fruit.

The most important difference between Tichý's work and 98.3 % of the rest of contemporary logic is that TIL uses an *interpreted* formal logic. After the advent of Hilbert's metamathematics and its maturation in the works of Gödel, Tarski, and Bernays from the early 1930's, the formal languages used in logic have been uninterpreted (meta)mathematical objects of study. A well-formed formula ϕ in such a language does not say or state anything. It is not intended for communication, and does not signify, but serves merely as an object of study. The wff's are not used for speaking; we only talk *about* them. Within the ensuing metalogical tradition a great logician is one who proves deep mathematical theorems concerning metamathematical formalisms. Here the paradigm is still constituted by the theorems of Gödel: Completeness, Incompleteness and Consistency of the Continuum Hypothesis.

Previously, though, the task of a logician was not to prove such theorems about formal systems; on the contrary, his task was to design a sizeable formal language in which mathematics could be carried out. In particular, the formal language of such a system had to be an interpreted one. Examples of such systems are provided in the great works of Frege and Russell: Frege's "conceptual notation" (*Begriffsschrift*) and Russell's Ramified theory of Types are both interpreted systems. Wittgenstein's *Tractatus*, on the other hand, can be seen as attempt to provide the required meaning-explanations for the systems of the Frege-Russell kind. Less well-known examples

are provided by the systems of Curry, Church, and Quine. Lesniewski must count as a major exponent of the trend. The logical work of Rudolf Carnap also falls within this paradigm. It is possible to view even the later Quine, at least the Quine of the time-slice corresponding to *Mathematical Logic*, as belonging to this group. After 1940, however, it is very rare to find authors that still adhere to the older paradigm. Alonzo Church's classic *Introduction to Mathematical Logic* from 1956 is an example of a later work which looks (or *longs?*) back to the earlier period.

2. The Charge

Of contemporary systems only the Intuitionistic Type Theory designed by Per Martin-Löf and Pavel Tichý's TIL even begin to approach the efforts of the older paradigm. My admiration for Tichý's courage is great; his surety of touch in opting for the construction of a sizeable interpreted formal system is impressive. When giving his meaning-explanations Tichý, like his mentor Gottlob Frege, strives for a full-blown, all-out realist version that validates all of a modal ramified *classical* type theory. And this is precisely the spot where I beg to be counted out. I simply do not believe that the meaning-explanations of Tichý do establish what has to be established.

However, without the valuable expositions unselfishly offered by Pavel Materna in his survey *Concepts and Objects*, I would not even have begun to understand "the great works of Tichý".² Accordingly, I will avail myself of the occasion and address my worries, not to Tichý, but to his namesake Materna, and in particular to the explanations offered in his monograph. These worries are largely variants of criticisms offered from within the constructivist camp in the philosophy of mathematics, from Kronecker onwards.

Tichý's TIL is a type theory, not unlike the simple type theory of Church, but it has more ground (or *base*) types than Church's type 0 (of truth-values) and type 1 (of individuals). The additional base types in (Materna's version of) TIL are the type ω (of possible worlds) and the type τ (of real numbers that also serve as indices for time-points). Thus, the base is the set $\{0, 1, \omega, \tau\}$. Furthermore the type-structure over this set of basic types is generated using *partial* instead of total functions:

- (i) Every base type is a type.
- (ii) When β_1, \dots, β_k , and α , are types then $(\alpha\beta_1, \dots, \beta_k)$ is the type of partial functions from (a subset of) $\beta_1 \times \dots \times \beta_k$ to (a subset of) α .

My worries concerning TIL largely pertain to these notions only. What Materna does using his framework has, *grosso modo*, my general blessing, especially, for instance, concerning "constructions". However, in my opin-

² Pavel Materna, *Concepts and Objects*, Acta Philosophica Fennica 63, Helsinki 1998. Arabic numerals within brackets are page references to this monograph.

ion, the explanations offered for the ground notions do not suffice to turn his formalism into an interpreted formal language: they simply do not deliver the required goods.

3. The Case against the Base Type ι

What is an individual? Materna's answer is

Individuals are simple material entities possessing no non-trivial property essentially. (27)

For sure, this explanation of the notion of an individual does not make it easy to know individuals. Are there any individuals at all? How do we know that something is an individual?

First, it seems, we have to know that something is a "simple material entity"? What is an entity, that is, what are the application- and identity-criteria for the type (kind, sort, ...) *entity*?³ Is *material* a property of entities? Or is *material entity* a type (kind, sort, ...)? If so, what are the application- and identity-criteria for *material entity*? Whatever the answer to these questions, there remains the answer to the questions concerning *simple*. Does it qualify *entity*, *material* or *material entity*? Depending on the answers here a whole tree of alternatives is generated. So the notion of a *simple material entity* seems anything but simple.

Secondly, when we finally have got to know a *simple material entity* we have to know that it has no non-trivial property essentially. What is a *non-trivial* property? What is it to have a property (be it trivial, or not) *essentially*?

Furthermore, an application criterion alone does not suffice to fix a type; there also remains the question of the appropriate *identity*-criterion. That is, we need also an answer to the question:

What is it for two individuals to be equal individuals?

This clearly demands an answer to the question what it is for two simple material entities to be equal simple material entities. This, however, is not enough. It must also be taken into account what effect, if any, that privation with respect to non-trivial essential properties has for the identity of simple material entities.

This battery of questions shows the lengths to which one has to go before one has the right to make a claim of the form

$t \in \iota$,

³ Concerning the need for application- and identity-criteria, see my "Identity: Absolute, Criterial, Propositional", *The LOGICA Yearbook 1998*, ed. Timothy Childers, Prague, Filosofia, 1999, 20–26.

that is,

t is an individual.

Whether I (or we) would ever be in that position, I feel very uncertain about.

4. The Case against the Base Type 0

What is a truth-value? From Materna's text the following answers can be culled (21). The truth-values are TRUE and FALSE. They are objects. Sentences denote truth-values. The denoting ("having") can depend on empirical facts. Indeed:

The entities which can be—depending on empirical facts—true or false (or lack any truth-value) are usually called *propositions*. (27)

Since the truth-values form a *type* 0 I will yet again press the questions concerning application- and identity-criteria. This time an answer seems to be forthcoming. TRUE and FALSE are both truth-values, where TRUE = TRUE and FALSE = FALSE, and the rule

$$\frac{S \in t \quad P[\text{TRUE}/z] \quad P[\text{FALSE}/z]}{P[S/z]}$$

is valid.

Materna's explanation of propositions, though, in the quote above, seems to make any entity into a proposition. Entities which can be true or false are propositions, but so are also entities which can, possibly even depending on empirical facts, *lack* a truth-value. But *anything* non-linguistic can lack a truth-value, so anything non-linguistic fulfils Materna's condition for proposition-hood. Surely this cannot be right? (In the sequel I will disregard this unfortunate rider of Materna's.)

When propositions are construed in such a (roughly) Fregean manner, as (ways of determining) truth-values, Kronecker's constructivist criticism of classical logic becomes acute. We consider, with Kronecker, a classical function $f \in \mathbb{N} \rightarrow \mathbb{N}$ that is defined by a non-decidable separation of cases:

$$f(k) =_{\text{def}} \begin{cases} 1 & \text{if the Riemann Hypothesis is true} \\ 0 & \text{if the Riemann Hypothesis is false.} \end{cases}$$

According to Kronecker, and I agree, f is *not* well-defined, that is, the rule does not give a function from \mathbb{N} to \mathbb{N} . Because consider $f(14)$, say. There is at present no way of evaluating this to primitive form as an Arabic numeral, since we cannot (yet) decide the Riemann hypothesis. Thus *the definition* of f introduces defined, non-primitive terms, which cannot be eliminated in favour of primitive terms. As such it goes against the canons of proper definition that were clearly stated three centuries ago by Pascal.

Similar considerations now show that under the (Frege-)Materna conception of propositions the universal and existential quantifiers will similarly introduce non-eliminable non-primitive expressions. The correctness of the \forall - and \exists -formation rules is not at all clear. We consider the Fregean explanation of the universal quantifier. Let F be a propositional function, that is, a function F such that

$$F(x) \in \circ, \text{ provided that } x \in D.$$

Thus, for any $a \in D$, $F[a/x]$ is, or, better, *evaluates to*, a truth-value in \circ .

Then we define with Frege:

$$(\forall x \in D)F(x) =_{\text{def}} \begin{cases} \text{true if } F(x) = \text{true, whenever } x \in D \\ \text{false otherwise} \end{cases}$$

The definiens contains a separation of cases that cannot be decided effectively as soon as the domain D is infinite or otherwise unsurveyable. The analogy to Kronecker's example above is obvious. Accordingly, Frege's classical \forall -formation rule (and, of course, \exists -formation as well) introduces defined, *non-eliminable* ways of determining truth-values. That is, the type \circ of truth-values will have to contain non-primitive elements that cannot be eliminated by means of evaluation to primitive form. Note also that it is quantification with respect to an infinite, or unsurveyable, domain D that poses the problem. Quantification with respect to (propositions, that is, ways of determining) truth-values is perfectly straightforward:

$$(\forall x \in \iota) F(x) =_{\text{def}} F[\text{TRUE}/x] \ \& \ F[\text{FALSE}/x],$$

since there are only two of them.

5. The Case against the Base Type ω

What is a possible world? Materna (25 ff.) pays allegiance to the current paradigm of possible worlds concerning the semantics of modality. He is aware of the need for careful explanations and offers:

Any possible world is a *consistent set of facts*. ... Further, in a very clear sense a possible world is a *maximum such set of facts*. (26)

The facts, I presume, are the ontological correlata to propositions, so that a proposition is true *at* a world if the corresponding fact holds *in* the world and false at the world if it does not hold *in* the world. In view of this, I am not happy with Materna's use of 'fact' here. Following Wittgenstein's *Tractatus*, I would prefer 'state of affairs' (*Sachverhalt*) or 'situation' (*Sachlage*) where Materna has 'fact'. Another alternative would be to use *circumstance*, a good, neutral term. The, or at least, a point of possible-worlds semantics is, I take it, that a proposition that is true at one world need

not be true at another. Accordingly, the ontological correlata to propositions, whatever we may call them, must be able to hold, or obtain, in some worlds, and fail to do so in others. If there be non-actual possible worlds (and if there are not, why bother about the whole machinery), each must contain something counterfactual, that is, a *circumstance* (situation, state of affairs) which obtains in the world in question, but not in the actual world, and similarly circumstances that obtain in the actual will not hold in the counterfactual world. Materna's 'fact'-terminology now forces us to speak about "non-obtaining facts". To my mind this comes close to a contradiction in terms and hence I would opt for one of the other alternatives. This, however, is a minor terminological point only, but one, the neglect of which makes the theory sound strange.

However, there are also real, rather than merely terminological problems with Materna's conception of a possible world as a maximum consistent set of circumstances. But for *circumstance*, about which enough said, the three operative terms are *set*, *consistent* and *maximum*. What conception of set does Materna have in mind here? Clearly not the constructive notion of sets as constructive type, but also the classical notion of set as an element in the cumulative hierarchy will not serve here, because in general the elements of the consistent sets will be circumstances, rather than mathematical objects. Thus one needs to develop a novel "empirical" theory of sets (which, to put it mildly, seems to be a major task indeed) prior to using the conception of sets with respect to facts.

The lack of a suitable notion of set is not the least of the problems that beset the conception of possible worlds as (maximally consistent) sets of circumstances. Let λ be the cardinal of the "set" of possible worlds. Then (at least classically) there are 2^λ "sets" of possible worlds. But each of these serve to single out a proposition, that is, a way of determining a truth-value. Thus there are, at least, 2^λ propositions. But circumstances, that is, the elements of possible worlds, are the ontological correlata of propositions. Thus, there would have to be also 2^λ circumstances. But then there are many more, or at least as many, maximally consistent sets of circumstances. That is, there are 2^λ possible worlds, but by assumption also λ is the number of possible worlds. Therefore, $\lambda = 2^\lambda$. However, we know from Cantor's theorem that $\lambda < 2^\lambda$. Thus we have a contradiction. I do not wish to imply that this reasoning will necessarily refute Materna's theory, but it is indicative of the difficulties that beset possible-worlds semantics when interpreted as a proper semantics for a sizeable language.

Furthermore, but for these difficulties that pertain to the notion of set and the resulting notion of proposition, there are also problems concerning the notion of a *maximum consistent* set of circumstances. Usually a set of propositions is consistent if no contradiction follows *logically* from it. Here,

though, the elements of consistent set are, not propositions, but circumstances ("facts"). (Here another unfortunate side in the choice of "fact" is revealed; *any set of facts is consistent*, since it is realised in the real world.) What does it mean for circumstances to be consistent? According to what logic? In the *Tractatus* the states of affairs are all logically independent, so the obtaining of one state of affairs (*Sachverhalt*) is entirely neutral with respect to the obtaining or non-obtaining of all other states of affairs. It is not at all clear in what consistency consists for sets of circumstances.

Finally, the maximality of the consistent sets poses problems. How do we know that there are any maximum consistent sets?. The standard ("Lindenbaum") technique for expanding consistent sets of propositions to maximally consistent sets makes use of an undecidable separation of cases: if a proposition can be added while preserving consistency add it, otherwise add its negation. In general, though, we cannot decide whether a set of propositions is consistent or not. In the case of predicate logic, such decidability is even mathematically ruled out by the Church-Turing theorem on the undecidability of predicate logic. The proposition ϕ is logically true (or, is a theorem) if and only if the singleton set $\{\neg\phi\}$ is inconsistent. So if we have a means for deciding consistency we also have a means for deciding theorem-hood; but the latter cannot be, according to Church and Turing. So how do we know that maximum-consistent sets exist? There is one kind of proof that does not make direct use of the Lindenbaum-construction; instead one observes that the notion of consistency is of finite character and applies one of the many classical equivalents of the Axiom of Choice, say, a maximum principle such as Zorn's lemma, or, perhaps most easily, the Teichmüller-Tukey Lemma. It is one of the great and persistent myths of 20th century mathematics that a constructivist is someone who rejects the Axiom of Choice. On the contrary, that AC is constructively true can be seen directly from the (constructive) explanation of the meanings of the quantifiers. Because a proof of

$$(\forall x \in D)(\exists y \in E) A(x, y)$$

yields a method, which applied to a given $d \in D$, produces a proof of

$$(\exists y \in E) A[d/x, y].$$

Such a proof, on the other hand, is nothing but an ordered pair $\langle a, b \rangle$, where $a \in E$ and b is a proof of $A[d/x, a/y]$. So piecing this together, by mapping $d \in D$ to the matching $a \in E$, one obtains a function $f \in D \rightarrow E$ that effects the required choice of a y for an x . Thus a proof has been given of

$$(\forall x \in D)(\exists y \in E) A(x, y) \rightarrow (\exists f \in D \rightarrow E)(\forall x \in D) A[x, ap(f, x)/y].$$

The myth contains some truth, though, in that many of the classical equivalents of the Axiom of Choice do not appear to hold constructively. In particular, the maximum principles are purely existential and offer no means of constructing the maximum element for which existence is claimed. Indeed,

the standard proofs of Zorn's lemma and other equivalents from the Axiom of Choice all make use of the principle of defining functions by means of undecided separation of cases, which as we saw above, *à propos* of Kroecker, is not justified.

6. The Case against the Base Type τ

Here we can be reasonably brief. In principle, I have no quarrel whatsoever with the use of the set of reals. Many perfectly constructive treatments are known, the smoothest being, perhaps, that which uses constructive Cauchy-sequences. However, Materna also uses the elements of the base type τ as points of time, and this requires more elaboration. Why do the points of time have to form a continuum? Does not a discrete conception agree as well with our intuitions? In that case the order-type of the points of time would be that of the integers \mathbf{Z} , rather than that of the reals \mathbf{R} , that is, order-type $(*\omega + \omega)$ rather than order-type θ . (Here, of course, ω are Cantorian order-types, and not Materna's type of possible worlds.)

How will Materna decide between these option in favour of the reals? And how will he treat of discrete time, given his choice of the reals as time-indices?

As it stands, his choice of the reals for the task of serving as indices of points in time appears without sufficient grounds.

7. The Case against Partial Functions

In standard type theory, one opts for *total* functions as the objects of the function type, and not without good reason, it seems to me. The application-criterion for the (in general, dependent) function-type $(x \in \alpha)\beta$ is that one must know the rule

$$\frac{a \in \alpha}{f(a) \in \beta[a/x]},$$

in order to have the right to assert that

$$f \in (x \in \alpha)\beta,$$

that is,

when one is entitled to go from $a \in \alpha$ to $f(a) \in \beta[a/x]$, one is also entitled to assert that $f \in (x \in \alpha)\beta$.⁴

In the case of partial function-types nothing as simple as this is available, because, given $a \in \alpha$, there is, in general, no means of knowing whether a

⁴ With Schütte and Martin-Löf I use $(\alpha)\beta$ for the (total) function type. The total function takes its arguments in α and its values in β . Note the difference in notation from Materna's $(\beta\alpha)$.

belongs to the sub-domain of α where f is defined. The superiority of the total-function type lies just in that this issue is side-stepped. For Materna's (and Tichý's) partial types, on the other hand, one can know that $f \in (\beta\alpha)$ —Materna's notation!—and that $a \in \alpha$, and still do not know whether f can be applied to a in order to get a value in β . Also, in general, the relation of type-membership will not be a decidable one. It is unclear how to supply further type-information as to the domain of definition so that one knows when f is applicable to a given argument and when it is not.

We should also note that Materna uses a *set-theoretic* conception of type: in his explanation of the type of partial functions—cf. section 2 above—the types are treated as *sets*. However, if we are prepared to use the classic set-theoretic machinery, why bother about types? Surely the sets are enough. If, on the other hand, we prefer to use a type theory, within that framework, the notion of set should be dependent on that of type, and not the other way round. Materna wants it both ways, and that, in my view is demanding, too much.

The prosecution rests.

It now only remains for me to offer my warmest congratulations to my friend Pavel and to await gingerly the broadside that he will fire, let no doubt remain, in response to my impertinent birthday-questions.

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