CONCEPTUAL DEPENDENCE OF VERISIMILITUDE VINDICATED.
A FAREWELL TO MILLER'S ARGUMENT

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There were published just two replies to my recent paper (Raclavský 2007; CDV for short), namely that of Miller (Miller 2008; NMH) and Taliga (Taliga 2008; WVS). The two critical papers contain many points I consider faulty or being simply a gross misunderstanding of CDV. This admissibly long (thus rather short) version of my reaction concentrates only on the most important matters.

First of all, I find it wrong that somebody who criticizes me does not adhere to the key semantical distinctions used by me. Miller's argument revolves around semantical matters such as language, meaning, translation, equivalence, and intensions, thus it is bad enough to hopelessly confuse them as Taliga and Miller do. Especially, one cannot accept their notorious identification of languages with conceptual systems, thus words with concepts (moreover, concepts were partly identified with meanings in CDV); logicians and most of philosophers always do distinguish these two things. There are other Miller's strange views on semantics, which have been unacceptable for many decades (for instance, to dismiss – in sequel to his nominalistic construing of proposition as a class of sentences – intensional logic at all, NMH 187 – 188). I am not going, however, to disprove any such untenable views of theirs.

In the next section, I reject Taliga's rebuttals of my conception for its putative overall inadequacy. Then I proceed to criticism of Taliga's and Miller's recalcitrant construal of translation. The section 'The fallacy behind Miller's argument' repeats 30 years old disclosure of the fallacious confusion leading to the erroneous acceptance of Miller's argument. The penultimate section examines Miller's misguided objections to the approach developed by Tichý and followed by Oddie and me. In the rest of this introductory section, I am going to recall key theses of CDV and expose our test example.

A theory is construed as a conjuction of sentences which expresses a certain compound concept-construction which determines a propo-

sition (a function from possible worlds to truth-values).[1] The truth of a sentence, or a verisimilitude of a theory, is derived from the truth, or the verisimilitude, of the concept-construction it expresses. Consequently, we may focus only on the verisimilitude of constructions which is language independent. Measuring of verisimilitude, which is relative to (dependent on) a conceptual system and 'the truth', is accomplished on constructions expressed by theories. For the sake of simplicity, assume that a conceptual system CS_i is a class of basic and derived (CDV: derivative) concepts. The truth, or more properly (a part of) the possible world we consider as actual, is stated within some conceptual system, say CS_i, deploying only basic concepts of CS_i. When any of the compared theories does not express a construction combined only from basic concepts of CS_i, it is to be exchanged for an equivalent construction which is composed exclusively from its basic concepts.[2, 3]

Let the basic concepts of the conceptual system CS_T be ^{0}Hot (briefly: **H**), ^{0}Rainy (**R**), ^{0}Windy (**W**) – and, of course, **&**, ~ (its bold face is not visible enough), etc., which I will not mention any more. Now let the truth T_{CST}, a partial segment of the world we consider as actual, be (the construction) that it is hot, rainy and windy. Within CS_T, the distance of the theory T_1, expressing $\lambda w [\sim\mathbf{H}_w \& \mathbf{R}_w \& \mathbf{W}_w]$, from T_{CST} is 1/3 (one wrong guess from all three guesses stated by T_1). It implies quite a good degree of verisimilitude (in CS_T). Within CS_T, the distance of the theory T_2, expressing $\lambda w [\sim\mathbf{H}_w \& \sim\mathbf{R}_w \& \sim\mathbf{W}_w]$, from T_{CST} is 3/3, which implies an entirely bad degree of verisimilitude (in CS_T). Relatively to T_{CST} and CS_T, the verisimilitude of the content of (construction) T_1 is higher than that of T_2.

Now consider that we wish to ascertain the propinquity to the truth relatively to a different conceptual system (note that the procedure for obtaining degrees of verisimilitude is the same as above). Take CS_M hav-

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[1]   An expression is said to express (mean) a meaning which is modelled as an abstract structured procedure, Tichý's construction (constructions are displayed by some sort of λ-terms); the construction determines (constructs) the denotatum of that expression, i.e. an intension (such as proposition) or a non-intension. What an expression expresses or denotes is relative to, dependent on language (if not indicated otherwise, the language in question is English). A language is construed as a function from expressions to meanings-constructions.

[2]   Two constructions are equivalent iff they construct the same object (in the case of constructions of propositions the same proposition). Two expressions are equivalent iff they express (in a particular language) equivalent constructions.

[3]   A convenient introduction to Tichý's, Oddie's as well as my approach is Oddie (1987).

ing **H**, ^{0}Minnesotan (**M**) and ^{0}Arizonan (**A**) as its basic concepts. Let the truth T_{CSM} be that it is hot, Minnesotan and Arizonan. Suppose that T'_1 and T'_2 express $\lambda w\,[\sim\!\mathbf{H}_w\&\sim\!\mathbf{M}_w\&\sim\!\mathbf{A}_w]$ and $\lambda w\,[\sim\!\mathbf{H}_w\&\mathbf{M}_w\&\mathbf{A}_w]$ respectively. We easily conclude that, relatively to T_{CSM} and CS_M, the verisimilitude of the content of T'_1 is lower than that of T'_2. Nevertheless, Miller's argument utilizes other assumptions. Firstly, each of CS_T and CS_M has no (relevant) derived concepts. But there is a conceptual system, say CS_{TM}, which contains all concepts from CS_T and CS_M. CS_{TM} utilizes the two following 'equivalence rules', 'objectual definitions': $\mathbf{M}_w \equiv_{df} [\mathbf{H}_w \leftrightarrow \mathbf{R}_w]$ and $\mathbf{A}_w \equiv_{df} [\mathbf{H}_w \leftrightarrow \mathbf{W}_w]$. It implies that **H**, **R** and **W** are basic concepts of CS_{TM} but **M** and **A** are derived concepts of CS_{TM}.[4] Now it follows that, within CS_{TM}, T'_1 and T'_2 are certain counterparts of T_1 and T_2. The discussion of relevant consequences is postponed to the section devoted to Miller's argument.

Vindicating overall adequacy of my conception

Taliga's most fundamental objection is utterly unconvincing because it is wholly unfounded. He repeatedly claimed that my approach directly implies a contradiction, namely $[Vs(T_1) < Vs(T_2)]\ \&\ [Vs(T_1) > Vs(T_2)]$ (WVS, 198; adapting his notation to some extent). But his verisimilitude operator Vs apparently stands for a unary function assigning numbers (degrees of verisimilitude) to certain constructions (loosely represented by him as T_1 and T_2). Contrary to this, the verisimilitude I defended in CDV is evidently a ternary function whose arguments are triples ⟨construction, truth, conceptual system⟩ (this function delivers degrees of verisimilitude). Thus when one really follows CDV, he should write something like $[Vs(T_1,T_{CST},CS_T) < Vs(T_2,T_{CST},CS_T)]\ \&\ [Vs(T'_1,T_{CSM},CS_M) > Vs(T'_2,T_{CSM},CS_M)]$. The illusion of contradiction immediately evaporates. To repeat: relatively to T_{CST} and CS_T, the verisimilitude of T_1 is higher than the verisimilitude of T_2 but, relatively to T_{CSM} and CS_M, the verisimilitude of T'_1 is lower than the verisimilitude of T'_2. This claim is not contradictory indeed.[5]

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[4] There is another noteworthy conceptual system, namely CS_{MT}, which we will mostly ignore. Its basic concepts are **H**, **M** and **A**, but its derived concepts are **R** and **W** (in sequel to $\mathbf{R}_w \equiv_{df} [\mathbf{H}_w \leftrightarrow \mathbf{M}_w]$ and $\mathbf{W}_w \equiv_{df} [\mathbf{H}_w \leftrightarrow \mathbf{A}_w]$).

[5] Taliga complained (e.g., WVS, 194) that my introductory example about measuring distances in different systems of measurement (CDV, 340) does not display all features

Taliga repeatedly exposed his second main caveat, namely that my approach is unable to objectively determine degrees of verisimilitude at all (e.g., WVS, 198 – 199, 200), since the dependence of verisimilitude on conceptual systems is not objective (e.g., WVS, 200). My response is straightforward and simple: a ternary function having conceptual systems (i.e. classes of constructions) as one of its parameters is clearly objective in the same way as any other *n*-ary function having some kind of abstract entities as one of its parameters. Realize that this 'logical view' is not any kind of 'bad' Relativism (thus reacting here to Taliga's accusation that my approach is committed to some kind of such Relativism, *cf.* WVS, 200 – 201).

Taliga also maintained that my relativization of verisimilitude to conceptual systems is advisable only when supplemented by the 'real objective factor': the way the world is (e.g., WVS, 202 – 203), which my approach lacks (e.g., WVS, 202). Both his claims are completely wrong. Due to CDV, the way the world is nothing but a possible world – a conceptual entity (a certain construction); thus I do not lack it and I do not need to add it to my conception.

There is perhaps something else in the background of this objection of Taliga. Taliga seems to claim that Tarski's T-convention combines a definiendum relative to conceptual systems and a definiens which somehow contains or captures the 'objective truth', the real way the world is. But due to CDV, there is no conceptually unmediated access to the 'real way the world is'. I refer here to the well known and widely accepted view that we cannot escape all our conceptual systems and then look, independently of all these systems, at the real, distortionless way the world is. (For more see Raclavský 2007a.)

(A note on Miller's and Taliga's misuse of Tarski's T-convention is perhaps needed. Everybody who knows what Tarski really did knows that Tarski's theory of truth is worthless for the explication of the concept of truth routinely used in natural languages (in which theories are often formulated). Tarski's definition was designed only for some formal languages; Tarski explicitly rejected to provide a definition applicable al-

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of Miller's argument. He overlooked, however, that this was not its real purport: it was intended to show that the units of measurements have to be exposed when comparing some numerical degrees; otherwise, what we obtain is nonsense. However, Taliga did not understand the moral of the story at all because the contradiction he exposed is based on a direct violation of that natural principle.

so to natural languages. The natural, primary concept of truth applies to propositions, not to sentences (as Tarski did). Tarski's approach left us in the dark about how meanings of all words from which the sentence is composed participate on the sentence's being true.[6] Amazingly, Miller proposed an analogue of Tarski's definition (NMH, 179, 188). Its definiens contains a sentence the truth of which should be achieved in a Tarskian manner and its definiendum contains a truth-predicate for constructions combined with a certain (displayed) construction. Without going into details, such suggestion is an obvious non-starter for it sus-tains most disadvantages of Tarskian approach and adds also some new. The key reason: on natural construal, a sentence is true in a particular language iff it is true what the sentence means in that language; however, Miller swapped this, thus it becomes a mystery why a sentence is true when not for what it means.[7])

What a translation is not

In the introduction of this paper, I have referred to Miller's and Taliga's exceptional (and unacceptable) understanding of basic semantical matters. But their opinions on the nature of translation are equally dubious. In the first half of this section I am going to expose direct consequences of their construal showing how much they deviate from the standard views on translation.

It is a platitude that a translator is a person enabling to communicate one and the same thing-meaning by means of different expressions of different languages, say English and Czech. The following definition (used by me in CDV) results from this. An expression E_2 of a language L_2 is a translation of E_1 of a language L_1 iff E_2 expresses in L_2 the same meaning as E_1 does in L_1. When asking for translation of some expression, we usually mention languages. (Other consequences will be men-

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[6] For better understanding of differences between so-called models (and thus the usual model-theoretic work with formal truth-predicate) and interpretations (or possible worlds in the approach Tichý, Oddie and me) consult Oddie (1986, esp. 66 – 69). Model theory is fruitful for the study of various features of formal languages, but it is not useful for an explication of natural languages which are already interpreted.

[7] All aforementioned objections are avoided by my approach (usable for both natural and formal languages) whose basic (originally Tichý's) idea was exposed in CDV and was rigorously developed in Raclavský (2008). (Miller's opinion on my approach is surprisingly negative, NMC, 188.)

tioned below.) When doing proofs, etc., logicians and mathematicians, do not translate − they perform equivalent transformations, conversions. From an objectual viewpoint, a construction C_2 is a conversion of a construction C_1 iff C_1 and C_2 are logically equivalent. When asking for an equivalent conversion, we usually mention not languages but a rule according to which the equivalence is guaranteed. Derivatively, an expression E_2 (of L_1) is an equivalent conversion of E_1 (of L_1) iff the meaning of E_2 (in L_1) is logically equivalent to the meaning of E_1 (in L_1).

Miller commented on such natural views as a quite strange doctrine, perhaps of a suspicious origin (NMH, 186, 184). Taliga is much more expressive in his criticism (WVS, 199); he speaks about a dogma, etc. However, their own views are entirely bewildering. First of all, they notoriously talk about translations of constructions which are meanings of theories. On the natural construal, however, when users of two languages already manage meaning(s) they wish to communicate, there is no need to make translation(s); translation concerns expressions (and preserving the same meaning). Further: Taliga and Miller evidently identified translation with equivalent conversion. Thus it seems to them impossible to make one thing without the other (*cf.* WVS, 199). Look at the examples of translations they give us, for instance, 'It is hot if and only if it is rainy' – 'It is Minnesotan'. It is clear that such couples are not mutual translations at all: a. sameness of meaning is apparently not preserved (for $\lambda w\,[\mathbf{H}_w{\leftrightarrow}\mathbf{R}_w]$ is not identical with $\mathbf{M}$), b. (usual) uniqueness of translation is not preserved (for 'It is Minnesotan and it is Minnesotan' is equivalent to those expressions too), c. no language is evoked (both expressions are English expressions). The point c. might be possibly challenged because Taliga and Miller view English as smashed into the shatters L_T, L_M, etc. As we will see, the consequences of this idea are bizarre. Firstly realize, however, that the point a. still makes a valid objection: with the exception of 'hot', 'not' and 'and', L_T does not encode the same meanings as L_M, and *vice versa*, thus a real translation between L_T and L_M is excluded. The critical point b. can be perhaps avoided, if they adopted a restriction due to which only one 'equivalence rule' may be applied during the 'translation', not more; e.g., when $\mathbf{M}_w \equiv_{df} [\mathbf{H}_w{\leftrightarrow}\mathbf{R}_w]$ is applied to 'It is hot if and only if it is rainy', which leads to 'It is Minnesotan', the immediate application of the rule $[P_w\&P_w] \equiv P_w$ (P being a variable for constructions of propositions) is prohibited. On this occasion notice that such rules are not, *pace* Taliga (WVS, 199), rules for trans-

lation at all: because no language is mentioned in them (two different languages must be mentioned) and because no expressions are mentioned (two different expressions must be cited). Now look closely at their idea of language shatters. It implies that everybody managing at least one such 'equivalence rule' (say deriving 'P and P' whenever 'P'), that is (almost) everybody under the Sun, is a translator. This simply flies in the face of common sense. It also implies that logicians and mathematicians are the best translators nowadays because they manage the utmost number of 'equivalence rules'. Nevertheless, logicians and mathematicians would be surely angry if they would be told that they perform mere linguistic translations. It is really odd to view each step of their proof (based on equivalent transformations) as a skip from one language to another. Hence Taliga's and Miller's views seem to me only points showing what a translation in fact is not.

When Miller tried to argue against Tichý's denial of intertranslatability of sentences expressing different propositions (Tichý 1976, 35 – 36; 1978, 193), he exposed another hardly acceptable view. Miller's rebuttal stems from his striking misunderstanding of how possible worlds are specified in Tichý's system. Miller maintained that (improving his claim slightly) a certain class of possible worlds (forming one logical space) is specifiable by any intensional basis (class of pre-theoretic concepts-attributes) you wish (*cf.* NMH, 186). In Tichý's system, however, a particular class of (pre-theoretic) possible worlds is specified by (generated from) a particular class of (realizable, complete) distributions of primary attributes of one particular intensional basis. It is thus simply excluded that a certain possible world can be specified by means of various distinct intensional bases (containing distinct primitive attributes). Tichý's (or my) possible worlds are really not 'alternative spaces' 'specifiable' or 'describable' by means of terms of various distinct intensional bases (or conceptual systems).

There is also another example showing Miller's unfettered, thus quite idle approach to fundamental principles of explication. He refused 'Tichý's insistence, fostered by Raclavský, that each proposition is tied by an umbilical cord to a unique intensional basis' (NMH, 188). In fact, Miller deceives the reader because more than one half of page 347 (in CDV) is nothing but a criticism of Tichý's claim suggesting that propositions are tied up with intensional bases. Using Tichý's own arguments (Tichý 1988, esp. 195 – 196) I said that a proposition is a function from

simple (unanalyzed), 'colourless' logical objects W_1, W_2, …, W_n. It does not mean, however, that there should be no link of any such proposition to any intensional base at all. For a system of functions does not explicate anything until it is established what (at least some of) these functions (or rather non-functions) explicate (CDV, 348, *cf.* also Tichý 1988, 194 – 200). A simple illustration: it should be decided whether a proposition P is an explication of our pre-theoretical notion 'it is rainy' or 'it is not rainy' or 'it is hot if and only if it is Minnesotan'. When the first option is chosen, then it is entirely not an arbitrary matter whether the proposition complementary to P is an explication of 'it is not rainy' or 'it is hot if and only if it is Minnesotan' – only the first one is admissible. When considering the hyperintensional explication, the crux is more visible. To explicate our pre-theoretical notion 'it is rainy' not by the concept **R** but by the concept λw [~$\mathbf{R}_w$] or λw [$\mathbf{H}_w \leftrightarrow \mathbf{M}_w$] is markedly materially inadequate. Nevertheless, Miller is committed just to this, when he advised to 'smuggle' constructions of propositions to various distinct conceptual frameworks: 'a proposition can be associated with many distinct bases…, into each of which the constructions that construct it can be translated' (NMH, 189).

Summing up the content of the two preceding paragraphs, Miller showed in his misconstrued criticisms of Tichý that he did not understand these fundamental features of Tichý's approach. His criticism is thus surely wholly unconvincing.

The fallacy behind Miller's argument

First recall the example given in the introductory section. Due to $\mathbf{M}_w \equiv_{df}$ [$\mathbf{H}_w \leftrightarrow \mathbf{R}_w$] and $\mathbf{A}_w \equiv_{df}$ [$\mathbf{H}_w \leftrightarrow \mathbf{W}_w$], the theory T_1 is equivalent to ('intertranslatable with') T'_1, the theory T_1 is equivalent to ('intertranslatable with') T'_2, and moreover, the truth T_{CST} is 'the same' as T_{CSM} (quoted terms belong to Miller and Taliga). It follows that the distance of T_1 from the 'one' truth T_{CST}-T_{CSM} reverses when we move T_1 to the second framework ('translating' it as T'_1); analogously for T_2. Thus according to Tichý's approach, T_1 is closer to the truth than T_2, but it is less close to the 'same' truth than the second theory is, when we exchange the conceptual system CS_T by CS_M. Hence Tichý's approach is materially inadequate. That is Miller's argument and its result.

Already in Tichý (1978, 192 – 194), Tichý detected a dangerous hidden equivocation on which the acceptance of Miller's argument is based,

i.e. what 'lends Miller's argument its air of plausibility and propensity to confuse' (*ibid.*, 192). Oddie followed Tichý and he discussed the hidden equivocation in Oddie (1986, esp. 137 – 141); I discussed the equivocation too (CDV, 343 – 344). Neither Miller, nor Taliga ever discussed this fallacious equivocation; in fact, they still continue to think in a direct correspondence to it.

My current presentation of the fallacy behind Miller's argument reveals the confusion of A-reading and B-reading of Miller's argument generated by the following incompatible premises which I call A-premise and B-premise:

> *A-premise*. The conceptual systems CS_T and CS_M are assumed as not 'mutually dependent' – each of them having its own basic concepts (sharing only **H**) and having no (relevant) derived concepts; i.e. **H**, **R**, **W**, **M** and **A** are mutually independent.

> *B-premise*. The conceptual systems CS_T and CS_M are assumed as 'mutually dependent' – interrelated by 'equivalence rules', 'objectual definitions' $M_w \equiv_{df} [H_w \leftrightarrow R_w]$ and $A_w \equiv_{df} [H_w \leftrightarrow W_w]$, thus rather both CS_T and CS_M are in fact assumed as parts of some other system, namely CS_{TM}, whereas CS_T forms the class of its basic concepts and the class of its derived concepts is adopted from CS_M; i.e. **M** and **A** are dependent on **H**, **R**, **W**.

Presupposing *A-premise* is inevitable in order that *Miller's verisimilitude appraisals hold* (i.e. that they are distinct degrees of verisimilitude for T_1 and T'_1, analogously for T_2 and T'_2). On the other hand, presupposing *B-premise* is inevitable in order that *Miller's 'intertranslatability' statements hold* (i.e. that T_1 is 'intertranslatable' with T'_1, analogously for T_2 and T'_2). We will check that 'on none of these construals [A-reading and B-reading; J.R.] do all of Miller's claims go through' (Oddie 1986, 138).

When accepting B-premise, evolving thus B-reading, we easily find that *Miller's verisimilitude appraisals* (developed due to A-premise) *do not hold*. Because a theory content compound of derived concepts has to be converted – in accordance to 'equivalence rules' – in order to contain only basic concepts of CS_{TM} (e.g., $\lambda w \, [\sim H_w \& \sim M_w \& \sim A_w]$ is converted to $\lambda w \, [\sim H_w \& R_w \& W_w]$). Realize that in such case *no reverses of degrees of verisimilitude appear* (check yourself or re-read CDV, 344, 349). Hence on its B-reading, Miller's argument cannot support Miller's claim that Tichý's method of verisimilitude counting is materially inadequate.

When accepting A-premise, evolving thus A-reading, we easily find that *Miller's 'intertranslatability' statements* (developed due to B-premise) *do not hold* because CS_T and CS_M are not assumed as interrelated by 'equivalence rules' $\mathbf{M}_w \equiv_{df} [\mathbf{H}_w \leftrightarrow \mathbf{R}_w]$ and $\mathbf{A}_w \equiv_{df} [\mathbf{H}_w \leftrightarrow \mathbf{W}_w]$. When we do not have these 'equivalence rules' at our disposal, we have no way how to check whether $\lambda w [\sim\mathbf{H}_w \& \mathbf{R}_w \& \mathbf{W}_w]$ (from CS_T) and $\lambda w [\sim\mathbf{H}_w \& \sim\mathbf{M}_w \& \sim\mathbf{A}_w]$ (from CS_M) are equivalent; thus 'translating' is blocked. Consequently, *no reverses of degrees of verisimilitude* are ascertainable again. Hence also on its A-reading, Miller's argument cannot support Miller's claim that Tichý's method of verisimilitude counting is materially inadequate. It should be added here that Tichý's, Oddie's and my further reasons for the denial of the intertranslatability for the case of A-reading are only a supplementary semantical matter. For the 'intertranslatability' presupposed by Miller needs only these 'objectual definitions'. It is Miller's main error that he did not realize the impossibility to admit them for the case of A-reading.

Oddie examined also some other readings showing that none of Miller's claims go through either. Oddie generalized Miller's argument and found that Miller's own theories about accuracy or confirmation of scientific theories can be discredited by his own way of reasoning. Oddie concluded that 'What these generalizations of Miller's argument reinforce is the conviction that there must be something wrong with it. An argument which purports to show that the notions of accuracy, truth-likeness, structure, change, sameness of state, confirmation and discon-firmation, are all spurious ... must harbour a defect somewhere' (Oddie 1986, 158). And it is pretty clear *what is wrong with it: deriving some conclusion from the two patently incompatible premises*, namely A-premise and B-premise.

Re-examination of Miller's objections to our approach

Miller is quite dissatisfied (NMH, 181 – 182, 183) with the approach developed by Tichý (upgraded to the conceptual, hyperintensional level by me) because it is unable to preserve the order of theories such as T_1 and T_2 when we move ('translate') them from one conceptual system (e.g., CS_T) to another (e.g., CS_M).[8] When posing such objection, however, one is apparently a victim of the fallacious conflation of A-reading and B-

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[8] Such ordering is yielded, for example, by their likeness to the truth.

reading. Firstly, one finds the ordering of T_1 and T_2 within CS_T due to A-reading; following our above example, T_1 is closer to the assumed truth than T_2. Then he applies B-reading and assumes that T_1 and T_2 are equivalent to the theories T'_1 and T'_2. Further, he shifts back to A-reading and gauges verisimilitude of T'_1 and T'_2 within CS_M; he finds that T'_1 is less closer to the truth (stated within CS_M) than T'_2. Before drawing the conclusion, he invokes B-reading according to which T_1 and T_2 are equivalent to ('same as') T'_1 and T'_2 respectively. Then he complains that (according to the appraisals based on A-reading) the ordering of T_1 and T_2 is not preserved when we move ('translate') these theories from CS_T to CS_M (which is possible only on B-reading). Nevertheless, the reader knows very well the culprit of this fault: it was indicated already by Tichý, Oddie and me – *cf.* the preceding section.

As an exercise, let one of Miller's examples of pairs of equivalent systems (NMH, 184 – 185), namely $\{\sim, \rightarrow\}$ and $\{\downarrow\}$ (Miller 2006, 217), be examined. (Miller omitted to properly comment on these examples, thus one is unsure what he intended to demonstrate.) Recall CS_T and CS_M; the basic concepts of the former system, BC_T, forms the class $\{\mathbf{H,R,W}\}$ and the basic concepts of the latter system, BC_M, forms $\{\mathbf{H,M,A}\}$; both CS_T and CS_M have the empty class of derived concepts, $DC_T = DC_T = \varnothing$. On the other hand, CS_{TM} is such that $BC_{TM} = \{\mathbf{H,R,W}\}$ and $DC_{TM} = \{\mathbf{M,A}\}$, but CS_{MT} is such that $BC_{MT} = \{\mathbf{H,M,A}\}$ and $DC_{MT} = \{\mathbf{R,W}\}$. Let the A-kind approach be applied to Miller's example: $BC_{\sim \rightarrow} = \{\sim, \rightarrow\}$, $DC_{\sim \rightarrow} = \varnothing$ and $BC_{\downarrow} = \{\downarrow\}$, $DC_{\downarrow} = \varnothing$; i.e. the situation is quite analogous to that with CS_T and CS_M. When applying the B-kind approach to the example, we get $BC_{\sim \rightarrow \downarrow} = \{\sim, \rightarrow\}$, $DC_{\sim \rightarrow \downarrow} = \{\downarrow\}$[9] and $BC_{\downarrow \sim \rightarrow} = \{\downarrow\}$, $DC_{\downarrow \sim \rightarrow} = \{\sim, \rightarrow\}$; i.e. the situation is quite analogous to that with CS_{TM} and CS_{MT}. In order to make an analogy to the verisimilitude problem, consider the property "being composed from the basic concepts $\sim$ and $\rightarrow$". Due to A-reading, $[\sim p \rightarrow q]$ is composed from the basic concepts $\sim$ and $\rightarrow$. Due to B-reading, $[\sim p \rightarrow q]$ is equivalent ('translated') to $[p \downarrow q]$. Applying then A-reading, the 'translated' $[\sim p \rightarrow q]$ is not composed from the basic concepts of $\sim$ and $\rightarrow$. One concludes that the extension of the property "being composed from the basic concepts $\sim$ and $\rightarrow$" unexpectedly changes, when we move $[\sim p \rightarrow q]$

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[9] Due to $[p \downarrow q] \equiv_{df} [\sim p \rightarrow q]$. For $DC_{\downarrow \sim \rightarrow}$ – due to $\sim p \equiv_{df} [p \downarrow p]$ and $[p \rightarrow q] \equiv_{df} [\ [[p \downarrow q] \downarrow q] \downarrow [[p \downarrow q] \downarrow q]\ ]$. (The variables for the truth-values, p and q, are basic concepts of all systems $CS_{\sim \rightarrow}$, $CS_{\downarrow}$, $CS_{\sim \rightarrow \downarrow}$, and $CS_{\downarrow \sim \rightarrow}$.)

from one conceptual system to another. Now we will be told that a theory which has such feature is wholly unacceptable. Nevertheless, we already know which fallacy causes such undesirable breakdown: it is the messy shift between the A-style and the B-style way of reasoning.

Miller objected to the approach developed by Tichý, Oddie, me and others that it is quite worthless, since it does not explain what to do when scientists reformulate their theories in other variants, making thus use of a (different) vocabulary with new primitive words (NMH, 184, 185, 186). However, this Miller's criticism is entirely misdirected, since Tichý, Oddie and I already offered an approach directly implying what happens when a theoretician admits a variant formulation of his theory. The rest of this section will show it clearly again.

First of all, a theoretician may translate T_1 from English to Czech (relatively to these languages, both T_1 and its translation express $\lambda w\ [\sim\mathbf{H}_w\&\mathbf{R}_w\&\mathbf{W}_w]$). Secondly, one can introduce to English – underlied by, for instance, CS_{TM} – a new word 'Z_1' which is synonymous with some original expression of English, say 'hot' (thus both 'hot' and 'Z_1' express $\mathbf{H}$). Analogously for an introduction of a new primitive word, say 'Z_2', synonymous with the compound expression 'It is not hot, but it is rainy and windy'. Of course, such cases of reformulations in a variant vocabulary ('hot', 'Z_i', 'rainy', 'windy', 'not', 'and', 'if and only if') are trivial (thus it was left to the reader of CDV to conclude himself how to manage them).

In a science we may face admitting of a new primitive word which is not synonymous (i.e. having the same meaning) but only equivalent to some old expression. Let the concept $\mathbf{F}$, expressed by the word 'Floridian', be introduced in CS_{TM} by means of the rule $\mathbf{F}_w \equiv_{df} [\mathbf{R}_w \leftrightarrow \mathbf{W}_w]$. We expand the system CS_{TM} in a 'conservative way'. Since this new system CS_F contains $\mathbf{F}$ as a derived concept, the class of its basic concepts does not differ from that of CS_{TM}. When counting verisimilitude of a theory such as 'It is Floridian, but not hot' (hereafter F_1), expressing $\lambda w\ [\mathbf{F}_w\&\sim\mathbf{H}_w]$, we have to convert this construction onto a construction composed only from basic concepts of CS_F, i.e. $\mathbf{H}$, $\mathbf{R}$, $\mathbf{W}$. A verisimilitude of F_1 will be then the same as that of some theory over the conceptual system CS_{TM}. Thus such 'conservative expansion' evidently does not influence 'conservativeness' of verisimilitude appraisals (*cf.* Tichý 1978, 194, CDV, 344; note that this kind of expansion of CS_{TM} to CS_F is exactly parallel to the expansion of CS_T to CS_{TM}). Hence, Tichý's approach behaves in full accordance with our expectation.

On the other hand, enriching the class of our basic concepts must influence verisimilitude appraisals (in comparison with those for CS_T) notably. Again, Tichý's approach behaves appropriately to this 'non-conservative expansion'. Suppose that the weather on the Earth will change so that there will be states of weather not definable by means of old terms, i.e. by some equivalences to hot, rainy or windy. Our descendants will adapt on the situation and make use of the new basic concept **U** and add it to {**H**,**R**,**W**}. From its very origin, Tichý's approach was designed for an arbitrary amount of basic concepts. Thus it is easy to deduce how to count verisimilitude of a theory such as 'It is not U, but it is hot and windy' (expressing $\lambda w\ [{\sim}\mathbf{U}_w\&\mathbf{H}_w\&\mathbf{W}_w]$) within CS_U having {**H**,**R**,**W**,**U**} as the class of its basic concepts. When the truth is, for instance, $\lambda w\ [\mathbf{U}_w\&\mathbf{H}_w\&{\sim}\mathbf{R}_w\&{\sim}\mathbf{W}_w]$, the distance of that theory from it is 2/3.

Conclusion

Miller commented on Tichý's, Oddie's and my approach to verisimilitude and our critical examinations of his argument as follows: 'the failing of Tichý's account of verisimilitude is ... that it continues to ignore straightforward criticism' (NMH, 186). In order to reply to this hard condemnation, I confine myself to mere enumeration of brute facts. There is a good refutation of Miller's argument in Tichý (1976, 35 – 36). In Tichý (1978, 191 – 196), Tichý continued in defence of his approach and made the refutation of Miller's argument much better by means of the identification of a hidden fallacious equivocation. In Oddie's book, there are exactly 31 pages (Oddie 1986, 136 – 166) of a detail discussion including an unambiguous rejection of Miller's argument which contains also Tichý's 1978-criticism. Recall also my CDV. It was recommended to me to read Miller's recent discussion of his argument (Miller 2006, 197 – 233). All these 37 pages contain only the quotation of Tichý's 1976-denial of intertranslatability (*ibid*. 216). Immediately under the quotation, we read Miller's laconic 'Similar claims are made by Oddie'. Instead of any attempt to discuss Tichý's 1976-denial, Miller listed (*ibid*. 217) various mathematical and logical systems such as {$\sim,\rightarrow$} and {$\downarrow$} but without any relevant comment. That's all. More generally, Miller as well as Taliga never disputed (or at least mentioned) Tichý's 1978-criticism which was repeated also by Oddie and me. Hence the direct counterarguments against Miller's argument have left Miller and his ally Taliga undisturbed already

for the whole 30 years. The reader is invited to decide himself/herself who in fact ignores straightforward criticism.[10]

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[11]   Tichý's (sometimes co-authored) papers are reprinted in Tichý (2004).