Orduality versus *Flausible* Falsifiability & Inference Criteria Explicated: None (All) is a Proof?

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ABSTRACT[1]

Orduality may bypass Goedels tradeoff while *subsuming* tautology (the latter's extension being but one aspect of the former) rather than merely 'contradicting' it. When reduced to representation-invariance, tautology may not apply to select *sub*sets or lower-level sub-objects (e.g. particular equations of a proof) the way it does to the object in its entirety as that might compromise the inner (deductive) and outer (inductive) relational consistency rather than enforcing or testing it. Mochizuki's IUT serves a benchmarking purpose throughout while likewise allowing for generalization along orduale lines.

Keywords: Orduality, tautology

Haste in Having Issues Raised

We've had the good fortune of seeing one of the recent critiques raise an issue on rethinking just what it is that amounts to things wrong, obvious, and long said. *Reduction to tautology*[2] has been posited as a surefire remedy (perhaps right up the *automated proving* hype alley as if to overlook [co]NP completeness and decidability issues), an acid test, and possibly a decision-procedure. Or is it? Does it really work, can it apply in every setup, or would it be nice to have—and have it working as well as workable for that matter?

I immediately responded on preliminary grounds by arguing that tautology (as the possibly coveted end outcome of *deductive validity* as per analytic truths) may not apply to *inductive cogency* (filling contingency)—which is what pervades my each and every demonstration (or have I claimed any proof thus far?) To showcase the meta-dilemma, I further pointed to a flip- or downside of deploying the *RAD* tool as part of approaching the *ABC*

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[1] WP201965-7 To Vyshinsky and all those Machiavelli'd or Orwelled "for freedom's sake," or for speaking their mind and convictions in good faith. To my late Grandma whose name connotes Hope.

[2] Whilst it might elude one's ken as to what it is exactly a '*denial*' of a proof (or a disproof of a *demonstration*?) amounts to, still this kind of methodology coinage might be that critique's central contribution subject to copyright. That said, we shall extend our coverage beyond the mysterious criteria or just as tenuous a solution allegedly acting as hurdles.

conjecture. Incidentally, that stumbling block amounted to a *recurring pattern* which shined through in many of my more recent expositions. On top of that, I hinted at one other ubiquitous pattern or structural regularity which will have shown to pervade the bulk of otherwise distant axiomatizations and applications alike. I now turn to treating these issues in greater detail while seeking to substantiate how they could be of importance in shedding light on the broader agenda.

None is a Proof? Extending One's Tautologies

As has been argued from the outset, most *generalizing* frameworks build on *in*duction yet to be scrutinized with respect to *cogency*, which may have little to do with *de*ductive *validity* per se and more with *mutual support*—be it *internal consistency* (within a particular axiomatization or demonstration) or recurring patterns and otherwise grand corollaries appearing similar (*across* distant frameworks or disciplines). For one thing, it may be fully desirable and 'reasonable' to expect that, insofar as tautology applies to inductive modules or layers, these either yield complete solutions (as with the problems that I have tended to reduce to functional or difference equations insofar as these allow for closed forms) or garner results that recur in other setups (as if to maintain the inductive sections as 'black boxes' whose deductive merit is neither testable directly nor relevant).

One may be led to presume that the targeted results (i.e. the very substance of conjectures or implications of hypotheses) constitute part and parcel of the expected or recurring results as tested by [*quasi, generalized*] tautology (spanning deductive *and* inductive domains alike). Needless to say, though, this could pose a paradox or moral hazard-like trap, with self-selection acting to sort out some side outcomes that may have resulted from the exact same line of reasoning or decision-procedure while seeming at odds with the core result that constitutes the conjectured scope. As one other extreme possibility, the proof (or an axiomatization allegedly implying one), may turn out to generate *a single* result, in which light the [self-serving] framework's merit could be questioned beyond its ad-hoc application no matter how far-reaching.

To some, this is what appears to have been the issue with Mochizuki-style proofs[3]. On the one hand, these have readily been embraced by some observers (cf. Fesenko) judging the interim results or select rationales and lines of reasoning as similar to theirs—which instance of recurring patterns or mutual support pertains to induction (or transduction as ML recognition) yet definitely not to deductive validity per se. Worse yet, insofar as the IUT paradigm posits about as long a shot as it is a single one (no matter how much there is at stake given the scope of inference that would result from ABC validity), this would question the effectiveness of such enterprises or

[3] It will be shown in a forthcoming preprint how ABC could be trivialized or indeed tautologized, in which light, somewhat paradoxically, most any proof would do—be it Chaldean or Beowulf style 'Hwaet!' succinct or the size of Mochizuki's. But that only really begs the issue of whether tautology makes a difference, and how does one go about undoing its costly sterility.

grand decision-procedures[4]. What is more, it would be most awkward imposing any prior match hurdles or similarity expectations on an altogether *novel* framework vis-à-vis familiar results, in which event a good chunk of 'inductive tautology' is waived as a hallmark.

Thus far, tautology may have seemed plausibly *sufficient* in some (largely deductive) setups yet far from *necessary* in most (heavily inductive first and foremost), such that its violation suggests nothing and its support at best amounts to reinforcing a failure to refute or 'falsify' the null hypothesis of invalidity (unsatisfiability). Invoke further Goedel's impossibility theorem, and the tautology criterion may appear downright irrelevant insofar as any framework (not necessarily tantamount to a line of reasoning drawing thereupon) will prove incomplete. Whilst this poses uncertainty or inconclusive judgment as to its [self-sustained] internal consistency (rather than necessitating contradiction as if to deny tautology ex ante)—the same going for any finite admixture of such frameworks so as to build on *extraneous* consistency criteria—this might render any tautology-based test prone to irrelevance or refutation bias. In contrast, attaining tautology *ad hoc* might likewise tell nothing on what this amounts to or whether the test could be replicated outside the setup.

In light of the above, tautology nets out to little if anything—and one need not decide this meta-scrutiny by merely tapping into alternate [past] paradigms, notably intuitionism (logic-validity insubstantive), in contrast to formalism (logic-validity empty) or logicism (logic-validity inadequate).

Denying Denialism & Unbegging the Begged: Beyond Double Negation Dialectics

We now revert to some of the early illustrative *counter*-criteria along the lines suggested from the outset. To begin with, my follow-up retort pointed to a thought experiment, not necessarily having to do with ABC immediately (as regards drawing upon coprimeness). Consider $a = X^m, b = X^n$, in which case it becomes largely a matter of convention whether or not $rad(ab)$ equals X or X^2. A generalized dilemma could feature a family of conventions over $\exists|X| \leq \infty: \varphi(X) = \varphi(X+1)$, with the factorial at X=1 and 0 being one case in point. Irrespective of whether deductive validation is a matter of efficiency (or *cost* parameter as a criterion attached to a decision-procedure while positing a *soft* form of *impossibility*), a matter of *convention* could be an extreme or corner case of induction largely orthogonal vis-à-vis tautology which may either obtain at a low to no cost while amounting to nothing or prove technically unfeasible without necessarily plaguing the attempted line of reasoning (or prior conceptualization) as invalid.

[4] This might be an instance of sheer *azimuthality*—the opposite of *gradiency*. Insofar as my humble judgment could be of interest, I would not discard IUT if only on the strength of the apparent overlaps it shares with some of my orduale constructs or approaches, my single gravest concern resting with whether its underlying Teichmueller legacy can possibly get it much further without 'going too far' with an eye on undue excesses verging on sheer absurdity or uncertainty (mounting past Mochizuki's 'three mild indeterminacies').

In passing, note that many of the aforementioned issues may in fact border on the tradeoff over Type I versus Type II error (*alpha* sigificance versus *beta* power residuals) pertaining to the odds of rejecting a true candidate as opposed to accepting a false one. As one interim or *Type III* setup, consider the possibility I discussed in the selfsame rush followup response to the critique, whereby some otherwise-valid results that may have ensured tautology *in-box* may not carry over satisfactorily in other conceptual setups. This seems to apply to the so-called 'trivial' roots of the Riemann zeta set at zero: After all, beyond seeing that $\Gamma^{-1}(-2m) = 0$—which may in fact question the accuracy of assessment of the recursive representation of zeta with an eye toward the fine multiplicative residuals (or mappings) of the gamma inverse—it would be awkward to buy anything like $\sum_{N=1}^{\infty} N^{2m} \equiv 0\ \forall m \in \boldsymbol{N}$ *outside* the RH specific setting. Arguably, the same would go for infinite series like $1 + 1 + 1 \ldots = -.5$ (instrumental as this may have seemed in outside areas sweeping infinities under the rug).

Incidentally, the dilemma of opting for and going with $rad(X^m X^n) = \begin{cases} X \\ X^2 \end{cases}$ as a matter of convention[5] may or may not technically be exogenized as an input—indeed a *prior* aspect of axiomatization whose tautology (truth value) is bypassed or swept under the rug as inductive discontinuity. In other words, tautology is not completely devoid of 'mental reservations,' which manipulability is what further undermines its meta-cogency as a cut-off. Or, at any rate, this would fare on par with [*indefinitely improvable*] criteria of rigor which have varied over time or across paradigms and might prove just that—a matter of school-specific 'political' coalescing or 'institutional' network support. Whilst deductive rigor might seem at odds with either intuitive 'clarity' or constructive completeness (me personally siding with the latter), if you dub anything so rigorous as to claim tautology, I would call for either more rigor *or* utter generality—which renders tautology a relativistic moving-average, far from a robust test which may work at small spans while failing miserably under any major extensions.

So, shall any flesh be spared? Can any proof be salvaged? As far as I am concerned, none of my [select] *demonstrations*—which have been aimed at recovering the candidate rationale behind the conjecture's claims (likely to prove as myopic at some levels as my treatment thereof at others)—have ever been intended as [ultimate] *proofs*. It's just way too foreign to and different

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[5] The committed onlooker will have observed a similar dilemma or pattern appearing in my recent works—*Part II* (0^2 versus 0 in the powers of 0-representations of N^{-s} terms) and *Part III* ($0^2 \leftarrow (\lambda s)^2 \sim \lambda s \to 0$ in the power of the exponential Mikusinski operator as in the respective footnote). As per the other recurring pattern yet to be unveiled in the forthcoming works, and possibly appearing as early as my ABC (being 'denied'), this amounts to $\Delta X = X - 1$. Induced to $\Delta^k X = X - k$ then restricted or 'narrowed' down to $X \equiv 1, k \equiv \Delta$, it yields a celebrated structure: $\Delta^\Delta = 1 - \Delta$ or $P^P = 1 - P$. This could further be narrowed to, say, $0^0 = 1 - 0 = 1$ for $\Delta \equiv 0$ (endogenizing the otherwise-arbitrary $0^0 = 1$ convention) or, on the contrary, unnarrowed back to, $\Delta f = f - f_0$ per $X \equiv f$ and f_0 relaxing 1 as will be proposed in some characteristic series. In a sense, this extends $X^2 = X$, for $\Delta X \sim X^{-1}$. The general pattern will reappear copiously throughout my forthcoming expositions. Come to think of it, the very problem of comparing 0-objects might be related to that of comparing infinities and complex numbers. Not only can complexity be shown to be reducible to zero-radius conic sections as in $(z - Re)^2 + Im^2 \equiv 0$, the line has been demonstrated to be fuzzy (cf. *Part I*) between it and infinite real solutions (applying to powers chiefly): $z = Re + iIm = Re - \frac{2\pi Im}{2i\pi} = Re - 2\infty\pi Im \equiv Re'$.

from what mine have targeted (when it comes to discerning and applying as dual of the judgment criterion—the two likely about as much disjoint yet intertwined as are the Type I versus II cutoffs).

All is a Proof! Tautologetics of the General

Wait, though: I've changed my mind! *All* of my demonstrations *are* [potentially] a proof whose validity is beyond [naïve] tautology. For starters, all of the various conceptualizations I have attempted would result in patterns that appear strikingly similar (call it mutual support or recurrent patterns if you will, or as contributing to inductive cogency). This should come as little surprise, bearing in mind they all have been inspired by my *Orduale* Program. I have for one hinted before I'm most interested in showing how *orduale* results could prove superior, whereas the success of select applications (tentatively qualifying as *pre*-ordual or sub-ordual and seen as ordual-cardinal *bridges* or *levels of narrowing*) might hover anywhere in between marginally desirable versus irrelevant.

Now, why did I attempt the ordual rethinking, in the first place? To be honest, it just has evolved that way, largely as a response to my aesthetic craves as well as the perceived shorcomings to whatever the aforementioned ones are but a minor subdomain of. The flip-side silvery lining, though, is that all of the competing alternates (testing criteria included) have been *subsumed or generalized* under the orduale paradigm.

To appreciate this within the proposed scope and context, consider the conventional, FOL tautology scenario (with the syllogistic and bijective schemes seconding each other):

$$\boldsymbol{X} \xrightarrow{p} \begin{cases} A \\ B \\ C \\ \ldots \end{cases} \xrightarrow{q} \begin{cases} A' \\ B' \\ C' \\ \ldots \end{cases} \xrightarrow{r} \ldots \begin{cases} \boldsymbol{X} \\ Y \\ Z \\ \ldots \end{cases}$$

It remains to be seen whether X (as one section) can [again] be arrived at in some stage with certainty (roughly the product of the stage likelihoods or composition of bijections) without either compromising the rest of the implications (or the induced as well as deduced objects' inner and relational entirety) arbitrarily or assigning disproportionately low truth values (product likelihood) thereto.

By contrast, the ordual setup would treat all these levels or transitions as relationships, marked by particular parameters, with the *select* nodes less well-defined outside the relational scheme and span (in line with orduale premises positing [higher-order] relationships as the ultimate objects). Better yet, the entire scheme is collapsed—along the orduale lines of *completeness yielding simplicity*—to [generalized] tautology as [*constructive*] *self-identity*:

$$(X,X) \equiv (X,X)$$

One way of approaching the constructive domain is to further inform (not necessarily restrict or specify) the symmetry, followed by allowing for a plethora of specific objects being constructed (*inter alia*, consider the wide variety of calculi as proposed early on, more to follow shortly):

$$(X,X) \equiv \begin{cases} \big((A,a),X\big) = \big((A,a),(A,a)\big) = (A,a)^{\rho} \\ \big(X,(a,A)\big) = \big((a,A),(a,A)\big) = (a,A)^{\frac{\rho}{\rho-1}} \end{cases}$$

Take this *relational domain* (rho as complete and unspecified simplicity) one 'level of variableness' down, with the implied 'level of narrowness' identical to the *operational space a la* CES/Lame:

$$V^{\rho} \equiv \sum_{k}^{M} X_k^{\rho}$$

One other level of narrowing, e.g. setting rho to 1 versus 0, would yield the equivalent of *addition* versus *multiplication*[6] (or, in CES terms, perfect substitution versus perfect neutrality):

$$V \xrightarrow{\rho\equiv 1} \sum_{k} X_k$$

$$\frac{V^{\rho}-1}{\rho} \equiv \frac{\sum_k (X_k^{\rho}-1)}{\rho} + \frac{M-1}{\rho}$$

$$V \xrightarrow{\rho\to 0^-} e^{\log \prod_k X_k} e^{R} = \prod_{k} X_k$$

This is how an operational space may boast plasticity beyond the linkage between Σ versus Π and certainly beyond ABC yet largely in line with Mochizuki's program[7] while still positing but a *special case* of orduality (indeed restricted or 'narrowed' down more than once,

[6] While at it, one may opt to consider an alternate log-as-limit convention, e.g. $logX = \lim_{\rho\to 0}\frac{X^{\rho}}{\rho}$ in place of $\frac{X^{\rho}-1}{\rho}$. Whereas the resultant gap may be on the order of infinity, the CES form (and any *ordual* representation for that matter) would be accurate and *convention-invariant*, in contrast to *cardinal* ones (e.g. standalone *values* of the above as opposed to their relationships). Whilst the more rigorous version yields a counterintuitive result because of the potentially infinite power residual (or zero factor), the latter may well be discarded if only because any finite phi residual, $\varphi \not\equiv M$ in the numerator of the right-hand side can be assumed [away] because of the special, infinitesimal denominator: $\frac{\sum_k (X_k^{\rho}-1)}{\rho} + \frac{\varphi-1}{\rho} = \frac{\sum_k [(X_k^{\rho}-1)+(\varphi-1)/M]}{\rho} \xrightarrow[\rho\to 0]{} \frac{\sum_k (X_k^{\rho}-1)}{\rho}$. It is straightforward to see how the two conventions are bridged for $\varphi \le M \le T \to \infty$: $\frac{\sum_k [(X_k^{\rho}-1)+(M-1)/M]}{\rho} \xrightarrow[M\to T]{} \frac{\sum_k (X_k^{\rho}-1+1-\frac{1}{T})}{\rho}$

[7] Since mine predates Mochizuki's at least a decade, actually dating back to the late 1990s, it could not possibly have been inspired thereby by the slimmest chance.

such that this cannot be undone or recovered in a trivial manner, much less be subject to an adequate tautology-based scrutiny)!

Moreover, even so much as a *pre*-orduale domain (loosely thought of as partial narrowness) has been earmarked by effective [generalized] tautology. For instance, *Part III* of my recent preprint has been aimed at bypassing the complex-values [non]orderability issue by embarking on averages. The presumption was, The same operation, when applied to any of the representations (interpretations) of a particular object or relationship (between which a tautology could be presumed based on prior equivalences or exogenously set relationships), will yield a [pairwise] identity between these. In particular, averaging of the same sort, as applied to what I have maintained as tautologous representations of the Euler-Riemann Equivalence (left-hand side), yields:

$$\widehat{N}^{-s} \overset{q\equiv 1}{\longleftrightarrow} \frac{log^{\widehat{N}}\zeta}{\widehat{N}!}$$

In other words, an average will identically map into itself as one-to-one, in contrast to *individual* [complex] series *terms* which cannot be compared reliably irrespective of how plausibly they seem to agree around the identical N-indices. In fact, this might be an instance of tautology confined to representation-invariance. That said, it doesn't seem like a very good idea applying an invariance-test to each and every *sub*set or *lower*-level object (e.g. standalone or interim equations of a proof) the way it may apply to a holistic object in its relational entirety. The moral hazard is that, instead of testing for consistency, the very criterion could compromise the linkages and transitions in the first place. Oh, anyone detect any manner [*ever higher*] generality lends itself to tautology?

A RehashTag

Needless to say, the above is but one aspect and application of the Orduale paradigm, whose emerging nature and existence could not possibly have been motivated by weak-form tautology alone (albeit fully met by orduale extension). None of my [standalone] demonstrations would amount to a perfect proof, if only because *only the complete will secure simplicity* capturing clarity and validity alike. Somewhat akin to how '*All* Scripture is God-breathed' (2 Tim 3:16-17)—probably referring to the scriptural complete and integrated whole at odds with manipulative citing—so too would an entirely developed and honed paradigm ensure inner consistency (deductive validity) in a way inseparable from outer or cross-discipline mutual support (inductive cogency).

But then it happens, *any* of my demonstrations—pre-ordual possibly less so than orduale—amount to complete proofs in line with one other aspect of orduality whereby *the special can be comparable to the whole* (their relation revealing simplicity if both are reasonably complete or orduale). In other words, insofar as the analysis tends to the orduale domain, its validity mounts even as prior (incomplete, special-case, or cardinal) validity hallmarks dissipate their relevance. After all, one may speculate at leisure that a *special* theory 'contradicts' a *general* one or vice versa (these hardly enabling equal tautology, much less symmetric co-refutability)—but the conceptual merit of such bare-bones phenomenological allegations remains to be seen [as nil].

The orduale domain, viewed as *constructive, non*-cardinal [self] *identity*, appears to capture *both* the notions of tautology—syllogistic as well as representational. Whilst conventional tautology (of either type) could be remedied or improved upon without having to be discarded downright, it is a *particular method* or '*usage*' that may plague it with sheer excesses. For example, if one were to insist on deploying tautology to sub-objects the way it may apply to complete objects, I would take one further step suggesting this hold for and carry over all the way down to operators. Now, since only a handful of these are known to boast invariance[8] (let alone convention-invariance), the entire enterprise (applying as meta-induction or automorphous self-reference *a la* liar paradox) is thus reduced *ad absurdum* in and of itself. Or would one spot much convention-invariance to the RAD operator of the ABC?

Simpliciter

A [true] proof does *not necessitate* its interim tautology (e.g. interpretation-invariance), nor does the latter *suffice* to ensure the former. A *single*-shot [proving] success may not validate the underlying *paradigm* nor deny alternates. *Interim* satisfiability valuation may apply to [complete] *axiomatizations* but not to *ad-hoc* proofs (*models*) drawing thereupon. Furthermore, *gray area* in between Goedel's *in*completeness (truth-amidst-undecidability) versus completeness (validity-inapplicable-to-induction) may in fact extend beyond that between Type I (alpha) versus II (beta). Orduality surpasses HOL.

[8] Suppose one symmetry requirement might be about the group action being preserved: $\varphi(g * f) = g * (\varphi(f))$. While this does hold for my rad' operator as in the ABC demonstration (being 'denied'?), need anything like that necessarily hold for *non*-linear or non-homogeneous (or homogeneous of degree other than 1, e.g. excluding CES forms) operators?