Ex falso and Proof-Theoretic Validity

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1 *Introduction*

This paper focuses on how the rules for absurdity are evaluated in the framework of proof-theoretic semantics. While this topic has been investigated thoroughly – both in classical and in recent works, which are mentioned in the following sections –, some key issues has been neglected. In particular, in dealing with absurdity researchers have focused almost exclusively on the satisfaction of a well-known prerequisite for proof-theoretic validity, usually called Inversion Principle, without taking in consideration the full account of validity. The main goal of this work is to fix this lack.

Apart from this introduction, the article is divided in two sections:

- In the first one, the Inversion Principle is presented and compared with a full notion of proof-theoretic validity. The need of an explicit definition for such a notion is defended, and the definition of validity of inferences (needed to evaluate *ex falso quodlibet*) is obtained from that of validity of derivations.

- In the second section, the role of *ex falso quodlibet* inside proof-theoretic semantics is evaluated. First of all, Inversion Principle is taken in consideration, and some proposed solutions to make this rule suits it are presented. Then these same solutions are discussed for proof-theoretic validity *tout court*. In the end, it is concluded that a full notion of proof-theoretic validity poses new and hard-to-solve problems for *ex falso quodlibet*.

A conclusion completes the paper, by taking stock of problems and solutions investigated and evaluating the status of the rules for absurdity in proof-theoretic semantics.

2 *Proof-theoretic validity*

2.1 *Inversion principle and justification of rules*

According to a common *vulgata* proof-theoretic semantics (PTS) distinguishes between meaning-conferring rules - which are self-justified - and rules which are justified by the meaning of the terms which occur in them. The reason to impose this distinction is that there are pairs of rules which behave so badly that they are apparently unable to give a clear meaning to the connectives which they speak about, and so should arguably be excluded from any good system. A standard example of this situation is Prior's rules for *tonk*, which allow to derive $AtonkB$ from A and to derive B from $AtonkB$. According to the received wisdom, the problem with these rules is that the meaning-conferring rule does not justify the other one.[1]

It is standard to consider I-rules as meaning conferring rules – essentially because they increase the complexity of their conclusion – and E-rules as justified.[2] Already (Gentzen, 1969b) had a similar position, and later Prawitz developed this idea in a series of works, starting from (Prawitz, 1965). Nonetheless, taking as an example Gentzen's system **NJ** of intuitionistic logic, it is immediately obvious that E-rules are not derivable from I-rules *stricto sensu*, since there are derivations that cannot be formulated without them. Moreover, even restricting our attention to closed derivations, E-rules seem to remain indispensable, so they are neither derivable, nor admissible.[3] In order to explain in which way E-rules can be justified by their introduction counterpart, Prawitz proposes his Inversion Principle (IP):[4]

> "Let α be an application of an elimination rule that has B as consequence. Then, deductions that satisfy the sufficient condition $[\cdots]$ for deriving the major premiss of α, when combined with deductions of the minor premisses of α (if any), already "contain" a deduction of B; the deduction of B is thus obtainable directly from the given deductions without the addition of α."

[1] In other words, to distinguish between meaning-conferring and justified rules means to adopt a normative approach toward meaning theory, instead of a purely descriptive one, like the one endorsed in (Popper, 1947a), (Popper, 1947b), (Popper, 1948a), (Popper, 1948b), against which Prior's objection was originally raised.

[2] It is nonetheless possible to consider E-rules as meaning conferring. This alternative is already evaluated in (Prawitz, 1971), and used in (Dummett, 1991) in order to define what he calls *stability*.

[3] (Moriconi, 2012), p. 70.

[4] (Prawitz, 1965), p. 33. For a historical account of the development of this principle, see (Moriconi and Tesconi, 2008).

In practice, a pair of rules for a logical constant suits IP iff there are some *reduction steps* which: take every derivation in which the major premise of an E-rule is derived using an I-rule as input; return a derivation of the conclusion of the E-rule which is constructed by combining the derivations of the premises of the I-rule and the (eventual) derivations of the minor premises of the E-rule. To make this intuition more precise, Prawitz introduces the notion of *maximal formula*:

Definition 1 (Maximal formulae) *Given a derivation $\mathfrak{D}$, a maximal formula in it is a formula that is the conclusion of an I-rule and the major premise of an E-rule.*

IP holds for a pair of I and E-rules iff there is a reduction step that removes the maximal formula that they generate. As an example, the maximal formula in the derivation in the left is remouved in that on the right:

$$\begin{array}{c} \supset\mathrm{E}\,\dfrac{\begin{array}{c}\vdots\\ A\end{array} \qquad \supset\mathrm{I}\,\dfrac{\begin{array}{c}[A]\\ \vdots\\ B\end{array}}{A \supset B}}{B} \\ \vdots \end{array} \quad \rightsquigarrow \quad \begin{array}{c}\vdots\\ A\\ \vdots\\ B\\ \vdots\end{array}$$

2.2 *Validity*

While this *naïf* formulation of PTS focuses on just the local property of the availability of a reduction step for a maximal formula, usually calling it *harmony*, in the more detailed picture this property is only a prerequisite of another property, called *normalizability*, which in turn is a prerequisite of what really is proof-theoretic validity.

Why IP is not enough

Someone could wonder why, even though IP already establishes that the E-rules are contained in the I-rules, we should go further in our analysis. Indeed, given the traditional characterization of analyticity, it seems that we have established that E-rules are analytic consequences of I-rules, since they are already contained in them. In order to show why this picture does not catch analyticity appropriately, let us consider the following pair of rules which, according to Stephen Read are a proof-theoretic version of the *liar*:[5]

[5] (Read, 2000), p. 141. They are essentially a reformulation of a pair of rules already evaluated in (Prawitz, 1965), p. 33.

$$\bullet\mathrm{I}\ \frac{\neg\bullet}{\bullet} \qquad\qquad \bullet\mathrm{E}\ \frac{\bullet \qquad \begin{matrix}[\neg\bullet]\\ \vdots \\ C\end{matrix}}{C}$$

These rules suit IP, so there is a reduction step for every maximal formula that they can generate. Nonetheless, they have two flaws: they generate derivations in which it is not possible to erase all the maximal formulae, even though it is possible to erase each of them; they can be used to give a closed proof of $\bot$ (passing through maximal formulae).

The mechanism behind this phenomenon is that even though each single maximal formula can be erased, other maximal formulae are generated in the process. This is not strange; it is what usually happens when we operate a reduction step. What is peculiar about this situation is that the process of removing maximal formulae moves in a circle, never accomplishing its task. Arguably, since this pair of rules leads to contradiction – a contradiction which should even be analytically valid –, we want our *criterion* to exclude them, and IP is useless for this purpose.[6]

According to Prawitz, this means that we should elaborate further on the idea of IP and ask for *normalizability* of each derivation. He defines as in *normal form* a derivation in which there are no maximal formulae, and *normalizable* a derivation which can be reduced to normal form by a *reduction procedure* – that is, the reflexive and transitive closure of reduction steps.[7]

Validity

Exploiting normalization, we can in the end formulate proof-theoretic validity explicitly. Let us start by defining *canonical derivations*:

Definition 2 (Canonical derivation) *A canonical derivation for a non-atomic sentence is a closed derivation that ends with an application of an introduction rule and such that it has only valid immediate subderivations.*

[6] Although the idea of contradictory analytical rules could seem absurd, (Read, 2015) defends this possibility.

[7] Here we are taking the reduction steps as fixed in advance. It is sometimes asserted that this is one of the main differences between Prawitz’s approach via natural deduction and Martin-Lof̈ approach via Intuitionistic Type Theory, in which on the contrary reduction steps are rules of the system, and so part of the explicit meaning of the logical constants (see (Moriconi, 2012), p. 71.). While this is surely true to some extent, especially if referred to Prawitz’s early writing (Prawitz, 1971), it is important to remember that Prawitz himself, starting from (Prawitz, 1973), considers reduction steps as “customizable” so to say, even though maybe not part of the meaning of the logical terms.

This is the central notion of the recursive definition of validity.[8] Another necessary ingredient for defining validity are *atomic bases*, which will be very important for our investigation regarding absurdity:

Definition 3 (Atomic Base) *An atomic base $\mathscr{B}$ is a set of rules (called 'atomic rules') that apply to atomic sentences and have atomic sentences as conclusions.*

An atomic rule can discharge an assumption, and there is no request of consistency for atomic bases, that is an atomic base $\mathscr{B}$ can allow a *closed* derivation of $\perp$. We obtain our goal through a definition of validity with respect to an atomic base, and a subsequent generalization that gives validity *tout court*. This detour is necessary because we need atomic bases in order to close open derivations:

Definition 4 (Validity in $\mathscr{B}$) *A derivation $\mathfrak{D}$ is valid in $\mathscr{B}$ iff either:*

1. *$\mathfrak{D}$ is a closed derivation of an atomic conclusion C and it can be reduced by normalization to a closed proof of the same conclusion C carried on in $\mathscr{B}$; or*

2. *$\mathfrak{D}$ is a closed derivation of a non-atomic conclusion C and it can be reduced by normalization to a canonical proof of the same conclusion C; or*[9]

3. *$\mathfrak{D}$ is an open derivation and for every extension $\mathscr{B}'$ of $\mathscr{B}$ every closure of $\mathfrak{D}$ (obtained by replacing each open assumption of $\mathfrak{D}$ by a closed derivation of it that is valid in $\mathscr{B}'$) is valid in $\mathscr{B}'$.*

Let us just note that, given the third clause of this definition, an open derivation can be valid in $\mathscr{B}$ because: it has only valid closures in every extension $\mathscr{B}'$ of $\mathscr{B}$; or it does not have closures at all in any extension of $\mathscr{B}$.

Definition 5 (Validity) *A derivation is valid iff it is valid with respect to each atomic base $\mathscr{B}$.*

So we have arrived in the end at the proof-theoretic definition of validity. Unfortunately, this definition works only in bundle with a controversial assumption:[10]

Assumption 2.1 (Fundamental assumption)**.** *For every valid closed derivation of B non-atomic, there is a canonical derivation with the same conclusion that can be found by reduction.*

[8] The term *canonical proof* is coined in (Dummett, 1978), but the idea that valid closed derivations that end with an application of an I-rule have a key role in the definition of validity is already established in (Prawitz, 1971).

[9] In this case a canonical derivation asks for subderivations valid in $\mathscr{B}$.

[10] (Dummett, 1991), p. 254.

This assumption is a theorem for valid closed derivations in **NJ**, that is for closed derivations in which no atomic rule is applied, since it is a trivial corollary of normalizability that in this case a closed derivation in normal form always ends with an I-rule.[11] Nonetheless, we need to assume this property also for closed derivations that use atomic rules, because these are needed to close open derivations.[12] This assumption is the reason why we can accept the second clause of the definition of validity in $\mathscr{B}$. Without this, there is no warrant that for every closed derivation there is a canonical one.

From validity of derivations to validity of inferences

With the previous notions in place, we can define valid inferences as valid (usually open) derivations. Someone could feel that we are inverting the natural order of explanation. Indeed, in PTS valid derivations are defined *before* valid inferences and the concept of derivation is even used in the definition of valid inference.[13] There are some ways to turn around this order of explanation, but it is not as easy as it could seem. As an example, Prawitz's attempt through *grounds* succeeds, but only because this notion is, again, defined in a global way.[14]

We already saw some difficulties which we encounter if we try to define validity in a *purely* local way, by using IP or some similar principles which apply directly to rules. Nonetheless, someone could be unconvinced by our counterexample which relies on Read's bullet, so let us see some possible objections and their respective answers.

Acceptability of the I-rule for bullet Read's bullet works as a counterexample to IP iff •I is an acceptable I-rule, and •I and •E suit IP.[15] There are objections to both these clauses.

Both Milne and Gabbay raise doubts about whether •-rules really satisfy IP. While their arguments are different, they are based on a common observation: •-rules taken in isolation seem to suit IP, but $\neg$ and $\perp$ occur in the rules for • and if

[11] This is the reason why Schroeder-Heister speaks of it as a corollary of normalization in (Schroeder-Heister, 2006) (p. 531), receiving some severe criticisms from Stephen Read: (Read, 2015), p. 146, note 17.

[12] Since in pure **NJ**, that is without atomic rules, we have closed derivations only for logical theorems.

[13] Prawitz (2019) pp. 494, 496.

[14] (Prawitz, 2019).

[15] Here we do not discuss the controversial position of Read, who accepts incoherent analytically valid sets of rules.

we take in consideration $\neg$-rules and $\bot$-rules as well, IP does not hold anymore.[16]

Furthermore, $\bullet$I seems already problematic *qua* introduction rule because, while I$\bullet$ should define the meaning of $\bullet$, this constant already occurs in the assumption of the rule, leading to a circular definition. According to Dummett's complexity condition, an I-rule could be 'circular' in the way just seen, as long as in any of its application its conclusion is more complex than its premises and discharged assumptions.[17] Anyway, it is clear that $\bullet$I cannot suit this condition, since $\bullet$ occurs negated in the premise but not in the conclusion, so its acceptability is still at least in doubt. Dyckhoff claims that Dummett's condition is too strong, since it excludes some well-behaving rules for natural numbers, but in the end he proposes a change that saves these arithmetical rules but still excludes $\bullet$I.[18] So, the argument goes, since there are good reasons to reject $\bullet$ as a counterexample to IP, maybe we could regain hope in a local characterization of validity. Unfortunately, there is another counterexample, which solves all these objections. The pair of rules:

$$?\mathrm{I}\,\frac{(a+c)/(b+d)=e/f}{(a/b)?(c/d)=e/f} \qquad ?\mathrm{E}\,\frac{(a/b)?(c/d)=e/f}{(a+c)/(b+d)=e/f}$$

lead to contradiction in any minimal system for arithmetic, and suits both IP and Dyckhoff's reformulation of complexity principle.[19]

Other local principles in general? Someone could object that our argument only applies to IP and so does not exclude the possibility of other local principles that characterize directly valid inferences. There is nonetheless a very general problem for this kind of approach: since valid derivations already occur in some meaning-conferring rules, we risk circularity in definition. Indeed, some valid applications of I-rules are defined using valid derivations, and valid derivations are defined compositionally from valid applications of rules. In **NJ** we have this problem for $\supset$I since its applications are valid iff they take as input valid derivations.[20] In PTS, this circularity is solved, at least in logic, with the inductive *global* definition of valid derivations that we already saw.[21] The moral seems to be that local definitions of validity are in general bad suited for rules of deduction - that is, rules

[16] (Milne, 2015), pp. 215-216, and (Gabbay, 2017), p. S113.

[17] (Dummett, 1991), p. 258.

[18] (Dyckhoff, 2016), pp. 82-83.

[19] (Ceragioli, 2019).

[20] Moreover, both $\supset$ and applications of E-rules can already occur in these derivations; chapter 11 of (Gentzen, 1969a) 11 and (Prawitz, 1971) p. 285.

[21] There is nonetheless a problem in extending the notion of canonical proof to the complete theory of intuitionism, as opposed to the purely logical fragment of this doctrine. This observation

which take derivations as inputs - and there are good reasons to believe that good systems for interesting logical calculi should contain this kind of rule.

3 *The problem with ex falso quodlibet*

Already Prawitz found a clear justification of minimal logic in PTS.[22] Nonetheless, the extension with *ex falso quodlibet* in order to obtain at least intuitionistic logic is problematic. Usually, the problem of including *efq* in PTS is analyzed only in relation to IP, while as emerges from the considerations of the previous section, a complete investigation regarding its PTS-validity is needed. Let us start with IP and then extend our investigation to validity *tout court*.

3.1 *IP and efq*

At the beginning of his investigation of IP, Prawitz adopted something like a deflationary account of *efq*. Indeed, since apparently this rule is neither an I nor an E-rule, IP is not a requisite for its acceptability and so he merely shows that it does not disturb the normalization procedure regarding the other rules.[23]

But of course this solution would be improper: when we impose IP we assume that every logical rule has to be considered as an I or an E-rule, otherwise the requirement loses its *raison d'être*.[24] So, considering *efq* as neither an I nor an E-rule means considering it as part of an atomic base, and hence valid only within some of these bases.

Another, more promising attempt consists in interpreting *efq* as an elimination rule, relabeling it $\bot$E. If we accept the label $\bot$E for *efq*, that is we decide to consider it as an elimination rule, then we have to justify it. The obvious problem is that there seems to be no introduction rule for $\bot$, so no chance of justification via IP. There are three proposals to solve this issue:

Dummett $\bot$I has the form $\bot\mathrm{I}\,\dfrac{b_1 \quad b_2 \quad \cdots}{\bot}$ where the b_i run through all the atomic sentences of the language;[25]

is related to one of Shapiro's criticisms of Tennant's proof-theoretical logicism ((Shapiro, 1998), p. 613), but it is essentially a consequence of the old Hilbert's problem of the *purity of methods*. See also (Dummett, 2000), p. 269-274.

[22] (Prawitz, 1971).

[23] (Prawitz, 1965).

[24] Dummett even suggested that harmony should be a requirement for the entire language, not only for logic; (Dummett, 1991), p. 287.

[25] (Dummett, 1991), p. 295.

Read $\bot$E is justified by the absence of an introduction rule for $\bot$;[26]

Milne $\bot$I is a non-logical and non-formal rule that depends on the context, like $\bot\mathrm{I}\ \dfrac{0=1}{\bot}$.[27]

The first two solutions treat $\bot$ as a logical constant, while the last one relegates it to the non-logical vocabulary. If we accept the first formulation of $\bot$I it is clear that IP holds. Indeed we have the following kind of maximal formulae and reduction step:

$$\bot\mathrm{I}\ \dfrac{\begin{matrix}\vdots \\ b_1\end{matrix} \quad \begin{matrix}\vdots \\ b_2\end{matrix} \quad \dots}{\bot\mathrm{E}\ \dfrac{\bot}{b_i}} \quad \rightsquigarrow \quad \begin{matrix}\vdots \\ b_i\end{matrix}$$

Nonetheless, this kind of solution has a big disadvantage: the meaning of $\bot$ is not independent of the non-logical fragment of the language. Indeed, since we treat it as a kind of conjunction of all the atoms, if we change the class of atoms then we change its meaning. So it seems that the meaning of $\bot$ is not fixed for every language, and it can change in different contexts. Dummett speaks on this regard of a lack of 'invariance' of $\bot$.[28]

Moreover, nothing says that $\bot$ has to be false, that is that there cannot be a valid derivation for it from correctly asserted assumptions, as observed by Nils Kürbis.[29] Indeed let us assume that $\bot$E and the first version of $\bot$I were enough to define absurdity. Let us now consider the following scenario: in our pre-logical language every atomic sentence is true.[30] If we do not need pre-logical knowledge to understand absurdity, the meaning of $\bot$ should be unaltered in this case, and as a conclusion absurdity does not need to be false, since it is tantamount to the conjunction of all atomic sentences, which is a true sentence in our toy-language.

[26] (Read, 2000), p. 139. To be precise, this justification was already present in (Cozzo, 1994) (p. 110) and the author claims that he heard Prawitz suggesting this idea during the 1980s.

[27] (Milne, 1994), p. 64.

[28] (Dummett, 1991), p. 296. Of course, the same lack applies to negation since its meaning depends on that of absurdity. We could even observe this *phenomenon* directly for negation, by the rule (see (Milne, 1994), p. 81): $\neg\mathrm{I}\ \dfrac{\begin{matrix}A \\ \vdots \\ b_1\end{matrix} \quad \begin{matrix}A \\ \vdots \\ b_2\end{matrix} \quad \dots}{\neg A}$ Nonetheless, I think that using $\bot$ distinguishes more clearly between the logical and the non-logical aspects of the meaning.

[29] (Kürbis, 2015a) and (Kürbis, 2015b).

[30] This of course can happen only for deeply deficient languages.

Indeed, if it were not for non-formal contradictions like 'this is a completely red and completely green coloured spot' or 'this body is heavy and light', or at least factually false sentences like 'the Moon is made of cheese', what problems there would be in accepting $\perp$ among our beliefs? According to Kürbis, from this observation we have to conclude that the rules for $\perp$ do not offer a complete characterization of the meaning of "absurd". Kürbis seems to say that the loss of invariance is enough to reject the idea that we can catch the meaning of $\perp$ using an inferentialist theory of meaning. Nonetheless I think, with Dummett, that all that we can conclude from this observation is that a part of the meaning of absurdity does not pertain to logic. (Dummett, 1991) indeed makes the same observation of Kürbis and concludes:[31]

> "It is, however, important to observe that no appeal has been made to the principle of consistency, and that the logical laws do not imply it. We may know our language to be such that not every atomic statement can be true; but logic does not know that. As far as it is concerned, they might form a consistent set, as they are assumed to do in Wittgenstein's Tractatus. The principle of consistency is not a logical principle: logic does not require it, and no logical laws could be framed that would entail it."

Hence, far from being a problem of proof-theoretic semantics, this situation seems to be a well-known aspect of this theory, in line with some positions regarding logic. Of course, philosophers who do not share this position see it as a problem, but this holds for every philosophical consequence of PTS.

The second solution, proposed by Stephen Read solves the problem of invariance of $\perp$. The idea naturally arises from a reformulation of IP proposed by the author, according to which E-rules are obtained as results of the application of a function to a set of I-rules. Nonetheless, I think that this proposal has nothing to say about the falsity of $\perp$, for which we have to accept Dummett's position.

The third alternative accepts in some way both Dummett's observation about the lack of invariance of $\perp$ and the conclusion, derivable from Kürbis's observation, that part of its meaning comes from the non-logical territory. We could say that while Dummett's solution isolates only the logical part of the meaning of $\perp$, Milne's solution accepts a completely non-logical meaning-conferring rule for this constant, leaving in some way open the question of the logical status of $\perp$E.

There have been a lot of logical investigations regarding IP and the rules for absurdity, but the problem of its PTS-validity has been guiltily overlooked.

[31] P. 295.

3.2 *Validity of efq*

Of course, inconsistent atomic bases pose some problems for the validity of *efq*. Since the validity of a rule in $\mathscr{B}$ (equated to an open derivation) asks the preservation of validity of *closed* derivations *in any extension* $\mathscr{B}'$ of $\mathscr{B}$ (clause 3 of definition 4), this rule cannot be valid in any atomic base that has incoherent but non-trivial extensions. Hence, this rule is invalid even in coherent bases, as opposed to what is usually believed. *A fortiori*, validity *tout court* of *efq* is out of the question.

To save *efq* at least in coherent bases, we could "update" the definition of validity in $\mathscr{B}$ as Prawitz did in his (Prawitz, 1973) and (Prawitz, 1974). In those papers, he substitute clause 3 with the following:

3'. $\mathfrak{D}$ is an open derivation and every closure of $\mathfrak{D}$ (obtained by replacing each open assumption of $\mathfrak{D}$ by a closed derivation of it that is valid in $\mathscr{B}$) is valid in $\mathscr{B}$.

That is, he fully neglects extensions of atomic bases.[32] In this way, he can claim that *efq* is valid at least for coherent atomic bases.[33] Following Dummett's suggestion that consistency is not a logical property, we point out that with this change *efq* is valid for incoherent trivial atomic bases as well.[34]

Someone could wonder why we should accept incoherent bases and especially incoherent non-trivial ones, since they are the main cause of all our problems in justifying *efq*. By excluding them from the class of acceptable atomic bases, this rule would become valid in general. This solution has a clear connection to the proposed I-rules that we saw in the previous paragraph: the adoption of Read's rule entails essentially a rejection of incoherent atomic bases, while the adoption of Dummett's rule entails that every incoherent base is trivial.

Before evaluating the philosophical opportunity of such a restriction on the class of atomic bases, let us focus on some technical details. This restriction is a workable solution only with the original clause 3 of definition 4 in place of the recently proposed alternative 3'. Indeed, if we evaluate open derivations in atomic bases without considering their extensions, we need incoherent non-trivial atomic bases in order to reject some unwanted open derivations. In other words, with

[32] Schroeder-Heister in (Schroeder-Heister, 2006) registers this change in Prawitz's definition, but decides to remain with the first formulation; see note 19 on page 567

[33] (Prawitz, 1974), p. 243.

[34] By selecting even smaller classes of atomic bases we could find justifications of other logics. As an example, by considering only decidable predicates it should be possible to justify classical logic. Interestingly enough, this resembles Beall and Restall's path towards logical pluralism: (Beall and Restall, 2006).

clause 3' we cannot have *efq* valid in general without having unwanted derivations as well. Let us see an example.

Of course, our definition of validity should not prove the open derivation

$$\frac{\neg p}{q}$$

as valid in general, if p and q are atomic. So this derivation should be invalid in at least an atomic base $\mathscr{B}$. According to clause 3', in order to do this, we need to have a closed derivation for $\neg p$ in $\mathscr{B}$. Indeed if it is impossible to close the derivation $\mathfrak{D}$ for $\neg p$ in $\mathscr{B}$, this derivation become vacuously valid in it. Since $\neg p$ is logically complex, if $\mathfrak{D}$ is valid in $\mathscr{B}$, then by fundamental assumption there is a canonical derivation $\mathfrak{D}'$ of $\neg p$ that ends with an application of ¬I. It must be closed and its form has to be

$$\begin{array}{c}[p]\\ \vdots\\ \neg\mathrm{I}\ \dfrac{\bot}{\neg p}\end{array}$$

Let us now consider the open sub-derivation of $\bot$ from p. This is an open derivation and so it is valid in $\mathscr{B}$ iff it can be closed by a valid derivation in $\mathscr{B}$ so that the result of the closure is again a valid close derivation. But the closure of a derivation of $\bot$ from p results in a closed derivation of $\bot$, so $\mathscr{B}$ has to be incoherent. Now, if $\mathscr{B}$ were trivial as well, q would be provable in it as well. So, if we accept only coherent or trivially incoherent atomic bases and use clause 3' of the definition of validity, there would be a valid derivation of any atomic sentence q from the negation of any other atomic sentence $\neg p$. The validity of this unwanted derivation makes unpalatable clause 3', since it makes *efq* valid (in general or in some specific bases) only together with some very repulsive rules.

As we already saw, with clause 3 in place of clause 3' *efq* cannot be valid only in coherent bases. Nonetheless, by restricting atomic bases to just coherent and trivial ones we have the validity of this rule in all atomic bases; that is, its validity *stricto sensu*. Moreover, the justification of $\frac{\neg p}{q}$ which we just saw regarding clause 3' is blocked for clause 3. Indeed, with this clause in its place, to falsify this rule we just need a base $\mathscr{B}$ that has at least an extension $\mathscr{B}'$ that closes the derivation of $\neg p$ and a further extension $\mathscr{B}''$ that closes a derivation of $\bot$. It must be noticed that while $\mathscr{B}''$ must be incoherent, $\mathscr{B}'$ need not, since it just closes the derivation of $\neg p$, requiring a derivation of $\bot$ possibly with p as an open assumption. Now, even if $\mathscr{B}''$ were trivial (as it must if we impose triviality to any incoherent base) and so proved q, $\mathscr{B}'$ would still be an extension of $\mathscr{B}$

which enables the proof of $\neg p$ without proving q as well. It should be remarked that this reply works only if we restrict atomic bases to coherent *or trivial* and not just to coherent ones. Indeed the incoherent (and possibly trivial) base $\mathscr{B}''$ is needed for the rule not to be vacuously valid. So, following the comparison between restrictions and rules, it seems that Dummett's rule is needed instead of Read's one, if we want *efq* not to have unpalatable companies.

We can so announce our first bad conclusion:

Theorem 3.1 (Lack of invariance of $\bot$)**.** *If efq is valid, $\bot$ is not invariant.*

Proof. While Read's choice of an empty set of I-rules for $\bot$ establishes both IP for *efq* and invariance for $\bot$, Dummett's I-rule for $\bot$ suits only IP. To assume an empty set of I-rules for $\bot$ is equivalent to assume only coherent atomic bases in the definition of validity, and this has unacceptable consequences. Dummett's rule, which is equivalent to assume only coherent *or trivial* atomic bases, solves this issue, but at the cost of the lost of invariance for $\bot$. $\square$

Let us now consider our third alternative: Milne's proposal of non-logical I-rules for $\bot$. This move is essentially a rejection of the logical status of this constant, and does not lead to any evident justification of *efq*. Indeed, how could such a rule even suit IP with *efq*? The only alternative, in this case, should be accepting the non-logical status of *efq* as well, which will become justified in those bases in which it is assumed. This alternative could be seen as a surrender, but there are nonetheless good reasons to defend Milne's setting. Indeed, it can clearly be identified with an unrestricted acceptance of all possible atomic bases, and this should be the standard position, lacking any reasonable reason to restrict them. Moreover, the need for incoherent, trivial bases makes the situation even worse, since while the exclusion of all the incoherent bases could be justified by pointing out (controversially and against Dummett) that logic teaches that the world is coherent, the adoption of only those incoherent bases that are trivial is obviously justifiable only through *efq*, and so circularly. Hence, we conclude:

Theorem 3.2 (Circularity of *efq*)**.** *In PTS efq can be justified at most in a circular way.*

4 *Conclusion*

In the first section of this paper, we defined validity in PTS and explained why such an explicit definition is not only desirable, but even needed for our justification of logic to be well-founded. Some objections and different starting points are evaluated and shown inadequate. In the second section, we discussed the rule of *ex falso quodlibet* and its problems in PTS. Three proposals are evaluated: Read's idea that there should be no I-rules for $\perp$, Dummett's idea that the I-rule for $\perp$ should have all the atomic sentences as premises, and Milne's idea that the I-rule for $\perp$ should be extra-logical. It is argued that, while both the first two rules suit IP, only Read's one suits invariance, that is it gives a meaning to $\perp$ which does not depend on the non-logical part of the language. Nonetheless, it is also shown that Dummett's rule is the only alternative that could make *efq* valid according to the definition that PTS gives of this notion, without at the same time making valid some obviously unacceptable rules. As a first conclusion, the validity of *efq* can be achieved only at cost of a lack of invariance for $\perp$. In conclusion, it is shown that Dummett's rule and so *efq* can be justified only in a circular way, making the case for Milne's defeatist proposal of a non-logical treatment of absurdity.

BIBLIOGRAPHY

Beall, J. and Restall, G. (2006). *Logical Pluralism*. Oxford University Press, Oxford.

Ceragioli, L. (2019). Peano's counterexample to harmony. *Theoria*, 85:459–484.

Cozzo, C. (1994). *Teoria del significato e filosofia della logica*. Clueb, Bologna.

Dummett, M. (1978). The philosophical basis of intuitionistic logic. In *Truth and other enigmas*, pages 215–247. Duckworth, London.

Dummett, M. (1991). *The Logical Basis of Metaphysics*. Harvard University Press, Cambridge (Massachussets).

Dummett, M. (2000). *Elements of Intuitionism*. Claredon Press, Oxford.

Dyckhoff, R. (2016). Some remarks on proof-theoretic semantics. In Piecha, T. and Schroeder-Heister, P., editors, *Advances in Proof-Theoretic Semantics*, pages 79–93. Springer International Publishing, Cham.

Gabbay, M. (2017). Bilateralism does not provide a proof theoretic treatment of classical logic (for technical reasons). *Journal of Applied Logic*, 25:S108–S122.

Gentzen (1969a). The consistency of elementary number theory. In Szambo, M. E., editor, *The Collected Papers of Gerhard Gentzen*, pages 132–213. North-Holland Publishing Company, Amsterdam, London.

Gentzen (1969b). Investigation into logical deduction. In Szambo, M. E., editor, *The Collected Papers of Gerhard Gentzen*, pages 68–131. North-Holland Publishing Company, Amsterdam, London.

Kürbis, N. (2015a). Proof-theoretic semantics, a problem with negation and prospects for modality. *Journal of Philosophical Logic*, 44:713–727.

Kürbis, N. (2015b). What is wrong with classical negation? *Grazer Philosophische Studien*, 92:51–86.

Milne, P. (1994). Classical harmony: Rules of inference and the meaning of the logical constants. *Synthese*, 100:49–94.

Milne, P. (2015). Inversion principles and introduction rules. In Wansing, H., editor, *Dag Prawitz on Proofs and Meaning*, volume 7 of *Outstanding Contributions to Logic*, chapter 8, pages 189–224. Springer, Cham, Heidelberg, New York, Dordrecht, London.

Moriconi, E. (2012). Steps towards a proof-theoretical semantics. *Topoi*, 31:67–75.

Moriconi, E. and Tesconi, L. (2008). On inversion principles. *History and Philosophy of Logic*, 29(2):103–113.

Popper, K. R. (1946-1947a). Logic without assumptions. In *Proceedings of the Aristotelian Society*, volume 47 of *New Series*, pages 251–292. Aristotelian Society, Wiley.

Popper, K. R. (1947b). New foundations for logic. *Mind*, 56(223):193–235.

Popper, K. R. (1948a). On the theory of deduction, part i. derivation and its generalizations. In *Koninklijke Nederlandsche Akademie van Wetenschappen, Proceedings of the section of sciences*, volume 51, pages 173–183.

Popper, K. R. (1948b). On the theory of deduction, part ii. the definitions of classical and intuitionist negation. In *Koninklijke Nederlandsche Akademie van Wetenschappen, Proceedings of the section of sciences*, volume 51, pages 322–331.

Prawitz, D. (1965). *Natural Deduction: A Proof-Theoretic Study*. Almqvist & Wiksell, Stockholm.

Prawitz, D. (1971). Ideas and results in proof theory. In Fenstad, J., editor, *Proceedings of the 2. Scandinavian Logic Symposium*, pages 237–309. North-Holland.

Prawitz, D. (1973). Towards a foundation of general proof theory. In et al, P. S., editor, *Logic, Methodology and Philosophy of Science IV*, pages 225–50. North-Holland.

Prawitz, D. (1974). On the idea of a general proof theory. *Synthese 27, 1974, pp 63-77*, 27:63–77.

Prawitz, D. (2019). The seeming interdependence between the concepts of valid inference and proof. *Topoi*, (38):493–503.

Read, S. (2000). Harmony and autonomy in classical logic. *Journal of Philosophical Logic*, (29):123–54.

Read, S. (2015). Proof-theoretic validity. In Caret, C. and Hjortland, O., editors, *Foundations of Logical Consequence*, pages 136–58. Oxford UP.

Schroeder-Heister, P. (2006). Validity concepts in proof-theoretic semantics. *Synthese*, 148(3):525–571.

Shapiro, S. (1998). Proof and truth: Through thick and thin. *Journal of Philosophy*, 95(10):493–521.