

## AGAINST ABSOLUTE CERTAINTY

ABSTRACT. I criticize Hoppe's concept of argumentation ethics, which is used to give a "Letztbegründung" (final, incontestable proof) of libertarian ethics and Austrian economics, from the point of view of Popper's critical rationalism.

I also evaluate various arguments against Popper in libertarian literature and find them misguided: They criticize only an empiricist straw version of Popper's critical rationalism.

I argue that the libertarian theory – ethics as well as economics – have to be based on critical instead of classical rationalism.

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## 1. INTRODUCTION

I'm libertarian, but also a natural scientist, physicist, interested in the philosophy of science. In this domain, Popper's fallibilism is popular and considered as almost self-evident. So that it was quite strange for me to recognize that libertarian theory, in ethics as well as economics, is currently based on a completely different and, in my opinion, outdated philosophy, on classical rationalism. One of the aims of this paper is, therefore, argumentation in favour of the philosophy of critical rationalism – the philosophy developed by Karl Popper and Hans Albert.

This does not mean that I want to propose here, as many empiricists have, to apply the methods of natural sciences, in particular physics, in the domain of economy and ethics.

First, I recognize very well that there are important differences between the domain of economics and natural sciences. Nonetheless, these are, however important, differences in degree, not in principle. Despite these differences, they can be based on a common philosophy – a philosophy which fixes only the most general ideas, but not the details. And for this general philosophy I propose to use critical rationalism.

Second, the philosophy I propose here – critical rationalism – is not simply fallibilism as used by physicists. Most physicists pay lip service to Popper’s fallibilism but remain heavily infected by positivistic influences, so what they propose is, in fact, something like a positivistic version of fallibilism. In particular, physicists remain heavily influenced by the general anti-metaphysical, anti-philosophical prejudices of empiricism, and therefore simply ignore philosophy of science. As a consequence, they remain unable to recognize the large philosophical differences between critical rationalism and their own trivialized versions of fallibilism. But even those who are nonetheless interested in philosophy usually have a heavily simplified idea about Popper’s philosophy. The presentation made by Sokal and Bricmont [28] is an unfortunate example.

But this trivialisation of critical rationalism is not only a disease among physicists. There is a whole “intellectual tradition” of gross misrepresentation of Popper’s theory. In his critique of Sokal and Bricmont, Niemann [18] has criticized this “intellectual tradition” in quite harsh words<sup>1</sup> and named Kuhn, Feyerabend, and Lakatos as the originators of this tradition of trivialization of Popper. In this sense, we can speak about an established philosophy of “naive fallibilism”, which is attributed to Popper, but has nothing to do with Popper’s original version of fallibilism: It is, in fact, not much more than old empiricism “improved” by using the word “falsification” instead of “verification”.

So Popper’s philosophy of critical rationalism remains widely unknown. Not because Popper is unknown, but because his true philosophy is hidden behind the fake of naive fallibilism. So, one of the aims of this essay is to give some introduction into critical rationalism. I combine this with a consideration of arguments against Popper I have found in libertarian literature, most of them made by Hoppe. They show that libertarian philosophy is infected by the same problem – what is criticized is not Popper, but naive fallibilism, the fake version of Popper’s teachings.

The historical accident that Hoppe’s PhD adviser was Habermas possibly explains Hoppe’s personal position. Habermas was Popper’s opponent in the German “Positivismusstreit” and probably deserves the title of a champion of misrepresentation of Popper. Habermas has to be blamed also as the originator of the second target of this essay: Together with Apel, he has created the theory named “Diskursethik” (ethics of discourse) – the German, social-democratic original of Hoppe’s libertarian argumentation ethics. There is not much more to say here about Habermas. What I attack here, as a libertarian, is Hoppe’s libertarian version. To attack the original from the position of critical rationalism is not necessary – it has been already criticized in detail by Hans Albert [23],[24],[25],[26]. Nonetheless, the arguments I propose here do not depend on the differences between the social-democratic and libertarian versions. What is attacked is the very philosophy, and in particular the thesis that one can derive in this way incontestable truths. The “truths” themselves – the social-democratic as well as the libertarian variants – remain contestable, questionable and hypothetical. In their social-democratic version, they are simply wrong. This is not what I want to show in the libertarian case. Instead, what Hoppe claims to derive are (in most cases) reasonable and

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<sup>1</sup>“Poppers Ansichten über Falsifikation sind leicht zugänglich; sie zu überlesen, ist deshalb nicht ein läßlicher Fehler, der en passant jedem unterlaufen kann, sondern ein aus wissenschaftlicher Sicht grober Fehler, dessen sich schon Kuhn, Feyerabend, Lakatos und alle ihnen folgende Kritiker des ‘naiven Falsifikationismus’ schuldig gemacht haben.”[18]

useful principles. And they are worth to be defended. But, please, not with invalid methods, which can be easily misused to “prove” totalitarian nonsense as well, as has been shown already by the original version.

But there is also a third target of this essay – classical rationalism. In fact, the popularity of argumentation ethics in the libertarian community can be easily understood if one takes into account that argumentation ethics is a quite natural improvement of key ideas of classical rationalism. So I argue not only against argumentation ethics, but against classical rationalism in general, in favour of critical rationalism.

It can be considered as a revenge for Hoppe’s misrepresentation of critical rationalism as a variant of empiricism that I criticize, in particular, classical rationalism as a minor and insufficient improvement of empiricism, which shares with empiricism some of its most fundamental errors. In fact, classical rationalists of the Austrian school leave natural sciences completely to empiricism. But the most important shared error of above philosophies is their quest for absolute certainty, the very idea that one can obtain absolutely certain knowledge outside the domain of mathematics. In empiricism, this idea leads to the concept of derivation of scientific theories from experience, a misconception which is shared and only mildly corrected by classical rationalists, who insist that such a derivation requires a few additional a priori principles.

The resulting practical consequences for the libertarian movement are fatal. The prejudice against uncertainty in Austrian economics leads to an unjustified rejection of whole branches of economics by Austrian economists, leaving these domains to pro-state economists. Libertarian economic science becomes splitted into an Austrian and a non-Austrian part. The belief in absolute certainty of the own teachings leads groups like the objectivists into isolation. And it is politically absurd that the most ardent defenders of an ideology which is, in its very essence, pro-state – the ideology of absolute certainty – are today the libertarians.

## 2. PERFORMATIVE CONTRADICTIONS

Let’s start with the consideration of the useful part of Hoppe’s “argumentation ethics” – the method to use so-called “performative contradictions” as logical arguments against certain theories.

A performative contradiction is a contradiction between a theory (hypothesis, claim, statement, ethical rule) and the behaviour of the proponent of this theory.

Such contradictions are something quite common, especially in the case of ethical theories, where the proponents of these ethical theories often enough violate their own rules. For this proponent, the contradiction is quite shameful, much more shameful than a contradiction with some external ethical rule which he has not proposed, not even acknowledged himself as just. He will be despised by everybody, not only by proponents of the rule in question, but even by those who do not acknowledge this rule or even violate it themselves.

**2.1. The irrelevance of simple performative contradictions.** On the other hand, such contradiction are only arguments against these particular proponents. Usually they tell us nothing about the theory in question itself. Even if many proponents of a given rule violate it, it may be nonetheless a useful ethical rule, worth to be widely accepted and followed.

This is an important qualitative difference between an internal logical contradiction of a theory and a performative contradiction. The internal logical contradiction is a decisive, final argument against a given theory. In fact, a true theory cannot have any internal logical contradictions. But it is quite possible for an ethical theory that those who propose it violate the very rules they propose. The same is possible for other types of statements. In particular, there may be statements about optimal strategies in certain situations. These statements may be true even if those who propose them violate them if they in fact appear in such a situation.

The situation is different in a physical theory. A physical (or any other empirical) theory is falsified if its predictions fail in reality. Here, of course, the performance of a proponent of the theory is as good as any other piece of empirical evidence – if it contradicts the prediction of the theory, the theory is falsified. But the very fact that the contradiction is a performative one made by a proponent of the theory plays no role at all. The performance of everybody else would be as good as an argument, and the falsification of the theory as a physical theory by the performance of the theoretician would be in no way shameful, but acknowledged as an important positive contribution to science. So, while it formally fits – a contradiction between a theory and the performance of one of its proponents – we can safely ignore the particular case of empirical theories from our considerations.

As an argument against ethical or strategical rules or theories, a particular performative contradiction is, therefore, simply irrelevant. It is only an *ad hominem* argument against the particular proponent.

So, there should be more than a particular case of a particular performative contradiction if we want to use it as an argument against the *theory* in question.

And, in fact, much more. It is, in particular, not sufficient to show that, say, violations of the rule in question are much more common among proponents of the rule than among a control group. There was, for example, quite good evidence for this in the case of rejection of homosexuality, good enough to introduce the special notion “homophobia” for this particular effect, and a similar effect of “paedophilia” may be behind the actual hatred against paedophiles. But does this provide an argument against these theories? It doesn’t, because it is a quite reasonable and natural self-defense of homosexuals in an anti-homosexual society to become anti-homosexual fanatics, as well as it is today for pedophiles to become anti-pedophile fanatics. The observation of this effect is in no way an argument in favour of homosexuals or pedophiles, and has, at best, an indirect influence: The fanatics become suspect themselves, which possibly weakens the corresponding hysteria. That’s all. So, even a general tendency for performative contradictions among the proponents does not give an argument against a theory itself. It remains to be not more than a weak *ad hominem*.

So, to become an argument against a theory, a performative contradiction needs another, completely different quality.

**2.2. Unavoidable performative contradictions as an argument against philosophical theories.** This new quality is that the performative contradiction is *unavoidable in principle*. That means, not only some of the defenders of a given theory, not even most of them, but every defender of the theory *necessarily* behaves in such a way that a performative contradiction with the theory appears.

How is this possible at all? The quite trivial case of an empirical theory contradicted by empirical facts about human behaviour I have already excluded, because

in this case it is completely irrelevant that the human being is a defender of the theory.

So the really interesting case is where it is the very behaviour of defending the theory, the act of argumentation in favour of the theory, which necessarily contradicts the theory in question.

Now, there are, without doubt, some theories such that every argumentation in favour of this theory contradicts the theory. Some of them can be invented simply for the purpose of having such examples: For example, the claim “it is not possible to argue” would be necessarily contradicted by a defender who argues in favour of this claim. But, of course, such theories are simply nonsense, invented for having an example which could be rejected.

Fortunately, there are some more interesting examples of philosophical theories, theories which have been seriously proposed and defended. The most important examples are various versions of relativism of truth as well as the metaphysical rejection of “metaphysics” which is part of empiricism.

Relativism claims that humans are in principle unable to find any truth. But what is, in this case, the status of relativism itself? Is it a true theory? He who seriously defends this theory behaves as if the theory is true. This is, simply, part of the meaning of the phrase “seriously defending a theory”. So, for a serious defender of this theory, a performative contradiction is unavoidable.

Similarly, empiricism claims that true theories have to be derived from experience. But this theory is, in itself, not derived from experience. A defender of empiricism in an argumentative discussion behaves, as well, as if there were possibilities different from empirical observation – namely the evaluation of his arguments – which allow to decide about the correctness of philosophical theories, in particular about empiricism.

So, there are interesting and important theories such that performative contradictions provide strong arguments against them. This is already sufficient to consider the presentation of unavoidable performative contradictions in particular theories as a reasonable method of criticism of these theories. In this way, it has its place in the large arsenal of different methods of criticizing theories which may be used in critical rationalism.

**2.3. The weakness of performative contradictions.** But unfortunately theories of this type are rare exceptions.

Indeed, I have already shown that these theories have to have a very strong special property – that *every* imaginable argumentative defense necessarily leads to a contradiction. I don’t see any reason to hope that this could be a common property of wrong philosophical theories.

And in fact even in the case of the few theories which may be rejected by the application of this method, minor modifications and corrections are usually sufficient to save these theories from this particular attack.

The classical example is Feyerabend’s version of relativism: With the rationalists search for truth Feyerabend rejects also the necessity of serious argumentation in the search for truth. So, if he participates in such a serious argumentation, a contradiction seems unavoidable. But, despite this, Feyerabend’s anarchism does not forbid to participate in serious discussions. One may participate for whatever reason. In particular, one may participate just for having fun by frustrating rationalists who take these argumentations seriously. So can one accuse Feyerabend

himself of not following his own rules, of showing a performative contradiction? Even if he sometimes looks like presenting serious arguments, one cannot. First, one cannot derive from observation of the argumentative behaviour that he really takes the argumentation seriously. He may only pretend to participate seriously, but really, in fact, this participation is only a joke. But even if we ignore this possibility, and conclude that he is participating seriously, Feyerabend's version of anarchism does not *forbid* to argue seriously. There is only no moral *obligation* to argue seriously.

So, even if the performative contradiction may really become an argument against the very theory in question, it appears quite unproblematic to save the theory from the performative contradiction. In the particular example, a quite minor modification of the theory was sufficient. This simplicity to save a theory endangered by unavoidable performative contradictions underscores the exceptional character of theories killed by this attack: Even in a sufficiently small environment of the theory in question one can find theories immune against this attack.

This does not save Feyerabend's relativism itself. Relativism of truth remains nonsensical as a philosophical theory, but for other reasons.

Another example is philosophical egoism. One has to distinguish here the Randian version of egoism – a very specific ethical theory which makes things obligatory which are forbidden in usual altruistic ethics – from the Stirnerian version of egoism, which does not impose any ethical restrictions at all. Once there is behaviour which is forbidden in Randian ethics, it may appear that Randians violate Randian ethics. The situation is different in Stirnerian egoism. Here, the possibility of a performative contradiction has been considered and rejected already by Stirner himself:

If to the extent of my powers I let a bit of daylight fall in on the nocturnal spookery, is it perchance because love to you inspires this in me? Do I write out of love to men? No, I write because I want to procure for my thoughts an existence in the world; and, even if I foresaw that these thoughts would deprive you of your rest and your peace, even if I saw the bloodiest wars and the fall of many generations springing up from this seed of thought – I would nevertheless scatter it. Do with it what you will and can, that is your affair and does not trouble me.<sup>2</sup> [16]

And even if we interpret Stirner's egoism as an ethical prescription not to worship any Idols, Gods, Kings, or Ideas like Humanity, Reason, or Truth (which is incorrect – Stirner rejects such worshipping not as a wrongdoing, but as a stupidity, as a form of mental illness) there is no danger of performative contradiction: The Egoist is free to follow whatever He considers as His own idea. What makes the difference is that the Egoist preserves the freedom to reject, in some future, His own idea in favour of some different own interest. But this difference is nothing which is visible in argumentative behaviour. So, observing some argumentative behaviour we can

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<sup>2</sup> Lasse Ich etwa darum nach Kräften ein Tageslicht über den nächtlichen Spuk einfallen, weil Mir's die Liebe zu Euch so eingibt? Schreibe Ich aus Liebe zu den Menschen? Nein, Ich schreibe, weil Ich meinen Gedanken ein Dasein in der Welt verschaffen will, und sähe Ich auch voraus, dass diese Gedanken Euch um eure Ruhe und euren Frieden brächten, sähe Ich auch die blutigsten Kriege und den Untergang vieler Generationen aus dieser Gedankensaat aufkeimen: – Ich streute sie dennoch aus. Macht damit, was Ihr wollt und könnt, das ist eure Sache und kümmert Mich nicht.

never conclude that there is a performative contradiction with (this interpretation of) Stirner's egoism.

**2.4. The consent theory of truth as a presupposition.** To become a really decisive argument against a theory, one needs even more than the necessary, unavoidable character of the performative contradiction. In fact, the two examples above of how to avoid this argument have illustrated this weak point: They have reinterpreted the nature of participation of their proponents in an argumentation.

For Feyerabend, argumentation may be a nice and interesting possibility to have fun by frustrating rationalists, for Stirner it is a nice possibility to distribute the own ideas. This not only preserves them from performative contradictions. There is also another subtle point: Even if there would be a performative contradiction, it would be less dangerous. In the worst case, it would be impossible for them to participate in argumentations without contradicting themselves. But so what? Feyerabend would be frustrated and bored, having lost a nice possibility to have fun, and Stirner would have to look for other ways to distribute His ideas or to do other things in His interest.

But this would not yet prove that their philosophies are false.

In fact, the argumentative power of the performative contradiction depends on a particular theory of truth. It is known as the "consent theory of truth". According to it, the decisive criterion for deciding about truth is intersubjective discussion, discourse. Apel, the founder of discourse ethics, openly proposes this theory of truth (see, for example, [27]). I have not found an explicit discussion of this question by Hoppe, but interpret the following quote (in particular the "must be decided" part of it) as at least suggesting that Hoppe presupposes this theory of truth too, and thinks one can justify it with a similar argument:

Whether or not something is true, false, or undecidable; whether or not it has been justified; what is required in order to justify it; whether I, my opponents, or none of us is right – all of this must be decided in the course of argumentation. This proposition is true a priori, because it cannot be denied without affirming it in the act of denying it. [2]

Instead, I follow (with Popper) the good old correspondence theory of truth: Truth means correspondence to the facts, to reality. The most important, classical objection is that in this case we have no way to find out what is true or not in a decisive, certain way. But in critical rationalism this is simply acknowledged as a fact of life – yes, we will never have absolute certainty that the hypotheses we believe in are true.

I think it is only the discomfort with this conclusion which makes such theories like consent theory of truth considerable as alternatives: We can hear all arguments and, then, make a final decision. At least in principle. What to do with the possibility of existence of arguments which have not yet been proposed? I don't know. But at least one does not have to give up the dream of absolute certainty.

I think it is also worth to mention that the consent theory of truth is ideal as a justification of democratic decision making. All what remains is to find some argument to replace consent by majority decision. But this is quite easy. There are stupid people, people who are not interested in truth, selfish people who do not like

to accept truths which contradict their interests, so we cannot expect absolute consent in real, non-ideal discussions. So majority decision (or some qualified variant of it) has to do the job.

The dangerous question if the “truths” established by majority decision have any correspondence to reality does not appear at all. It is based on the rejected correspondence theory of truth and, therefore, misguided. In consent theory of truth, it is not reality which counts, but consent – which, in practice, reduces to the majority decision of the Congress or the Supreme Soviet.

This is an example of a general tendency: The connection between the hope for absolute certainty – the main target of this essay – and etatist ideology. Of course, the dream of absolute certainty should be rejected not because of this connection, but because it is a false, misleading hope, because our knowledge about the world is in fact uncertain and remains open to doubt.

**2.5. The status of performative contradictions in correspondence theory of truth.** The point of emphasizing the role of consent theory of truth is that if we follow the correspondence theory of truth, the status of performative contradictions becomes much less decisive. In fact, in correspondence theory of truth what really matters is reality itself, not our human ability to establish true theories of reality.

Of course, as humans we are restricted to theories which humans are in principle able to invent and to evaluate. Intersubjective argumentation is an extremely important method for this purpose. But in correspondence theory of truth it is only a *particular method* to find out something about the truth. It is part of the method to criticize proposed theories, and therefore plays a central role in critical rationalism. But it is nonetheless only a particular method. It does not give us any warranty that it leads to the correct result, to truth. And it is, in particular, imaginable that a true theory cannot be defended argumentatively in intersubjective discussion.

Thus, even an unavoidable performative contradiction would not be an unquestionable, final argument against a given theory. Its status is reduced to that of a sufficiently strong argument. But it is, in comparison, weaker than an internal logical contradiction.

All this said, let’s clarify that performative contradictions, if they appear really unavoidable and therefore more than simple ad hominem arguments, remain strong arguments against particular theories. In this property, they are part of the methods of critical rationalism: They allow to criticize and falsify theories.

### 3. LETZTBEGRÜNDUNG

But there is another idea about this – the idea to use performative contradictions as a “Letztbegründung” of ethics.

A “Letztbegründung” is the Holy Grail for ethical philosophers uncomfortable with rational ethics. Here, rational ethics is the quite simple and straightforward recommendation to follow the own educated self-interest. Educated means that they should take into account all the risks, side effects, and back-reactions of their actions. And therefore they have to take into account also the interests of other people influenced by their actions. Rational ethics is quite sufficient to solve the problems of cooperation. And, which is the point, it does not require any Letztbegründung – it is simply a recommendation for rational decision making.

But rational ethics does not seem sufficient to some moral philosophers. They prefer a theory of ethics which requires more. People should give up their own

interests in favour of the interests of something else – a Community, God, The People, Humanity, Reason, some Categorical Imperative or other Higher Values. Whatever it is, there is a common denominator – people are obliged to behave irrationally from point of view of simple rational ethics, to do something stupid from point of view of their own interests.

I personally consider rational ethics as completely sufficient and do not see any reason for ethical systems beyond that, and suspect that the main interest in such ethical systems is a quite unethical one, in particular the justification of unethical institutions like states. But this would be worth to be considered in detail in another paper. Whatever the intentions of those who propose such irrational ethics, they would like to have a good justification for their proposals. The theory of an Almighty God, with ability and intention to punish us for our amoral behavior after death, was a nice tool. But with Enlightenment, the idea of such a God was no longer as popular as before among moral philosophers. Something else was necessary to replace it – a rational justification for the ethical, that means irrational, behavior. At best, an unquestionable one, which does not depend on any questionable axioms or assumptions – a *Letztbegründung*.

Now, Hoppe claims to have found this Holy Grail – at least this is suggested by the repeated use of attributes like “incontestable” or “indisputable”. And the aim of this section is not to question the motivation for this search, but to evaluate if he has really found it. So let’s see how it looks like.

**3.1. The basic idea.** The idea how to obtain a “*Letztbegründung*” – a final, incontestable proof, which does not depend on any contestable assumptions – Hoppe has taken from the “*Diskursethik*” (ethics of discourse) which has been proposed by Apel and Habermas (see for example [27]). Hoppe has correctly observed that what Apel and Habermas claim to prove – in particular, heavily social-democratic ideas about equality – does not follow at all. But, instead of recognizing that the whole approach is doomed to fail, he thinks that one can use the same (faulty) method to derive, instead, libertarian ethics.

This is not the only domain where one can apply the idea. One can try to apply it also in the domain of scientific methodology, to find a final proof of some basic principles, principles which cannot be derived from observation because they are necessary to interpret observations and to derive something from them. Here the similar “transcendental tradition goes back to Kant.

As I will try to show, the idea of a “*Letztbegründung*” is invalid in above cases. And it is not simply invalid, but even extremely faulty, so that it makes sense to ask why such a faulty idea is worth to be considered in detail at all. But philosophy is known to be extremely open to extremely faulty ideas, and if a faulty idea is worth to be explored in detail or not mainly depends not on its popularity. That argumentation ethics is extremely popular in the libertarian community is, therefore, sufficient justification to take it seriously.

Roughly speaking, argumentation ethics is the attempt to use the technique of performative contradictions considered in the last section – a useful technique for criticizing various philosophical theories – beyond its domain of applicability. This domain of applicability is the critique of certain, well-defined philosophical theories. If every attempt to justify argumentatively such a well-defined theory leads, necessarily, to a performative contradiction, this is a strong argument against this particular theory.

But argumentation ethics promises more – it promises to establish some positive truths, to prove some philosophical theories. And, moreover, it makes heavily exaggerated claims about the certainty of these theories, naming them incontestable.

**3.2. An example: The a priori of argumentation.** The following is a typical example:

Whether or not persons have any rights and, if so, which ones, can only be decided in the course of argumentation (propositional exchange). Justification – proof, conjecture, refutation – is argumentative justification. Anyone who denied this proposition would become involved in a performative contradiction because his denial would itself constitute an argument. Even an ethical relativist would have to accept this first proposition, which is referred to accordingly as the apriori of argumentation. [1]

At another place, the “a priori of argumentation” is defined as “humans are capable of argumentation and hence know the meaning of truth and validity” [2], and as the “proof” the following is presented:

The validity of the axiom, like that of the action axiom, is indisputable. It is impossible to deny that one can argue, as the very denial would itself be an argument. In fact, one could not even silently say to oneself “I cannot argue” without thereby contradicting oneself. One cannot argue that one cannot argue. Nor can one dispute knowing what it means to make a truth or validity claim without implicitly claiming the negation of this proposition to be true.[2]

Indisputable? I disagree. I can dispute the claim “humans are capable of argumentation” without contradicting myself. I can, for example, claim that some humans are not capable of argumentation. Or I can argue that the notion of argumentation is not sufficiently well-defined, so that the whole sentence does not have a sufficiently well-defined meaning, as required for a fundamental axiom of a scientific theory.

Of course, this is not the straightforward rejection of the type “I cannot argue”. But above examples clearly *dispute* the original claim. Thus, the original claim is *not indisputable* at all. And I see no reason to fear that by disputing them I risk a performative contradiction.

In the first case, I have made a claim about some humans, not about myself. So no contradiction with my behaviour is possible even in principle. One may argue that I have misinterpreted the claim itself, that it should be understood as “some humans are capable of argumentation” and not as “all humans are capable of argumentation”. But even if we accept this,<sup>3</sup> the point in question was a different one. I do not argue that all the hypotheses Hoppe claims to prove are wrong. I

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<sup>3</sup>There is, by the way, no reason to accept this. In mathematics, an expression like  $A(x)$  which depends on an unspecified variable  $x$  does not have any truth value at all. One needs a quantor:  $\forall xA(x)$  or  $\exists xA(x)$  or something else which specifies the meaning. What is the quantor for “humans”? “Some humans” or “all humans”? Or something else? Like “almost all humans”? The last one seems the most reasonable interpretation: Exceptions are possible, but only a few. But are those who cannot argue, in the sense that they think that giving names to the opponent and proposing hypotheses about the behaviour of his mother is argumentation, so that a reasonable, civilized argumentation with them is impossible, only a few? If not to mention those who think

argue only that they remain contestable. I have contested the claim, and there was not even a minor risk for a performative contradiction in this contest.

In the case of my second contest, one could at least try to construct a performative contradiction. Maybe I also use vague notions in my own argumentation? Possibly yes, but necessarily? I doubt. Moreover, I may have different requirements for certainty in axioms than in argumentation.<sup>4</sup> So I'm not afraid at all of a performative contradiction.

And, again, even if one succeeds, one would have at best closed one loophole in the proof. But my claim is that the "proof", as presented, has yet loopholes. If we find one and close it, the proof, and therefore the proven statement, was *disputable*, even if it has survived the particular dispute.

**3.3. The various possibilities of disputing a claim.** It would be an easy exercise to add more examples where the Letztbegründung fails. We will consider some of them below. But here I would like to proceed with the consideration of the general problems of this type of argumentation.

The first great problem of argumentation ethics is that performative contradiction – a reasonable method for *falsifying* particular theories – is used for something very different, namely to *prove* a particular theory. But how one can prove a theory, having only a method for falsification at hand? This would require the falsification of *all imaginable alternatives* of the given theory. But one cannot construct a performative contradiction between the theory and the behaviour of a defender of the theory if we know nothing about the theory in question, except that it somehow disagrees with the theory we want to prove.

But isn't it enough to negate the original proposition? Whatever the proposition  $A$ , we always have  $A \vee \neg A$ . Or  $A$  is true, or its logical negation  $\neg A$ . So what is the problem, if we find a performative contradiction in  $\neg A$ ? Isn't this an indisputable proof of  $A$ ? Unfortunately, not. There are two important problems hidden behind the denotation  $A$ : First, the interpretation of the meaning of  $A$ . Second, the very question if  $A$  has a meaning at all. So there remain some extremely important possibilities to question  $A$  even if  $\neg A$  seems to give a performative contradiction: To question the meaning of  $A$  in various ways.

**3.4. The theory-dependence of the meaning of words.** The point which is much more problematic for the whole idea of a Letztbegründung is the uncertainty about the meaning of the words used in the proposition.

Is it possible to leave this uncertainty out of the discussion? Certainly not. The truth value of "almost all humans are able to argue" depends on the meaning of "argumentation" and of "ability". Using a sufficiently restrictive notion of "ability" like "can do this whenever he decides to do this as long as he decides" instead of "sometimes he does it, even if only unintentionally, accidentally, and without distinguishing this from other behaviour", and a high culture notion of argumentation, which requires adequate, civilized behaviour, a high culture of discussion, the

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that hitting the face is an argument. This was, by the way, a third possibility to dispute the claim – it does not have a well-defined meaning because "humans" is too unspecific as a quantor.

<sup>4</sup>In fact, I really have them. Vague notions in an argument are unproblematic as long as above opponents don't question the meaning of the notion. So one is free to use them, but obliged to specify the meaning if the opponent objects. Instead, in an axiom vague notions have no place at all. Axioms are, in my understanding, to be left to mathematics and exact theories of natural sciences.

knowledge of typical argumentative fallacies and the ability to identify them, then the number of human beings who are able to argue unfortunately decreases to a small minority, so that the typical human being appears unable to argue.

This is a quite typical situation if words of common language are used. The problem is not that big in natural sciences. But it exists anyway. The meaning of the words which define chemical elements is well-defined by the current theory of chemistry. But what does this mean? It means, essentially, that the meaning of these words is defined by a complex theory which is in the background. A proposition using these notions obtains its meaning (and as a consequence its truth value) depending on this background theory.

The same holds for the meaning of words of everyday language: They depend on the background knowledge. The additional problem is that this background knowledge is very different for different people. Today the theory of chemistry is the same for all people, except may be some crackpots. The theory of meaning of “argumentation” is not. In a sufficiently specific background, “argumentation” can be used to describe street fight.

But even if we ignore the differences in the backgrounds of everyday language and restrict our consideration to scientific notions, the very problem remains: The meaning of a proposition depends on theories which define this meaning.

This is fatal for the attempt to make incontestable derivations. Once the very meaning of the proposition in question depends on background theories, one way to contest the proposition itself is to contest one of the involved background theories. We have usually not *specified* them if we write down a proposition. But we *necessarily have* them.

We have possibly even *different* background theories. In extremal cases, this can lead to the situation that the proposition is true for me but false for you, because we interpret the notions used in the proposition differently.

But this is not yet the very problem. The problem appears even if we all use the same background theories. And it is the large number of alternatives for disputing the proposition by disputing the background theories. If the background theory is disputed successfully and, as a consequence, replaced by another one, the truth value of the proposition *may* have changed. And this *possibility* alone is sufficient to reject the status of incontestability.

Or may have even *disappeared*, because one of the notions is no longer used in the modified background theory and the proposition is now meaningless. Therefore disputing one of the background theories is a valid method of disputing the proposition itself.

**3.5. No hope to restrict the number of background theories.** One can imagine a hope that it may be possible to restrict the number of theories of meaning which one has to consider. Last but not least, there are only a few words in each proposition. So we need only a few theories of meaning for them. Maybe it is possible to handle them all? At least in principle?

The first problem is that if we reduce the theory of meaning of a word to a few sentences of a definition, the problem reappears. The definition contains words, and these words have meanings, and these meanings may be contested as well. And so on. So the background is a more complex thing – it consists of rather complex theories.

Then, there is the even more problematic case where the “theory of meaning” of a word X is quite simple: “The word X is meaningless.” In itself, this seems to be a rather nonsensical way to dispute a claim. Confronted with such a claim, one would require further explanation. OK, no problem – the further explanation consists of a completely different background theory, a background theory which describes all the things we have previously described with propositions containing X in other ways, but without X, so that the word X is completely unnecessary and undefined in the new background theory. Modern chemistry does not assign a meaning to the word “phlogiston”, the theory of relativity does not define a meaning of the word “ether”, and praxeology does not assign a meaning to “utility of the whole society”.

Is presenting a new background theory, where X is meaningless, a way to contest, to dispute a proposition containing X? Certainly, yes. If somebody talks about “utility of the whole society”, we do dispute this with a reference to praxeology and that this notion is simply not defined there. And the very idea that this is not a valid objection against such propositions is funny.

But now, having this example in mind, let’s think about the possibility to “prove” a proposition by rejecting all legitimate ways to dispute the proposition. It means, we would have to reject all imaginable refutations of this proposition, that means also those which follow this scheme: To present a completely new background theory which does not define one or more words used in the proposition, so that the proposition becomes meaningless.

Up to now, we have had at least a hope – the alternative theories of meaning would have to give the words of the proposition a meaning. But now?

**3.6. The theory-dependence of the interpretation of behaviour.** And even this is not all. In the attempt to prove a proposition by constructing a performative contradiction in every alternative we have not only to care about the theory-dependence of the meaning of the words. We also have to care about the theory-dependence of the behaviour of the defender of the theory.

I have already mentioned Feyerabend’s relativism, which is a nice example. What one can see is some seriously-looking guy proposing sentences which sound like serious arguments. This may be interpreted as a really serious guy who really proposes arguments. But this may be, as well, interpreted as a guy making fun out of his opponents, who only pretends to be serious, but, in fact, has a lot of fun looking at the stupid guys who take him seriously. These are different interpretations of the same behaviour – interpretations in the light of different theories about the aims and intentions of the guy, which remain invisible.

It should be noted that these theories are of a completely different nature. The proposition consists of a few words, and the background theory has to give these words a meaning. But now we have to interpret some behaviour. To describe this behaviour, we are in no way restricted to the use of these words. We can use completely different theories, using completely different words.

Then, this is what we have to meet if we consider a particular real life situation and want to use a simple ad hominem. But the performative contradiction has to appear in every imaginable situation. So we have to interpret not only a single discussion, but very different ones.

And, the other complexity, we have to use the interpretation which corresponds to the theory we want to reject. In fact, it should be a contradiction between the theory proposed by the defender and the behaviour of the defender. This is

meaningful as an argument only if we find an internal contradiction, that means, a contradiction between the theory and the behaviour of the defender interpreted in the light of his own theory.

Indeed, a contradiction between the theory I defend and my behaviour in defending it, interpreted from point of view of your theory, is not impressive at all. Are you impressed if a Marxist tells you that your argumentation against Marxism leads to a performative contradiction, because it follows from your behaviour – proposing anti-Marxist positions – that you lie and distort the truth to defend your class interests?

But, in the case considered in the last section, we have no information at all about the theory in question, except that one of the words used in the proposition is meaningless in this theory. So we also have no information at all about how to interpret the behaviour of the defender. Except for a quite minimal one – that we should not use all the words used in the proposition, because at least one of these words is meaningless. I doubt this information is very helpful.

**3.7. The logical asymmetry between ethical restrictions and their negation.** We have already observed that contesting a truth claim can be done in quite unexpected ways, related with the dependency of the very meaning of the claim on various background theories. This dependency on background theories is an effect which is symmetrical with regard to negation – it holds in the same way for the claim itself as for its negation.

But in the case where we want to derive something nontrivial about ethical theory using the method of performative contradictions, we observe yet another problem which does not have this negational symmetry.

In fact, what can be done with the method of performative contradictions is to *falsify* a particular ethical *restriction*: The performance of defending the restriction in an argumentation may, necessarily, violate this very restriction. What follows? It is the *negation* of the particular restriction. But the negation of a restriction is not a restriction. It does not prescribe any sort of behaviour. It simply tells us that the particular restriction does not exist, that it is not an ethical rule. The negation of “you should not do X” is *not* “you should do X”. It simply tells us that doing X in itself does not violate any ethical rule. It is something I will name here an allowance:<sup>5</sup> You are allowed to do X. But you are in no way obliged to do X.

This logical asymmetry between ethical restrictions and allowances is fatal for the aim of deriving ethical restrictions from performative contradictions. A performative contradiction is possible and easily imaginable for restrictions – it may be that the defender of the restriction has to violate, necessarily, the very restriction he wants to defend. But this proves only the negation of the restriction – the corresponding allowance. To derive a restriction in this way, we would need a performative contradiction for an allowance. But this is impossible: Whatever the defender of the allowance does, it does not violate the allowance, because the allowance to do X *does not restrict* behaviour: The defender can do X, but is not

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<sup>5</sup>Even naming it “allowance” seems too strong: One could think that such an allowance modifies other restrictions. Say, a restriction to do Y would be irrelevant as long as you do X. But there is not even such a consequence. The negation of the restriction to do X tells nothing about restrictions to do Y.

obliged to do X. So, nor doing X, nor not doing X leads to a performative contradiction. So what could be a performative contradiction with an allowance? There is no such thing.

This point is central. So, in a defense of argumentation ethics, Eabrasu argues:

If performative contradiction is a valid argument, then it shows that it is self-contradictory (and illogical) to establish a norm allowing people to bash someone else on the head. [22]

But arguing for a “norm allowing” something simply cannot lead to performative contradictions. If something is allowed, I can do it or leave it, this is my free choice. So if I do not bash you on the head during the discussion, I do not violate my own proposal. Performative contradiction can be used to reject proposals for normative restrictions and obligations. That’s all.

**3.8. How does argumentation ethics work?** Given the results of the previous sections, one asks himself how it is possible that argumentation ethics seems to work at all.

The main point is of course that the most serious problems – the theory-dependence of the meaning of words, as well as the theory-dependence of the interpretation of observations – are usually ignored. They are part of critical rationalism, and recognized by some more philosophers, but this remains to be a quite small community.

But this seems strange – in fact theory-dependency we all meet in everyday discussions all the time. I think everybody has been confronted with misunderstandings of his claims based on different theories of meaning of the words used there, and was forced to object against occurrences of misinterpretation of his own behaviour based on unfavorable theories about his intentions. So why it remains hidden and unacknowledged in a large part of philosophy?

I think the key is the false hope for certainty. We recognize, more or less, that we live in an uncertain world. But we want to have at least a little bit certainty. And here it is, this little bit of certainty – argumentation ethics. We really want to have it, and we are therefore ready to ignore the quite obvious obstacles. I think even the defenders realize that argumentation ethics is at best an idealization, that it cannot be realized. But should this prevent us to construct, at first, the ideal, and then, later, to see how much of it can be realized? So we are ready to ignore the theory-dependence of the meaning of the words and of the interpretation as parts of our dirty, non-ideal reality in our construction of the ideal world of certainty.

But if we, because of wishful thinking, are ready to ignore all the difficulties, what remains is sufficiently simple: All we have to do to prove the obligation to do *A* is to reject its negation. As the negation of the obligation to do *A* we use, instead of the allowance to do *A*, the obligation not to do *A*. This is something which at least can lead to a performative contradiction. Moreover, it is usually a sufficiently nonsensical rule, so that nobody likes to defend it anyway. Complications related with possibly different meanings are ignored, but so what.<sup>6</sup> And we have the behaviour of the defender. This also can be interpreted in simple terms – in the same terms used in *A*. That other interpretations are possible we ignore. It remains

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<sup>6</sup>I think, another point plays a role here too: Self-contradictions are not the most trivial and most common logical constructions and confusing for the inexperienced. So he has to focus his interest on the logical part. With this focus, the other, problematic parts like the uncertainties of the meaning of words will be easily ignored.

to evaluate the behaviour only from the point of view of the following question: Does it correspond to the obligation to do  $A$  or to the obligation not to do  $A$ ? That's all. In above cases, we have "proven" the other obligation in an "incontestable" way.

Possibly a large role is played here by the fact that the notions used here are words of everyday language. So it can be implicitly assumed that we all know their meaning. Moreover, we all hate disruptive requests from nitpickers asking for definitions of every word. This natural emotion prevents us from questioning the meaning, as well as from considering such questioning as a valid way of contesting a theory.

Then, the performative contradiction counts only if every imaginable defender, in every imaginable attempt to defend it ends in such a contradiction. So we do not have to consider real defenders, but some abstract, purely theoretical defenders. Therefore the problem of interpreting real behaviour is automatically hidden. And we tend to describe the behaviour of this abstract, theoretical defender in the same words, assuming the same meaning, as used in the proposition in question.

The very possibility of a word not having a meaning (as phlogiston) is, of course, almost automatically ignored if we use words of common language which have a meaning (even if only an uncertain one). So, it is quite natural that arguments related with the uncertainty of meaning will be ignored.

It is much harder to understand the confusion between the negation of an ethical restriction ("you should not do  $X$ " is false) and the ethical obligation ("you should do  $X$ "). This is a quite trivial and obvious confusion, and every libertarian who has argued against various prohibitions not because he likes drugs, tobacco, pornography, prostitution and alcohol, but because he likes freedom, is aware of this difference.

Here, I have no explanation. But I have the very interesting personal observation that I have recognized this simple and strong argument only after the first version of this essay was finished and published on my web-page. Philosophy seems to be a domain where it is easy to make horrible errors and extremely hard to recognize them.

#### 4. EXAMPLES

Given the conceptual rejection of the method of argumentation ethics, is it worth to consider further details? I think it is. They show, in fact, other points which are interesting from libertarian point of view.

**4.1. Norms for argumentation or battlefield of ideas?** Are there norms for argumentation, and if there are, which? Hoppe uses the following argument:

Arguing does not consist of free-floating propositions. It is an activity. If aside from whatever is said in its course, however, argumentation is also a practical affair and if argumentation is the presupposition of truth-claiming and possibly true propositions, then it follows that intersubjectively meaningful norms must exist – namely those which make an action argumentation – which must have a special cognitive status in that they are the practical preconditions of truth. Once more, this is true a priori, so that anyone, such as an empiricist-positivist-emotivist who denied the possibility of a rational ethics and who declared the acceptance or rejection of

norms an arbitrary affair would invariably get caught in a practical contradiction. For contrary to what he would say, he would in fact have to presuppose the norms which underlie any argumentation whatsoever as valid simply in order to say anything at all.[2]

What shows up here is the ugly face of the consent theory of truth: Norms for argumentation as a precondition of truth.

How to make such a theory compatible with the way how children learn to argue is beyond my understanding. We have, first, to be able to distinguish truth from falsity. Only with this presupposition we can learn to speak at all. In fact, how a child learns to speak? It observes our verbal behaviour and develops theories about the meaning of the different words. We try to help them as we can. But what we can do is not that much: We show a green ball and say “ball”. Or, sometimes, “green”. A single occurrence of this is certainly not sufficient to clarify the meaning of these words, nor of “ball”, nor of “green”. The child may end up calling a red ball “green” and a green book “ball”. It has to recognize itself that these theories of meaning are false, and to correct them. And it recognizes this, not because it reaches a consent in an intersubjective discussion, but because this theory does not correspond to reality: The other people do not give him the green book it likes if it asks for “ball” or the red ball if it asks for “green”.

But if we exclude argumentation by crying, knowledge of language is a presupposition for argumentation, and, therefore, the ability to distinguish truth from falsity, as a presupposition for learning language, is a presupposition for argumentation too. Thus, the situation is the reverse one. But one needs, of course, the correspondence theory of truth to recognize this.

The other question raised by this quote is if there are norms of argumentation. I will use this as an example of contesting the proposition by presenting an alternative theory such that the notion “ethical norms of argumentation” becomes meaningless.

Let’s consider the process of learning how to argue. It seems quite obvious that argumentation develops as a method of manipulation of others. A baby uttering “ball” wants to manipulate the parents to give him the ball. Then it improves his techniques for manipulation, for example by using full sentences better understood by the parents. Other examples are insisting if the requests are rejected. Initially simply by repeating. Then it learns to offer something in exchange or to promise something. It is not truth which it seeks initially, but to make others behave as it wants. The aim of argumentation is to win.

Truth becomes important once the child learns that it is sometimes useful to lie, and that other may hurt its interest by lying. Then it starts to learn the rules of another game of manipulation – the game of detecting and hiding lies. It learns this game by doing. Partially in the role of a truth seeker – if it wants to identify if somebody else is lying. Often enough also in the role of the liar, with the aim to hide the truth. And the first strategic lesson in this role is that it has to pretend to search for the truth – even if it is really lying.

The ideal argumentative discussion, with the aim to establish truth, is only a variant of this battle. A variant where the liar simply does not participate. But the shared interest of the truth seekers remains the same – to establish the truth. The role of the liar is taken over by the erroneous ideas which the liar could have distributed to confuse us. But they appear even without intentional liars. And we

have to think about the same things we have to think in the battle against the liar. Even in details.

In battles of every type there always are “rules” of rational behaviour. If one violates them, one will lose. These strategic rules have characteristics which are quite different from ethical rules. Indeed, consider the strategic rules in a chess game: If you violate them, you will lose the game, but you will not be morally condemned for this. At worst, you will be considered as stupid. And the great chess master is the guy who invents a new strategy, a new opening move, in other words, the one who violates the old rules but wins. So, violation of strategic rules does not lead to moral rejection or excommunication. In this sense, the strategic rules of the battlefield are very different in their nature from ethical rules, where the violator is despised, condemned and rejected as anti-social.

So, even if we cooperate with each other in argumentation, with the shared aim to find the truth, the rules of this activity are strategic rules, rules of the battlefield, where everything is justified if it helps to win – that means, to find the truth. To make this common fight for truth a more pleasant experience for all of the participants, it is, of course, useful to accept, beyond these rules of fight, some social, cultural norms of civilized behaviour. This is especially necessary because we have learned all our techniques in the fight with despised enemies, with liars. So we have a strong natural tendency to attack those who defend wrong ideas as if they were despised enemies. But these social norms of cultural behaviour during a discussion are not the rules of argumentation. One has to distinguish these two types of rules. “Oh, this son of a b... is so f... stupid that he thinks an *ad hominem* is a decisive argument” is – as an *argument* – as valid as “Dear friend, I think you recognize very well that this argument is *ad hominem* and therefore not decisive”. So the ethical rules of politeness have a different character and are, therefore, irrelevant, not presuppositions for argumentation.

One may object that there are, nonetheless, ethical norms too – the norms against liars, against deliberate distortions of the truth. These are, without question, ethical norms. Nonetheless, they are not prerequisites for argumentation: One certainly can (and often has to) argue with liars. Even more: The liar, even if he violates these ethical rules, does not violate the rules which make an argumentation an argumentation – his lies have the same form as truths, his distortions the same form as undistorted truths. And even if he intentionally hides some good counter-arguments, the arguments of the liar remain valid arguments. Nor the victim of the liar, nor an external observer are able, without additional background knowledge (or without seeing the faces in bad movies, where it is always possible to distinguish the bad guy from the good guy without even knowing the language) to distinguish this exchange from a honest argumentation. Else, the lies could not reach their aim, even if believed. So he would not only be a liar, but also a stupid liar. That means, even an argumentation distorted by lies and manipulations remains an argumentation. The ethical norms of honesty should therefore not be classified as prerequisites of argumentation.

Therefore I reject the very notion of “ethical norms of argumentation” as a prerequisite for argumentation. While there are ethical, cultural norms of polite, civilized behaviour in argumentations, as well as norms of honesty which forbid lies and distortions, these norms are in no way prerequisites for argumentation, because they are not what distinguishes meaningless babble from argumentation. It is possible

to argue in a valid way even if one violates the cultural norms of politeness. And for liars it is even a necessity to camouflage their lies and distortions in such a way that all the *necessary prerequisites* of an argumentation are present.

Instead, the rules of argumentation itself, the rules which distinguish well-formed arguments from meaningless babble, do not have the character of ethical norms. They are better understood as rational strategies in the search for truth.

A particular consequence of the strategic character of the rules of argumentation is that they are different for different people. Indeed, every fighter has to choose his own strategy, in dependence of his own abilities and his own preferences. So there is no single, once and forever established set of rules. As a particular example, consider the argumentative value of a reference to authority. It is well-known that there are large differences between people here: There are people who accept such references as decisive arguments. Others ignore them completely, or at least pretend to. From the point of view of strategic rules, depending from the own abilities and interests, this is quite natural: If you are a specialist in the field in question yourself, you will prefer to ignore it and to evaluate the arguments of the authority in their own value. If not, and if learning the relevant facts about the field takes a long time, it is more reasonable to accept the position of an established authority without questioning it. Then, it depends on your interests – how much the argument hurts you. If it is sufficiently irrelevant, one can believe the authority. If it really hurts, it may be worth to spend the time to become a specialist in the field yourself.

A distinguishing feature of rational, strategic rules is the possibility of their improvement. This seems to look like a counterargument: The basic rules of elementary logic have been well-known already to the old Greeks. But, nonetheless, there is progress in this domain too. The mathematical formalization of logic has been done only in the last centuries. And as a particular and very interesting example I would like to refer here to the insight that the logic of consistent plausible reasoning is described by the mathematics of probability theory. This is a quite recent insight, and not accepted by many (all those who follow the frequentist interpretation of probability theory) even today. For the insight itself I recommend to read Jaynes [29].

So I think I have made a sufficiently good case in favour of an alternative theory, a theory where the notion “ethical norms of argumentation” (as different from the ethical norms of polite behaviour and of honesty) is meaningless, so that the original claim, despite its “indisputable” proof by argumentation ethics, is successfully disputed. Do I have to be afraid of a performative contradiction? I don’t see why. Of course, by presenting my case, I have not violated the ethical norms of civilized behaviour, but these are not rejected in my theory as well. And I have not violated nor elementary logic, nor more sophisticated rules of argumentation – or at least I hope so. But in my theory such rules exist too – as rules of rational strategy – and part of my theory is that it would be stupid to violate them. Maybe I have followed some rules I don’t know about, which are not part of my theory. Even in this case I see no contradiction, because I nowhere propose the rule “don’t follow any other rules”.

One can interpret my argumentative behaviour in the light of some other theory, as a behaviour which does (not) violate the ethical norms of argumentation, however defined. But this would be irrelevant, because it is not part of my theory that these other norms should (not) be violated. These norms simply are not part of my theory,

and so I'm allowed to violate as well as to follow them without any contradiction with my theory.

So I'm not afraid of a performative contradiction. And I challenge the defenders of the original thesis to find such an unavoidable contradiction. Of course, if you find avoidable ones, this will be appreciated too – it would allow me to improve my presentation.

**4.2. Different purposes of different participants.** But even if one rejects the argument against ethical norms of argumentation as presented above, it does not follow at all that there is a single set of ethical norm of argumentation which has to be presupposed.

The following argument presented by van Dun in defense of such norms fails too:

Clearly, engaging in argumentation entails a commitment to abide by a number of norms, because any violation of or departure from these norms impairs and possibly even destroys the purpose of argumentation itself. [19]

My theory about the strategic character of the rules of argumentation clearly meets this objection. Another problem with this argument is that there may be no single or shared purpose of an argumentation. The single purpose – to establish truth – is in fact only an idealization. Even in natural sciences truth is not, at least not necessarily, the final aim. There are, in particular, various possibilities for technical applications, and the hope for such technical applications is quite important as a motivation for scientific research. Even more obvious are the differences of the interests in ethical discussions. Here, a quite typical aim is that the other participants accept the moral norms one prefers.

There are also other aims: For the teacher there is the aim to make money by teaching. Then there is competition in the field of argumentation, the wish to present the own behaviour in a favorable light, or simply to have fun. As well one may ask for help, or, on the other hand, one may like to help somebody else. All these different aims do not contradict each other. Even in the same discussion different participants may have different aims. This may be a cause of conflict, but such a conflict is in no way a necessity.

**4.3. About seriousness.** The last observation allows to contest the following thesis “proven” by van Dun:

A [performative]<sup>7</sup> contradiction emerges when someone states: You ought to take seriously the argument that you ought not to take argumentation seriously. ... Thus, when the claim is made that one ought not to take argumentation seriously and this claim is presented not as a joke but as a serious proposition for argumentation then the opposite norm, “One ought to take argumentation seriously,” is in any case simultaneously posited or presupposed as valid and binding, and is, moreover, argumentatively or dialectically irrefutable. [19]

First of all, this is a nice example of a confusion of the negation of the norm “you are obliged to take argumentation seriously” (which is the allowance “you are not

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<sup>7</sup>Here van Dun uses “dialectical” instead of “performative” – a choice I personally do not like because of memories of Marxist indoctrination.

obliged not to take argumentation seriously”), with another norm, the “opposite” norm “you are obliged not to take argumentation seriously”, a confusion which has been discussed in section 3.7.

But this is also an example where the claimed principle itself can be even seriously questioned. The point is that even if one does not take an argumentation seriously, one can make arguments which deserve to be taken seriously. This is, in particular, the case for the various arguments proposed by Feyerabend. Should we ignore his arguments simply because he does not pretend to take them seriously himself? I don’t think so. In the case of Feyerabend I finally reject them, but not without taking them seriously, that means, without identifying appropriate counterarguments. An argument is an argument, and if it is a serious one does not depend on the intentions of the proponent, but on its argumentative power. In fact, presenting serious arguments in the form of jokes has even some advantages – reading them becomes a more pleasant experience, and remembering the argument becomes easier too.

As a consequence, the moral rule “One ought to take argumentation seriously” would lead to adverse consequences – people like Feyerabend would not participate in discussions, so that their arguments would be lost, or they would present their arguments in a much more boring way. This would be a serious loss.

So, I think, I have made an argument against the moral rule “One ought to take argumentation seriously” which deserves to be taken seriously. Is there any danger of a performative contradiction in my own argumentation? In no way. The point is that I argue *against* a moral restriction, not in favour of one. What I defend is an allowance: Everybody is welcome to argue – those who take argumentation seriously, those who prefer to make jokes, as well as those who sometimes argue seriously but sometimes joke. So it doesn’t even matter how one classifies my argument: as a serious argument or as a bad joke. There is no performative contradiction in above cases.

**4.4. Letztbegründung in Austrian economics.** For the libertarian reader educated in Austrian economics this rejection of Hoppe’s Letztbegründung of ethics raises the natural question: What about the foundations of Austrian economics, of Misesian praxeology? If one reads Hoppe about economics, one also finds expressions like “incontestable axiom of action” [6], suggesting some Letztbegründung of this axiom too.

In this case, my personal relation is a different one than in the previous example. I like Mises’ “Human action” very much, consider it as one of the best books on economics ever written, and have not found very much to object.<sup>8</sup> The target is therefore only the “indisputable” proof itself.

Let’s consider the particular example the “axiom of action”. Hoppe names it an

“incontestable proposition... (such as that man acts and knows what it means to act)”. [5]

and thinks one can prove this:

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<sup>8</sup>There is, of course, of Mises’ defense of a minimal state. I’m also critical about the frequentist interpretation of probability theory and the rejection of cardinal utilities. In general, I also reject the much too rigorous rejection of alternative methods, in particular of application of mathematics, and the role of empirical predictions and observation. These are unnecessary restrictions, but they do not make the results of praxeology invalid.

... the knowledge of what it means to act must be considered knowledge about reality which is a priori. The very possession of it could not be undone or disproved, since this would already presuppose its very existence. [7]

But to dispute it is an easy exercise: For this, it is sufficient to consider in a little bit more detail the meaning of “action” in Misesian praxeology. One will find, in particular, that Mises has written quite a lot of text to define this meaning – a whole introductory section. This seems a lot of work to describe something everybody already knows a priori in an incontestable way. But would you suggest that it should be simply omitted? I wouldn’t. In particular, I have found there the following: “The vigorous man industriously striving for the improvement of his condition acts neither more nor less than the lethargic man who sluggishly takes things as they come. For to do nothing and to be idle are also action, they too determine the course of events.” That’s fine, a remarkable idea.<sup>9</sup> But this is not self-evident, but a remarkable and counter-intuitive theoretical invention. For the common man, action means something different, and certainly does not include “doing nothing”. So men, at least those who have not read “Human action”, don’t know what it means to act in the Misesian meaning of the word.

Hair splitting? Of course, but incontestable propositions ask for this. Then, the point of this example is that not only in mathematics and natural sciences, but also in praxeology scientific notions differ from common sense notions. In particular, they have much more certain definitions, and even if a word from common language is used, it is often used there in a different, more specific way. But the trick of *Letztbegründung* works only if we forget about different meanings, which works much better in common language where we usually don’t care much about this, at least much less than in a scientific context.

If one ignores the possibility of modifications of the meaning, one can easily end up in ridiculous absurdities – all one needs is a sufficiently rigorous modification of the meaning. I would propose to apply this in the case of the “axiom of action” (that man acts and knows what it means to act) with the restricted meaning of “to act” as denoting only the sexual act, or, even more specific, the sexual intercourse between a man and a women. If, now, the “axiom of action” is indisputable, what about homosexuals? Maybe they are not men? Maybe they are animals, so that one can kill them?

Ridiculous, of course. But if one ignores the possibility of different meanings, they tend to come in from the backdoor, as hidden shifts in the meaning. Scientific precision in the specification of the meaning of scientific terms is the very method to avoid this danger. And even in humanities, where the scientific definitions are not (and should not be) as precise as in mathematics or mathematical physics, this danger is at least minimized by such detailed introductions into the notions as that given by Mises for “action” in *Human action*.

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<sup>9</sup>Educated as a mathematician, I highly value the idea to incorporate trivial cases as regular into the definition. So zero becomes a number, the empty set becomes a set, the zero-dimensional space (a single point) becomes a linear space. The great advantage of this counter-intuitive idea is that we have to make much less exceptions for these exceptional cases in our theorems and proves. Mises applies this idea in the definition of action, defining action in such a way that the trivial action – doing nothing – becomes by definition an action too.

**4.5. An attempt to defend argumentation ethics.** That theories of meaning are important has been recognized by some defenders of argumentation ethics, for example by Eabrasu:

Besides the debates on self-ownership definition, it is important to note that Callahan and Murphy’s critique does not affect the success of the argument by performative contradiction in justifying the self-ownership axiom. Their critique deals only with the definition of self-ownership. For example, disputing the fact that the owner should use fences for delimiting her land does not specify who should own the respective land. There are two different matters: defining the self-ownership and justifying the self-ownership axiom. Even though they are both essential features of the libertarianism, they are entirely independent one from another. [22]

This is a quite strange position, and I’m unable to make much sense of it. Without a specific definition, the notion of self-ownership is simply meaningless, undefined. And, as a consequence, we cannot prove anything about it. We can of course make a difference between definition and the argument, but the definition should be fixed before we can decide if the argument works.

Else, the example above of the various definitions of action provides a nice counterexample. It clearly depends on the definition of action if there appears a performative contradiction. Using the meaning “coitus” of action, no performative contradiction is possible, because this notion of action is completely unrelated to argumentative behaviour. With other definitions of action, performative contradictions become at least possible.

**4.6. The argument for the Golden Rule.** This identifies yet another potential source of error: A shift in the meaning after the proof. An example of such a shift in the meaning is the argumentation in favour of a “universalization principle”. Indeed, let’s look at the first part of the argument:

But what are the strictures of the ethics-implied-in-argumentation whose validity cannot be disputed because disputing it would implicitly presuppose it? Quite normally it has been observed that argumentation implies that a proposition claims universal acceptability . . . Indeed as it is implied in argumentation that everyone who can understand an argument must in principle be able to be convinced by it simply because of its argumentative force, . . . [2]

This is, indeed, an interesting point in favour of some “principle of universality”: A valid argument should be accepted, as an argument, by everybody who is able to understand it. And, as a consequence, if the argument proves a thesis, the thesis should be accepted by everybody. And at least some part of argumentation – strong logical arguments – really have this character. And even weak arguments, like appeal to authority, ad hominem, or plausible reasoning, usually have such a universal character: Even if one is attacked ad hominem as being prejudiced because of a personal interest, one has to accept that this may indeed increase the probability to err in favour of the personal interest – the tendency to err in this way is a general human weakness.

Moreover, this is also quite natural. Truth is truth, correspondence to reality, and if a claim corresponds to reality, it corresponds to reality for everybody. It is this

fact which makes intersubjective cooperation such a powerful way of establishing scientific truth – the correct truth value is the same for everybody, but everybody observes something very different, thus, receives different information about this. So, even if I would (out of principle) refuse to assign to this principle the status of indisputability, I would not attempt to dispute it.

And now about something completely different. Let's take a look at the completion of the quote:

...or should it be a norm proposal, that it be "universalizable." Applied to norm proposals, this is the idea, as formulated in the Golden Rule of ethics or in the Kantian Categorical Imperative, that only those norms can be justified that can be formulated as general principles which without exception are valid for everyone. Indeed as it is implied in argumentation that everyone who can understand an argument must in principle be able to be convinced by it simply because of its argumentative force, the universalization principle of ethics can now be understood and explained as implied in the wider a priori of communication and argumentation. [2]

The Golden Rule is certainly also an important principle, even a very important one. But, sorry, what does it have to do with the universality principle in argumentation we have considered above?

This fundamental difference between the two becomes obvious in the following example. Let's imagine an argument proposed by a slave-owner that he is not morally obliged to give the slave freedom: "I have bought the slave in a honest way on the slave market, paid for him with my own, hard-earned money, and I don't want to throw my property away for nothing, and not obliged to." The appeal of the *argument* is clearly universal: It appeals to universal property rights, and therefore has appeal even to the slave – he would also not like to give away his property, even if, at the current moment, he owns nothing but is owned. In this sense, it is in full agreement with the universality principle of argumentation: It claims universal acceptability. But it is, on the other hand, in obvious disagreement with the Golden Rule. Which does not make it self-contradicting or so – the two "universality principles" are simply very different principles.

Rereading the quote, one observes that the difference between the two principles is not denied: It is talked explicitly about an idea, applied to norm proposals. So, one can think about this in such a way: Argumentation ethics "proves" the universality principle in argumentation – the claim of universal acceptability. From this follows an undefined "idea of universalization", and this idea we simply have to apply to ethics. But, sorry, the vague idea of universalization is, then, not proven at all. It is, at best, a generalization of something which has been "proven".

**4.7. The derivation of self-ownership.** Let's continue with the Letztbegründung of self-ownership:

...right to property in ones own body and its standing room must be considered apriori (or indisputably) justified by proponent and opponent alike. Anyone who claimed any proposition as valid vis-a-vis an opponent would already presuppose his and his opponent's

exclusive control over their respective body and standing room simply in order to say “I claim such and such to be true, and I challenge you to prove me wrong.” [1]

Simply in order to say something I have to presuppose almost nothing. Control over my standing room is certainly not necessary. All that is necessary is to be somewhere. That may be a cell in a jail. My body may be completely fixed, leaving minimal freedom only for my mouth to speak. My opponent may be fixed in a similar way close enough to hear me. What else? So, this type of presuppositions is so minimal that it is much less than the amount of self-control usually left to slaves. In fact, to fix slaves in such a way would make them completely useless for the owner, except for the purpose of sadistic games.

So a theory which *necessarily* leads to a performative contradiction for every imaginable defender for this reason has to be a very strange one. (Remember that, else, it is not decisive as an argument against the theory, but only an ad hominem against a particular defender.) Say, the theory “humans have to remain silent all the time” or so.<sup>10</sup> But what else? All imaginable alternatives to the libertarian ideal of complete self-ownership? That’s simply laughable, sorry.

Murphy and Callahan [20] have already criticized in particular this derivation. One argument is that a slave-owner can allow a slave to participate in a discussion. In particular this illustrates the confusion of use and ownership: The participant in a discussion uses his body, but need not own it. Let’s consider some responses which illustrate some of the errors behind.

Eabrasu objects:

The difference consists on the fact that contrary to the ownership on land, the ownership on the body cannot be denied or abandoned. It is conceivable that a person does not own a piece of land. But it is inconceivable that a person does not own herself. By definition, self-ownership can be withdrawn only by canceling the agent’s intentionality (free-will and conscience), i.e., by transforming her into a zombie or robot. [22]

But if we *define* self-ownership in such a way, then self-ownership is simply a tautology without any non-trivial ethical content, another word for intentionality. In this meaning of the word, fine, self-ownership cannot be consistently denied in an argumentation. But, on the other hand, what follows? Nothing. Self-ownership in *this* meaning of the word is compatible with slavery, taxation, mandatory military service and all the other etatist restrictions of self-ownership in the usual libertarian meaning, so there would be no point proving it.

Now we can see better that the only way to assert non-contradictory that “it is moral to use force to compel a healthy person to give up one of his kidneys” . . . is to use a very restrictive definition of self-ownership (which excludes kidneys). But even in this case one may say that kidneys are homesteaded by the self-owner.[22]

Unfortunately the case for homesteading is even weaker, so this does not save the game too.

As it can clearly be observed, Hoppe’s definition of self-ownership refers to the body as a whole without excluding specific parties of

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<sup>10</sup>And even this only if we ignore the possibility of aliens or artificial intelligence.

the body. Any leaving being capable of argumentation is treated as a single unit. Therefore, even if the kidneys or the legs are not required in argumentation they are nonetheless included in Hoppe's definition of self-ownership. [22]

That's fine, and Hoppe has obviously the right to define his own notion of self-ownership and to use it. At least as long as he does not make claims that *every* other theory leads to contradictions. If he makes them, he has (somehow, I don't know how, and it is not my problem) to prove the existence of a contradiction in all alternative theories. This includes theories based on different notions of self-ownership too. So we cannot restrict ourself to Hoppe's definition. Eabrasu tries this too:

To be sure, there is no logical bond between the definition of self-ownership and the justification of the axiom of self-ownership. On the one hand this type of critique has no effect at all on the justification of the self-ownership axiom using the argument from performative contradiction. [22]

But how this is supposed to work? An argument which shows a contradiction using a notion which is not yet defined? Sorry, this doesn't work. A definition of the meaning is *presupposed* by any meaningful sentence or argument using a given notion. Fortunately explicit definitions are not a necessity in philosophy, because, different from mathematics, common sense supports us with such meanings. But this does not change their logical connection. The job which has to be done – to find a performative contradiction in *every* alternative theory – simply cannot be done. It would have to cover *every* alternative meaning of the words too.

In another defense of argumentation ethics, Heinrich objects:

It merely shows that individuals can be inconsistent in their application of normative ethical principles. By allowing the slave to debate freely and make up his own mind as to the validity of arguments given during discourse, the slave-owner recognizes the slave's right to self-ownership implicitly. He does not merely recognize that the slave possesses his own body – that only the slave can control his body in the peculiar and direct way that a self-owner can control his own body – but also doesn't in any way attempt to control the slave's body during debate. [21]

But there is no inconsistency: There is a right, but no obligation, to control the own property. Then, I can allow other people to use my property without giving up my property rights or implicitly acknowledging property rights of other people.

If after the debate, the slave-owner says, "Ok, now get back to picking my cotton, or I'll whip you," that does not show that discourse ethics is invalid. Such is not an argumentative justification for the initiation of aggression. [21]

But the question is not if there exists a valid argumentative justification for the initiation of aggression or for slavery. The question is if there exists, necessarily, a performative contradiction. Now, having the slave as his property does not lead to any moral obligations of the slave-owner – according to the ethical theory of slavery, he can do what he likes with his property, from killing to giving him complete freedom.

Last but not least, let's note that there are large parts in the literature defending argumentation ethics which I'm unable to understand – I'm unable to see how the particular claim follows. The only thing which comes into mind in reading them is the repetition of the simple question “why” after almost every sentence (or even part of a sentence).

**4.8. The derivation of property rights.** Let's continue with property rights:

Furthermore, it would be equally impossible to engage in argumentation and rely on the propositional force of one's arguments if one were not allowed to own (exclusively control) other scarce means (besides one's body and its standing room). If one did not have such a right, then we would all immediately perish and the problem of justifying rules – as well as any other human problem – would simply not exist. Hence, by virtue of the fact of being alive property rights to other things must be presupposed as valid, too. No one who is alive can possibly argue otherwise.<sup>[1]</sup>

This is not better. Slaves do not own anything, but they live nonetheless. Slave-owners have to give them something to eat if they want to use them in future for work. But this does not mean ownership for the slaves: If the slave does not eat what has been given to him, in the hope to exchange it later for something else, the slave-owner can take the uneaten portion away. It remains in his ownership, even if he allows the slave to eat it, in the same way as the uneaten food he gives to his animals remains his property. So something to eat and to survive I can have also without any ownership. Even worse, if the slave rejects eating, preferring death to slavery, the owner may force him to eat.

Here, again, these possibilities are hidden behind a chain of plausible simplifications: He is alive, so he has to have eaten, people usually eat what they own, so he has probably been the owner of something to eat. So ownership becomes a prerequisite for live.

But ownership is not a prerequisite for being alive.

At least not the absolute form of ownership used in libertarian ethics. Of course, there may be a notion of de-facto ownership applicable to almost every society, which rejects the official notions of ownership of a given society and introduces a notion of ownership to those who make the real decisions. But this concept of ownership, even if it is useful in the fight against communistic propaganda lies about “peoples ownership”, leads to a quite different, insecure notion of ownership: The “owner” (in this sense) of a firm in Stalin's Soviet Union could become, without any volitional exchange, the “owner” of a completely different firm, or even a Gulag slave, tomorrow. This notion of ownership has not much to do with ownership in libertarian ethics, which is created by original appropriation of unowned things and may be changed only by volitional decision of the owner.

But, again, the plausibility of the whole method requires ignorance of possible differences in the meaning.

**4.9. Estoppel.** Kinsella [11] identifies the legal principle of estoppel – that claims which are in contradiction with past behaviour should be rejected – as a variant of a performative contradiction, and, then, proposes a justification of libertarian principles based on estoppel.

Let's note here that estoppel itself – as far as applied in this particular way, as an argument against a particular claim or behaviour of a particular person – does not have the many general problems I have established for the Letztbegründung. It is a reasonable and unproblematic way to reject particular claims.

But to apply this unproblematic technique to establish, positively, libertarian principles immediately faces all these problems. And Kinsella's application of estoppel, indeed, does not consider and reject all imaginable alternatives.

He tries to prove that punishing aggressive behaviour is justified but punishing non-aggressive behaviour is unjustified. I do not object against the claims he tries to prove, but only against the logical conclusiveness of the argument.

In fact, in his argumentation the difficult points of justifying these claims have simply disappeared. The most difficult being, of course, that the state has to be considered and evaluated as being on equal foot with an individual – the very point of disagreement between anarchists and etatists. The second point being that the libertarian classification is the right one: That we have to distinguish two classes of behaviour, aggressive and non-aggressive, and that it is this difference which matters, which is ethically relevant. And, not, for example, the many other distinctions, like verbal vs. non-verbal, aesthetic vs. unaesthetic, accepted by the majority or not accepted by the majority. So, where he, in particular, discusses a murderer who argues against the penalty against him, Kinsella forgets the non-contradictory possibility that the murderer does not classify his action as aggression, but, instead, as justified self-defense – the guy who has been killed may have named him, say, “son of a b. . .”, and killing him was reasonable and justified punishment in defense of the honor of his family, in particular his mother, and therefore not an act of aggression.

Considering the case of imprisonment for non-aggressive behaviour, Kinsella considers the example of Susan imprisoned for distributing pornography. He argues:

Now the government may attempt to be clever and use the estoppel argument against her, to estop her from objecting to her imprisonment. However, Susan is not estopped from complaining about her confinement. She is complaining about the aggression against her. Her prior action in question was the publishing of a pornographic magazine. This action is in no way aggressive; [11]

But why is the government that stupid? Logically it could prefer to use the estoppel argument in another way: What she has done, is some non-verbal behaviour. So, she cannot object if the state applies some non-verbal behaviour – like imprisonment – against her. Or, more close to real arguments, the defender of the state will distinguish between behaviour accepted as just by the majority and that not accepted as just by the majority. She claims that their imprisonment is unjust. But what she has done is not accepted by the democratic majority. So she is estopped to argue that the state is doing something not accepted by the democratic majority (if applied against innocent people) in reaction against her behaviour.

Of course, the opinion of the democratic majority about justice is irrelevant, the only thing which matters is justice itself. But this is a difference of content. It is not a point which could be made based on pure logic, even if extended by the possibility of estoppel argumentation.

In particular, the following argument can be seen to be logically faulty:

The conduct of individuals can be divided into two types: coercive or aggressive (i.e., involving the initiation of force) and non-coercive or nonaggressive. This division is purely descriptive. It is unobjectionable, because it does not assume that aggression is invalid, immoral or unjustifiable; it only assumes that (at last some) action can be objectively classified as either aggressive or nonaggressive. [11]

In fact, the same can be said about the other classifications I have considered above. Verbal or not, accepted by the majority or not, these are purely descriptive divisions. So, it follows that to use them is as unobjectionable. At least some actions can be objectively classified as either verbal or non-verbal, either accepted or not accepted by the majority.

Of course, I don't want to question the libertarian position that the distinction between coercive and non-coercive behaviour is much more important than these other distinctions. The point is that to establish this much higher *degree of importance* the purely *logical* methods proposed here are insufficient.

**4.10. Confusing the claim with the proof of the claim.** Last but not least, the very fact that the claims one wants to prove are quite plausible anyway – at least for the libertarian participants of the discussion – leads to another danger: the confusion between the claim itself and the proof of the claim. An argument that the proof of the claim is incomplete or erroneous is usually made only by opponents of the claim itself. So there is a natural tendency to confuse an attack on the proof with an attack on the claim itself. And therefore it seems to be a reasonable answer to defend the claim itself.

In the case in argumentation ethics this is invalid. What is attacked are not the claims themselves. Even in the cases where I have attacked the claim itself too, this is not the main target. Instead, it was only a way to strengthen the argument against the “proof” that in some cases the “proven fact” appears to be even wrong. So one has to be extremely careful here. Else, one may think, for example, that van Dun has been able to refute an attack by Murphy and Callahan in the following quote:

They [Murphy and Callahan] then assert “We cannot convince you of anything by clubbing you, but we may quite logically try to convince you that we should have the right to club you” (M&C, p.58). True, they may try to convince me that they ought to have the right to punish me for my crimes, if I have committed any. There is a good chance that they will succeed. But how on earth do they hope to convince me by means of logical arguments that they should have the right to club me, regardless of what I may have done or will do? [19]

A nice rhetorical question. And, I would guess, the critics have no idea how convince van Dun that they should have this right, because they, as libertarians, don't believe they have this right. So, a point for van Dun? In no way. What has happened here is simply an illegitimate shift of the burden of proof. Nobody questions the claim itself. But it is not the job of Murphy and Callahan to prove that the claim is wrong, but the job of Hoppe and van Dun to show that their *proof* is valid and has no loopholes. And, even more, that this *proof* is *incontestable*, once they make such claims.

Now, Murphy and Callahan have shown a particular loophole in the proof. And this loophole cannot be closed by a rhetorical question, because rhetorical questions are not legitimate parts of logical, incontestable proofs. Imagine a mathematician who claims to have proven, say, the Riemann conjecture, by exclaiming “how on earth can somebody hope to construct a counterexample of the Riemann conjecture?”

Rhetorical questions are legitimate and reasonable parts of plausible reasoning. And the argument provided by van Dun is a meaningful plausible argument, and it would be legitimate if the claim itself would have been questioned. But the claim itself is not questioned at all, it is plausible and acknowledged as plausible by all participants of the discussion. The only question is if there is a strong, logical, even incontestable proof of it. There is none. The aim of the argument was to show that there are loopholes in the proof. And in fact there are. Every in principle imaginable way to answer this rhetorical question defines such a loophole, even if nobody actually tries to use them.<sup>11</sup> The question is not if clubbing can be justified. All participants of the discussion agree that it is not. The disagreement is about a different thing: If there is a strong, incontestable proof of it, or if there is, instead, only a weak, contestable plausibility argument, which justifies that clubbing is unjustified.

**4.11. About the logic of uncertainty.** Why not reject my counter-argumentation given in the previous sections as nitpicking? Once we agree about the principles, why bother about the details of the justification?

On the one hand, this seems to be a position which is quite reasonable. What we really have to care about is truth, not certainty about truth. The very title of this essay, “against absolute certainty”, seems to be directed against the idea that one has to consider certainty as important.

But in fact the situation is quite different. Indeed, it is truth what we really have to care about, not certainty. But the *degree of certainty* becomes even more important in a situation where we do not believe into absolute certainty. As long as we follow the dream of absolute certainty, the world is simple: We have some incontestable, absolutely certain truths, and everything which follows from these truths is absolutely certain too. Everything else is, instead, uncertain and not worth to be considered at all.

In the real world, the situation is a little bit more complicate. There is no longer a simple subdivision of things into absolutely certain and absolutely uncertain things. Certainty becomes a question of degree. And even if we do not assign, in real life, real numbers to degrees of certainty, it is an interesting and important result that the correct, consistent mathematical formalization of plausible reasoning is described by a well-known exact mathematical formalism – the formalism of probability theory. If we would like to construct a robot with artificial intelligence who is able to do what we are doing in our plausible reasoning, and able to do

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<sup>11</sup>For example, one may try to convince van Dun that a quite innocent critical remark he has made in the past is, in fact, a form of verbal aggression strong enough to justify strong retaliation. Or that clubbing is not at all an aggressive act, but, instead, a friendly gesture in a given culture. There are in fact large differences between different cultures in such questions, and acts, verbal as well as nonverbal, considered in one culture as informal friendly gestures (at least if exchanged between friends) appear in other cultures as intolerable aggressions which require deadly retaliation.

this in a really consistent way, without the usual and unavoidable human weakness, this robot would have to assign real numbers between 0 (for certainly false) and 1 (for certainly true) to every statement. And this robot would have to use the standard mathematics of probability theory. Everything else would be, in one way or another, inconsistent.<sup>12</sup> And, surely, if we use this logic of plausible reasoning, the degree of plausibility is a quite important, even central notion for the evaluation of truth. For example, there may be different, conflicting arguments. On the one hand, there will be an argument  $A \rightarrow C$ , on the other hand, an argument  $B \rightarrow \mathcal{C}$ . What is more plausible now,  $C$  or  $\mathcal{C}$ ? Which of the two arguments is more important? This depends on the comparison of the degrees of plausibility of  $A$  and  $B$ . If, say,  $A$  is much more plausible than  $B$ , then  $C$  becomes more plausible than  $\mathcal{C}$ . But if, instead,  $B$  is much more plausible than  $A$ , the situation is the reversed one, with  $\mathcal{C}$  becoming more plausible than  $C$ .

So, good expectations of the degrees of plausibility of various claims are, in a world without absolute certainty, a quite important tool for the evaluation of the truth of other propositions. And therefore it is very important to reject approaches which do not give reasonable expectations of plausibility, approaches which aim only for absolute certainty.

Performative contradictions are a fine thing: If there is more than an accidental ad hominem, if the contradiction is really a necessity for a consistent defender of a theory, it is a nice argument against this theory. They have their place in critical rationalism, as a possibility to criticize theories or ethical rules. Of course, it remains open to doubt. But this is, from point of view of a critical rationalist, only a self-evident triviality, so I would not even mention it.

But whenever somebody claims to prove something in an incontestable, indisputable way, questioning this self-evident triviality, he invites me to nitpicking. This is what I have learned – mathematics, or, in other words, the art of extremal nitpicking.

**4.12. Summary.** I think the examples above have given sufficient illustration of the various failures which appear in various attempts to find a Letztbegründung of various things.

In particular they have been nice illustrations for the complete failure to consider alternatives. There is one theory of ethics – libertarian ethics, with absolute self-ownership and property rights. To prove it in an indisputable way by a method which only allows to reject, any other theory of ethics should be rejected, should lead to a self-contradiction. But in fact almost every alternative theory of ethics – in particular the one of the etatist societies we live now – is not at all in necessary self-contradiction. The people which defend this theory are around us, they argue, they use standing room, and they are alive, and the theory they defend, the status quo, gives them this possibility to argue, to use standing room, and to be alive. And it is quite obvious that not only the theory which defends the status quo, but much worse theories, in particular slavery, give people the possibilities to speak, to use standing room and to be alive.

Other examples, where the failure to consider alternatives is less obvious, have illustrated that the method itself is fallacious. There always remain possibilities

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<sup>12</sup>See the first chapters of Jaynes [29] for details.

to dispute the claims, because the very meaning of the claims depend on complex background theories which are far away from being indisputable.

In fact, the very idea depends on the naive hope that the meaning of particular propositions is well-defined, does not depend on complex background knowledge which is presupposed but nonetheless questionable.

But I would like to note what I have not done. Let's again consider here an argument made by van Dun:

Quite a number of Hoppe's critics like to argue that the ethics of argumentation binds only at the moment of argumentation itself and then only those who take part in it. ... If these critics were right, they would not only have "scored" against Hoppe, they would also have deconstructed the entire edifice of reason, law and justice without which the West would never have risen above the level of barbarism. [19]

Here, I'm also on the side of the attacked critics. But I think these critics have been misunderstood here too. At least the point where I agree with the critics here is that one cannot use the *method* of performative contradictions to *prove* something beyond the very moment of argumentation. Indeed, a theory which agrees with libertarian theory about the very moment of argumentation itself, but essentially differs from it in other question, cannot be rejected because of a performative contradiction – if this would be possible, it would be possible to reject libertarian theory in the same way, using the same performative contradiction.

But this does not mean that there are no other methods to argue for ethical theories. It is only the particular method – to use performative contradictions to *prove* theories – and the resulting pretension of *incontestability* which is questioned and rejected here.

## 5. THE FAILURE OF CLASSICAL RATIONALISM

Having rejected the concept of Letztbegründung as proposed by Hoppe, it seems necessary to consider the philosophical background of this idea – classical rationalism. In particular, Hoppe's Letztbegründung shares the most important psychological aspect with classical rationalism – the wish for certainty of knowledge, the fear of uncertainty.

**5.1. Simplified versions of classical rationalism and empiricism.** So let's introduce here, at first, short and simplified versions of the main opponents – classical rationalism and empiricism. I'm certainly not a specialist in the history of philosophical thought, and I would concede without even questioning that the philosophical theories I name here "classical rationalism" and "empiricism" don't have much to do with the philosophies proposed by real rationalist or empiricist philosophers. They are probably as far away of them as Hoppe's version of Popper from Popper's critical rationalism, as discussed below. But simplified, "straw" versions of philosophical theories sometimes serve a useful purpose – they focus the interest on some key features of the theories in question.

The focus of our interest is, in this case, the hope for absolute certainty of human knowledge.

But let's start. In the philosophy of science before Popper we can distinguish two large camps: Classical rationalism and empiricism.

The main thesis of empiricism is that scientific theories have to be derived from experience.

Classical rationalism objects, with good reasons: We simply cannot derive anything from experience alone. At least some theoretical presuppositions are necessary as support. Without any theoretical background it is simply impossible to derive anything from experience. In particular, we need logic, the laws of reasoning. To derive something, we always need logic. So, if we want to derive something from observation, we also need logic. So we cannot derive logic itself from experience. But even logic alone is not sufficient.

Given such a powerful argument against empiricism, one wonders how empiricism is able to survive it. This is possible because of a powerful counterargument of the empiricists – the “argument” of ignorance. According to empiricism, anything not derived from experience is metaphysics, unscientific, therefore pure speculation, and should be rejected. All we have to care about is experience. Rationalist have, naively, tried to use this as an argument against empiricism – empiricism is metaphysics, not derived from experiment, thus, meaningless. Surprisingly, the empiricist is not convinced. For a very simple reason: Together with empiricism, all arguments against empiricism are metaphysical, thus, nonsensical too. So the empiricist is obliged to ignore them. In this sense, empiricism appears to be indisputable – every attempt to dispute empiricism is necessarily metaphysical (because it attacks a metaphysical theory), and therefore should be ignored by empiricists. And I have to acknowledge that, at least in physics, positivists are quite consistent in following this rule and, consistently avoiding performative contradictions, usually simply refuse to argue about philosophy.

Once I do not share the positivist prejudice against metaphysics, I do not ignore these arguments, and therefore empiricism is nothing I would take seriously (except as a dangerous mental illness). So the more interesting question is what proposes the alternative camp – classical rationalism.

Classical rationalism proposes to solve the mentioned problem of empiricism in the following way: One acknowledges that to derive something from observation one needs something more, some theoretical presuppositions. These presuppositions cannot be derived from observation, so they have to be justified in a different way. So, what one needs is, on the one hand, a minimal set of theoretical principles, strong enough to allow to derive all the content of science from observation, and, on the other hand, an independent justification for these principles. If this would be reached, one would obtain the beautiful situation that all scientific theories can be derived, derived from a few independently justified, unquestionable theoretical principles together with observation.

A few words about some of the simplifications in this picture: Of course, some empiricists recognize that observations are inaccurate, and that it is hard to claim that one can derive accurate knowledge from inaccurate observation. But one can save the very promise of certainty: With increasing accuracy of the measurements, the derived theories become more and more certain, approaching in the limit of ideally accurate observation absolute certainty. Then, there is the Austrian variant: One recognizes the conceptual impossibility to predict human behaviour accurately: Human beings can learn, and this influences their behaviour, but one cannot know today what they will learn in future. The Austrian solution is the following: Once there is no hope for certainty of the prediction even in principle, one has to reject

any attempts to make empirical predictions of human behaviour, and to restrict oneself to results which can be obtained with absolute certainty.

And there is, of course, more to say. But I leave this to my opponents.

One purpose of these simplifications is a quite obvious one: It allows a sort of revenge for the presentation of Popper's critical rationalism as a variant of empiricism, a revenge which is, I hope, better justified than Hoppe's misrepresentation of Popper I criticize below.

In fact, it is now easy to present central common features of classical rationalism and empiricism. First, above philosophies share, as their Holy Grail, the promise of absolute certainty of scientific knowledge. In empiricism, this promise is justified by restriction to experience, to observation, and the refusal to participate in theoretical speculation. In classical rationalism, the situation is a little bit more complex, because it is recognized that observation taken alone is not sufficient for the derivation of scientific theories. One has to add a few theoretical principles. But these principles are true a priori, necessities of thought, incontestable truths, justified by an unquestionable *Letztbegründung*. So, the resulting promise is the same: All scientific theories obtain absolute certainty.

The second central point which is shared is the picture of natural sciences, with theories as derived from experiments. In particular, Austrians simply stay away from the consideration of natural sciences, leaving them to empiricists, and fight only against the applications of the methods of natural sciences in economics. In any case, they accept the classical subdivision which goes back to Kant between knowledge a priori – a few incontestable general principles of thought, to be proved by some transcendental argumentation – and knowledge a posteriori – theories derived from experience.

From the point of view of this concept of classical realism it becomes clear why one likes to insist on the incontestable character of the basic principles: It is not only the absolute certainty of these basic principles which is at stake. The whole promise of absolute certainty for the whole of science depends on the absolute certainty of these basic principles. Without certainty about the fundamentals there can be no certainty for the whole building of scientific knowledge. So, there is a lot at stake.

And the danger seems great: The other side of the dream of absolute certainty is the nightmare of absolute skepticism.

**5.2. The Popperian revolution.** Then came the Popperian revolution. The very foundations of the philosophy of science have been turned upside down. What has been shared by above camps – the dream of absolute certainty of scientific knowledge – has been openly rejected. All our scientific theories are, according to Popper, only hypotheses, open to doubt, and we can never be completely sure that they are true.

And, second, it is not theory which is a posteriori, but observation. All our theories are a priori, free inventions of the human mind, not at all derived from observation. Observation is only a method of evaluation of our theories. The new logical order is the following: First, there are theories, freely invented, without any prior justification. Then, one derives testable predictions about reality from these theories. And only after this we can compare these predictions with observation.<sup>13</sup>

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<sup>13</sup>The ordering in real time may be different, the experiment may have been done before the theory has been developed. But, anyway, even in this case the comparison can be done only after formulation of the theory, and after derivation of the prediction about the outcome from the theory. In this sense, the theory is logically a priori.

Thus, all theories are logically a priori, and all observations are logically a posteriori. In classical rationalism, a priori principles have been a small, very special subgroup of scientific theories. The great majority, all those theories connected with observation, have been a posteriori in classical rationalism. This has changed now in a radical way. In Popper's approach all scientific theories appear to be a priori.

It seems that to give up the dream of absolute certainty of scientific theories was emotionally much more disturbing.

But it was a necessary step. Necessary, because classical rationalism has simply failed. First, the idea of absolute certainty of scientific theories has failed in reality. The Einsteinian revolution has shown that the most cherished classical theory, Newtonian mechanics, was simply wrong, not more than an approximation for small velocities. The quantum revolution has shown that this was not a particular accident, but that Newtonian mechanics was wrong in another domain – for very short distances – too. And it was easy to recognize that this can happen again and again. However accurate, our observations are never completely exact, and there will always be a possibility that we mingle the true theory with some approximation, simply because our observations are not accurate enough to distinguish them.

The failure was as big in the domain of pure philosophical theory. The deadly problem here was the problem of induction. It was clear that to obtain a general theory from observations, which are restricted to the past, one needs some additional principle – a sort of “principle of induction”. But, different from some other problems on the way from observation to theory, it was quite obvious that there is no justification for induction. Induction is speculation. Common sense may not recognize that observations are theory-laden, have to be interpreted, and that these interpretations already heavily depend on theories. So it was possible to ignore this problem. Not so in the case of induction. That induction in a changing world is speculation, that it is often helpful, but also often fails, that one cannot derive future from the past, is sufficiently obvious already for common sense.

So it was simply necessary to give up the hope for absolute certainty in science.

**5.3. The danger of dogmatism.** Given the fact that one has to give up the hope for absolute certainty of all our scientific theories anyway, there is no longer a theoretical reason to prefer absolute certainty for a small subset of fundamental principles. Anyway, given that a “principle of induction” has no chance to reach the status of absolute certainty, the remaining set is quite useless because not even a single general empirical theory can be derived.

Emotionally, the attraction may be understood – the fear of uncertainty remains strong. Even if we accept that there remains uncertainty in the theories of natural sciences – if there are at least some points of certainty, uncertainty at other places is something tolerable. If necessary, we have a safe place to go back. But if not? If everything can be doubted?

Moreover, classical rationalists have already found some “justifications” for some of the principles. What to do with them? Throw them away? Why? To know something with absolute certainty is obviously better than hypothetical knowledge, even if one is not afraid of uncertainty.

Of course, the main reason why one should throw away such justifications is that they don't fulfill their promises. They simply don't prove what they claim to prove with absolute certainty. They are at best plausibility arguments. Usually

they are not even good plausibility arguments, but rather weak. I have presented some examples above.

Nonetheless, one could imagine that there are some better examples, where the justification which claims to prove a principle with absolute certainty is really good. One can imagine, for example, justifications of classical logic. A pretty nice example of a derivation of the rules of probability theory as the rational rules of plausible reasoning can be found, for example, in Jaynes' book [29]. But even in the case of such nice derivations I refuse to accept claims about absolute certainty (fortunately, Jaynes does not make them). Here is my argumentation why.

Human beings often err. It is, therefore, reasonable to see what happens if we err. So, assume there is a proof with absolute certainty, and I err, and continue to think that to doubt the proven principle is possible. What happens? In general, nothing. As long as nobody makes a reasonable proposal to replace the principle I will continue to use it. That I consider it to be only hypothetical doesn't change the situation, because I believe that everything is only hypothetical, and I have to make decisions anyway, so I'm used to make decisions based on hypothetical theories and not afraid of this. In this particular situation, where no reasonable alternative is available, my choice is trivial anyway.

The situation changes if somebody proposes an alternative. Once we have assumed that the principle is absolutely true, a reasonable alternative simply does not exist. So all what happens is that I loose some time to find the error in some unreasonable alternative proposals. Sometimes something even worse can happen, and I may believe an unreasonable proposal. But even in this case I don't assign to the alternative theory the status of an absolute truth. So I remain open to arguments in favour of the correct theory. Once the arguments in favour of the true theory will be much better, it will not take a long time before I recognize my error. So what I risk is some loss of time for studying nonsense, and, in the worst case, to believe some time into a wrong theory.

What happens if the classical rationalist errs? There is a principle, claimed to be proven with absolute certainty. He believes this. Now assume that this is an error. It may be that only the proof is wrong, but the principle is nonetheless true. In this case, nothing dangerous happens. But what if the principle itself is wrong too? In this case, the rationalist is trapped. The principle is a dogma, proven with absolute certainty. Therefore it makes no sense at all to consider criticism. So, our dogmatic will never read any argumentation against the principle. Indeed, this is the whole point of absolute certainty. We can be sure that alternative proposals are nonsense, and do not have to loose time studying them. So, our dogmatic will believe into his false dogma forever, without even a theoretical possibility to recognize his error. The only way to recognize his error would be to study criticism of the principle. But this is meaningful only if we accept that, at least in principle, our dogma is open to doubt.

So I risk much less – some loss of time, and only in the worst case a correctable error. In other words, I risk nothing in comparison with the uncorrectable, fatal error of the dogmatic who believes into a false dogma. But this is not even the whole story. Because I do not even loose time. At least this is what experience suggests.

Indeed, the best imaginable candidate for an incontestable, absolutely certain truth is, of course, classical logic. But there have been some interesting attempts to refute classical logic, and it is worth to consider what happened with them.

One such proposal was made by Brouwer, named “intuitionistic logic” [31]. It has been considered by Gödel, who has found that a minor redefinition is sufficient to return to classical logic. That means, assume we accept Brouwer’s intuitionistic logic. Now, almost all we have to do is to redefine the meaning of “exists”. We have to define  $\exists xA(x)$  (there exists an  $x$  so that  $A(x)$ ) as a shortcut of  $\neg\forall x\neg A(x)$  (it is false that for all  $x$   $A(x)$  is false). And, for the new notion of “exists” all rules of classical logic are valid. Even more interesting is that we do not lose anything. If we give Brouwer’s notion of “exists”, which does not follow the rules of classical logic for “exists”, another name, all the results of Brouwer’s intuitionist mathematics become results of classical mathematics and do not contradict classical logic – because for the renamed notion of “exists” the rules of classical logic of “exists” should not be applied. So, the consideration of intuitionist logic by Gödel was not a loss of time at all. The result was a new part of classical mathematics.

And there was also another attempt to modify classical logic, named “quantum logic” [32]. This was a proposal for a better understanding of quantum theory. The result was quite similar. The mathematical apparatus of “quantum logic” was another interesting mathematical structure named “non-distributive, orthocomplemented lattice”. And the consideration of the foundations of quantum theory has shown that one does not have to modify classical logic. Today we know that there are interpretations of quantum theory which do not require any modifications of classical logic or classical realism. An example of such an interpretation is the de Broglie-Bohm interpretation of quantum theory. Again, simple ignorance of quantum logic would not have been the best choice. We would have lost the mathematical results of quantum logic.

Remarkably, considering these alternatives has also given some insight into classical logic. We have now a better understanding of classical logic, and, in particular, we have also obtained some knowledge about how to handle such attacks. Indeed, the solution was in above cases quite similar – it was necessary to reconsider the meaning of some notions. So, if there will be another attack on classical logic, it is already clear where to look first: at the meaning of the basic notions, and in particular of the notions “exists” and “is”. There are a lot of different common sense meanings of these words, and it is easy to confuse these meanings. And if one uses another meaning of “is” instead of the logical meaning, one should not wonder that the rules of classical logic become invalid.

So there have been even two interesting cases where the most certain part of human knowledge, classical logic, has been questioned. And in above cases, the consideration of the alternative proposals has given us interesting insights – even if classical logic survived this.

Is this an accident? I don’t think so. Simply, if even reasonable people question such a fundamental notion like classical logic, they have some motivation for this. And one would better care about these problems, even if one does not think that classical logic requires modification. So, considering such alternatives does not even seem to be a loss of time. It gives a better insight into the very nature of the principle in question. We can learn effective ways how to defend it. And we can solve the problems which have motivated this attack.

So we can summarize that dogmatism is dangerous high risk behavior even if there would be truths provable with absolute certainty. One risks to be trapped in believing a false dogma, but gains nothing – not even the time one would have to spend to study alternative proposals.

## 6. POPPER’S CRITICAL RATIONALISM AND HOPPE’S OBJECTIONS

There are good reasons for libertarians not to care very much about Popper. Last but not least, Popper is an etatist. And the state he prefers is far away from being a minimal nightwatchman state. Even if Popper favors an open society, this open society is an interventionist, social-democratic state. His proposal for “piecemeal social engineering” instead of bold modification minimizes, of course, the harm caused by such small modifications of government intervention, but in the long run leads to an even more dangerous increase of the power of the state.

Nonetheless, it is Popper who has developed the philosophy of critical rationalism, which is the appropriate replacement for the failed ideologies of empiricism and classical rationalism. And this philosophy is worth to be studied. In particular, it is a much better base for libertarian economics and ethics.

Of course, instead of giving here an introduction myself, it seems much more reasonable to refer the reader to Popper himself. Popper is a very good and clear writer, and books like “Conjectures and refutations” are nice reading and a much better introduction as what I would be able to present here. I can also highly recommend to read Albert – the other founder of critical rationalism.

But some sort of introduction I have to give here anyway, for other reasons. There are some critical remarks against Popper in libertarian literature, especially by Hoppe. They are quite unjustified, as I will try to show below. What is criticized is not Popper’s critical rationalism, but some simplified, empiricist version of fallibilism. It seems necessary to correct this. These corrections necessarily give also a short introduction. And, maybe, even a helpful one, because it is focused on points which are really misunderstood and misrepresented.

I would not overemphasize the importance of Hoppe’s anti-Popperianism. Hoppe had the unfortunate fate that his teacher was Habermas, the enemy of Popper in the “Positivismusstreit” and probably the champion in the domain of misrepresentation of Popper. It would be unfair to blame Hoppe for not recognizing all errors of his teacher. Moreover, Habermas is not alone. I have already mentioned the whole intellectual tradition of simplification and misrepresentation of Popper started by Feyerabend, Kuhn and Lakatos – people much closer to Popper himself as well as to the domain of natural sciences.

And in fact Hoppe is not the real target. Most of his misunderstandings can be interpreted as derived from a single cause – a preference for absolute certainty. Hoppe’s Popper seems to be, in other words, Popper as misunderstood by a believer in absolute certainty. It is this dangerous belief which is the main target of this paper, and it is shared by a large part of the libertarian movement, from Austrian economics to objectivism.

But, enough, let’s look at the details.

**6.1. Naive fallibilism.** Let’s start with the following quote:

Popper would have us throw out any theory that is contradicted by any fact, which, if at all possible, would leave us virtually empty-handed, going nowhere. [6]

This is simply false. From the start, Popper has never proposed to throw away a falsified theory. The theory is false, so, it has to be corrected and improved, or replaced by a better theory. Let's quote page 16 of his "Logik der Forschung" – his first book about this question: "Nach unserem Vorschlag kennzeichnet es diese Methode, daß sie das zu überprüfende System in jeder Weise einer Falsifikation aussetzt; nicht die Rettung unhaltbarer Systeme ist ihr Ziel, sondern: in möglichst strengem Wettbewerb das relativ haltbarste auszuwählen." [17] The crucial point is here "*das relativ haltbarste auszuwählen*", which I would roughly translate as "to choose the system most robust in comparison".

Hoppe continues:

In recognizing the insoluble connection between theoretical knowledge (language) and actions, rationalism would instead deem such falsificationism, even if possible, as completely irrational. There is no situation conceivable in which it would be reasonable to throw away any theory – conceived of as a cognitive instrument of action – that had been successfully applied in a past situation but proves unsuccessful in a new application – unless one already had a more successful theory at hand. [6]

Fine. Only that this is exactly what Popper himself proposes. The primitive version of fallibilism criticized by Hoppe is, instead, Hoppe's own invention (or possibly of one of his teachers) and has nothing to do with Popper.

But, as promised, this simplification may be understood from the point of view of a believer into absolute certainty. If we believe in absolute certainty, a false theory is of no use at all, and has to be simply thrown away. A false theory may be useful for rational men, if no better theory is available. It is not useful for those who search only for absolutely certain theories.

**6.2. The hypothetical character of falsification.** It is a characteristic feature of Popper's critical rationalism that all our theories are only hypothetical. Not only general physical theories, but also the basic propositions – theory-laden interpretations of observations – are hypothetical. And, as a consequence, falsification is also only hypothetical. Here is what Hoppe thinks about this:

... the hypothetical character of basic propositions invalidates Popper's claim, central to his entire falsificationist philosophy, that an asymmetrical relationship between verification and falsification exists (i.e., that one can never verify a hypothesis, but can falsify it). [6]

Let's compare this with Popper:

Unsere Auffassung stützt sich auf eine Asymmetrie zwischen Verifizierbarkeit und Falsifizierbarkeit, die mit der logischen Form der allgemeinen Sätze zusammenhängt; diese sind nämlich nie aus besonderen Sätzen ableitbar, können aber mit besonderen Sätzen in Widerspruch stehen. [17]

That means, the asymmetry is related with the logical form of general propositions, not with their degree of certainty. General propositions may be in conflict with

particular propositions, but they cannot be derived from particular propositions. This is a clear logical difference. It has nothing to do with the status of certainty of the propositions, and holds for uncertain propositions as well: A hypothetical particular proposition can logically contradict a general hypothetical proposition, but the last cannot be derived from particular hypothetical statements.

Again, for a believer in absolute certainty there is a point in Hoppe's argument. If the basic proposition is only a hypothetical one, then the falsification is also only hypothetical. And, indeed, it is. But for our believer in absolute certainty such an uncertain falsification is completely useless.

Sometimes it seems as if very idea of different degrees of certainty seems foreign to Hoppe:

... the idea of perfect or radical uncertainty (or ignorance) is either openly contradictory insofar as it is meant to say "everything about the future is uncertain except that there will be uncertainty – about this we are certain," or it entails an implicit contradiction if it is meant to say "everything is uncertain and that there is nothing but uncertainty, is uncertain, too." (I do know such and such to be the case, and I do not know whether such and such is the case or not.) Only a middle-of-the-road position between the two extremes of perfect knowledge and perfect ignorance is consistently defensible: There exists uncertainty but this we know for certain. Hence, also certainty exists, and the boundary between certain and uncertain knowledge is certain (based on certain knowledge). ([10] p. 50-51)

But there is another position – and one at least very close to the one described by Hoppe himself in the next section – that nothing can be known with absolute certainty, everything is, at least in principle, open to doubt. But that there are degrees of certainty, and there are a lot of cases where it is rational to ignore the remaining possibility of doubt, a sort of pragmatical certainty, and it is this pragmatical certainty we use if we say "I know" or if we make certain statements without mentioning any qualifications.

**6.3. Popper's "empiricism".** Hoppe classifies Popper as an empiricist. If one uses Hoppe's "definition" of empiricism as "the philosophy which thinks of economics and the social sciences in general as following the same logic of research as that, for instance, of physics." [3], one would even have to agree. I nonetheless don't take this definition seriously. It may be a characteristic property of empiricists in economics, but empiricists in physics don't care about this question at all.

A much more reasonable criterion of empiricism has been provided by Mises:

The viciousness of positivism is ... to be seen ... in the fact that it does not acknowledge any other ways of proving a proposition than those practiced by the experimental natural sciences and qualifies as metaphysical – which, in the positivist jargon, is synonymous with nonsensical – all other methods of rational discourse. [13]

And here, Popper is clearly on the side of the rationalists.

Part of the explanation of this misqualification may be that the role of falsification in critical rationalism is heavily overestimated. This has historical origins. Falsification was the idea Popper started with: A theory is empirical if it makes

falsifiable predictions. This very idea was implicitly quite heavily laden with empiricism, in particular with the wish of the empiricists to distinguish empirical, scientific theories from metaphysics, to get rid of the “metaphysical nonsense”.

But Popper has well understood that this does not exclude metaphysics from physical theories. And the very idea of his later philosophy of “critical rationalism” is a generalization of his approach to purely philosophical, metaphysical, and ethical theories. These theories are, as well, a priori, free inventions of the human mind, and can as well be criticized. And Popper has developed criteria for criticizing them: Such theories are proposed to solve philosophical problems. Therefore they may be criticized for not solving them. Or for other problems they create, which do not appear in alternative theories. And there are a lot of other criteria.

The interesting point is that all these different criteria for criticizing philosophical theories may as well applied to criticize empirical theories. And real scientists apply them heavily: Explanatory power, internal consistency, compatibility with other theories, simplicity, beauty, unification are criteria which may be applied to empirical as well as metaphysical or ethical theories. So the empirical content of a theory and its falsification or corroboration are not everything scientists take into account. Their role is heavily overestimated as a consequence of empiricism.

Only from the point of view of a simplified version of fallibilism, probably one which claims that theories which do not allow empirical falsification are metaphysical nonsense, one can at least understand a misqualification of Popper as an empiricist. But from the point of view of the complete Popper this becomes nonsensical.

**6.4. A priori true propositions.** One argument to justify the claim that Popper is an empiricist is that there exist “no such things as a priori true propositions about reality” [6], or, in more detail:

For in fact, Popper is in complete agreement with the fundamental assumptions of empiricism . . . and explicitly rejects the traditional claims of rationalism, i.e. of being able to provide us with a priori true empirical knowledge in general and an objectively founded ethic in particular. [6]

But what are the facts about a priori true propositions in Popper’s approach?

First, in critical rationalism all theories are a priori. That means, they are constructed logically prior to observation. They appear as free inventions of the human mind, not as derived in any way from observation. Observation comes only later. First, we need the theory. Then we derive predictions about reality. And only after this we compare these predictions with observations. Even if these observations themselves have been made earlier in real time, they can be used only later, after the derivation of the prediction, to test the theory. So, theories are always a priori.

Then there is Popper’s theory of truth, which is nothing but good old correspondence theory of truth: Truth means correspondence with reality. According to this theory of truth, we can know absolute truths.

So, according to Popper, there are a priori true propositions.

Of course, a believer in absolute certainty tends to mingle absolute truth with absolute certainty about this truth. These are quite different things – some completely unjustified hypothesis may be true. For example, take an arbitrary meaningful statement, say  $A$ . Then or  $A$ , or its negation  $\neg A$  is true. But above claims may be completely unjustified, pure guesses, and have no certainty at all.

**6.5. About rabbits.** To prove his point that Popper is an empiricist, Hoppe writes:

See, for example, . . . where he advances the traditional empiricist thesis that “only if we are ready to accept refutations do we speak about reality” (p. 212) and “refutes” the idea of the rules of logic and arithmetic being laws of reality by pointing out that “if you put  $2 + 2$  rabbits in a basket, you may soon find seven or eight in it” (p. 211). [6]

But this has nothing to do with empiricism. It has to do with uncertainty. But uncertainty is not a characteristic feature of empiricism. Last but not least, the main idea of empiricism is that theories have to be *derived* from observation. But derived things are certain, as certain as the thing they are derived from. So there is no uncertainty in the ideal of empiricism. Some empiricists, as well as some rationalists, may have recognized accidentally that human measurements are uncertain, and it is quite obvious that one cannot derive certain theories from uncertain observations. But the ideal of absolute certainty is shared by empiricists and classical rationalists (or, to be accurate, at least by my straw versions of empiricism and classical rationalism). So it is a distinguishing feature of Popper that the ideal of absolute certainty is given up completely.

**6.6. Methodological conventionalism?** Another accusation against Popper’s approach is that of methodological conventionalism:

It is correct that Popper has always been aware of the possibility of immunizing ones hypotheses from falsification. . . His answer to such a threat to his falsificationism, however, can hardly be accepted as a solution. For he actually admits that he cannot show such "conventionalism" to be wrong. He simply proposes to overcome it by adopting the methodological convention of not behaving as conventionalists do. Yet how can such methodolical conventionalism (i.e., a methodology without epistemological foundation) claim to establish science as a rational enterprise and to stimulate scientific progress? [6]

I disagree. One can consider in detail how Popper’s method is justified as a rational enterprise. It is certainly rational, from point of view of praxeology, to search for tools which allow to make accurate predictions about the future, because such tools allow us to predict and therefore to optimize the outcome of our actions. And there are clear and simple rules which characterize the quality of these tools: A predictive tool is good only if its predictions about future events are really correct. Indeed, a tool which makes wrong predictions would be fatal. That’s why, if such tools are proposed, it is rational to test if their predictions are really correct. Now, scientific theories are nothing else but such tools, and what Popper proposes is exactly to test them.

Given that several such tools survive our tests, which of these tools is preferable, more useful for our purposes? Of course, it is the tool which makes more, more accurate and more detailed predictions. But this is, again, simply Popper's criterion of empirical content. So the basic rules of Popper's methodology are simply the rational rules for making choices between prediction-making tools. Of course, the rational variant here is Popper's variant, where the rules are used to find out the best among the available tools, not Hoppe's simplified variant rejected above, where every tool which makes a single wrong prediction should be thrown away immediately.

And, different from the empiricist variant, Popper's method is not a method to construct such tools. It doesn't care how such tools are constructed. Popper's method only specifies how to choose the best among all the proposed tools. This is rational too: Once there is some predictive tool which makes correct predictions, why should we care how it has been constructed? It works, and this is what is important from the rational point of view for the customer. Therefore the tools are considered as given a priori, and our job as potential customers of these tools is to evaluate them, to test them, and to find out the best of them.

One can trace the rationality of the Popperian method in all details, in particular also in the case of immunization. If a prediction fails, this is certainly bad and dangerous. But, if most other predictions are correct, it would be nonetheless stupid to throw the tool away. One would, instead, try to correct the device. An easy way would be to identify and remove the wrong predictions preserving the correct ones, at the cost of making less predictions. This way is known as immunization: The immunized theory no longer makes the prediction which had been wrong. Some ad hoc excuse is used to invalidate the prediction, so that it makes no prediction at all in this particular situation, and, as a consequence, in sufficiently similar situations too. Everything else is, as much as possible, unchanged. The immunized tool, once it does no longer make false predictions (as far as we know), is better than the original one. But, of course, one would prefer a tool which makes a correct prediction about the result of the critical experiment. Thus, immunization is sometimes a useful method, but not the optimal one – it decreases the empirical content.

Justified from a rational point of view is also another key property of Popper's method – the rejection of verificationism. We have no proof that the available tools for prediction make only correct predictions. Of course, it would be nice to have such a tool – therefore we try to find such tools, true theories, and reject theories which are known to be false because they have been falsified. But it would be stupid to throw away a prediction-making tool only because we have no certain proof that it always works correctly.

Thus, Popper's method is, in all its parts, justified by what Hoppe names in footnote 105 of [7] the “rationality criterion of instrumental success”.

**6.7. The problem of induction.** Let's consider now an argument about the problem of induction.

Popper, with great self-assurance, claims to have solved – through adopting his falsificationist methodology – the Humean problem of induction and thereby to have reestablished science as a rational enterprise. (See in particular Karl R. Popper, *Objective Knowledge*, Oxford, England: Oxford University Press, 1972, p. 85ff.)

Alas, this is simply an illusion. For how can it be possible to relate two or more observational experiences, even if they concern the relations between things that are perceived to be the same or similar, as falsifying (or confirming) each other, rather than merely neutrally record them as one experience here and one experience there, one repetitive of another or not, and leaving it at that (i.e., regarding them as logically incommensurable) unless one presupposed the existence of time-invariantly operating causes? Only if the existence of such time-invariantly operating causes could be assumed would there be any logically compelling reason to regard them as commensurable and as falsifying or confirming each other. [6]

Obviously Hoppe has completely misunderstood the way the “induction problem” is solved in the Popperian approach. The point is that theories are no longer derived from observation, but a priori, free inventions of the human mind. It doesn’t matter *what* has motivated their human creator to invent them. That may be some astrological considerations, as in the case of Kepler, some strange dream, as in the case of the ring structure of Benzol, or something else – it doesn’t matter, because the justification of the theories, the consideration of their quality, does not depend at all on this. If the theory has been developed based on some process of induction – so what? This induction plays no role in the evaluation of the theory too. There is therefore no reason to care if induction can be justified. Induction may be understood as pure speculation, this does not change anything at all.

So the problem which Hoppe discusses does not even appear in Popper’s approach. There is no necessity at all to relate different observations or to derive something from such a relation. Theories are a priori, even theories based on induction, because any *justification* by induction, even if it exists, is irrelevant and ignored. Moreover, even if observations may be used to falsify or confirm something, then not each other, but, instead, general theories. And even the phrase “to confirm general theories” is misleading, because it suggests that a theory “confirmed” by an observation is established as true. Popper has proposed the weaker notion “corroborate”. Hoppe continues:

However, Popper, like all empiricists, denies that any such assumption can be given an a priori defense (there are for him no such things as a priori true propositions about reality such as the causality principle would have to be) and is itself merely hypothetical. [6]

I have already mentioned that there are, in critical rationalism, a lot of a priori true propositions. I have to add here that there are also a priori defenses. In fact, all defenses of a theory against attacks on its logical consistency, simplicity, beauty, empirical content, and explanatory power can be done conceptually prior to comparison with observation, and are, in this sense, a priori. So, the only thing which is correct here is that theoretical hypotheses are merely hypothetical.

**6.8. Skepticism as another danger of dogmatism.** What follows leads to another important point – the danger of skepticism:

Yet clearly, if the possibility of constantly operating causes as such is only a hypothetical one, then it can hardly be claimed, as Popper does, that any particular predictive hypothesis could ever be

falsified or confirmed. For then the falsification (or confirmation) would have to be considered a hypothetical one: any predictive hypothesis would only undergo tests whose status as tests were themselves hypothetical. And hence one would be right back in the muddy midst of skepticism. Only if the causality principle as such could be unconditionally established as true, could any particular causal hypothesis ever be testable, and the outcome of a test provide rational grounds for deciding whether or not to uphold a given hypothesis. [6]

Here we are back to a question already considered – the problems related with the requirement of absolute certainty. Hoppe seems to think that if there is no absolute certainty, one ends in skepticism.

This may be the most important motivation of those who believe in absolute certainty. They are afraid of skepticism – the philosophical theory that, once we cannot be sure that our theories are true, they are completely useless.

But the situation is different. Absolute certainty is only a dream of philosophers. The only domain where one can reasonably talk about it is pure mathematics. But humans are so prone to err that even in mathematics they err often enough. Not even proof checker programs would guarantee *absolute* certainty – at least up to now they have to be written by erring human programmers, so may contain errors themselves.

And even if one would concede, for the sake of the argument, that not only mathematical propositions, but also some claims about reality are absolutely certain – it would not change the fact that there are a lot of situation where we have nothing even close to certainty. Nonetheless, these are situations which appear in our life, and we have to act in these situations too. And we are used to act in such uncertain situations. What we use in these situations is hypothetical knowledge, without even a claim of certainty. Nonetheless, it would be stupid to reject it. We have methods to evaluate them, methods which allow to distinguish more questionable from less questionable theories.

Any reasonable theory of human action has to cover human action in situations of uncertainty too. Else, it would be far too incomplete to be of any use. Human life is full of uncertainty, and we have to act and act under uncertainty. And a reasonable theory of human action has to tell us something about these actions under uncertainty. It has, at least, to accept that reasonable methods for deciding about actions under uncertainty exist and are successfully applied by human beings in such situations. The philosophy of skepticism is, in this sense, unreasonable.

Instead, the aim of Popper's critical rationalism is exactly the consideration of these methods – methods to evaluate hypothetical theories. These methods, of course, do not become invalid if one of the theories is a certain one. But their main domain of application are hypothetical theories and propositions.

Of course, from the point of view of our hypothetical believer in certainty such methods do not make sense. He doesn't care about degrees of certainty except for the ultimate one – absolute certainty. So methods which allow to evaluate theories with other, lower degrees of certainty are useless in his belief.

And this is the other side of the danger related with the belief in absolute certainty. It is not only that one risks to believe in a false dogma. It is the ignorance of uncertain knowledge based only on its uncertainty in principle. The fact that some

proposition is not absolutely certain is, for the dogmatist, already strong evidence against it. Instead, the critical rationalist does not have to care. His reasoning is straightforward: “This proposition is in principle open to doubt? Okay, but so what? Everything is in principle open to doubt. So please present some specific reason to doubt.” A way of thinking which is not open to the dogmatist, who subdivides the whole domain of knowledge into absolutely certain dogma and unproven speculation.

It goes without saying that doubt is an important and helpful emotion if it is justified, in case when there are several reasonable alternative, or in case the theory in question has never been tested in comparable situations. But unjustified doubt is not helpful, but, instead, dangerous. The general doubt which the dogmatist assigns to all unproven propositions is an example of such an unjustified doubt.

In this context, one has to recognize the existence of an emotional barrier for the dogmatist to accept critical rationalism. He is simply afraid that in future he *always* has to rely on unproven speculation. This fear is nonsensical. One easily becomes used to the new situation, which is not that new at all. In everyday life, the dogmatic does not care about relying on such “unproven speculations” too. It would be impossible to survive otherwise.

**6.9. Kuhn, Feyerabend and Lakatos.** Classical examples of criticism of Popper have been given by Kuhn, Feyerabend and Lakatos. Hoppe refers to them. So, in [6], Hoppe supports the following claim of McCloskey:

...have we not known since Thomas Kuhn’s Structure of Scientific Revolutions that the actual history of natural science does not seem to come anything close to the Popperian illusion of science as a rational enterprise steadily advancing through a never-ending process of successive falsification

We haven’t, because the things Kuhn observes are nice and interesting, but not in contradiction with Popper (only some of Kuhn’s erroneous conclusions are).

At another place, Hoppe tries to show that “Popperianism” leads to skepticism, relying on Feyerabend:

Was it not Feyerabend who first and most forcefully drove the relativist message home to Popperianism? And was not he himself a leader of this very school who simply drew the ultimate logical conclusions of Popperianism? [6]

He certainly wasn’t, because there is no such relativistic message.

Hoppe also refers to Lakatos as a “neo-Popperian philosopher”, as discussed below (section 7.4).

Even if these three points are quite different, I have unified them here because all three philosophers share a common misunderstanding of Popper. Let’s explain it on the toy example of a “methodology of football”: There are rules of the game named “football” (also named “soccer” in some provinces). These rules define how the teams have to behave on the field, what counts as a foul, what as a goal, and that the team with more goals is the winner. But there are other interesting rules connected with football: Rules how to teach children playing football, rules of strategy and tactics on the field, rules how to form a winning team. These rules are of different character. The rules of the game itself are quite simple and certain. The rules how to form a winning team are quite subtle and complex. Moreover, they

are extremely uncertain – what is helpful for one team makes the things worse in another team. In other words, it is success which decides, and otherwise “anything goes”.

In a similar way, one has to distinguish different types of rules in scientific methodology. There are the rules how to evaluate the various scientific theories which have been proposed at a given moment of time. Roughly, these could be named the “Popperian rules”. (Only roughly, because Popper has sometimes cared and written about the other type of rules too, and is therefore not completely innocent in the confusion between these two types of rules.) And there is the other domain – the question how to develop scientific theories which have a chance to win the Popperian evaluation in some future.

Having in mind these distinctions, I can roughly classify the positions of the various philosophers:

Popper has proposed rules of the game, rules which prefer the best actually existing prediction-making tools.

Lakatos has tried to develop some set of rules for how to develop a winning team. Indeed, a methodology for evaluation of research directions is what a scientist needs to develop successful theories in the future. So he has tried to solve a completely different problem. One may like his proposals or not, they are certainly not an improvement of Popper, but something completely different.

Feyerabend has proposed “anything goes” as a replacement of Popper’s rules. For Popper, “anything goes” is the rule how to form a winning team, for Feyerabend it is the rule of the game. So he has certainly not “drew the ultimate logical conclusions of Popperianism”. “Anything goes” as a replacement for the rules of football would destroy the very game.

Kuhn has, as a historian, considered records of what scientist tell. They tell us something analogical to football players after the game: “we have played very good, only because of some unhappy accidents and some wrong decisions of the referee the other team has made some goals”. In other words, they evaluate the current state from the point of view of future development, which tries to ignore points which are irrelevant for the future (like referee decisions) from the relevant ones (the own good play). The actual score is usually not even mentioned. So no wonder that by evaluating such statements Kuhn was unable to identify Popperian rules.

What Kuhn has done is, in fact, similar to an empirical falsification of praxeology: Popper’s rules are based on understanding of the problems scientists try to solve, and can be understood as a praxeology of scientific research. The idea to “falsify” this insight is an empiricist one.

All three critics have missed the point.

There are, of course, explanations why it was much simpler to ignore this conceptual difference between rules of the game and rules to form a winning team. The difference is much more obvious in the case of football. (This is why I have introduced this analogy.) There are several points which hide the difference in the case of science.

First, there is no subdivision into training and games in science, no fixed time for a game with a certain score after this and a next round next week, no table of results, no champion of the year. In history of science there is no replacement for this information. Future historians of football may become confused because today

an increasing number of other statistics is available and published too, but all they have to understand is that the count of corners doesn't matter in the championship. For historians of science the situation is much worse. The actual score is important, but evaluated only by individual scientists. But the scientists them-self care as well about the future chances of their research direction. If a historian thinks that the number of scientists working in a particular direction defines the actual winner he is wrong: The decision where to work depends heavily on the future promises of a research direction, not only on the actual situation.

Next, the rules them-self are less certain: Football rules have the aim to increase the fun of the game, but cannot be derived from this aim. To minimize frustrating discussions on the field, they have to be very certain. The rules of the evaluation of scientific theories have the aim to give us the best possible prediction-making tools. But there is an uncertainty in the resulting rules because of a conflict between two criteria for quality: security (no false predictions) and predictive power (more predictions). And there are no reason to fix such uncertainties, because there is no necessity to establish a unique winner. As a consequence, there remains some uncertainty not present in the case of football. This makes it easier to mingle them with the uncertain rules of forming a winning team.

Last but not least, the scientists them-self have often had quite wrong theoretical ideas of what they are doing in practice. Their analogy would be a football player who thinks that what counts are nice beautiful tricks before making a goal. As long as he recognizes that making goals is also important, he may be, nonetheless, a good football player. Similarly, these scientist are, often enough, remarkable scientists. But the job of a historian of science does not become easier.

All this does not change the conceptual difference between these two sets of rules. And if one misses this distinction, one gets confused.

There is another point shared by all three critics: They are, in different degree, but all influenced by the empiricist idea that one can derive the rules of science from observation of history of science. One cannot, for well-known reasons. And even if one removes the empiricist nonsense about derivation, and replace it by falsification, and even if one ignores the difference between rules about what should be done by scientists and the claim that real scientists have followed these rules (unconsciously, without having even formulated them) in the past, such a falsification remains extremely problematic.

There is a difference between me and Hoppe about this: Hoppe thinks falsification in humanities is impossible in principle, while I think it is possible in principle but extremely difficult. This difference is discussed below. If Hoppe is correct here, we simply have to throw away Kuhn (and Feyerabend as well). I do not object against falsification based on history in principle (and therefore consider them nonetheless as interesting reading). But this would be an extremely difficult thing. Not only because falsification based on human history is difficult in general, but also because of the special situation discussed above.

**6.10. Other examples.** Hoppe is certainly not the only libertarian with an oversimplified view of Popper. Huelsmann gives another example. For example, he claims that

[Popper] also claimed that only falsifiable statements can be 'scientific,' while the Vienna Circle posited that all non-verifiable statements are simply nonsensical.[15]

Wrong. Popper certainly has not made such claims. He names scientific *theories* empirical, if they, as a whole, allow to derive falsifiable statements, not if they consist of falsifiable statements only. Moreover, Popper was also aware of the problem Quine has named holism, namely that it is not a single theory taken alone, but a complex of several scientific theories, together with assumptions about initial and boundary conditions, which allows to derive falsifiable predictions. So, scientific theories contain a lot of non-falsifiable *statements*. Even all of the statements of a particular theory may be non-falsifiable. But, once it is possible to derive falsifiable statements, even if only in combination with other theories, the theory is nonetheless an empirical theory.

Again, the believer in absolute certainty will share this misunderstanding: Falsifiable statements can be tested, we can obtain certainty about them – so only they have to be scientific.

Mises is also not free of simplification of Popper:

The popular prestige that the natural sciences enjoy in our civilization is, of course, not founded upon the merely negative condition that their theorems have not been refuted. . . . Sitting in a room that is lighted by electric bulbs, equipped with a telephone, cooled by an electric fan, and cleaned by a vacuum cleaner, the philosopher as well as the layman cannot help admitting that there may be something more in the theories of electricity than that up to now they have not been refuted by an experiment. [13].

Indeed. Popper would agree as well, last but not least because he has, for this purpose, developed the “degree of corroboration” of theories, which describes exactly this “something more”. One could argue that he has not succeeded to give an appropriate definition for the degree of corroboration, but this would be another point. (I find the logic of plausible reasoning – probability theory as presented by Jaynes [29] – more convincing.) The purpose of the degree of corroboration is exactly to distinguish a well-tested theory like that of electricity in the example from almost untested theories which are not refuted by observation only because nobody has tested them.

Here, the believer in absolute certainty also agrees with Mises: We can be absolute certain about electricity.

So my hypothetical believer in absolute certainty shares not only almost all of the misrepresentations of Popper I have found in Hoppe’s writings, but also those of Huelsmann and Mises.

I have intentionally introduced this artificial person. I don’t want to make arbitrary guesses about Hoppe. There is a whole tradition of misrepresentation of Popper. Hoppe is only one representant of this tradition, and I have focused here my interest on him only because he is a fellow libertarian, and because searching for “Popper” has given the most hits in Hoppe’s writings. My believer in certainty is not a speculation about Hoppe, but my guess about what makes this whole intellectual tradition attractive for so many intelligent people. And I think, this guess explains a lot.

The fear of uncertainty is dangerous, and I conclude that the misunderstanding of Popper, the popularity of the distortion of Popper’s critical rationalism into a variant of empiricism, is one of the unfortunate and dangerous consequences of this widely distributed fear.

## 7. PRAXEOLOGY AND THE UNITY OF SCIENCE

Let's continue with another field of disagreement, a disagreement between me and whole Austrian school. It is the question of unity of the scientific method. The unifying idea of the Austrian school is that there is a strong, conceptual difference between economics and natural sciences, so that in economics one needs a completely different philosophical foundation.

The justification of this claim combines some errors which have been already discussed with some other arguments that these two domains of knowledge are too different to be covered by a unique scientific method.

An error I have already discussed is the misinterpretation of Popper as an empiricist. It has a fatal consequence: It is not recognized that there is a rational alternative to empiricism in natural sciences. So the natural sciences are left, with some reservations, to empiricists:

There is no doubt that empiricism and pragmatism are right as far as they merely describe the procedures of the natural sciences. . . . the experience to which the natural sciences owe all their success is the experience of the experiment in which the individual elements of change can be observed in isolation. The facts amassed in this way can be used for induction, a peculiar procedure of inference which has given pragmatic evidence of its expediency, although its satisfactory epistemological characterization is still an unsolved problem. [14]

But leaving natural sciences to empiricism is wrong. Empiricism is invalid not only in economics, but in natural sciences as well. There is no place at all where empiricist methodology is valid – it is invalid everywhere. A consequence of this error is that two things become completely confused: Reasonable arguments against empiricism, arguments which largely remain valid even in the domain of natural sciences, and the consideration of the epistemological differences between economics and natural sciences. The following quote from Mises is a nice example of such confusion:

It is impossible to reform the sciences of human action according to the pattern of physics and the other natural sciences. There is no means to establish an a posteriori theory of human conduct and social events. [14]

The second part is a correct argument against empiricism in economy, but does not prove the first – the Popperian pattern for physics does not establish any a posteriori theories too. Similarly the following quote:

Praxeology is a theoretical and systematic, not a historical, science. Its statements and propositions are not derived from experience. They are, like those of logic and mathematics, a priori. They are not subject to verification and falsification on the ground of experience and facts. They are both logically and temporally antecedent to any comprehension of historical facts. They are a necessary requirement of any intellectual grasp of historical events. [14]

Everything is correct (except for the single word “falsification”), but unnecessarily restricted to economy. Popper could say the same in almost the same words about the theories of natural sciences.

The second error is Hoppe's idea of a *Letztbegründung*, which I have discussed in section 4.4. It assigns an unjustified level of certainty to the foundations of praxeology, making it different from the theories of natural sciences (assumed to be derived a posteriori from experience).

What is left, if we extract anti-empiricism (shared by Popperian natural science) and the *Letztbegründung* (invalid everywhere), about conceptual differences between economics and natural sciences?

**7.1. History vs. Experiment.** There is, first, the difference that economics has to rely on history while natural sciences can design experiments:

... there is no experience of future happenings. But the experience to which the natural sciences owe all their success is the experience of the experiment in which the individual elements of change can be observed in isolation. ... History can neither prove nor disprove any general statement in the manner in which the natural sciences accept or reject a hypothesis on the ground of laboratory experiments.[14]

Unfortunately for the argument, there is a classical natural science which cannot arrange any experiments – astronomy. As economics, it almost completely depends on historical events, and on the very limited information about these events which can be seen in our neighborhood. Only in the last time we can do some experiments with satellites inside the Solar system. Despite this almost complete restriction to historical observation, lots of different astronomical theories are used to make predictions and are tested (and often falsified) by observation.

Moreover, let's not forget that it was the observational evidence provided by *astronomy* about the history of movement of the planets which has given us Newtonian mechanics – one of the most important physical theories we have ever had.

And let's not forget that another natural science – biology – also depends in a large part on the very special history of evolution on Earth. One should not forget the great role of historical evidence – in particular fossil records – for another great theory – Darwinian evolution.

And what about geology?

As one can see, a lot of theories of natural sciences have found their decisive support in history, not in especially designed and controlled experiments. It is the focus of empiricism on derivation from observation which is the base of this argument. The only more or less plausible scenario for such a derivation was a process of induction supported by repeatable controllable experiments. In this scenario the only problem was the problem of induction. Hard enough, unsolvable, but there was at least some plausibility that after thousands of repeated experiments one becomes bored and ignores the loopholes in the "derivation". No such plausibility could be reached if only history is available. Real science doesn't work this way, has never worked that way, and lived nicely with the evidence provided by history. In natural sciences as well as in economics.

**7.2. Direct access to the fundamental theory.** Another claimed but not that important difference is that in natural sciences we do not have direct access to the most fundamental theory, while in economics no such problem exists – we know a lot about human behavior because we are human beings our-self.

As a consequence of this, the postulates of praxeology are quite uncontroversial. One does not have to test them – they are more or less self-evident.

But the situation is really different only in fundamental physics. Everything else can be derived today, at least in principle, from the equations of fundamental physics. For chemistry, biology, geology and condensed matter physics and in large parts even for astronomy there is no problem of an unknown fundamental theory. The fundamental theory is known, one can take it from the current state of fundamental physics. There is no doubt that this current state is good enough for almost all of chemistry, biology, geology, and condensed matter theory. Really new physics in astronomy we have to expect only in the theory of the very early universe and near black holes.

The job of the theoreticians in these domains is to find good approximations. The fundamental theory is much too complicate to allow to predict anything in many applications. But this is a situation quite similar to economics. To derive predictions, we have to make simplifications in a situation where the fundamental theory is known.

So, in fact only an extremely small part of all natural scientists works in a domain where this difference exists today. In other words, this difference is also not relevant.

**7.3. Impossibility of exact predictions.** In economy, there are conceptual limits of the accuracy of predictions. In particular, human economy heavily depends on the actual human knowledge. But the actual theory of economics is certainly unable to predict accurately the state of human knowledge in future. Hoppe thinks that this argument proves that the Popperian concept of unity of science is invalid:

Interestingly, this argument was first advanced by Karl R. Popper in the Preface to his *The Poverty of Historicism* (London: Routledge & Kegan Paul, 1957). However, Popper entirely failed to notice that such an argument actually invalidates his own idea of a methodological monism (*Einheitswissenschaft*) and demonstrates the inapplicability of his falsificationism in the field of human action and knowledge.

Of course, it doesn't. It only adds another source of uncertainty. But falsification is hypothetical anyway, in natural sciences too. The hypothetical character of falsification is, therefore, a general property and does not contradict methodological monism.

The argument itself is nicely explained using the example of demand for sugar:

A person cannot know today what he will know about sugar one year from now. And he cannot know all the goods that will be competing against sugar for his money in a year. He can make a guess, of course. But since it must be admitted that future states of knowledge cannot be predicted on the basis of constantly operating causes, a person cannot pretend to make a prediction of the same epistemological type as, for instance, one regarding the future behavior of the moon, the weather, or the tides. Those are predictions that could legitimately make use of the assumption of time-invariantly operating causes. But a prediction about future sugar demand would be an entirely different thing.<sup>[3]</sup>

So there are limits of the predictability in the domain of economics. But does this prevent us from making predictions? Certainly not. These predictions are, of course, in some sense only guesses. In particular, we can guess that nothing special happens with the demand for sugar in the next year. This guess may appear wrong. But is the situation in physics really that different? Simplifications and approximations are the everyday tool in physics as well. Let's take the examples mentioned. One can predict the trajectory of the moon for a quite long time using the approximation that the moon is a point mass. An assumption which is clearly wrong – I can see the difference between a point and the moon even without a telescope.

What about weather prediction? A theorem by Arnold ([30] p. 341) about this uses even much funnier “approximations”, in particular “[t]he atmosphere is a two-dimensional homogeneous non-compressible non-viscous fluid” and “[t]he earth has the shape of a torus”. Do results based on the assumption that the demand of sugar will not change next year have an epistemological status worse than results about weather predictions based on such “approximations”? The Earth is certainly not a torus.

Nonetheless, the “approximation” is sound in the following sense: Arnold establishes an absolute boundary for the accuracy of the prediction of the weather. An accurate prediction of the weather for a period of more than two months is impossible. But the predictability becomes even worse if the weather becomes more turbulent. And the weather on Earth is reasonably even more turbulent than the weather considered by Arnold on the torus, which is approximately a “tradewind current” parallel to the equator with sinusoidal velocity profile. An example of another principle: If an approximation is justified or not also heavily depends on what one wants to derive.

So absolute boundaries for the certainty of predictions in economics are nothing special too. Chaos, which leads to similar absolute boundaries in natural sciences, appears not only in weather prediction. The unity of science is not endangered.

But what about the very conceptual difference that natural science have “time-invariantly operating causes” not available in economics? If one looks at quantum theory, in the variant used by most physicists, namely its minimal interpretation, there are no such causes. The absence of such a causal explanation for standard quantum theory has been criticized by people like Einstein and Schrödinger. As a defender of the “causal” de Broglie-Bohm interpretation of quantum theory, which gives such a causal explanation, I don't want to defend this minimal interpretation too. Nonetheless, it has to be acknowledged that this interpretation is today the mainstream interpretation, supported by a large majority. In other words, most physicists don't believe that a causal interpretation of quantum theory would be an important improvement, but consider this as some sort of old-fashioned prejudice.

And even if I don't think so, I have to accept that the pragmatic, empirical part of quantum physics does indeed not depend on these differences in the interpretation.

The misguided idea that Popperian falsification is certain shows up in Hoppe's argumentation against the unity of science too:

In the natural sciences, success means that so far your hypothesis has not been falsified; apply it again; and failure means that your hypothesis as it stands is wrong; change it. In our dealings with our fellow men, the implications are not, and never can be, as

clear-cut. Maybe our prediction was wrong because some people, as can happen sometimes, acted out of character – in this case we would want to use our hypothesis again even though it had been apparently falsified. ([10] p. 73)

The situation in natural science is, in fact, far away from being clear-cut. There is no empirical falsification which cannot, according to Popper, be criticized and rejected as well.

**7.4. Unfalsifiability of research directions.** Unity of science is not endangered as well by Hoppe’s argument that falsification cannot reject whole research directions, like, in economics, socialism:

And whatever the observable results are, the original socialist idea – the “hard-core” of ones “research programme” as the neo-Popperian philosopher Lakatos would have called it – can always be rescued easily by pointing out some previously neglected, more or less plausible variable, whose noncontrol is hypothesized to be responsible for the negative result, with the newly revised hypothesis again needing to be tried out indefinitely, ad infinitum.[7]

The observation is correct in principle: What is falsified is always a particular theory, not a whole research direction. But this is the case in natural science as well as in economics. In particular, research programs in natural sciences may revive after a long death. So, the particle theory of light, created by Newton, was dead for a long time after interference effects have been observed and only the wave theory of light was able to predict them correctly. But, at least partially, as a research program, it has been revived by Planck and Einstein in early quantum theory. Today the particles of light, the photons, are an established part of quantum physics.

Of course, sometimes an experimental refutation is strong enough to kill whole research directions. Say, for example, the theory that stars are heated by burning coal or oil instead of thermonuclear reactions. The case of socialism is similar. Here I almost completely agree with Hoppe (I would only, as the reader will already guess, omit the “irrefutably”):

The difference in the results has been striking. Yet no social experiment was necessary to find this out. Naturally not all empirical details, but the fundamental outcome of the German experiment could have been predicted with certainty by those familiar with the principles of economic theory, and in particular the theoretical economic analyses of socialism by the Viennese (Austrian) school, most notably Ludwig von Mises’. In his famous “Die Gemeinwirtschaft: Untersuchungen über den Sozialismus”, of 1922, Mises irrefutably demonstrated what the East Germans were forced to find out the hard way: that socialism must end in disaster. [?]

The ineducable communist is of course able to explain away even the striking differences between East and West of Germany. The East has always been poor, had to pay more reparations after the war, there have been errors of the political leaders and so on. My favorite in the list of funny excuses is that the capitalists in the West, afraid of the example of the East, have paid more to their workers to prevent

a socialist revolution. But is that a point against the power of Mises' scientific prediction of the failure of socialism? Does this change the fact that socialist theory has been refuted by observation too? Are such lame excuses more than a joke for a scientific minded person?

Of course, my hypothetical believer in absolute certainty disagrees. He likes only certain falsifications. And a falsification which allows the socialists to invent some excuses, however lame, is not absolutely certain. But I don't think that we should throw away one of the most impressive arguments against socialism – the not very attractive results of the socialist experiments.

By the way, these refutations have an important effect even if, in principle, one can propose a new socialist model, different from the ones which have been tried. The classical Marxist strategy to *refuse to discuss* socialist models from point of view of economic theory can be rejected now in a much more powerful way. The old Marxist-Leninist "let's make the revolution and see what happens" would be obviously a path to repetition, and, given the results, such a repetition is far too dangerous to allow it. But this refusal to discuss models was a key of the success of Marxism. To destroy detailed theoretical proposals for socialist societies is, instead, an easy exercise to be left to undergrads.

**7.5. Predictions about the consequences of actions.** It does not follow from my arguments that there are no important methodological differences between economics and natural sciences. One certainly should not mingle fundamental conceptual differences with gradual differences. Fundamental conceptual differences would require the application of a conceptually different method, like classical rationalism in economics but a variant of empiricism in natural sciences. Gradual differences allow for a unique method of science. But the gradual differences may become very large, and, in this case, will lead to great qualitative differences of what really has to be done by the scientists.

So it is important to point out such important differences even if they, from a purely philosophical point of view, are only gradual.

Such an important difference is that, in applications of economics, we are interested in a question which is usually only of minor importance in natural sciences. It is the question of prediction of the consequences of our actions.

This is something we can compute in natural sciences too: We have everything we need to compute how a modification in the initial conditions modifies the final result. And sometimes such predictions are interesting: Say, in applications like ballistics. Ballistics does not make much sense if uncontrollable influences modify the result in unpredictable ways. Instead, such results may be nonetheless interesting in economics. We want to know if our action improves our fate or not. If, say, my different action leads to a difference in the result of ten dollar, this is interesting for me even if the unpredictability of external influences is much greater and does not allow to predict if I will win 1010 instead of 1000 dollar or lose 990 instead of 1000 dollar. This is, from a philosophical point of view, only a gradual difference, not a conceptual one. Ballistics with too much uncertainty may become useless if I want to hit a particular point. But the situation may be similar if the aim is to throw something away as far as possible.

Nonetheless, this difference has a great influence on the method of economics. The situation as described is the typical one: We don't know enough about the actions of all the other people. The action of interest – even if it is that of the

government – is only the action of one actor. The actions of the other people are unpredictable in principle. The best we can make here are simplifying assumptions. Nonetheless, the overall result will be unpredictable, or predictable only with extremely large error bounds. Despite this, the situation is different if we are interested only in the question how the situation changes in dependence of the single action of interest. To compare the two situations, we have to assume that everything else remains unchanged. So all the uncertainties are no longer relevant – we have to consider them as fixed. Thus, to derive the result we are interested in appears much easier than to derive a quantitative prediction in the usual sense.

So, we have, as special differences in the domain of deriving predictions, a special type of economic prediction which is, on the one hand, especially interesting, and, on the other hand, easier to derive. But above parts of the difference are only gradual. We may be interested in such predictions in natural sciences too (ballistics), and they remain falsifiable at least in principle. So I have to disagree with Mises claim that

Neither experimental verification nor experimental falsification of a general proposition is possible in its field.[14]

Indeed, the fact that other influences lead to big errors in the predictions about the two results taken separately does not mean that the errors are too big to allow their application as a falsification. It simply means that this becomes much harder. It is a gradual difference.

Consider, for example, the prediction that minimum wages lead to unemployment for the poor. Let's try to test this using history – conceptually in no way different from the use of history in astronomy. The claim predicts that historical events of states which establish or increase minimum wages will be followed by increasing numbers of unemployed poor, where poor people are those who had, before the event, wages below the new minimum. In each particular case, there are lots of alternative explanations why this prediction may fail. The most important reason would be, I guess, that the law is only for the books and not really enforced. Other things may happen – a boom, or a war so that unemployed young poor will be forced into the army. But there have been enough of such events in history, and so one can use statistics to extract more information. And I would say that if in almost all cases increase of minimum wages is followed by less unemployment for the poor something is wrong with the theory. Or, in other words, this would be a falsification. Of course only in the Popperian meaning of the word, that means, it has only hypothetical character.

Once the differences have only gradual character, the philosophical foundations of science remain the same for all domains of science. So there is something like a unity of science, of the scientific method. But, on the other hand, these gradual (in the philosophical sense) differences lead to important differences in what people are actually doing. Simply copying what natural scientists do is stupid. But it is also stupid to exclude predictions and observations completely from economics, restricting it in an artificial way to derivations from a few postulates of praxeology.

**7.6. Freedom of stupidity?** The following argument of Mises I don't understand:

In the case of natural phenomena the interpretation of an event must not be at variance with the theories satisfactorily verified by experiments. In the case of historical events there is no such

restriction. Commentators would be free to resort to quite arbitrary explanations. Where there is something to explain, the human mind has never been at a loss to invent ad hoc some imaginary theories, lacking any logical justification. [14]

Whatever it means, the human mind is as free in natural sciences too. Okay, if one believes in empiricism, one may think that there is no such freedom, because theories are derived from observation. But in Popperian science, theories are free inventions of the human mind, not restricted by anything, except that they are open to criticism. But this is what we have to do with such inventors of ad hoc imaginary in economics too – to criticize their nonsense. In natural sciences as well as in economics.

## 8. CONSEQUENCES OF THE AMPUTATION OF THE SCIENTIFIC METHOD

The hope for absolute certainty has a natural fatal consequence – the rejection of methods which are unable to give absolute certainty. Of course, from point of view of critical rationalism, except for pure mathematics no scientific method allows to give absolute certainty, so that, as a consequence, all scientific methods would have to be rejected. But, of course, this is not recognized by those who hope for absolute certainty. They accept some methods as able to give absolute certainty and reject other methods as insufficient, unable to give certain results.

But from point of view of critical rationalism, the rejection of scientific methods because of an impossibility to obtain absolutely certain results is completely nonsensical, and a dangerous and harmful amputation of the scientific method.

While the main problem considered in the last section was the defense of unity of science, as a part of critical rationalism, I have defended, in fact, also economic science from this amputation. In particular, the consideration of the role of empirical predictions in the last section was also directed against a dangerous amputation of libertarian economics – the complete refusal to make empirical predictions. Here I want to consider some other points where the amputation of the scientific method leads to unfortunate consequences for libertarian economics.

All the cases below have to be considered only as rough ideas. Each of these points deserves, in fact, a more detailed consideration in a separate paper.

**8.1. Good laws as public goods.** An established part of Austrian economics is the rejection of the theory of public goods. Rothbard has justified this rejection in the following way:

As for the recipients, they are being forced by the State to pay for benefits that they otherwise would not have purchased. How can we say that they “benefit”? A standard reply is that the recipients “could not” have obtained the benefit even if they wanted to buy it voluntarily. The first problem here is by what mysterious process the critics know that the recipients would have liked to purchase the “benefit.” Our only way of knowing the content of preference scales is to see them revealed in concrete choices. Since the choice concretely was not to buy the benefit, there is no justification for outsiders to assert that B’s preference scale was “really” different from what was revealed in his actions. [12]

But this argument shows only that we cannot prove, cannot be certain, that the recipient would have liked to purchase the “benefit”. In a physics context, I would have rejected this as typical positivistic rejection of unobservables. In the humanities, the extremist positivistic position is known as behaviourism, and the argument is, indeed, one which could be easily made by a behaviorist. But to criticize Mises and Rothbard – classical rationalists – as behaviorists seems nonsensical at a first look.

But it is not that nonsensical if one recognizes that classical rationalism and positivism share the preference for absolute certainty, and, as a consequence, the prejudice against uncertain, hypothetical knowledge. Then it remains to remember that to reject hypothetical knowledge completely is simply impossible – one would have to reject science completely. So, what is rejected as uncertain is quite arbitrary. And so there is nothing strange with the observation that a particular classical rationalist may reject some parts of scientific knowledge which a particular empiricist doesn’t reject. Anyway, the rejection is not based on objective properties of the rejected theory.

In this particular case, the rejection of public good theory seems motivated by the misuse of this theory for the justification of the state. This was, in fact, an important application of this theory in the past. But this has changed with public choice theory. The point is that the decision-making process in a democratic state has an own public good problem: Good laws, which benefit everybody, are a classical example of a public good. Instead, bad laws, which harm the majority in favour of a small interest group, are a private good of this interest group. The smaller the group of those who benefit from a law, the less important is the public good problem for this group. As a consequence, public good theory, applied to the state, predicts that democratic laws tend to be bad laws, laws in favour of small, concentrated interest groups which are harmful for the majority.

So, what has been, in the past, an argument in favour of the state, is today an argument against the state. Indeed, the public good problem on markets is restricted only to some goods. The public good problem of the state exists for all decisions of the state, for all laws. So there are now a lot of questions where the market solution is clearly better, because for these questions there is no public good problem on the markets. On the other hand, it is not clear at all if for the remaining questions, where a public good problem exists for markets as well as for states, the state solution or the market solution is less evil.

Thus, with the complete rejection of public good theory we loose a strong and important argument against the democratic state.

We also loose a strong argument for decentralization. Indeed, the greater the state, the greater the corresponding public good problem for good laws. By returning the decision making to smaller entities we can clearly reduce the related public good problem of the corresponding decision making.

**8.2. The rejection of cardinal utility.** A standard Austrian position is that utility makes sense only as an ordinal, not as a cardinal. For example, Rothbard notes:

Value scales of each individual are purely ordinal, and there is no way whatever of measuring the distance between the rankings; indeed, any concept of such distance is a fallacious one. [12]

I disagree. Simple introspection tells me that utility differences have a quantitative aspect. The counterargument is an essentially positivistic one – unobservability:

The Austrian takes a position midway between that of the behaviorist and Caplan’s neoclassicism. For the behaviorist, there is no such thing as motive, introspection; all is human behavior. For Caplan, all is motive and introspection; no behavior whatsoever is necessary to tie these phenomena to the real world. In the praxeological perspective, there is both, and the latter is necessary to demonstrate the former. ([33] p. 24)

So because you cannot measure how important the difference between the green and the blue sweater is for me, the (subjective) fact that I don’t care at all but, to avoid the fate of Buridan’s Ass, have nonetheless made a random choice, does not exist?

But the cardinality of our preferences (about which I’m sure because of introspection anyway) influences my decisions and has, therefore, implicit observable consequences. This is something rejected by positivism – every bit should be observable – but accepted by Popperian science.

Indeed, take the decision to accept or refuse a proposal for a bet. Ordinal utility gives me only the following order: Winning > refusal > losing. Introspection tells me that my decision depends not only on this, but also a comparison of the cardinal aspects – how much I win, in comparison with how much I lose.

**8.3. The rejection of subjective probabilities.** It also depends on my personal expectation of my chances to win. Which is also rejected, following the positivistic frequency interpretation. For example, Hoppe supports here Richard von Mises (also author of the “Lehrbuch des Positivismus”):

As Richard von Mises, the originator of the frequency interpretation of probability, has unambiguously stated: the application of the term probability to a single event is “utter nonsense.” It is possible to speak about numerical probabilities only in reference to a properly defined collective ([9] p.19),

against subjectivist attempts to apply the apparatus of probability theory to plausible reasoning. How unreasonable this rejection is can be estimated reading Jaynes [29].

In above cases, Austrian economics is on the side of positivism against subjectivism.

## 9. CONCLUSIONS

The dangerous dogmatic dream of absolute certainty was the main target of this paper. This dream is dangerous because of several reasons. First, it leads to dogmatism. Some hypotheses become dogmas, are considered as indisputable, incontestable, unquestionable, and critical evaluation of the dogmas is prevented. Even if, by a happy accident, the dogmas appear true, this prevents further development of arguments in their favour. But usually at least some of them are wrong. Then the consequence is really fatal – the natural way to detect errors, by considering arguments of those who doubt, is closed – the dogmas are beyond doubt.

As dangerous is the other side of dogmatism – skepticism about disputable, hypothetical knowledge, ignorance of plausible reasoning. Without a possibility to estimate the plausibility of hypothetical knowledge – plausible reasoning is rejected too – the resulting decisions what to reject as uncertain have to be arbitrary. So it is no wonder that the rejected theories appear in fact often more plausible than those accepted as dogma.

And there is a third danger, a danger especially interesting to libertarianism – the connection between the ideology of certainty and etatist ideology. Markets and free societies cannot promise certainty. Prices and wages vary, and it is impossible to predict with certainty how free people will behave. It is the state which promises certainty, with fixed laws and regulations about almost everything. Of course, this certainty is illusory, and almost the only certain thing about regulations is that they have adverse side-effects. But there is not much disagreement about this among libertarians, and, therefore, this part of the danger of certainty was not the focus of this essay. I have focused my interest on other points – points where the hope for absolute certainty misleads the libertarian movement.

One fatal consequence is the rejection of critical rationalism – the philosophy which accepts, from the start, and as one of its main points, that our human knowledge about reality always remains hypothetical. I have considered here the arguments which can be found in libertarian literature against Popper’s critical rationalism and found that they criticize, instead, a trivialized, empiricist version of fallibilism. And I have identified the hope for absolute certainty as the possible explanation of this distortion.

The hope for absolute certainty leads also to an uncritical acceptance of proposals for a “Letztbegründung”, a final, incontestable justification, of libertarian ethics. Our evaluation of this justification – “argumentation ethics” – has found that its “proofs” not only fail to give the promised certainty. They appear to be, at best, plausibility arguments. Even worse, even as plausibility arguments they appear quite weak, often weaker than classical plausibility arguments which do not even pretend to give incontestable proofs. Even more, I have shown not only errors in the particular applications of the method of argumentation ethics. The method is in itself unable to reach the promised results: All what it is able to give is a critique of particular theories. And even these reasonable applications do not give much: Philosophical theories against which it may be applied have to have very special properties, so they seem to be rare exceptions and it seems easy to immunize them against such arguments.

The influence on economic theory is equally fatal. Here, classical rationalism is used as the methodological foundation. The result is an artificial methodological amputation of libertarian economics. Only some “aprioristic” methods are accepted as valid, as able to give absolute certainty. Other methods are rejected as unable to give absolute certainty. This leaves a lot of reasonable scientific methods to defenders of the state, methods which, instead of supporting the state, are able to give strong arguments in favour of a libertarian society.

Dogmatism would be dangerous even if there would be methods to establish truth with absolute certainty. Humans err, and they may err even in the application of these methods. But there are no such methods. I have evaluated the most popular one, and it appeared to be completely misguided. No claim of indisputability can be made based on such argumentation, because there remain a

lot of various possibilities to dispute these claims. The main loopholes are the uncertainty of the meaning of the words of common language, the theory-dependence of the meaning of scientific notions, and the theory-dependence of the interpretation of the performances of those who dispute the claim. One cannot avoid the possibility to dispute the theoretical background which is necessary to interpret the meaning of the proposition in question – a background which is a prerequisite for understanding the meaning of the terms used in the proposition, but which is seldom specified explicitly.

The consideration of particular examples has shown that all the propositions considered remain disputable. Moreover, even if in some cases the “proof” survives as an interesting and useful plausibility argument in favour of the “proven” thesis, in other cases the argument itself is completely misguided (the proof of the Golden Rule, of self-ownership and property rights), and in other cases the “proven” proposition is simply wrong (men don’t know what it means to act in the Misesian meaning of the word, and there is no norms for argumentation). The arguments themselves, without the background of hope for absolute certainty, often seem so bad that their popularity can be explained only by wishful thinking.

As a part of argumentation ethics, I have identified a meaningless theory of truth – the consent theory of truth – which seems preferable only if the dream of absolute certainty distorts our thinking. In fact, the only reasonable objection against the correspondence theory of truth is that there is no criterion which gives us absolute certainty of our knowledge – something we would better simply accept as an unfortunate fact of life.

The dream of absolute certainty is also behind the errors of the two great philosophies of science – empiricism and classical rationalism. Above philosophies try to find the Holy Grail of absolute truth. They have failed miserably, leaving important domains of human knowledge as victims of the rejection of “uncertain” knowledge.

The victims of empiricism have been metaphysics and ethics – two important domains of scientific knowledge which have been completely removed from the domain of empiricist science. As a consequence, these domains have been left to obscurantism and relativism. The very idea that a majority opinion about laws and morals may be deeply wrong has been lost.

The victims of classical rationalism, in particular Austrian economics, have been all the uncertain, approximate methods of economics, in particular the very possibility of making testable empirical predictions.

Above have rejected the logic of plausible reasoning – the rules of rational decision-making under uncertainty and insufficient information.

Let’s end with the observation that the very dream of absolute certainty is a powerful weapon in the hands of etatist propaganda. The state sells itself as the organization which makes everything certain and clear, by establishing certain laws obligatory for everybody and by enforcing them with police, by regulating and controlling the “chaotic, unhampered” market, and by providing social security.

Thus, the fear of uncertainty is dangerous for the libertarian movement. As I have tried to show in this paper, it leads to dangerous theoretical and philosophical misconceptions. Its misuse by etatist propaganda is the other part of this danger, a part which I have not considered here.

The other question which I have not considered here is what libertarians can do about this.

There is something to be said to the common man. Certainty is something valuable for the weak, for those afraid of uncertainty, for those lost if something goes not as expected. For those who believe in their own abilities, in particular their ability to handle a new, unexpected situation, certainty is not the highest value. But even those who are weak, afraid, but not stupid, are able to recognize that our world has the unfortunate property of being uncertain, that certainty would be nice but not realistic. So let's leave the dream for absolute certainty to those who are too weak to face uncertainty and stupid enough to mingle their dreams with reality.

The common man can defend himself. The preference for certainty in everyday life is a rational, reasonable feeling. It suggests to prefer the actual environment, with its well-known dangers and risks, in comparison with a new, completely unknown one. This may be the choice of the weak, but it is a rational choice.

But at least for us, scientists and philosophers, there is no such excuse. It is our job to investigate the new, the unknown, and it is the job of our choice. And if there remains some unconscious fear of uncertainty, we should detect it and get rid of it consciously. And we should give a positive example: "Uncertain? So what? Everything is uncertain." should be our guiding rule.

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