Philippe van Basshuysen

Dawid et al.'s [2015] no alternatives argument: an empiricist note

Article (Published version) (Refereed)

Original citation:
van Basshuysen, Philippe (2015) *Dawid et al.'s [2015] no alternatives argument: an empiricist note*. Kriterion: Journal of Philosophy, 29 (1). pp. 37-50. ISSN 1019-8288

Dawid et al.'s [2015] No Alternatives Argument: an empiricist note.[1]

Philippe van Basshuysen

Abstract

In a recent paper [2], Dawid, Hartmann and Sprenger claim to prove the possibility of non-empirical theory confirmation via the *No Alternatives Argument*. In this note, I argue that from an empiricist point of view, their "proof" begs the question in the sense that it cannot convince someone who has not already been convinced of non-empirical theory confirmation before.

Keywords: No Alternatives Argument (NAA), theory confirmation, Bayesian networks, empiricism, Principle of Indifference

1 Outline

In a recent paper [2] which has turned out to be rather influential (cf. [4]) – even in popular scientific discussions ([6], [7]) –, Dawid, Hartmann and Sprenger (DHS) argue in favour of the validity of the *No Alternatives Argument*. The No Alternatives Argument (NAA), often used by scientists, politicians, journalists, in the court room and in everyday life – roughly concludes from a lack of alternatives to a hypothesis or theory to the truth of that hypothesis/theory.[2] The argument is regarded as highly problematic in many contexts. For example, in the courtroom, where the principle "in dubio pro reo" is in force, NAA is usually not sufficient a reason for a conviction. In the following, I will restrain the analysis (as do DHS) to scientific hypotheses. The reason is that the applicability of NAA to other contexts may not be equivalent (e.g. because of different standards of what counts as an alternative hypothesis) – and would be an interesting field for further research.

DHS claim that "if valid, [NAA] would demonstrate the possibility of non-empirical theory confirmation" ([2], 2). They give a probabilistic analysis of the argument, representing the target probability

Kriterion – Journal of Philosophy (2015) 29: 35–47.
http://www.kriterion-journal-of-philosophy.org

function with the help of a Bayesian network, and conclude that under certain (plausible) conditions NAA can indeed give confirmation to a hypothesis. Thus, the proof of the possibility of non-empirical theory confirmation. In this note I argue that from an empiricist point of view, however, the "proof" begs the question in the sense that it cannot convince someone who has not already been convinced of non-empirical theory confirmation before.

I sketch DHS's argument in (2); in (3), I argue that at least one of the assumptions needed for their argument presupposes non-empirical theory confirmation and thus stands on shaky grounds. In (3.1), a counterexample to this assumption shows that it must in fact not be expected to hold. I conclude in (4), urging caution in the application of Bayesian networks to normative problems.

2 DHS's argument

It is usually undisputed that an empirical hypothesis H is confirmed or disconfirmed by a piece of evidence E that at least partially falls into the hypothesis' domain, or does not fall into its domain but "can be related to H by another scientific theory" ([2], 2). If one is in favour of Bayesian confirmation theory, the degree of confirmation can be calculated with the help of probability theory and using some confirmation measure. This is due to the hypothesis being *probabilistically dependent* on the evidence. Irrespective of which confirmation measure (if any) one wishes to use, the evidence incrementally confirms the hypothesis in the Bayesian sense just in case $P(\mathrm{H}|\mathrm{E}) > P(\mathrm{H})$.

In the case of NAA, the situation appears to be more difficult because the "evidence" does not seem to raise/decrease the probability of the hypothesis since it does not fall even partially into its domain: the fact that – despite considerable effort – we have not found any alternative to the hypothesis in question is not usually a fact the hypothesis talks about,[3] nor can it *prima facie* be related to it by another theory. Thus, the hypothesis is on the face of it not conditionally dependent on the evidence; so the evidence does not confirm or disconfirm it.

Let's consider DHS's argument for NAA – more precisely, that NAA does in fact constitute confirmation in the Bayesian sense – in detail. What DHS claim is that there is a common cause – a mediating statement that *does* establish a probabilistic dependency between a hypothesis, H, and us not having found suitable alternatives to H: namely, *that there really aren't many alternatives to* H. This mediating statement supposedly has direct influence on both the scientists not having found suitable alternatives and on the probability of H. This is because arguably, less alternatives make it less likely that scientists will find any of them; and arguably less alternatives to H make it more likely that H is the true hypothesis (this will be the critical assumption challenged in this note). According to DHS, via this mediating statement confirmation is possible.

I give a (simplified) formal representation of this line of argument. Define the propositions:

- H: the hypothesis we are interested in;
- NA: the scientific community has not yet found an alternative to H satisfying conditions Ω;
- $\mathrm{Y_k}$: there are k alternatives to H satisfying conditions Ω.

A couple of remarks:

(1) What counts as a hypothesis? Let us assume a very inclusive definition: any set of mutually non-contradictory (scientific) statements is a hypothesis.

(2) According to DHS, Ω subsumes the following conditions: to satisfy a set of theoretical constraints C; to be consistent with existing data D; and to give distinguishable predictions for the outcome of some set ϵ of future experiments ([2], 4). The conditions are deliberately left unspecified, since it is on the scientists to decide which hypotheses count as real alternatives.

(3) It might seem odd that H is a set of statements in the object language while NA and $\mathrm{Y_k}$ are statements in a metalanguage. This shows that we are not concerned with normal evidence that falls into H's domain, but rather with observations about H. Nothing hinges on this language-metalanguage distinction though. We could form a statement equivalent to H in a metalanguage (let T: "H is true").[4]

DHS [2] then specify their target probability function with the help of a Bayesian network. As usual when dealing with Bayesian nets, let an italic proposition denote the respective random variable. NA and H are propositional variables taking the values y and n, whereas Y_k takes

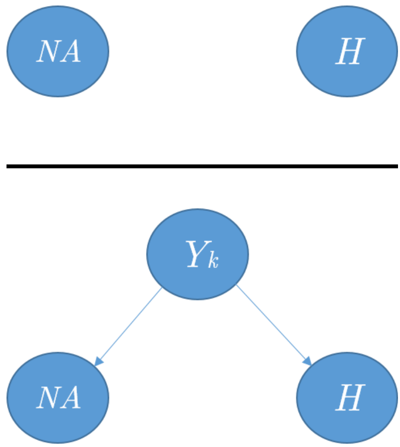

Figure 1: Below part: Bayesian net representing the target probability function in [2] (simplified); the mediating variable Y_k establishes a dependence between NA and H which, before learning about Y_k, are independent (above part).

values in the natural numbers. Fig. 1 shows two Bayesian nets, one representing the situation before learning about Y_k (above), and one a simplified model as targeted by DHS [2] for the situation after learning about Y_k (below).

Under some supposedly plausible conditions on the probability table, it can then be calculated that $P(\mathrm{H}|\mathrm{NA}) > P(\mathrm{H})$, i.e. NA confirms H. We do not need to specify all these conditions here since they do not affect this analysis (for details see [2] or [4]). To give an example, one crucial condition for a successful confirmation (which might appear to be a problematic one but is not considered here) is that $P(Y = \infty) < 1$. Note that two very important conditions are the assumptions given above, i.e. that both NA and H are dependent on Y_k in a specific way – which will be the object of the next section.

3 Problems

What I would like to draw the attention to – although I am not saying all the other assumptions specified in the Bayesian network in Fig. 1 are indisputable – is the dependency of H on Y_k. In assumptions A4 and A5 ([2], 8), DHS specify the nature of the alleged dependency formally: namely, that the conditional probability $P(\mathrm{H}|\mathrm{Y_k})$ is non-increasing in k, and that there is at least one pair $(i, k) \in \mathbb{N}^2$ with $i < k$, for which $P(\mathrm{H}|\mathrm{Y_i}) > P(\mathrm{H}|\mathrm{Y_k})$; i.e. *more alternatives don't make the hypothesis more likely, and there is at least one case where more alternatives make the hypothesis less likely.*

Now why should this dependency be the case? Just like NA, the mediating statement $\mathrm{Y_k}$ does not usually fall into the hypothesis' domain,[5] so this dependency is not trivial but in need of an argument. What is their argument? They give an informal justification earlier in their paper ([2], 4):

"We assume that scientists who develop a theory in accordance with available data do not have a perfectly reliable method to select the true theory if false theories can be constructed that are also consistent with the available data. Under this condition, a lower number of possible scientific theories that can account for a certain set of empirical data increases the degree of belief that the theory developed by scientists is adequate."

Note that this is a stronger claim than the non-increasing conditional probability needed in the formal execution of NAA (see above). This doesn't matter though since I argue that any direct dependency is suspicious from an empiricist's point of view.

Let's consider this argument in greater detail. DHS claim that a smaller set of alternatives makes it more likely for the scientists to choose the correct hypothesis, thus *raises the degree of belief in the theory chosen by the scientists.* First of all, the question is what they mean by the indefinite "degree of belief" – whose belief exactly?

It could be some ordinary person's, or some scientist's, or some scientist community's (etc.) belief. Now it might well be that there are situations where ordinary people, some scientists, or scientific communities think this way. This claim is wholly descriptive and would need an empirical backup. However, the goal of the argument is to show why NAA is *valid.* A description or psychological investigation into how people actually reason does not suffice for this claim – we are in need of reasons for why this way of inferring is a good one. In short, the subjective route has no normative grip and does not succeed. I do think, however, that the subjective interpretation of the argument might yield a plausible psychological explanation of why NAA is used (cf. (4)) – this could be the object of an empirical investigation.

Dawid et al. choose the more objective route (cf. [2], 10 f.) in which it is a *rational constraint* that $P(\mathrm{H}|\mathrm{Y_k})$ be non-increasing in k. But why is this rational? Why should the probability of a hypothesis depend on the number of alternative hypotheses satisfying Ω? Their argument (as cited above) is that the smaller the set of rival hypotheses, the more likely it is that scientists chose the true one when they "picked" their hypothesis, since they "do not have a perfectly reliable method to select the true [hypothesis]" (4). However, this formulation is confusing. First of all, scientists don't pick hypotheses like lottery tickets but rather put them forward or learn them from other scientists, and then theory choice is a complex matter (e.g. Kuhn [5]). Now in this case, NAA, there is no choice whatsoever involved since there is only one hypothesis H the scientists have arrived at, and one could possibly choose from. We are interested in the probability of H, conditional on the number of its alternatives. To sum up: we are not talking about the probability of scientists choosing the true hypothesis, but about the probability of some hypothesis – the one the scientists worked out.

Now there is not yet any argument to the effect that the probability of H be different if there are a hundred alternative hypotheses than if there are a million alternative hypotheses to it. That scientists "do not have a perfectly reliable method to select the true theory" is – at least *prima facie* – completely useless a reason for this claim. One could argue that the probability of scientists "picking" the true hypothesis together with the fact that they picked H, have an influence on the probability of H. This, however, confuses the logical structure of the argument because it *presupposes non-empirical evidence.* The structure is thus: we want to evaluate the probability of a hypothesis. This evaluation is based on empirical evidence. Sometimes, there is also reference to an authority involved – particularly in the layman's belief in scientists' theories. These "arguments from authority" are made in many contexts, and may be rational in many situations. The point here is, however, that it is another attempt to make use of non-empirical evidence and – if successful or not – cannot prove the possibility of this kind of evidence. From an empiricist point of view, the fact that scientists have a probability x of choosing the true hypothesis and that they choose H *prima facie* have no influence on the probability of H.

The point becomes clearer when we ask who is to evaluate a hypothesis. In principle, anyone can; but we are concerned with the validity of an argument for non-empirical evidence, so our standards are high: a "perfectly rational perspective", if you will. The agents that come closest to this perspective in the case of scientific hypotheses arguably are the scientists themselves. Now it would be odd if someone in a scientific community said, "our subjective probability of H raises *because* we believe in it". The fact that we believe in a hypothesis does not give extra boost to its probability – if it did, by iterated conditionalisation we could boost any hypothesis to some desired threshold (which may be certainty).

Once we leave references to other non-empirical evidence aside, there is no obvious reason to the effect that the probability of a theory be dependent on the number of its alternatives (in the described non-increasing way, or in any way). To establish such dependence would imply reference to some principle of rationality governing the probability function. The natural candidate for such a principle (although there might be others) is the *Principle of Indifference* (PI). Applying PI, we get $P(\mathrm{H}|\mathrm{Y_k}) = 1/\mathrm{k}$. However, it seems suspect to think about the truth of scientific theories like the winning of a lottery. Besides the usual arguments against PI,[6] its application is particularly questionable in this case, for *we are not in a situation of ignorance*, of the absence of any reason to expect one event rather than another. Rather, we hold H because we believe it is true. How could we reasonably assign the same probability to H as to a number of unknown hypotheses – hypotheses we have absolutely no idea about?

In conclusion, instead of proving that there is non-empirical evidence, the argument in fact seems to imply a non-empirical move: it presupposes that the probability of a hypothesis is dependent on how many alternatives it has, and to argue for this claim seems to involve either a regress to non-empirical evidence (argument from authority), or some doubtful application of some doubtful principle (PI). Anyone unconvinced of this principle will remain unconvinced of the possibility of non-empirical evidence.

3.1 A counterexample

The principled argument in (3) will become stronger if there is a case in which $P(\mathrm{H}|\mathrm{Y_k}) > P(\mathrm{H}|\mathrm{Y_i})$ for some H, with $(i, k) \in \mathbb{N}^2$ and $i < k$. One situation where this may be the case is the following. There are ten mutually exclusive and jointly exhaustive hypotheses, $\mathrm{H_1} - \mathrm{H_{10}}$, relative to Ω. Now suppose one of the hypotheses, $\mathrm{H_1}$, becomes impossible – maybe as a result of an additional restriction on Ω, or of learning about contradicting data (in which case that data must of course be assumed to be certain). Now suppose further there is some background hypothesis, $\mathrm{H_b}$, whose probability diminishes after learning $\neg\mathrm{H_1}$. This in turn makes some of the alternative hypotheses positively related to $\mathrm{H_b}$, $\mathrm{H_2}$ say, less likely, while the probability of other hypotheses not dependent on $\mathrm{H_b}$ (say $\mathrm{H_{10}}$) overproportionally increases. So learning about one alternative

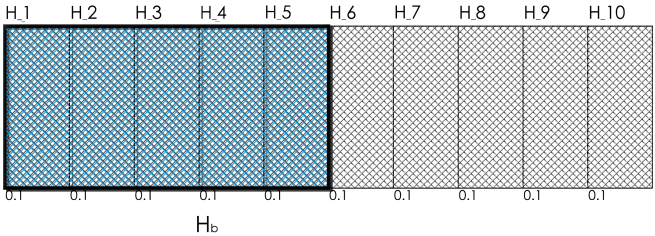

Figure 2: Boolean algebra representing probability function P_i; there are ten rival hypotheses, and a background hypothesis positively related to H_1 – H_5.

"falling out" of the sample space *can reduce* the probability a rational agent may assign to hypotheses.

The idea can be realised through different probability distributions (and could be generalised but for our purpose one plausible instance is sufficient). Consider the following probability distribution. Imagine

Case1. There are ten hypotheses, so $Y = 9$ for each of $\mathrm{H}_1 - \mathrm{H}_{10}$; and we have a background hypothesis $\mathrm{H_b}$. We form a probability function, P_i, obeying the following assumptions:

- Let the initial probabilities:
  - P_i is equally distributed over H_1 through H_{10}, so $\mathrm{H}_1 = \ldots = \mathrm{H}_{10} = .1$
  - $P_i(\mathrm{H_b}) = .5$
- $\mathrm{H_b}$ follows from H_1, H_2, H_3, H_4, and H_5, and its negation follows from the rest of the hypotheses: $P_i(\mathrm{H_b}|\mathrm{H}_1) = \ldots = P_i(\mathrm{H_b}|\mathrm{H}_5) = 1$ and $P_i(\mathrm{H_b}|\mathrm{H}_6) = \ldots = P_i(\mathrm{H_b}|\mathrm{H}_{10}) = 0$.

Case1 is displayed in the Boolean algebra in Fig. 2.

Case2. Now assume H_1 is not an alternative anymore. Thus a new probability function, P_j, must be formed, for which the following is the case:

$$P_j(\mathrm{H}_1) = 0, \text{ or } P_j(\neg\mathrm{H}_1) = 1 \tag{1}$$

Also, learning about $\neg\mathrm{H}_1$ decreases the probability of $\mathrm{H_b}$, by conditionalisation on $\neg\mathrm{H}_1$:

$$P_j(\mathrm{H_b}) = P_i(\mathrm{H_b}|\neg\mathrm{H}_1) = \frac{P_i(\mathrm{H_b} \cap \neg\mathrm{H}_1)}{P_i(\neg\mathrm{H}_1)} = .4/.9 = .\overline{4} \tag{2}$$

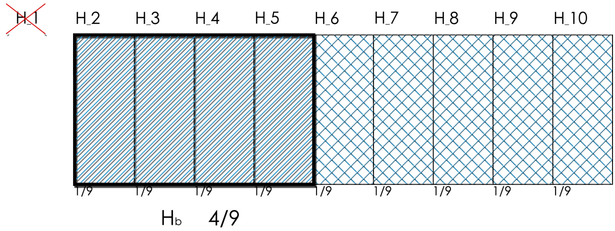

Figure 3: Boolean algebra representing probability function P_j: simple conditionalisation after learning about hypothesis H_1 "falling out" of the sample space.

Now the question is, how should we rationally update our belief in hypotheses $H_2 - H_{10}$? What is the evidence we should conditionalise on?

We could conditionalise on (1) which yields a result satisfying DHS's condition, namely a normalised, equal distribution over $H_2 - H_{10}$. For example, for H_2:

$$P_j(H_2) = P_i(H_2|\neg H_1) = .1/.9 = .\overline{1}$$

Equally for $H_3 - H_{10}$. Figure 3 shows the resulting probability distribution.

This way of conditionalising, however, ignores (2), the fact that the background hypothesis is less likely than under P_i – a fact that should favour hypotheses not positively correlated with H_b over the ones that imply H_b. Arguably, a rational agent should take (2) into account, as a real piece of evidence, and may thus gain another probability distribution P_{j*} through conditionalisation on (2). Jeffrey Conditionalisation (which is needed because the "evidence" we conditionalise on is not certain) yields for H_2:

$$P_{j*}(H_2) = P_i(H_2|H_b) * P_j(H_b) = .1/.5 * .\overline{4} = .0\overline{8}$$

, parallel for $H_3 - H_5$; and

$$P_{j*}(H_6) = \ldots = P_{j*}(H_{10}) = P_i(H_6|\neg H_b) * P_j(\neg H_b) = .1/.5 * .\overline{5} = .\overline{1}$$

This way of conditionalising respects the intuition that the hypotheses not correlated with H_b should benefit from the decrease of its probability. However, it in turn ignores (1), and is not normalised. Through successive conditionalisation of $P_j(H_2) - P_j(H_{10})$ on $\neg H_1$, (1) can be taken into account and the resulting distribution is normalised over the remaining $H_2 - H_{10}$:

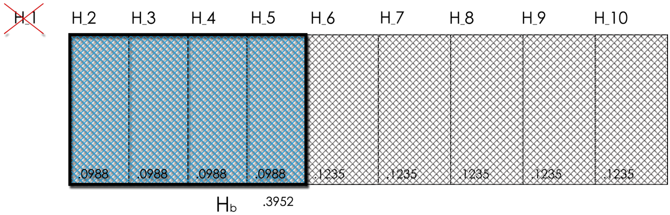

Figure 4: Boolean algebra representing probability function P_{j**}: taking into account that the background hypothesis, and thus hypotheses positively correlated to it, becomes less likely after learning $\neg H_1$.

$$P_{j**}(H_2) = \ldots = P_{j**}(H_5) = \frac{P_{j*}(H_2 \cap \neg H_1)}{P_i(\neg H_1)} = .0\overline{8}/.9 \approx .0988$$

; and

$$P_{j**}(H_6) = \ldots = P_{j**}(H_{10}) = .0\overline{1}/.9 \approx .1235$$

P_{j**} is normalised and seems to be a rational distribution, respecting the intuition that the background hypothesis' decrease in probability should favour hypotheses 6-10 over hypotheses 2-5.

Thus, we have a case where a rational procedure yields lower probabilities for some hypotheses ($H_2 - H_5$), relative to less alternatives – a violation of DHS's condition. If we successively conditionalise H_b on the new probabilities of $H_2 - H_{10}$, its probability decreases further. Fig. 4 shows the resulting probability distribution under P_{j**}.

To sum this hypothetical example up, there may be cases where it is rational to assign lower probabilities to some hypotheses for a lower number of alternatives, while the probabilities of other hypotheses overproportionally increase. Finally note that the probabilities of $H_2 - H_5$ do not decrease because of the learning of the extinction of an alternative *per se*; they increase because of the specific assumptions about $H_2 - H_5$ and the situation they are embedded in. Learning about (the extinction of) other alternatives, our belief in hypotheses may increase, decrease, or remain constant. The expression "probability of a hypothesis given k alternatives", or $P(H|Y_k)$, is in fact underspecified, and to assume it decreases with k (or increases, or whatever dependency you might want to come up with) is suspicious, even from a somewhat empirically minded Bayesian's point of view.

4 Conclusions

Why rain on a success story of the Bayesians' parade? I do not intend to dispute the ingenuity of DHS's analysis. I think they succeed in showing why NAA has an intuitive appeal, and why scientists in some contexts reason this way. Even more: they show that, relative to some lines, or principles, of thinking, NAA increases the subjective probability of a hypothesis. Now these lines or principles may be conceived of as more or less good. What DHS don't achieve is to give a proof for the general validity of NAA – those who weren't convinced before won't be convinced after reading their [2].

What is the lesson from this? The most important lesson is, I think, that we should be careful with the use of Bayesian nets, particularly when there is some strong notion of normativity involved – which is often the case in philosophy. In [1], Bovens and Hartmann claim that "integrating Bayesian Networks into philosophical research leads to theoretical advances on long-standing questions in philosophy and has a potential for practical applications", and that "philosophers have been sadly absent in reaping the fruits from these new developments [the development of Bayesian nets and their applications] in artificial intelligence". As much as I agree to the general spirit of the statement – the mathematisation of philosophical problems and the use of scientific tools to tackle them –, it seems to me that in some philosophical applications Bayesian nets are particularly convenient to hide assumptions which, made explicit, would be considered problematic. Bayesian nets are very useful if we want to make machines reason like humans, make or explain probabilistic conclusions, make diagnoses, predictions and decisions, for troubleshooting, or to find hidden causes. But in rationalisations of arguments or certain ways of reasoning, we must be careful about which kinds of (in)dependencies to specify and how they are justified.

Notes

1 I would like to thank Dominik Klein, David Makinson, Stefan Schubert and Jan Sprenger for helpful comments on an earlier draft of the paper. Thanks also to two anonymous referees of this journal. Last but not least, many thanks to Kristel Zarate Leon for producing the graphs.

2 For simplicity, let me in the following just use the term "hypothesis". The difference is not important for our purpose since "hypothesis" will just denote any set of rather coherent scientific statements (see below).

3 There might be interesting cases where it does – but for the sake of simplicity, assume it does not here and in the following.

4 In particular, Dawid et al. [2] use the statement "H is empirically adequate", meaning H is consistent with past and future observations (3). They claim that this prevents the possibility of there always being an infinite number of rival hypotheses that are empirically indistinguishable (cf. 5, 9). However, the condition in the second remark that two hypotheses that make the same predictions count as one and the same hypothesis seems to take care of that; so I don't understand why this move is needed, but since it does not touch the extension of this investigation (we could always easily replace "the probability of (the truth of) X" by "the probability of the empirical adequacy of X"), let's just leave it here.

5 Again, there might be interesting cases where it does – but let's not consider them here.

6 A discussion I don't intend to enter here. For an extensive list of literature concerned with it, as well as an own *defusing of Bertrand's Paradox*, cf. Gyenis & Rédei [3].

Philippe van Basshuysen
London School of Economics and Political Science

<philippe.v.basshuysen@gmail.com>
<http://lse.academia.edu/PhilippevanBasshuysen>

References

[1] L. Bovens, S. Hartmann. Bayesian Networks in Philosophy. B. Löwe et al. (eds.), *Foundations of the Formal Sciences II: Applications of Mathematical Logic in Philosophy and Linguistics*, 39-46. Dordrecht: Kluwer, 2002.

[2] R. Dawid, S. Hartmann, J. Sprenger. The No Alternatives Argument. *Brit. J. Phil. Sci.* 66 (1), 213-234, 2015.

[3] Z. Gyenis, M. Rédei. Defusing Bertrand's Paradox. *Brit. J. Phil. Sci.* (forthcoming, online: doi: 10.1093/bjps/axt036. Preprint: http://philsci-archive.pitt.edu/id/eprint/9539, 2014.

[4] F. Herzberg. A note on "The no alternatives argument" by Richard Dawid, Stephan Hartmann and Jan Sprenger. *European Journal for Philosophy of Science* 4 (3), 375-384, 2014.

[5] T. Kuhn. Objectivity, Value Judgment, and Theory Choice. *The Essential Tension: Selected Studies in Scientific Tradition and Change*, 320-329. Chicago: Chicago University Press, 1977.

[6] M. Krämer. There is no alternative! Discussion in *The Guardian*, http://www.theguardian.com/science/life-and-physics/2013/may/04/no-alternative-bayes-penalties-philosophy-thatcher-merkel, 2013.

[7] P. Woit. String theory and post-empiricism. *Scientia Salon: Philosophy, Science, and all interesting things in between* (blog), https://scientiasalon.wordpress.com/2014/07/10/string-theory-and-the-no-alternatives-argument/, 2014.