

## THE HYPOTHESIS THAT SAVES THE DAY. AD HOC REASONING IN PSEUDOSCIENCE

MAARTEN BOUDRY

### *Abstract*

What is wrong with *ad hoc* hypotheses? Ever since Popper's falsificationist account of adhocness, there has been a lively philosophical discussion about what constitutes adhocness in scientific explanation, and what, if anything, distinguishes legitimate auxiliary hypotheses from illicit *ad hoc* ones. This paper draws upon distinct examples from pseudoscience to provide us with a clearer view as to what is troubling about *ad hoc* hypotheses. In contrast with other philosophical proposals, our approach retains the colloquial, derogative meaning of adhocness, and calls attention to the way in which the context of a theoretical move bears on the charge of adhocness. We also discuss the role of motivations implicit in the concept of adhocness, and the way *ad hoc* moves draw on theory-internal rationalizations.

### 1. *Introduction*

In both academic and popular discussions on the scientific status of controversial theories, a hypothesis or explanation is often rejected as being *ad hoc*. In philosophical discussions about the demarcation project, i.e. the problem of distinguishing bona fide science from non-science, the practice of resorting to *ad hoc* moves in the face of anomalous data is often regarded as a distinguishing feature of bad science or pseudoscience (Pigliucci and Boudry 2013). Traditional analyses of adhocness, however, inspired by falsificationist philosophy, suffer from a number of problems, in line with well-known shortcomings of Popper's solution to the demarcation problem (Nickles 2006; Hansson 2009; Pigliucci and Boudry 2013). As with many concepts that are intuitively clear at a first glance, it has proven a bit trickier to unpack the notion of adhocness.

In this paper, we draw on distinct examples of bad reasoning from disciplines that are widely regarded as ‘pseudoscience’, in order to clarify what is objectionable about *ad hoc* moves. Rather than rehearsing the standard examples from the history of science (e.g. the postulation of an extra-Uranian planet, Pauli’s neutrino hypothesis, the Lorentz-Fitzgerald contraction hypothesis), on which philosophical opinion is divided, our strategy is to focus on specimens of reasoning that are *blatantly* fishy in a sense that we intuitively recognize as *ad hoc*. Taking these clear-cut examples as a starting point, we may be better able to explicate what underlies our intuitions of ‘ad hocness’, and we should be more capable of evaluating more complicated examples. We discuss the motivational and psychological component of *ad hoc* reasoning and rely on the concepts of immunizing strategies and epistemic defense mechanisms, as explored in Boudry & Braeckman (2011).

## 2. *Falsificationism and ad hoc reasoning*

Karl Popper famously argued that the distinguishing feature of the scientific attitude is the willingness to make bold empirical conjectures and subject them to successive attempts at refutation. According to Popper, a theory can only be regarded as scientific if it forbids certain states of affairs, and the paragon example of a scientific theory is one that takes the boldest empirical risks. A hypothesis can be corroborated if it survives attempts at falsification, but when it runs against empirical observations, it needs to be abandoned. However, sometimes an auxiliary assumption is added to the theory in order to *rescue* it from falsification. According to the traditional Popperian view, this resort to *ad hoc* reasoning is illegitimate and even the hallmark of pseudoscience:

Such a procedure [...] rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (Popper 1963/2002, p. 48)

In accordance with the Pierre Duhem’s problem of underdetermination, however, philosophers of science after Popper have acknowledged that, in order to bring a hypothesis into contact with reality, one always needs a number of auxiliary hypotheses. In other words, hypotheses are always tested in conjunction and never in isolation. If a “bundle” of hypotheses is tested and the observations do not accord with what was predicted, from a logical point of view any one of the auxiliary hypotheses (or the core hypothesis) could be blamed. Indeed, when scientists devise a test for such a conjunction of hypotheses, what counts as the central hypothesis under test and what counts as background knowledge is a matter of methodological decision. Imre Lakatos

has attempted to correct Popperian falsificationism taking this problem into account:

No theory forbids some state of affairs specifiable in advance; it is not that we propose a theory and Nature may shout NO. Rather, we propose a maze of theories, and Nature may shout INCONSISTENT. (Lakatos 1968, p. 162)

Falsificationists after Popper — as well as Popper himself in his more cautious moments — have allowed for modification of auxiliary hypotheses in the face of refutation, provided that the latter are independently testable and do not *reduce* the empirical content of the theory. If these conditions are not met, according to the falsificationist, the auxiliary hypothesis has to be discarded as *ad hoc*. Thus, a more sophisticated falsificationist philosophy of science accepts that every scientific research program builds up a “protective belt” of auxiliary hypotheses around its “hard core” claims (Lakatos and Musgrave 1970; Lakatos 1968). As such, adjustments and revisions in the face of empirical anomalies are not necessarily problematic. Scientists routinely resort to auxiliary hypotheses to rescue a theory from apparent refutation, and significant progress has been made by doing so. The example of Leverrier’s and Adams’s successful postulation of an extra-Uranian planet (Neptune) to account for the perturbations in the orbit of Uranus provides a case in point.

Alas, the explication of adhocness in terms of reduced empirical content still runs into trouble. Although an auxiliary may not have *known* testable consequences at the time of its introduction, further developments and new experimental procedures may render it testable after all (Grünbaum 1976). Bamford (1993), who argues that Popper equivocated between colloquial and technical senses of adhocness, draws attention to episodes of genuine scientific progress in which the appeal to an ‘*ad hoc*’ auxiliary did *not* increase the empirical content of the original theory. According to Bamford, the very idea of a hypothesis which has no testable consequences other than the observation it was introduced to account for, is difficult to make sense of in any case. Finally, even if it were possible for an auxiliary not to be independently testable from the main hypothesis at all, this is not to say that such a modified theory would be necessarily false. At most, one may argue that adhocness is generally not *conducive* to scientific progress.

In light of these and other problems, some authors no longer treat adhocness as a term of epistemic abuse, instead portraying the concept as a neutral methodological or epistemic attribute. Grünbaum (1976) offered a purely descriptive hierarchy of three senses of adhocness, and Laudan (1977) has even suggested that the extent to which a theory allows for *ad hoc* moves is a

*redeeming* feature of that theory. However, others have unpacked the notion of adhocness in a way that retains the colloquial, pejorative meaning. For example, Leplin (1975) reserved the term for auxiliary hypotheses whose introduction results in a loss of theoretical simplicity that is *not* off-set by a proportionate gain of fit with the data (see also Kitcher 1982).

### 3. *Adhocness in pseudoscience*

Although Popper's demarcation criterion has been decidedly out of philosophical fashion for several decades, the charge of adhocness, in the sense of a move for dodging criticism and refutation, is still often leveled against controversial theories. Indeed, an increasing dependence on *ad hoc* moves is widely regarded as a telltale sign of pseudoscientific discourse (e.g. Derksen 1993; Hines 2003; Carroll 2003; Pigliucci 2010; Kitcher 1982). Are these critics of bad science completely misguided, or does the 'ad hoc' charge really point to some interesting epistemic principle?

The charge of adhocness has played a role in many episodes in the history of science. In real-life scientific disputes, however, it is not always clear when the charge is (or becomes) justified. What we find instead are a lot of close calls and borderline cases. Examples include phlogiston theory, phrenology, Lamarckist evolution, the steady state model of the universe, or more recently, the Duesberg hypothesis on the non-infectious nature of AIDS. In many of these cases, there is no clearly defined point at which a theory requires too much patchwork and gerrymandering, and thus when it ceases to be rational to defend it. Often enough, crucial experiments and conceptual death knells are only to be identified in retrospect, when the dust has settled and the theory has disappeared from the stage. This shows that demarcating instances of adhocness may be a complicated affair, but not that the concept is fundamentally incoherent or normatively useless.

Indeed, if we accept that non-adhocness is an epistemic virtue or liability, and given that we are dealing with cutting-edge scientific disputes, we can reasonably *expect* that charges of adhocness are open to reasonable discussion. If advocates of a theory X are confronted with empirical anomalies, they will reasonably try to defend X in a way that is least open to the charge of adhocness (or other epistemic sins), at least as long as this is possible. When the evidence against a theory is so overwhelming that it would require blatant *ad hoc* moves to save it, that theory is unlikely to be the subject of serious scientific debate for a long time, and will either disappear from the stage or merely persist on the fringes of science. Outright pseudoscience is only rarely the subject of philosophical discussions about adhocness, but this is unfortunate. By having a look at blatant examples of *ad hoc* reasoning

from the hinterland of pseudoscience, we may be better able to make sense of charges of adhocness in more complicated cases.

### 3.1. *Escape Clauses*

In the field of parapsychology, a number of auxiliary assumptions about the phenomenon of psi have been put forward that are suspiciously evasive. For example, upon being confronted with negative experimental results, some parapsychologists have speculated that the presence of inquisitive observers can disturb the salience of psi phenomena. In particular, a negative form of psi cancelling out regular psi activity may be emitted by skeptical observers and experimenters who take pains to install strict experimental controls (Wiseman 2010; Humphrey 1996). A recent example of this strategy can be found in Sheldrake (1995). One of the founding fathers of parapsychology, J.B. Rhine, already remarked that “precautionary measures” against deception and information leaks (i.e. methodological controls) may hamper psychic performance (Gardner 1957, p. 307). Some authors have given this counteracting force impressive labels like “catapsi”, which is defined as “the generation of ‘static’ that cancels out regular *psi* powers within its range” (Bonewitz 1989, p. 55).

In the same vein, parapsychologists have sometimes explained away the low quality of evidence for psi by suggesting that methodological errors tend to *enhance* experimental results, because these errors *stimulate* psi activity. They have termed this the “error phenomenon” (Rao 1968). The famous psychological researcher John Beloff argued that psi phenomena are “actively evasive” (Beloff 1994, p. 7) and coined the term “decline effect” (1994, p. 11) to describe the puzzling tendency of psychics to lose their powers as they are tested more extensively. Some parapsychologists have hypothesized that the primary function of psi is to “induce a sense of mystery and wonder”, which allegedly explains its elusive character (Kennedy 2003, p. 67). For a sympathetic discussion of other explanations of the elusive nature of psi, see Kennedy (2001).

The most striking features shared of such theoretical moves is that (1) they are conveniently tailored around some empirical anomalies, while (2) they are too vague and non-specific to allow for novel predictions or explanatory reward. With concepts such as these up their sleeves, parapsychologists can explain away not just one particular anomaly, but potentially *any number of negative experimental results*. Hence, the error phenomenon and catapsi effect account for precisely what would have been expected if the alleged psi phenomena were entirely due to deception, sloppy experimental design and methodological defects. Martin Gardner has made the same observation with regard to the bag of excuses used by dowsers: “it is quite clear they are

so numerous and intangible that he [the dowser] has a ready excuse for every dowsing failure.” (Gardner 1957, p. 104)

What do we mean when we reject such moves as *ad hoc*? What is problematic is not just the logical relation between main hypothesis and auxiliary, but rather the circumstances in which the latter is introduced, and its (potential) range of application. To further illustrate this point, consider the case of an astrologer who belatedly invokes the formation of stars at the moment of *conception* — which is of course very hard to determine — when his prediction on the basis of the birth date has failed. Or similarly, consider the advocate of biorhythm theory who argues that some people are “arrhythmic” some of the time, when his predictions do not fit the observed patterns (Carroll 2003, p. 7). Why are we entitled to reject these moves as *ad hoc*? Because we realize that, if we would allow such moves, *any* type of observation can be accommodated, and hence we are left with *no constraints* on the use of the (type of) auxiliary hypothesis in question. In Freudian psychoanalysis, the so-called “hereditary factor” was often invoked when traces of allegedly repressed infantile desires were unforthcoming. This meant that, if the patient had not personally experienced the sexual traumas required by the theory, Freud argued that traces of “phylogenetic memory” — recollections of ancient traumas inherited over the generations — could fulfill the same role (Cioffi 1998, p. 108).

The other side of the coin, as far as the ‘no constraints’ objection is concerned, is that the concept on which the *ad hoc* move relies is versatile enough to be conveniently *ignored* as long as the data align with the theory. Consider the same astrologer who, as long as his birthdate-based predictions do not clash with the data, fails to mention the moment of conception as an avowedly crucial factor, boasting of his numerous predictive successes. Or consider Freud’s curious neglect of phylogenetic inheritance when, on some other occasion, he explained a case of neurosis by arguing that the patient had missed a powerful father in his childhood who could threaten the boy with castration (which Freud deemed necessary for healthy psychosexual development). Such concepts are invoked on particular occasions to explain away predictive failure, but they are inconsequentially ignored on other occasions. Thus, there is an *asymmetry* in the use of the auxiliary hypothesis in question.

As is clear from these examples, the broader context of a theoretical move is relevant for making the *ad hoc* charge. Although the presumption of *ad hoc* reasoning is strongest when we have actually witnessed several instances of opportunistic and inconsequential use of an auxiliary hypothesis, suspicions

can also be raised on the basis of a single case. Sometimes the lack of constraints is pretty obvious, and it is clear that the hypothesis can yield nothing beyond explaining away particular failures.

### 3.2. *Patterns of systematic ad hoc reasoning*

When we dismiss a theoretical move as being *ad hoc*, we seem to be saying that it was *motivated* by the need to solve a particular problem, and that there were no other good or proper reasons for making it. In some cases, however, the habit of making *ad hoc* moves may have deeper theoretical roots. In a discussion of pseudoscience and bad science, Boudry & Braeckman (2011) distinguish two ways in which a theory might achieve invulnerability from criticism and empirical refutation:

We define an ‘immunizing strategy’ as an argument brought forward in support of a belief system, though independent *from* that belief system, which makes it more or less invulnerable to rational argumentation and/or empirical evidence. By contrast, an epistemic ‘defense mechanism’ is defined as an internal *structural feature* of a belief system, which has the same effect of deflecting rational arguments and empirical refutations.<sup>1</sup>

In the case of epistemic defense mechanisms, the theory *itself* creates opportunities for rationalizations and *ad hoc* responses in the face of counterevidence. In a typical instance of such epistemological quicksand, believers posit an unobservable force or cause to account for a range of observations, while they have no precise understanding of the causal mechanisms involved, and at the same time go about inferring the activity of this invisible force *ex post facto* from its effects. For the purpose of illustration, consider shamanistic healing practices based on the invisible working of magic rituals. Suppose that the success of such magic depends on a number of elaborate procedures, taboos and prescripts, but that the proper performance of the ritual is partly inferred from the therapeutic outcome itself.

Because of the structure of this ‘theory’, believers are invited to interpret unexpected outcomes not as a failure of magic per se, but as an apparent sign that ‘something must have gone wrong’: the intervention was not of the appropriate type, the wrong material has been used, the ritual was not performed properly, or some other and equally invisible force interfered

<sup>1</sup> The use of intentional concepts such as ‘strategies’, ‘evasions’ and ‘maneuvers’ should not always be taken literally. Boudry & Braeckman (2012) have argued in some detail that the overall impression of *strategic design* we are left with when confronted with immunizing strategies and defense mechanisms may well derive from the latter’s internal *epistemic rationale*, rather than from conscious deliberation and strategic planning on the part of believers.

with the ritual.<sup>2</sup> In his seminal study on witchcraft and magic among the Azande, Evans-Pritchard has termed these *ad hoc* moves “secondary elaborations” (Evans-Pritchard 1965 [1937]). They have the effect of making the belief systems impervious to refutation, though internally coherent. A second repertoire of such secondary elaborations is used when the alleged effect is itself ambiguous or difficult to observe. In such cases, believers may insist that the ritual has been efficacious after all, but that the effects are not (yet) visible, or have a more spiritual dimension. For example, the effect may be super-empirical (Talmont-Kaminski 2013): having one’s energy balances ‘restored’ again, or being freed from evil spirits or witchcraft, or having one’s ‘chakras’ cleansed, etc.

In such cases, subtle feedback loops often arise from observations to theoretical explanations and back again, which will keep the theory forever outside the reach of empirical refutation. For example, the efficacy of a magic amulet to chase away evil spirits may be assessed on the basis of how the patient’s condition develops. But typically, the question whether the patient is possessed of evil spirits itself derives support from the *reputation* of the magical amulet as a ‘real’ one, which may involve the circumstances in which it was found and the events that have befallen its carriers (Boyer 1994, p. 144).

It will be clear that, in the field of parapsychology, a poor causal understanding of the paranormal forces at play likewise invites a systematic *pattern* of *ad hoc* reasoning in the face of alternate successes and failures. Feedback loops between the interpretation of observations and the theoretical characterization of *psi* will almost inevitably arise. *Psi*-activity may be postdicted whenever the results are successful, and virtually any contextual factor may be invoked as an explanation for failure: “Ok, that was just warming-up” — “The connection is weaker now, we have been trying too much” — “There are obviously negative energies around” — “If you’re too skeptical it won’t work” (Wiseman 2010). Note that, according to parapsychologists, *psi* powers may disappear over time or work only intermittently. And all of this can only be learned through experience. What is objectionable about parapsychology and other magical belief systems is not so much the introduction of a single auxiliary hypothesis, but rather a systematic practice of *ad hoc* reasoning, which is part and parcel of the pseudoscientific theory itself. In other words, we find that there are simply *no constraints* on the use of such evasive auxiliaries.

<sup>2</sup> Again, similar moves may be made by a scientist making an unexpected observation under a microscope, but in such a case there are indirect and independent checks available.

#### 4. Discussion

##### 4.1. Unobservability

As many of the examples of *ad hoc* reasoning we have reviewed rely on unobservable entities and forces, one may wonder whether this has any bearing on the explication of adhocness. Leplin (1975) argues that the charge of adhocness depends on whether the entity in question is unobservable *in principle*, or just *de facto*. The failure to detect X is less damaging if we possess of good, independent reasons to presume that X is intrinsically unobservable. By contrast, if there is no good theoretical reason for X's unobservability, then we have no excuse for *failing* to detect X. For example, in the 19th century astronomers like Le Verrier postulated the existence of an invisible planet, Vulcan, or an invisible asteroid belt, to account for anomalies in the orbit of Mercury. These auxiliaries to the Newtonian model of the solar system were quickly discarded, however, mainly because nobody could give a good explanation as to why these objects were supposed to be invisible to terrestrial observers. In other cases, such as the postulation of quarks and other subatomic particles, auxiliaries can derive support from other considerations: accordance with well-established theories, indirect experimental support, unification of a range of data etc. In the pseudoscientific examples we reviewed, however, the activity of X in question is merely postdicted on the very phenomena which X was supposed to account for, without any explanatory offset. This, and not the unobservability of X per se, is why the label *ad hoc* is justified.

##### 4.2. Adhocness and psychological motivation

An important problem confronting the reconstruction of adhocness is the distinction between the methodological and psychological components of the concept, both of which are often conflated in traditional discussions of *ad hoc* reasoning (Bamford 1993; Bamford 1999). As Laudan (1977, p. 117) writes, many discussions of adhocness tacitly assume that "there is something suspicious about any change in a theory which is motivated by the desire to remove some anomaly". Is our discussion of *ad hoc* reasoning in pseudoscience guilty of this confusion as well?

In the received view, a move is dismissed as *ad hoc* because it is said to be *motivated to* explain away some bothersome anomaly. There is a kernel of truth in this justification, but it is still misleading. A move is not invalid simply because it happens to suit someone's purpose, for example fulfilling her desire to rescue a theory. Even deliberately searching for a suitable auxiliary

assumption “to save the day” is not a bad thing *per se*. If the scientist introducing auxiliary *X* intended to rescue his cherished theory, this does not preclude that *X* has other (perhaps unintended) testable consequences (Bamford 1999, pp. 379–380). Our scientist may just be lucky that his wish to be proven right is granted by nature, so to speak. That being said, few would deny that, if someone has a strong desire to rescue a theory, this is usually a good *proxy* for epistemic trouble. If *A* is desperate to save one particular theory at all costs, and *B* has no stakes in the debate, it is more likely, other things being equal, that the resolution *A* comes up with is more biased, compared to *B*’s take on the issue. Thus, it is not unreasonable to suspect *A*’s solution to have been motivated *solely* by the desire to protect his cherished ideas.

This is a general problem pertaining to the role of motivations in the formation of beliefs. For instance, the very fact that *A* is motivated by his religious views to favor a theory (say, Big Bang cosmology) does not mean that he is wrong. Nonetheless, if *A* favors a theory that fits well with his religious views, at least the suspicion may be raised that *A*’s religious zeal has trumped epistemic considerations. More importantly, in absence of good epistemic reasons, *A*’s religious views constitute the best candidate for explaining why *A* has endorsed such or such theory. Thus, if construed along these lines, there is nothing wrong with dismissing a theory as “ideologically motivated”, or with objecting to an *ad hoc* move because “he just hates to give up his pet theory”. We should read that as shorthand for the charge that, in the *absence of proper epistemic reasons*, the only plausible motive for his move is the desire to rescue the theory at all costs.

#### 4.3. Core theory and ad hoc auxiliary hypotheses

In most philosophical accounts, an auxiliary is labeled as *ad hoc* in relation to some original theory or core hypothesis. In philosophy of science, however, some authors have insisted that one should not conflate *ad hoc* moves extraneous to a theory with legitimate moves within the theory-as-such. Notably, with regard to Popper’s verdict on Freudian psychoanalysis as an unfalsifiable pseudoscience, Adolf Grünbaum commented:

The (revocable) falsifiability of the theory-as-such in the context of its semantic anchorage is a logical property of the theory itself, whereas the tenacious unwillingness of the majority of its defenders to accept adverse evidence as refuting is an all too human property of those advocates. (Grünbaum 1979, pp. 137–138)

According to Grünbaum, the latter argument merely comes down to the “*sociological*” objection that Freudians are evasively unresponsive to criticism

of their hypotheses” (Grünbaum 2008, emphasis in original). Alas, clean distinctions between the theory-as-such and extraneous *ad hoc* moves can be hard to come by (Boudry and Braeckman 2011; Cioffi 1985, 1998). For example, the pattern of *ad hoc* reasoning found in parapsychology has been given a theory-internal explanation, viz. the elusive nature of psi forces, its wonder-inducing function, its intermittent efficacy, etc. For many parapsychologists, this feature of psi is one of the central tenets of their research program (Kennedy 2003, 2001). Whom do we have to consult to find out whether the resort to the shyness of psi is an *ad hoc* maneuver which has nothing to do with proper parapsychology, or whether it is an intrinsic part of parapsychological theory?

Grünbaum’s own example of Freudian psychoanalysis is particularly interesting in this regard. Many critics of Freudian psychoanalysis have argued that the psychoanalytic habit of *ad hoc* reasoning stems from (i) the malleable and incoherent conceptual structure of the theory (ii) Freud’s dynamic system of repressions, inversions and projections between different mental subsystems, in which anything can stand for either itself or its counterpart; (iii) the countless theoretical joker cards and immunizing gambits in psychoanalytic theory, which are almost impossible to resist in interpretive practice (Macmillan 1997; Cioffi 1998; Boudry and Braeckman 2011). If Freudian theory *itself* is the source of persistent *ad hoc* reasoning, we submit that there is something more going on than the mere “tenacious unwillingness of the majority of [psychoanalysts]” to accept disconfirming evidence. (for an in-depth treatment of psychoanalysis, see Boudry and Buekens 2011; Buekens and Boudry 2012).

## 5. Conclusion

What, if anything, distinguishes *ad hoc* reasoning from the legitimate introduction of auxiliary hypotheses in science? In order to get a clearer view on this matter, we have tried to find some particularly poignant examples of *ad hoc* reasoning, in the colloquial and pejorative sense, borrowing examples from pseudoscience and superstition.

First, we have reviewed the opportunistic resort to escape clauses that are conveniently tailored around observational anomalies. Second, we have shown that some pseudosciences, structured around ambiguous causal mechanisms and unassessable effects, provoke a systematic pattern of *ad hoc* reasoning. We concluded that a move deserves to be labeled as *ad hoc* if it *merely* explains away particular anomalies without yielding any independent prediction or other explanatory offset. This normative conception of adhocness, which captures the term’s colloquial meaning, is sensitive to the

particular context in which a theoretical move is made. Two additional conditions make the move even more suspect: (1) if it can be shown that there are no demonstrable constraints on its application (i.e. the move can be used to explain away any bothersome anomaly), and (2) if it is being used inconsequentially (i.e. it is conveniently ignored on other occasions).

In its standard use, the charge of adhocness is often supported by making reference to psychological motivations. This way of framing the issue is strictly speaking inaccurate, but it is acceptable as a shorthand. If a scientist is strongly motivated to rescue a theory at all costs, this is a good reason to be suspicious. If no proper epistemic reasons for his move are apparent, we may safely conclude that he has indulged in wishful thinking. If we put this motivational component of adhocness into proper perspective, and if we move beyond a naïve logicist view about unobservables and the theory-in-itself, the concept of adhocness may be retained as capturing an important epistemic sin, in science as well as in everyday reasoning.

Ghent University  
Belgium

E-mail: maartenboudry@gmail.com

#### REFERENCES

- Bamford, G. (1993). Popper's Explications of Ad Hocness: Circularity, Empirical Content, and Scientific Practice. *The British Journal for the Philosophy of Science*, 44(2), 335–355.
- Bamford, G. (1999). What is the Problem of Ad Hoc Hypotheses? *Science & Education*, 8(4), 375–386.
- Beloff, J. (1994). Lessons of history. *Journal of the American Society for Psychical Research*, 88(7), 7–22.
- Bonewitz, I. (1989). *Real Magic*. York Beach (Maine): Samuel Weiser.
- Boudry, M. and J. Braeckman (2011). Immunizing strategies & epistemic defense mechanisms. *Philosophia*, 39(1), 145–161.
- Boudry, M. and J. Braeckman (2012). How Convenient! The Epistemic Rationale of Self-validating Belief Systems. *Philosophical Psychology*, 25(3), 341–364.
- Boudry, M. and F. Buekens (2011). The Epistemic Predicament of a Pseudoscience: Social Constructivism Confronts Freudian Psychoanalysis. *Theoria*, 77(2), 159–179.
- Boyer, P. (1994). *The naturalness of religious ideas: A cognitive theory of religion*. Berkeley (Calif.): University of California Press.
- Buekens, F. and M. Boudry (2012). Psychoanalytic facts as unintended institutional facts. *Philosophy of the Social Sciences*, 42(2), 239–269.

- Carroll, R.T. (2003). *The skeptic's dictionary: a collection of strange beliefs, amusing deceptions, and dangerous delusions*. Hoboken, NJ: John Wiley & Sons.
- Cioffi, F. (1985). Psychoanalysis, Pseudoscience and Testability. In G. Currie and A. Musgrave (Eds.), *Popper and the human sciences*. Dordrecht: Martins Nijhoff Publishers.
- Cioffi, F. (1998). *Freud and the question of pseudoscience*. Chicago: Open Court.
- Derksen, A.A. (1993). The seven sins of pseudo-science. *Journal for General Philosophy of Science*, 24(1), 17–42.
- Evans-Pritchard, E.E. (1965 [1937]). *Witchcraft, oracles and magic among the Azande*. Oxford: Clarendon press.
- Gardner, M. (1957). *Fads and fallacies in the name of science*. New York: Dover Publications.
- Grünbaum, A. (1976). Ad hoc auxiliary hypotheses and falsificationism. *The British Journal for the Philosophy of Science*, 27(4), 329–362.
- Grünbaum, A. (1979). Is Freudian psychoanalytic theory pseudo-scientific by Karl Popper's criterion of demarcation? *American Philosophical Quarterly*, 16(2), 131–141.
- Grünbaum, A. (2008). Popper's Fundamental Misdiagnosis of the Scientific Defects of Freudian Psychoanalysis and of their Bearing on the Theory of Demarcation. *Psychoanalytic Psychology*, 25(4), 574–589.
- Hansson, S.O. (2009). Cutting the Gordian Knot of Demarcation. *International Studies in the Philosophy of Science*, 23(3), 237–243.
- Hines, T. (2003). *Pseudoscience and the paranormal (2nd ed.)*. Amherst, NY: Prometheus Books.
- Humphrey, N. (1996). *Soul searching: Human nature and supernatural belief*. London: Vintage.
- Kennedy, J.E. (2001). Why is psi so elusive? A review and proposed model. *Journal of Parapsychology*, 65(3), 219–246.
- Kennedy, J.E. (2003). The capricious, actively evasive, unsustainable nature of psi: A summary and hypotheses. *The Journal of Parapsychology*, 67(1), 53–75.
- Kitcher, P. (1982). *Abusing science: The case against creationism*. Cambridge (Mass.): MIT Press.
- Lakatos, I. (1968). Criticism and the methodology of scientific research programmes. *Proceedings of the Aristotelian Society*, 69, 149–186.
- Lakatos, I. and A. Musgrave (1970). *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
- Laudan, L. (1977). *Progress and its problems: towards a theory of scientific growth*. London: Routledge & Kegan Paul.
- Leplin, J. (1975). The concept of an ad hoc hypothesis. *Studies in History and Philosophy of Science*, 5(4), 309–345.

- Macmillan, M. (1997). *Freud Evaluated: The Completed Arc*. Cambridge, MA: MIT Press.
- Nickles, T. (2006). The problem of demarcation, *The philosophy of science: An encyclopedia* (Vol. 1, pp. 188–197).
- Pigliucci, M. (2010). *Nonsense on stilts: How to tell science from bunk*. Chicago: University Of Chicago Press.
- Pigliucci, M. and M. Boudry (Eds.). (2013). *Philosophy of Pseudoscience: Reconsidering the Demarcation Project*. Chicago: University of Chicago Press.
- Popper, K.R. (1963/2002). *Conjectures and refutations: The growth of scientific knowledge*. London: Routledge.
- Rao, K.R. (1968). Spontaneous ESP in laboratory tests: the error phenomenon. *Journal of the American Society for Psychical Research*, 62, 63–72.
- Sheldrake, R. (1995). *Seven experiments that could change the world: a do-it-yourself guide to revolutionary science*. New York, NY: Putnam Publishing Group.
- Talmon-Kaminski, K. (2013). *Religion As Magical Ideology: How the Supernatural Reflects Rationality*: Isd.
- Wiseman, R. (2010). ‘Heads I win, tails you lose’. How parapsychologists nullify null results. *Skeptical Inquirer*, 34(1), 36–39.