WHO HAS WON THE SCIENCE WARS?

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Bogdanov affair in astrophysics is strikingly similar to Sokal's in "cultural studies". This paper discusses similarities between Sokal and Bogdanov affairs, especially the outrageous methods and behaviour of brothers Bogdanov, and concludes that the latter affair has shown that natural sciences and natural scientists are not beyond reproach, beyond criteria of cogency, validity and criticism, as was once suggested by Sokal's affair. This has a broader morale: Since "high science" is understood by fewer scholars, such science is sometimes more prone to outrageous hypothesis which would not be tolerated in the more common ones. Therefore, there has to be at least a symmetry in critical approach to scientific claims: neither the type of science, nor the fame of scientists should provide a guarantee of proper conduct and scientific methodology. The paper discusses various meanings of symmetry in scientific approach to science, and discussing "trust" and "distrust" in science suggests a description of the s.c. "circle of credibility".

Keywords: Bogdanov affair, Sokal affair, symmetry of criticism, circle of credibility

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The goal of the paper[1] is to describe the state of the art in the social studies of science. By enlisting problems with scientific fraud of various kinds, it tries to answer the question about the importance of relativism as a standard commitment in the social studies of science (SSS), to assess the weight and long term consequences of the previous affairs in SSS, most notoriously the Sokal's affair, and it tries to answer the question:

have affairs from the last ten years left any visible scars on SSS, and whether the scholars from the field have changed their attitudes as a consequence? My answers to the questions are positive: it is my contention that SSS has changed substantially, in part as a reaction to the scandal in the science wars.

But my general purpose is to compare types of reactions to scandals in science. In order to describe what happened in the years following Sokal's affair (in the s.c. "science wars") in the social sciences, I shall first describe a more recent scandal, the Bogdanov affair in astrophysics and cosmology, to show that SSS was neither the only nor the premier target of intentional fraud and spoof in science in principle, and my conclusion is that at the moment all is not rotten in social science, but that something might be rotten in other, allegedly "hard core" scientific disciplines.

Further, I explore what was theoretically wrong in SSS, and what is nowadays wrong in cosmology. I draw consequences for social studies of science, by rejuvenating the "symmetry principle" known from the times of the "strong programme" in the sociology of science, to be sure, with a new definition of the symmetry principle. It says that scientific status of a discipline or of the hypothesis, is not a valid reason to treat scientific claims differently from say pseudo-scientific claims. We should not have an *a priori* asymmetrical explanation, just because some sciences or scientists have a better reputation than others. This new definition of symmetry may also be called "circle of credibility".

THE BOGDANOV AFFAIR

In 2002 French twins, physicists Igor and Grichka Bogdanov published at least six papers under various titles: "Topological theory of the initial singularity of spacetime", "Topological Origin of Inertia", "The KMS state of spacetime at the Planck scale" and "Topological field theory of the initial singularity of spacetime", in highly respected scientific journals, such as *Annals of Physics, Classics of Quantum Gravity*, and a host of others (like *Chinese Journal of Physics, Czech Journal of Physics*), on the conditions that occurred at the Big Bang. *Annals of Physics* and *Classics of Quantum Gravity* are highly respected, and have Nobel Prize winners in their editorial boards.

> The Bogdanov papers asserted that there is evidence of what happened during the beginning of the Big Bang, known as the Planck era. According to the Big Bang scenario, the Universe (or at least the part of it we can observe) experienced an extremely dense and hot era (bottom), possibly starting from a gravitational singularity. Since then, space has expanded, carrying matter and later galaxies with it. The history of this expansion is now well-

> -known, back to the time of primordial nucleosynthesis; however, there is less certainty about the events of earlier epochs. Recent analysis of the cosmic microwave background suggests that an inflationary era occurred at a much hotter and denser epoch than nucleosynthesis. The even earlier epochs are currently a matter of speculation, ranging from a singular beginning of space-time to a bouncing epoch (e.g., the Big Bounce); an eternal, non-singular expansion (e.g., chaotic inflation); and string theory-inspired scenarios such as the ekpyrotic universe. Some of these scenarios may eventually replace the now-standard inflationary framework. In the case of a singular beginning (a hypothesis which is presently impossible to prove either theoretically or observationally), present knowledge tells us nothing about what happened from the time of the singularity until approximately 10^{-43} seconds afterwards, an epoch known as the Planck era. This is because space-time at this point has to be described in terms of a quantum theory of gravity, about which the scientific community knows little. The Bogdanov publications purport to have discovered what happened during this earliest epoch, and even before the moment of the singularity itself.
> (The Bogdanov Affair, Wikipedia)

At least three well known physicists, Roman Jackiw from Massachusetts Institute of Technology, Costas Kounnas from École normale supérieure Paris and Jack Morava from Johns Hopkins University, among other peers, reported favourably on Bogdanov brothers articles. First rumors on the possibility of fraud occurred in 2002, when German physicist Max Niedermaier reported that these papers were pseudoscientific,

> consisting of dense technical jargon written to sound scientific without having real content. In Niedermaier's view, the Bogdanovs had tried to prove the existence of weaknesses within the peer-review system, much in the same fashion that physicist Alan Sokal had published a deliberately fraudulent paper in the humanities journal *Social Text*.[2] On 22 October 2002, Niedermaier wrote an email to this effect which was then widely distributed. An eventual recipient, the American mathematical physicist John Baez, created a discussion thread on the Usenet newsgroup sci.physics.research titled "Physics bitten by reverse Alan Sokal hoax?" which quickly grew to hundreds of posts in length.
> (Bogdanov affair, Wikipedia)

This was just the beginning of the new stage of the Scientific Wars, a series of disputes among postmodern scientists of humanist bent, and realistic scientists mostly from natural sciences, which was storming public media and humanist journals ten years ago.

A number of curious facts concerning the Bogdanov affair strike us immediately. First, it is a sort of a reverse Sokal's affair, which ten years ago purported and succeeded in showing that the peer-review system in some social science journals (like *Social Text*) malfunctions if the paper uses the jargon of the editors, if the theses conform to their general world view, if it generously quotes from the sources favorable to the editors' view, or endorses his political stances. Bogdanov affair showed that such a procedure (of publishing fraudulent work, with the intent to show weaknesses of the peer-review system) is not constrained to postmodern humanist science, but can affect even more respectable physicists and mathematicians as well.

Unlike Sokal, who immediately admitted a hoax (Sokal, 1996a, 1996b), the brothers Bogdanov never admitted it. In fact, they vigorously used to defend their ideas up till today, and even created a ""Bogdanov affair" affair", by publicly defending their rights to change the contents of the public article on their affair. When some commentators tried to prevent their interference with the contents of the site describing the affair, they created a ""Bogdanov affair" affair", moving the focus of the discussion from the contents of their "theory" to the topic of their rights to publicly defend their ideas. (Baez, 2006)

It is rather important to review Bogdanov affair in more detail. Unlike Sokal, who was a respected physicist when he performed his hoax, the Bogdanovs have received their Ph. D.s (in 1999 and 2002) from University of Burgundy with barely passing notes, and with referees' notes full of disclaimers. Secondly, from the 1980s the brothers Bogdanov were rather popular TV presenters of funny popular science and science fiction programmes *Rayons X* and *Temps X* on TV channel France 2. Thirdly, with the money earned from the shows, they have established an internet domain under the fake name "International Institute of Mathematical Physics" (th-phys.edu.hk), and "created erroneous suggestions amongst forum participants as to a possible link with The University of Hong Kong or the Hong Kong University of Science and Technology" (Bogdanov affair, Wikipedia). This fake "institute" intended to corroborate their credibility, and a number of alleged physicists have been used to defend the Bogdanovs on the internet.[3]

Further, they have published a book *Dieu et la Science* (*God and Science*), which has "provoked a dispute of its own in 1992, when University of Virginia astronomy professor Thuan accused the Bogdanovs of plagiarizing his book *The Secret Melody: And Man Created the Universe*" (Bogdanov affair, Wikipedia; Monastersky, 2002).

These facts, the Bogdanovs' role as entertainers, their lawsuit (settled out of court) concerning the plagiarism, and

their plagiary credibility in general (while constructing fake research "Institutes"), could easily have made referees of reputed scientific journals more alert, skeptical and weary of the contents of their papers. But they have not. In 2004 and 2006 the Bogdanovs published two more books – *Avant la Big Bang* (2004) and *Voyage vers l'instant zéro* (2006) in which they continue with the semi-serious spoof of their creation, mocking the whole science of physics.

The affair has continued to this date in an untimely fashion, creating a host of usenet groups, blogs, additions of their texts and the texts of their critics, and together with previous facts, created a picture of a truly postmodern situation – in which it is barely recognizable whether the whole affair is a postmodern setup, a funny show, or a veritable, real scandal. A reader who encounters the description of the affair for the first time is not really sure whether the whole story is an elaborate hoax, whether some parts of the story present a hoax, and the others criticisms, whether discussions on the usenet groups are made up or written by idiots, where exactly to draw the line between fiction and faction, and whom to believe. In ways more than one, the Bogdanovs have scored: they published nonsense in the best physical journals, but also, they made a lot of effort to stifle criticisms and outrage, not by preventing anyone to discuss their work, but rather by adding further nonsense to the already existing one. By claiming that they mean everything they said, by suing critics of their plagiarism, by convincing critics to withdraw parts of their public utterances, they almost succeeded in blurring the boundaries of fact and fiction, of right and wrong, of reality and fiction, private and public, true and meaningless.

But it is rather significant that this was done by professed physicists, not by sociologists.

Apart from the fact that respectable scientists, referees and journals, with Nobel Prize winners in the editorial boards have been fooled a number of times in succession, physicists who have bothered to read all the Bogdanovs' texts have claimed that the texts are just variants of the same fraudulent article (Baez, 2002). In this sense, I think we are justified to say, the Bogdanov affair proves to be even more serious than Sokal's.

Sokal's hoax (Sokal, 1996a, 1996b; Sokal and Bricmont, 1998), created a public uproar, and as we shall soon see, in fact reversed the style of thinking in social sciences and its previous public prestige. It made social scientists weary of commenting scientific claims that they do not understand, and it made scientific research more vigorous than before. Although Bogdanov affair made it to the public, (see for instance: Monastersky, 2002; Overbye, 2002; Economist, 2002; Drösser and Schna-

bel, 2002; Morin, 2002; for a discussion among physicists see Baez, 2002) Bogdanov affair, however, did not create a similar public impact, and did not, at least not immediately, create new publishing or research standards in physics.

Why is this so? Can we learn anything from the Bogdanov affair as we did from Sokal's? If we agree that Sokal showed what was wrong with cultural studies, we may now justifiably ask: what is wrong with physics now?

One of the points of Sokal's affair was his implicit claim, that there are two sorts of sciences: rigorous ones, with credible sources and methods (viz. empiricist and rigorous, clearly defined theories in natural sciences), and postmodern cultural studies, with all sorts of mumbo-jumbo written all-over, on the other. However, Bogdanov affair proves, if anything, that rigorous sciences, like physics, may be even more prone to such hoaxes, at least by considering the fact that it has been perpetrated several times, and that included the highest ranks of physicists. One reason why such hoaxes may be more frequent in physics than in cultural studies is the fact that the contents of such papers, unlike in the case of postmodern cultural studies, may be evaluated just by a rare few. Therefore, we might have expected that a more stringent set of rules towards fraudulent research will be put in place precisely for such research. The other possible explanation is that something is deeply wrong with physics, or at least with cosmology.

WHAT IS WRONG WITH CURRENT (NATURAL) SCIENCE?

Let's see then what is going on in contemporary physics. Let us hear what respectable physicists have to say.

In 2005 a well-known editor of the Internet site *Edge*, John Brockmann asked 100 most influential scientists to say what they believe but cannot prove. A number of physicists have given some striking answers. Philip Anderson, a Nobel laureate from Princeton, for instance, claimed:

> String theory is the first science in hundreds of years to be pursued in pre-Baconian fashion, *without any adequate experimental guidance* (italics mine). It proposes that Nature is the way we would like it to be rather than the way we see it to be; and it is improbable that Nature thinks the same way we do.
>
> Anderson, 2005

What Anderson says in effect, is that theoretical physics of contemporary cosmology uses s.c. anthropic principles, which lack empirical support, constructs universes from the facts known to us today, and creates nature's laws upon our own image. In a more speculative way, we may add to it by saying that contemporary string theory lacks the rigor of the previous scientific age.

Another physicist and Nobel Laureate, Leon Lederman from Fermilab, answered the same question in the following fashion:

> To believe without knowing it cannot be proved (yet) is the essence of physics. Guys like Einstein, Dirac, Poincaré, etc. extolled the beauty of concepts, in a bizarre sense, placing truth at a lower level of importance.
> Lederman (Brockman, 2006)

Lederman claims that for contemporary physicists it is of the essence to have beliefs that cannot be proved. The truth of the belief is a minor, even an irrelevant add-up to the fact that we have convictions. This claim can be understood as saying that for physics of today, standard scientific criteria (truth, empirical content etc.) may not be valid. Is it a wonder then, that Bogdanov affair occurred, that it has been repeated a number of times, and that it did not create a scandal as Sokal's affair did in the humanities? (To see other similar quotes from eminent physicists, see my papers (Polšek, 1999, 1998, 1997).)

A more reasonable and a more sober voice was uttered by a respected physicist Lee Smolin, from Perimeter Institute. Unlike his Nobel Laureate colleagues, he feels that this description of contemporary physics presents at least a scandal of the kind. According to Smolin, this is not the state we should wish for, but rather a description of the "Crisis in Physics". For us lay people in physics, it is of some interest to quote Smolin at some length, to get a grip of the extent of crisis in physics.

> For more than two hundred years, we physicists have been on a wild ride. Our search for the most fundamental laws of nature has been rewarded by a continual stream of discoveries… (but t)he last time there was a definitive advance in our knowledge of fundamental physics was the construction of the theory we call the *standard model of particle physics* in 1973. *The last time a fundamental theory was proposed that has since gotten any support from experiment was a theory about the very early universe called inflation, which was proposed in 1981.*

Explaining adjustable parameters in string theory, Smolin continues with a serious criticism of contemporary astrophysics:

> *This means that the theory is unlikely to be definitively tested in upcoming experiments. Even if the theory is not true, many possible outcomes of the experiments could be made consistent with some choice of the parameters of the theory* (italics mine)… No matter what future experiments see, the results will be compatible with vast numbers of theories, *making it unlikely that any experiment could ei-ther confirm or falsify string theory. This realization has brought the present crisis to a head. Steven Weinberg and Leonard Susskind have argued for a new definition of science in which a theory may be believed without*

> *being subject to a definitive experiment whose result could kill it…* Among infinity of theories and infinity of universes, the only predictions we can make stem from the obvious fact that we must live in a universe hospitable to life. If this is true, *we will not be able to subject our theories to experiments that might either falsify or count as confirmation of them. But, say some proponents of this view, if this is the way the world is, it's just too bad for outmoded ways of doing science… When the contact with experiment disappeared in the 1980s, we were left with an unprecedented situation… We do not have a precise definition of the theory, either in terms of physical principles or mathematics. Nor do we have any reasonable hope to bring the theory into contact with experiment in the foreseeable future.* We must ask how likely it is that this style of research can succeed at its goal of discovering new laws of nature…
> Smolin, 2006

If we take these words as a fair description of contemporary physics, and this does not seem to be too unreasonable, we should pose even more important questions in the science wars of the future than the ones raised by the Bogdanov affair, and definitely more important ones than Sokal's. For instance, if the majority of cosmological theories today (and Smolin speaks of hundreds) do not have even a potentially testable realistic verification, if they do not envisage potential experimental proofs, is it reasonable for a society or a friendly-hostile community of scientists to invest time and energy in such research (science?), or for a society to invest huge amounts of money in it, instead to dedicate such funds to improve human condition, or at least in theories and scientists who purport to do so?[4] I am not an activist, but such a conclusion seems to me unavoidable.

Further, it seems that the Bogdanov affair pinpoints a fundamental weakness in the "strong sciences", (in this case – in cosmology): the weakness, sociologists of science were once prone to – to treat speculations as legitimate theories.[5]

Apart from the myth that natural sciences always stick to the methodological rules, one of which is to envisage for a theory a possible test with reality, there are other myths, like the myth of progress, exposed, for our present purposes by John Horgan. Further argument for the crisis in "higher" types of science (natural science) is provided by John Horgan (Horgan, 2006). A notorious skeptic towards scientific progress, science journalist John Horgan, in his article written for a 10--year's anniversary of his famous book *End of Science* reinstated his general thesis: We cannot expect any dramatic scientific discoveries in the future. But it is for the new arguments he brings about, that we quote him:

> Scientists' attempts to solve these mysteries often take the form of what I call ironic science – unconfirmable speculation more akin to philosophy or literature than genuine

> science. (Science is ironic in the sense that it should not be considered a literal statement of fact.) A prime example of this style of thinking is the anthropic principle, which holds that the universe must have the form we observe because otherwise we would not be here to observe it. The anthropic principle, championed by leading physicists such as Leonard Susskind of Stanford University, is cosmology's version of creationism… Another example of ironic science is string theory, which for more than 20 years has been the leading contender for a "theory of everything" that explains all of nature's forces. The theory's concepts and jargon have evolved over the past decade, with two-dimensional membranes replacing one-dimensional strings, but the theory comes in so many versions that it predicts virtually everything – and hence nothing at all. Critics call this the "Alice's restaurant problem", a reference to a folk song with the refrain, "You can get anything you want at Alice's restaurant." This problem leads Columbia mathematician Peter Woit to call string theory "not even wrong" in his influential blog of the same title, which refers to a famous put-down by Wolfgang Pauli.
> Horgan 2006

Again, the formulation of the present malaise in the natural sciences (notably in astrophysics and cosmology) is very similar to Sokal's verdict on the social sciences of the 1990s. Speculation abounds, and in the overwhelming number of cases, we cannot even say whether the proposed theory of a hypothesis is wrong. According to Horgan, such formulas are more akin to literature than science proper.

> They adhere to the postmodern position that we do not discover truth so much as we invent it; all our knowledge is therefore provisional and subject to change. If all our scientific knowledge were really this flimsy and provisional, then of course science could continue forever, with theories changing as often as fads in clothing or music. But the postmodern stance is clearly wrong. We have not invented atoms, elements, gravity, evolution, the double helix, viruses, and galaxies; we have discovered them, just as we discovered that Earth is round and not flat.
> Horgan, 2006

This description applies to other disciplines as well, for instance neurology and psychiatry: "The postmodern perspective applies all too well to fields that attempt to explain us to ourselves." (Horgan, 2006). What Horgan is saying, in effect, is that the whole of science has become postmodern, in Sokal's sense: it has become "ironic", skeptical, not serious, and vacous. We may not go that far, but for our purposes, it suffices to say, that there are disciplines (and the most respected ones at that), apart from SSS, where this description really holds.

So, all is not too-well in natural sciences either, although we should not always keep Horgan for his word.

WHAT WAS WRONG WITH PRE-SCIENCE WARS SOCIAL STUDIES? ORIGINS OF TROUBLE

There are many accounts on what went wrong with science studies, and how science studies have run into trouble. The most respectable critics, the most ardent proponents of realism, like Kitcher (2000) and Laudan (1996) are interestingly looking for the sources of trouble in the *philosophy* of science dominating the seventies and eighties, not in the social sciences themselves.

I shall review just a few descriptions of the malaise in the social sciences, but let us first see what has actually been attacked and disputed.

Apart from Sokal's affair, one of the major and early contributions to dismantling fraudulent or irrelevant social science was a collection of articles gathered in a book *A House Built on Sand*, edited by Noretta Koertge (Koertge, 2000). Social sciences which the authors are attacking consist, according to Koertge, of the following common tenets:

- Every aspect of that complex set of enterprises that we call science, including, above all, its content and results, is shaped by and can be understood *only* in its local historical and cultural context.
- In particular, the products of scientific inquiry, the so-called laws of nature, must always be viewed as social constructions. Their validity depends on the consensus of "experts" in just the same way as the legitimacy of a pope depends on a council of cardinals.
- Although scientists typically succeed in arrogating special epistemic authority to themselves, scientific knowledge is just "one story among many." The more epistemological authority that science has in a given society, the more important it is to unmask its pretensions to be an enterprise dedicated to the pursuit of objective knowledge. Science must be "humbled."
- Since the quest for objective knowledge is a quixotic one, the best way to appraise scientific claims is through a process of political evaluation. Since the "evidence" for a scientific claim is never conclusive and is always open to negotiation, the best way to evaluate scientific results is to ask who stands to benefit if the claim is taken to be true. Thus, for the citizen the key question about a scientific result should not be how well tested the claim is but, rather, *Cui bono*?
- "Science is politics by other means": the results of scientific inquiry are profoundly and importantly shaped by the ideological agendas of powerful elites.
- There is no univocal sense in which the science of one society is better than that of another. In particular, Euroscience is not objectively superior to the various ethnosciences and shamanisms described by anthropologists or invented by Afrocentrists.

- Neither is there any clear sense in which we can talk about scientific progress within the European tradition. On the contrary, science is characterized chiefly by its complicity in all the most negative and oppressive aspects of modern history: increasingly destructive warfare, environmental disasters, racism, sexism, eugenics, exploitation, alienation, and imperialism.
- Given the impossibility of scientific objectivity, it is futile to exhort scientists and policymakers to try harder to remove ideological bias from the practice of science. Instead, what we need to do is deliberately introduce "corrective biases" and "progressive political values" into science. There is a call for "emancipatory science" and "advocacy research."

Koertge, 2000: 8

We can summarise some general points of dispute between "realists" and "relativists" by some famous quotations:

1. Constructivist epistemology. Paul Forman speaks approvingly of "our postmodern works" with its social constructivist epistemology and a "morality based, rather that truth based *Weltgefuehl*".

2. The natural world is not a criterium of truth. According to Harry Collins (Collins, 2007: 3) "(t)he natural world has a small or non-existent role in the construction of scientific knowledge".

3. Reality does not matter. According to Latour (1979: 237) "reality is the consequence rather than the cause of the social construction of facts".

According to Alan Sokal who made several valuable contributions in the Koertge volume (2000) and elsewhere (Sokal, Bricmont, 1998), the problem with postmodernism and philosophy of science leading to postmodernism is in conflating levels of trouble and (possibly) their explanation. According to Sokal there are various levels of explanation:

1. *Ontology*. What objects *exist* in the world? What statements about these objects are *true*?
2. *Epistemology*. How can human beings obtain *knowledge* of truths about the world? How can they assess the *reliability* of that knowledge?
3. *Sociology of knowledge*. To what extent are the truths *known* (or *knowable*) by humans in any given society influenced (or determined) by social, economic, political, cultural, and ideological factors? Same question for the false statements erroneously believed to be true.
4. *Individual ethics*. What types of research *ought* a scientist (or technologist) to undertake (or refuse to undertake)?
5. *Social ethics*. What types of research *ought* society to encourage, subsidize, or publicly fund (or alternatively, to discourage, tax or forbid)?

Sokal, 2000: 14-15

Consider first the two issues that seem to be at the core of the problem, since relativists and constructivists always maintain that the truth of the sentences belongs to our con-

structed explanations, to our framing of the notion of reality. According to Sokal, in postmodernist works, "(w)e often find phrases like "the social construction of facts" that intentionally elide the distinction between facts and our knowledge of them". This was partly due to the conflation of the two realms (reality and our knowledge of reality), and partly, as we shall soon see, upon the postmodernist' intention to change social conditions by theorizing, especially in situations presenting facts as something immutable and unchangeable. In general, both claims (social construction, and changing social circumstances) have merged, and were used as if the notion of social construction automatically brings liberation.

A good way to describe, and to solve the first problem (conflation of reality and knowledge) is given by James Robert Brown. Brown (Brown, 2001), along with Sokal, schematizes the trouble, and provides an answer, by following a useful diagram (Table 1).

TABLE 1
Confusing ontological and epistemological senses of objectivity and subjectivity

	Objective	Subjective
Ontology	Water is H_2O. (A fact about independent nature.)	Water is tasteless. (A fact based on us, not about independent nature.)
Epistemology	Belief that water is H_2O. (Based on standard evidence from chemistry.)	Belief that water is Zeus's urine. (Stemming from smoking too much dope while reading the Iliad.)

Source: Brown, 2001: 103

According to Sokal, Brown, Kitcher (2000) and others, this kind of a confusion led postmodernist, constructivist social scientists to the extreme: to doubt in reality, and in any objective common-sense truth, at least if it serves to attack political views with which they disagreed. Interestingly, social studies of science (SSS) have recently changed their relativist stance, not by accepting the arguments above, about conflating epistemological and ontological levels, but rather under the influence of another argument: when it was shown that radical relativism jeopardized any possibility to criticize political opponents – right wing creationists for instance, or that it would put in question some "indubitable" facts – like global warming (see Latour's remark below).

Philip Kitcher, a well known British philosopher summarized the postmodern trouble itemizing the s.c. "four dogmas of science studies" (Kitcher, 2000). According to Kitcher, these troubles belong to the philosophical realm: 1. *Theory Ladenness of Observation*: There is no truth save social acceptance; 2. *Underdetermination of Theory by Evidence* (Duhem-Quine thesis): no system of belief is constrained by reason or reality, and no system of beliefs is privileged; 3. *Variety of Belief* (Bloor's idea

of symmetry) – that "there shall be no asymmetries in explanation of truth and falsehood, society or nature" and 4. "*Actor's Categories" and the Writing of History*: whereby "narratives must be constructed in terms of "actor's categories".

There are no easy solutions to the first two dogmas, but just as the notion of construction all-too-easily conflated the epistemological and ontological realm, Kither proves that theory ladenness and underdetermination of theories need not provide sure ground for relativism and postmodernism. In the text that follows, we shall discuss "dogma of variety of belief", and reinterpret it in the light of the previously said, and leave dogma #4, about "actor's categories", for some future discussion.

While discussing the first two "dogmas", Larry Laudan (1996) went even further, and blamed the whole tradition in philosophy of science (positivists as well as post-positivists) for using the idea of "framing", "linguistic representation of reality", for a "discourse" in which the notion of representation of reality by definition cannot reach reality.

These two critics testify to the fact that sociologists of science were not the only, not even the primary causes of postmodernist malaise. But, to be sure, they carried already prepared philosophical notions to rhetorical extremes.

The second type of problem stems from the postmodernist, and modernist, idea that science should be engaged.

The idea that social construction of reality, as conceived by constructivists, automatically brings liberation was analyzed by the Canadian philosopher Ian Hacking. In his book *Social Construction of What?* (1999), after analyzing a range of social phenomena, Hacking concludes with the following:

> The idea of social construction has been wonderfully liberating. It reminds us, say, that motherhood and its meanings are not fixed and inevitable, the consequence of childbearing and rearing. They are the product of historical events, social forces, and ideology… Unfortunately social construction analyses do not always liberate. Take anorexia, the disorder of adolescent girls and young women who seem to value being thin above all else. They simply will not eat. Social construction theses are liberating chiefly for those who are on the way to being liberated. For all their power to liberate, those very words, social construction can work like cancerous cells.
>
> Hacking, 1999: 2

Furthermore, Hacking, in effect, is proving a very simple idea that some constructed facts are not good as such or not worthwhile, while some facts that can be constructed need not necessarily be changed.

> One may realize that something, which seems inevitable in the present state of things, was not inevitable, and yet is not thereby a bad thing. But most people who use the social construction idea enthusiastically want to criticize, change, or destroy some X that they dislike in the established order of things.
> Hacking, 1999: 7

Sokal's point in his hoax (Sokal, 1996a, 1996b) was to show that postmodernists fail in their professed goals. The idea of engaged science (especially as the only way of measuring relevance in science) ended up precisely with its opposite.

So, instead of automatically deriving liberation from the idea of "social construction", some scholars have translated the political *engagement* into four distinct categories. According to Brown (2001), science and politics do not match. There are various combinations of scientific and political stances (Table 2).

TABLE 2
Combinations of political and scientific stances

	Political Left	Political Right
Hostile to orthodox views of science	Some social constructivists, postmoderns	Religious conservatives, anti-Darwinians
Friendly to orthodox views of science	Sokal, Chomsky, Gould, Lewontin, the Vienna Circle	Some sociobiologists, race and IQ theorists

From: Brown, 2001: 26[6]

WHO HAS WON THE SCIENCE WARS?

So, on the basis of all things discussed, one can pose the question: who has won the science wars?

Reviewing issues raised by Sokal's hoax, James Robert Brown (Brown, 2001) mentioned several on-going historical battles between camps or among various scientific fractions; the first allegedly being between the protagonists of enlightenment and romantics at the beginning of the XIX century, the second between the first and the second culture, described by C. P. Snow, in the 1950s. Brown's broad perspective gives us a possibility to pause, and review the present situation more thoroughly.

Sokal's hoax was a blow to many humanities. A number of scholars in the social studies of science felt at the time that there would be no immediate recovery. But a striking fact was that humanities (sociology of science, cultural studies) did recover. For instance, even the still living proponents of cultural studies, of postmodern "theory", felt (with a significant time-lag) that something with humanities went seriously amiss, and therefore proposed different styles of thinking, and different ways of "doing theory". One such example is Terry

Eagleton, who begins a review of cultural studies in his *After Theory* with the following words:

> The golden age of cultural theory is long past. The pioneering works of Jacques Lacan, Claude Levi-Strauss, Louis Althusser, Roland Barthes and Michel Foucault are several decades behind us. So are the path-breaking early writings of Raymond Williams, Luce Irigaray, Pierre Bourdieu, Julia Kristeva, Jacques Derrida, Helene Cixous, Jurgen Habermas, Fredrick Jameson and Edward Said. Not much that has been written since has matched the ambitiousness and originality of these founding mothers and fathers. Some of them have since been struck down. Fate pushed Roland Barthes under a Parisian laundry van, and afflicted Michel Foucault with Aids. It dispatched Lacan, Williams and Bourdieu, and banished Louis Althusser to a psychiatric hospital for the murder of his wife. It seemed that God was not a structuralist.
>
> Eagleton, 2003: 1

Other primadonnas of cultural studies have voiced the same disappointment with the state of the art in the cultural studies. Bruno Latour, in his "Why Has Critique Run out of Steam" for instance claims:

> I myself have spent some time in the past trying to show "the lack of scientific certainty" inherent in the construction of facts… But I did not exactly aim at fooling the public by obscuring the certainty of a closed argument – or did I? After all, I have been accused of just that sin... Was I foolishly mistaken? Have things changed so fast?... While we spent years trying to detect the real prejudices hidden behind the appearance of objective statements, do we now have to reveal the real objective and incontrovertible facts hidden behind the illusion of prejudices? Was I wrong to participate in the invention of this field known as science studies? Is it enough to say that we did not really mean what we said? Why does it burn my tongue to say that global warming is a fact whether you like it or not? Why can't I simply say that the argument is closed for good?... Should I reassure myself by simply saying that bad guys can use any weapon at hand, naturalized facts when it suits them and social construction when it suits them? Should we apologize for having been wrong all along? Or should we rather bring the sword of criticism to criticism itself and do a bit of soul-searching here – what were we really after when we were so intent on showing the social construction of scientific facts? Nothing guarantees, after all, that we should be right all the time.
>
> Latour, 2004

This disappointment with cultural studies is felt elsewhere, by the protagonists of the so-called Theory (i.e. by relativists and constructivists alike).

But in other, more stringent humanities and social sciences, most notably in the field of economics and psychology, there are other visible signs that humanities and social sciences have recovered from Sokal's blow. We see a serious move towards finding facts, facts, facts, out of which as yet no visible paradigm has come to the fore.[7] But at least this drive to collect new facts on human nature, on rationality and cognition, on selfdelusion and bias, on societal facts and correlations, gives us promise that one day new humanities will get their own Einstein. And this analogy brings us to the issue: it is the humanities and social sciences nowadays that give us hope, and the promises for the betterment of humankind, not by criticizing science, but by doing science proper: by making and testing theories, by experimenting and designing applications. And we, not the physicists are looking for a new Einstein, a genius who would be able to make sense of the vastness of already collected pure data.

Noretta Koertge wittily said in 2000 that "(a) respectable account of the broader historical context of the Science Wars awaits the touch of a future Gibbon." (Koertge, 2000: 6) It is true, that describing circumstances surrounding Science Wars (in all its phases) feels like an immense task. But we need no future Gibbon to tell us what to think now. We need no social circumstances, or politics, or change in society to bring the question to the fore, whether postmodern posturing was a proper way of doing "theory", and we also need no external circumstances to explain and believe that something wrong was going on in theoretical physics.

And precisely with this conclusion we reach the true and original message of the SSS and perhaps of cultural studies.

WHAT HAS BEEN THE EXIT STRATEGY? WHAT MIGHT BE AN ADDITIONAL EXIT STRATEGY?

One "exit strategy" from the constructivist-realist clash was to frame legitimate claims of both camps, by replacing outrageous postmodernist claims with the ones sensible people would have to endorse. According to Kitcher and Sokal (Koertge, 2000), the sides in the science wars may be grouped in two clusters: The first is the *realist-rationalist cluster* which endorses the following claims:

1. In the most prominent areas of science, research is progressive, and this progressive character is manifested in increased powers of prediction and intervention.
2. Those increased powers of prediction and intervention give us the right to claim that the kinds of entities described in scientific research exist independently of our theorizing about them and that many of our descriptions are approximately correct.

3. Nonetheless, our claims are vulnerable to future refutation. We have the right to claim that our representations of nature are roughly correct while acknowledging that we may have to revise them tomorrow.
4. Typically our views in the most prominent areas of science rest upon evidence, and disputes are settled by appeal to canons of reason and evidence.
5. Those canons of reason and evidence also progress with time as we discover not only more about the world but also more about how to learn about the world.

The second cluster is the so called socio-historical one which endorses the following:

1. Science is done by human beings, that is, by cognitively limited beings who live in social groups with complicated structures and long histories.
2. No scientist ever comes to the laboratory or the field without categories and preconceptions that have been shaped by the prior history of the group to which he or she belongs.
3. The social structures present within science, affect the ways in which research is transmitted and received, and this can have an impact on intratheoretical debates.
4. The social structures in which science is embedded affect the kinds of questions that are taken to be most significant and, sometimes, the answers that are proposed and accepted.

Kitcher, 2000: 34-35

When science wars are described in such a way, as a war between these two camps, it becomes immediately visible that it is very easy to find a "middle ground". According to Sokal and Kitcher, the description above is precisely the middle ground we are searching for: "Here the middle ground – based on a respect for both the "realist-rationalist cluster" and the "socio-historical cluster", even as we may debate their relative importance in specific cases – is so eminently sensible that nearly all scientists and philosophers of science would give their assent, as would most (though apparently not all) sociologists of science." (Sokal, 2000: 18)

There are perhaps many other ways to solve problems between two cultures and between sides in science wars. However, I would like to suggest another simple strategy that is completely oblivious and impartial towards sides in the science wars.

Let me call this idea – the revitalization of the symmetry. Drawing upon the description of Bogdanov and Sokal affairs, let me draw a simple idea: *since both sides on the science wars are susceptible to fraud and prone to perform bogus science, we should be alert in both cases, towards any such claims.*

I am calling this idea "symmetry", although it has a troubled history. For some historians of SSS, this point was even pinpointed as the origin of trouble – within the so-called "strong programme" of the Edinburgh school, most notably in David Bloor's work (Turner, 2007; Zammito, 2007). I would like to modify some of Bloor's claims to fit the present situation. And by doing this, I hope to show that strong programme is not beyond correction. As a matter of fact, I shall claim not only that it becomes unproblematic, but rather that we can still keep its normative flavour.

Strong programme's claims are, no doubt, well known: causality, impartiality, symmetry, reflexivity (Bloor, 1976). Some scholars doubted that reflexivity claim can be fulfilled, especially if you work in the field of sociology of scientific knowledge, since then, allegedly, you run into a paradox. But I am not going to deal with self referentiality.

The most disputed claim of the strong programme was the symmetry claim (see among many, for instance Hollis, 1982; Hollis and Lukes, 1982; Slezak, 1989; Brown, 1984). This claim means that we should investigate the sources of scientific claim's credibility independently from its truth content. We should, so to say, put the truth in brackets, and investigate the question what kind of social and scientific ecology has given a claim its status of truth.

But, let me rephrase the symmetricity claim, so that we get rid of its problematic character. We should have no positive prejudices for certain kinds of scientific investigation. Scientific claims, as we have seen in the Bogdanov affair, are not infallible, even when they are uttered by the most popular and well known scientists. "High" science (like cosmology) has prima facie no privileges in that respect. All beliefs are, to start with, on *a par*.

The usual framing of the symmetry problem is in terms of which camp is closer to the truth (for instance: the first culture of cultural scientists, or the second, of natural scientists and engineers, or: Steven Hawking or perhaps Uri Geller). But, there is another way to frame the problem: which kind of science is more prone to fraud? In which area are we more likely to expect fraudulent or bogus science?

Let us see the difference between the usual picture, a hierarchical view, and the one I would like to argue for. In the usual picture (for scientists), there is a world of difference between pseudo-scientific claims (say of Uri Geller) and high-energy physics (or cosmology of Steven Hawking's kind). Science starts with pseudo-science, transforms itself into purely problematic, but perhaps testable claims, then it becomes ordinary (normal) science, and it ends up with the queen of sciences, with the high science of physics.

TABLE 3
Asymmetrical approach to scientific claims

This hierarchical view is rigidly asymmetrical. Consider the "romantic" proposals provided by the famous cosmologist Carl Sagan.

Scientific claims	Pseudo-scientific claims
Speculation allowed	Speculation not allowed
Claims proven wrong are incorrect	Claims proven wrong are irrational
Fraud in the name of science allowed	Fraud in pseudo-science should be illegal

(according to Carl Sagan, 1986)

Sagan considers two types of claims as varying in kind, but does not give us a clue what should *prima facie* distinguish them. Take an example. Sagan himself (Sagan, 1994) claimed that several meteorites found on Earth are of Marsian origin. Is this claim *prima facie* scientific, or is it bogus, pseudo-scientific or perhaps even intentionally fraudulent? Should we *prima facie* allow such speculations, or should we discard them right away? Well, we do not know before we test them. Moreover, it is not clear why should it matter in the first place. In what sense is it important to ascribe such a claim scientific or pseudo-scientific status even before we test the hypothesis? Why should we make an a priori demarcation? Is it sensible? What sorts of purposes would it serve to make such *a priori* ascriptions?

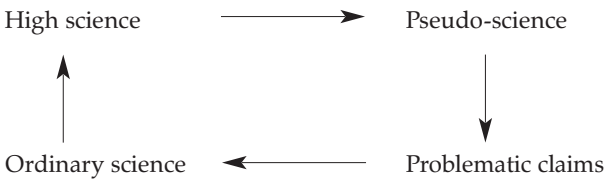
It is more sensible to withdraw from judgement, and to forget about whether the claim was uttered by a respected scientist, or by some bogus lay man. The truth content of such a claim has to be investigated no matter who has claimed it. Or at least so we hope science should work.

In the discussion on the Bogdanov affair and contemporary high physics, we have seen that "high" science or high-energy physics is not absolved from ordinary human mistakes and human conduct in general. It is prone to the same types of mistakes as the ordinary pseudo-science (unverifiability, speculation etc.). So, instead of keeping a rigidly hierarchical view towards science claims, if we want to keep the categories at all, we should re-draw the picture in the way showed in Diagram 1.

If this is the proper picture of scientific "developments", we see that in a sense we should expect more fraudulent or speculative science the higher we climb the ladder. It is in the area of high science (cosmology), not in the area of ordinary science (like cultural studies) that we should expect to find more speculation and more unverifiable claims.

But the whole argument for symmetricity does not hang on the truth of the description (picture). My point is that we should not *a priori* absolve high science from fraud and wild

speculation, and that precisely because of it, we should stay impartial, and treat both (or perhaps all) kinds of claims in the same way. This is the revitalized, or perhaps, this should have been the original, idea of symmetricity.



➲ DIAGRAM 1
Circle of credibility

From: Polšek, 1997

In short, the idea of symmetricity does not jeopardize the idea of truth, neither of independent reality, as Bloor was interpreted to have it, but rather it is a methodological claim to treat all statements as *a priori* equal before testing. And instead of treating symmetry as a hallmark of irrationality, this version of symmetry should be treated by the same token as a gatekeeper of rationality, sensibility and justice.

This does not seem to be all-too-problematical. What stays problematical is whether such *a priori* skepticism (or trust) necessarily leads to skepticism towards science as such. Let me consider examples:

TABLE 4
Categories of skepticism (type of science studies in brackets)

	Attitude: pro-science Methodology: asymmetrical	Attitude: anti-science Methodology: symmetrical
Confidence/Trust	Shermer (pseudoscience) Gardner (pseudoscience) Sagan (pseudoscience) Rothman (pseudoscience) Perutz (science and war) American Skeptics (pseudoscience) Gross & Levitt (leftist science) Sokal (leftist science)	Strong programmers Wallis (Rejected knowledge)
Distrust	Broad & Wade (misconduct) Lewontin (social use of science) Dewdney (science classics) Grinnell (misconduct) Bell (science financing, nepotism) Freeman (psychology of scientists)	Milton (suppressed research) Heideggerians Marxists Creationists

(see: Polšek, 1997, 1998, 1999)

Table 4 shows different attitudes towards science along the axis trust/distrust in science, and pro-science/anti-science. But in a way, there is a perfect solution for all cases, if we

agree with the revised idea of simple symmetricity. Whether anyone accepts scientific claims in advance or not, and whether anyone has sufficient trust in science should be irrelevant, if we accept the very simple idea to treat all beliefs on *a par* before they are put to test.

Of course, pragmatically we never do it. There is no sufficient reason to test again the hypothesis that the world is flat, or that the Earth is the center of the universe. But if anyone comes again with such a brilliant hypothesis, we can nevertheless show him the accumulated evidence to the contrary.

My point in reviving the idea of symmetry is not to exclude wild guesses, but to put them initially on *a par* with rather obvious nonsense, to make a point: that no claim should be a priori treated differently, and that all claims (no matter whether they come from Uri Geller or Carl Sagan) should be put to the same kinds of tests, the tests of reality and of accumulated human experience. We should be equally trustful and equally critical towards claims, independently from the question of their social validity. The Bogdanov affair was another very good lesson for doing so.

CONCLUSION

The Bogdanov affair has shown that a postmodern social situation (speculation, building artificial institutes, proliferation of unimportant discussions, outright fraud, blurring the boundaries of scientific importance and truth) may invade high science of astrophysics, and that even the most respected or respectful scientists are not infallible and beyond reproach. Human and social sciences have learnt a lesson from Sokal's affair: the level of (self)criticism has been raised, and outrageous claims from previous times have been severely criticized not only by external scientists, but by the proponents of postmodernism themselves. (Perhaps, they have done so by fulfilling their "reflexivity claim".) Have other sciences done the same? I wonder. But there is a reconciling way out. It consists in framing legitimate theses of both sides in the science wars in a plausible way for all. I have added another simple, and perhaps more achievable criterion, a rather well known claim in science: to be as critical, or as "symmetrical" as possible towards all scientific and anti-scientific claims. Only in that way may we expect progress, not only within disciplines, but in joint ventures of all scientific endeavours. It goes without saying: only in that way may we expect to diminish the number of fraudulent and bogus research, and to be less prone to "scientific" hoaxes.

NOTES

[1] I am grateful to an anonymous reviewer for a very thorough reading of the text, valuable comments and corrections. In several places (s)he requested from me a reconsideration of my theses from the

perspective of science financing. Also, there was a request to pinpoint the place of biological, especially genetic sciences within the proposed matrices. While such comments raise very interesting issues, their elaboration would expand the already oversized article. They need not change my overall thesis, so I shall leave them to some future occasion.

[2] For a description of Sokal's affair, see text below.

[3] Since the time of writing this article, several "respectable" web sites at Hong Kong University have been created by Andrej Bogdanov. See for instance http://www.cse.cuhk.edu.hk/~andrejb/

[4] This kind of criticism is usual among "leftist" social scientists, especially of the "critical theory". See for instance: Adorno & Horkheimer 2002., Marcuse 1991.

[5] I use "strong" ("hard") and "soft" sciences as vaguely corresponding to the "natural" and "human" sciences. However, it is a truism among scientists to treat physics and mathematics as being the "hardest" of them all.

[6] Note my analogous quadrant below (from Polšek, 1997).

[7] At this point, an anonymous reviewer suggested an explanation for the lack of the new paradigm which would purport to show that "facts are in the service of politics and economy, not in the service of important content". I do not believe that collecting facts in the field of economy and psychology are necessarily, *a priori*, politically laden, but even if that were true, this could not explain the absence of a visible paradigm in humanities.

REFERENCES

Adorno, T. W., Horkheimer, M. (2002), *Dialectic of Enlightenment*. Trans. Edmund Jephcott. Stanford: Stanford UP.

Anderson, P. (2005), *What you believe but cannot prove*. http://www.edge.org/q2005/q05_10.html

Baez, J. (2002), *Physics bitten by reverse Alan Sokal hoax?* http://groups.google.com/group/sci.physics.research/msg/9f8fde48d7c3566?q=g:thl3105378894d&dq=&hl=en&lr=&ie=UTF-8&safe=off

Baez, J. (2006), *The Bogdanoff Affair*. June 21. http://math.ucr.edu/home/baez/bogdanoff/

Bloor, D. (1976), *Knowledge and Social Imagery*. London: Routledge & Kegan Paul.

Bogdanov Affair. *Wikipedia*. http://en.wikipedia.org/wiki/Bogdanov_affair

Brockman, J. (Ed.) (2006), *What We Believe but Cannot Prove: Today's Leading Thinkers on Science in the Age of Certainty*, New York: Harper Perennial. http://www.edge.org/q2005/q05_print.html

Brown, J. R. (Ed.) (1984), *Scientific Rationality: the Sociological Turn.* Dordrecht: Reidel.

Brown, J. R. (2001), *Who Rules in Science. An Opinionated Guide to the Wars.* Cambridge Mass.: Harvard University Press.

Collins, H. (2007), A new programme of research? Introduction. *Stud. Hist. Phil. Sci*. 38: 615-620.

Drösser, C. and Schnabel, U. (2002), Die Märchen der Gebrüder Bogdanov (Fairy tales of the Brothers Bogdanov). *Die Zeit* 46. http://www.wissenschaft-online.de/artikel/611412

Eagleton, T. (2003), *After Theory*. London: Basic Books. http://www.amazon.com/gp/reader/B00081H01I/ref=sib_dp_pt#reader-link

Economist (2002), Another physics scandal? *Economist*. Nov 14th 2002 http://www.economist.com/science/displayStory.cfm?story_id=1441745

Hacking, I. (1999), *Social Construction of What?* Cambridge Mass.: Harvard UP.

Hollis, M. (1982), *Social Destruction of Reality. M. Hollis, Steven Lukes. Rationality and Relativism*. Oxford: Blackwell.

Hollis, M. and Lukes, S. (Eds.) (1982), *Rationality and Relativism*. Oxford: Blackwell.

Horgan, J. (2006), Final Frontier. *Discover Magazine*. 22. 9. 2006. http://discovermagazine.com/2006/oct/cover

Kitcher, P. (2000), A Plea for Science Studies. In: N. Koertge (Ed.), *A House Built on Sand* (pp. 32-56), Oxford UP.

Koertge, N. (Ed.) (2000), *A House Built on Sand – Exposing postmodernist myths about science*. Oxford UP.

Latour, B. (2004), Why Has Critique Run out of Steam. From Matters of Fact to Matters of Concern. *Critical Inquiry* 30 2 (Winter): 25-248. http://www.bruno-latour.fr/articles/article/089.html

Latour, B., Woolgar, S. (1979), *Laboratory Life*. Princeton UP.

Laudan, L. (1996), *Beyond Positivism and Relativism. Theory, Method, and Evidence*, Boulder: Westview Press.

Marcuse, H. (1991.), *One Dimensional Man*, Boston: Beacon Press.

Monastersky, R. (2002), *French TV Stars Rock the World of Theoretical Physics. Chronicles of Higher Education*. Tuesday, November 5, http://chronicle.com/free/2002/11/2002110501n.htm

Morin, H. (2002), La réputation scientifique contestée des frères Bogdanov. *Le Monde*. 19 December. http://www-cosmosaf.iap.fr/Le%20Monde_fr%20%20La%20r%C3%A9putation%20scientifique%20contest%C3%A9e%20des%20fr%C3%A8res.htm

Overbye, D. (2002), Are They a) Geniuses or b) Jokers?; French Physicists' Cosmic Theory Creates a Big Bang of Its Own. *New York Times*. 9. November. http://query.nytimes.com/gst/fullpage.html?res=9401E4DB1431F93AA35752C1A9649C8B63

Polšek, D. (1997), Two Ways of Dealing With Scientific Fraud. *Sociološka revija*, 28 (3-4): 163-173.

Polšek, D. (1998), *Wer hat mit den Wissenschaftskriegen angefangen?*, http://www.uni-graz.at/vizewww/archiv/sokal/polsek_zitate.html

Polšek, D. (1999), *Sokal's Overkill: Wissenschaftliche Kriege setzen sich fort*. ReSoWi, Universität Graz. http://www-gewi.kfunigraz.ac.at/moderne/index.html

Sagan, C. (1986), *Broca's Brain. Reflections on the Romance of Science* New York: Ballantine Books.

Sagan, C. (1994), *Pale Blue Dot: A Vision of the Human Future in Space.* New York: Random House.

Slezak, P. (1989), Scientific Discovery by Computer as Empirical Refutation of the Strong Programme. *Social Studies of Science* 19.

Smolin, L. (2006), *A Crisis in Fundamental Physics*. New York Academy of Sciences. Jan/Feb. http://www.nyas.org/snc/updatePrint.asp?updateID=41

Sokal, A. (1996a), A Physicist Experiments with Cultural Studies. *Lingua Franca*. 23 May.

Sokal, A. (1996b), Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity. *Social Text*, 46-7: 217-252.

Sokal, A. (2000), What the Social Text Affair Does and Does Not Prove. In: N. Koertge (Ed.), *A House Built on Sand* (pp. 9-22), Oxford UP.

Sokal, A. and Bricmont, J. (1998), *Fashionable Nonsense. Postmodern Intellectual's Abuse of Science*.

Turner, S. P. a. M. W. R. e. (2007), *Philosophy of Anthropology and Sociology.* Elsevier B.V. http://www.sciencedirect.com/science/book/ 9780444515421

Zammito, J. (2007), What's 'new' in the sociology of knowledge? In: S. P. Turner, M. W. Risjord (Ed.), *Philosophy of Anthropology and Sociology. Handbook of the philosophy of science* (pp. 791-857), Amsterdam: North-Holland, http://www.sciencedirect.com/science/article/B87FM-4PCJC74-D/2/b54c8c07ca8299563d34b8a0d99fee45

Tko je pobijedio u znanstvenim ratovima?

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Afera Bogdanov u astrofizici vrlo je nalik na Sokalovu aferu u kulturnim studijama. U članku se raspravlja o sličnostima među njima i posebno o zastrašujućim postupcima i metodama kojima su se koristila braća Bogdanov, pa se iz rasprave izvlači zaključak da prirodne znanosti i znanstvenici nisu "izvan domašaja" kritike, da ih bavljenje "visokom znanošću" ne abolira od standardnih znanstvenih (i moralnih) zahtjeva, upravo kao što je to Sokalova afera tvrdila za društvene znanosti. U tome leži i dublja pouka: budući da "visoku znanost" razumije manje stručnjaka (i laika), ona je katkada podložnija "ludim" hipotezama, koje se ne bi tolerirale u običnijim disciplinama. Stoga trebamo rabiti barem "simetrični pristup" u kritici znanstvenih tvrdnji: ni vrsta znanosti ni slava znanstvenika ne smije biti garancija ispravna ponašanja i znanstvene metodologije. U članku se razmatraju razna značenja simetričnosti u znanstvenom pristupu, a rasprava o "povjerenju" i "nepovjerenju" u znanost zaključuje se opisom tzv. "kruga vjerodostojnosti".

Ključne riječi: afera Bogdanov, Sokalova afera, simetrija kritike, krug vjerodostojnosti

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POLŠEK, D.:
WHO HAS WON...

Wer ist der Sieger in den Wissenschaftskriegen?

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Die Bogdanov-Affäre in der Astrophysik erinnert stark an die Sokal-Affäre in den Kulturwissenschaften. In der vorliegenden Arbeit werden die Ähnlichkeiten zwischen den genannten Kontroversen erörtert, vor allem aber die erschreckende Vorgehensweise und Methoden der Brüder Bogdanov. Der Verfasser kommt zum Schluss, dass Naturwissenschaften und Forscher durchaus in den Einzugsbereich der Kritik fallen und die Beschäftigung mit der „erhabenen Wissenschaft" sie nicht davon freisprechen kann, den Forderungen wissenschaftlicher (und moralischer) Standards Genüge zu leisten – im Gegensatz zu den Behauptungen, die im Hintergrund der Sokal-Affäre laut geworden waren. Darin liegt auch der tiefere Sinn dieser Vorfälle: Da die „erhabene Wissenschaft" von einer geringen Zahl von Forschern (und Laien) verstanden werden kann, ist sie zuweilen stärker der Gefährdung durch „verrückte Thesen" ausgesetzt, die man in gewöhnlicheren Disziplinen nicht tolerieren würde. Daher sollte in der Kritik wissenschaftlicher Theorien zumindest ein „symmetrischer Ansatz" gepflegt werden: Es dürfen weder ein bestimmter Wissenschaftszweig noch der Ruhm eines Forschers als Garantie für korrektes Verhalten und korrekte wissenschaftliche Methodologie betrachtet werden. Der Verfasser erörtert verschiedene Bedeutungen eines „symmetrischen Ansatzes" und schließt seine Ausführungen über „Vertrauen" und „Misstrauen" in die Wissenschaft mit einer Beschreibung eines sog. „Glaubwürdigkeitszirkels".

Schlüsselbegriffe: Bogdanov-Affäre, Sokal-Affäre, Symmetrie der Kritik, Glaubwürdigkeitszirkel