

# *What Would an Informant Tell Me after Reading My Paper? On the Theoretical Significance of Ethical Commitment and Political Transparency in Symmetrical Practice of Studying Religion(s)*

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This paper resulted from my ethnographic research into dance improvisation. Even though dance improvisation does not have anything inherently in common with religion, it provided a unique chance to test new theoretical and methodological approaches which are established in other research fields and are now slowly being developed within the study of religion. These new research avenues began to emerge a few years back during my doctoral research,<sup>1</sup> when I started to experience the suffocating limits imposed on the study of religion(s) by the very use of the concept “religion” as an analytical tool not only for the study of European society but also for intercultural comparison. I have presented my argument on this issue in an essay entitled “Why Not to Study Religion”.<sup>2</sup> In this essay I demonstrated how the concept “religion” prevents us from fulfilling some important initial aims of the founders of the discipline and how this concept brings theoretically counterproductive asymmetries into the research practice. In the following text I will elaborate this argument, take it a step further, and present a practical demonstration of the importance and implications of subscribing to the principle of symmetry.<sup>3</sup> I will try to

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1 Milan Fujda, *Akultura hinduismu a formování moderní religiozity: K sociálním dějinám českého okultismu 1891-1941*, Praha: Malvern 2010.

2 Milan Fujda, “Proč nestudovat náboženství: K sociologickému uspořádávání skutečnosti” [Why Not to Study Religion: On the Sociological Ordering of Reality], *Sociální studia* 10/3, 2013, 13-43.

3 The concept was first developed in the field of the Science and Technology Studies (STS) by David Bloor, “Afterword: Attacks on the Strong Programme”, in: id., *Knowledge and Social Imagery*, Chicago–London: The University of Chicago Press

show why the symmetrical approach contradicts some established notions of value neutrality and political non-engagement while it facilitates theoretical objectivity.

I presented the first analysis, based on the data from the above-mentioned ethnographic study on dance improvisation, at the conference of the European Association for the Study of Religions in Stockholm (2012) in a paper entitled “Agency in Instructed Action: A Dance Lesson for the Students of Religion”. While revising this paper I discovered some crucial analytical problems which led me to reflect systematically on the process of data analysis that I employed in that paper. The following demonstration of the significance of the symmetrical approach is based on this reflection on the relations between the theoretical significance of ethical commitments and political transparency on one hand, and research objectivity on the other.

Before I get to the auto-ethnography, however, I will present a short review of the asymmetries inscribed in academic work based on, or utilizing, the concept “religion”. This is necessary (1) to open up a space for the introduction of the complex meaning and implications of the symmetrical approach, and (2) to highlight the relevance of studying dance improvisation (and particularly with regard to the topic of instructed action) within the disciplinary boundaries of the academic study of religion(s). This review aims to introduce the concept of *insecure practices of pragmatic management of uncertainties and ambivalence* and its relation to the study of religion(s).

### **Insecure and instrumentally rational practices of problem solving: Towards postcolonial study of religion(s) without “religion”**

In the spring of 2014 I engaged in a conversation with a choreographer and dance teacher of Slovak origin.<sup>4</sup> After being asked about my occupation I explained to him the sociological relevance of dance improvisation as a laboratory for studying the ways in which people manage uncertainties. After listening carefully to my explanation of the importance of studying the human ability to act in situations characterised by a lack of control over the consequences of ongoing (inter)actions, he replied that it is rather strange that some people believe that they can control what is happening to, and around, them.

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<sup>2</sup>1991 (first ed. 1976), 163-185: 175. A detailed introduction to the concept of symmetrical study is provided later in this article in the section “Introducing the principle of symmetry”.

<sup>4</sup> Informal conversation with the choreographer and dance teacher Milan Kozánek, Prague, 26 March 2014.

Indeed, if early modernity can be characterised by the belief that we can build a desired future by rational calculation, prediction and political action, supported by scientific knowledge, then understanding late modernity in terms of the risk society<sup>5</sup> infers that this belief might have been naïve. This reading of Ulrich Beck has far reaching consequences for democracy and the role of religion in the late modern society. If the early modern political process mitigated unpredictability by discarding the sources of irrationality (including religion) then, these uncertainties must be reintroduced into the political process in the context of the risk society. The lack of actual control and the need to improvise thus creates the space for the legitimate use of a much wider range of decision-making and problem-solving strategies than those understood as rational in the modern perspective. Moreover, it highlights the fact that (untested) “tradition”, “emotionally based estimation”, “intuition”, “oracles and magic”, “dogmas” and “prejudices” have never ceased to be employed in modern politics.

“Tradition”, “dogmas”, “oracles”, “prejudices” etc. have often been related to “religion” in political and scholarly discussions. However, the “prejudiced” one has always been the political opponent and the label “religious” a way to silence his/her claims. For that reason I prefer to speak of *insecure practices of pragmatic management of unpredictabilities and ambivalence*.<sup>6</sup> At the same time I do not only relate to these practices in politics, but also to the management of ordinary everyday personal problems. The concept of insecure practices is defined by its opposition to *instrumentally rational practices of pragmatic problem solving*. The idea of “instrumental rationality” is borrowed from Max Weber. The need to distinguish between “insecure” and “instrumentally rational” means of pragmatic problem management lies in the fact that the possibilities of instrumentally rational calculation itself are very limited in this realm. Ernest Gellner consequently pointed out that various analogies of oracles are utilised in modern society while dealing with serious problems where incommensurate and multiple criteria enter into the assessment of success. “[N]o Delphic oracles are needed in the case of small issues, where reason

5 Ulrich Beck, *Risikogesellschaft: Auf dem Weg in eine andere Moderne*, Frankfurt am Main: Suhrkamp 1986.

6 I am aware that by speaking of religion in this sense I am limiting the range of the meanings it has been given in the field of the study of religion(s). However, I do not attempt to construct an all-comprehensive study of religion. I am interested in a limited range of issues and use the concept accordingly and without any aim to formulate any general statement on what religion is or what the study of religion(s) should be about. This kind of discussion I find theoretically unproductive, i.e. not providing any support for empirical research.

prevails,” he says, “but for really big questions, oracle-surrogates remain in use.”<sup>7</sup>

To illustrate the issue at stake I will quote a Facebook status by Helen,<sup>8</sup> one of the dancers in the improvisation project I studied. The status was published shortly before the premiere of the performance, which was based on a six-month experimentation with various conditions for improvisation; “Don’t we want to change the text of the program? I think that this nicely expresses our semi-annual achievement :))”. The photograph shared below this post contained the following text:

Theory is when you know everything but nothing works.  
Practice is when everything works but no one knows why.  
In our lab, theory and practice are combined: Nothing works and no one knows why.

This status shows at least three things: First, that six months of hard systematic work does not necessarily result in establishing a comprehensive set of instrumentally rational means. Second, that joking is an important means of handling the feeling that things are not under control. Third, the dancers are not the first ones to deal with such problems and accordingly, they can use publicly available, i.e. institutionalized, cultural resources for humour. Yet, one more thing is implied in the situation and supported by further data: namely that joking is just one of the strategies for managing a situation, which is defined by unpredictability and the loss of control, pragmatically. While joking is a way of freeing some shared experience of tension, there are further ways through which people elevate tension, provide some measure of assurance, facilitate optimistic expectations, help to overcome stage fright, and enable themselves to ride through events without having full control over them. They do this by acknowledging the existence of the insecurity and by anticipating possible complications. Other practices range from “cleaning” the stage and backstage by a shaman, distributing good luck-charms, personal good-luck wishing and performing collective, supportive rituals in a circle before entering the stage. Moreover, these activities occur without the actors feeling obliged to have full faith in their efficacy, pondering over the mechanisms behind their effects or even knowing how to engage in them correctly.

The only reason we do not see, report and analyse practices of this kind, present in everyday ordinary life of our society, is that they are so common that we are blind to their existence. They are something we refer to as “religion” or “magic” when we encounter them in unusual settings or

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7 Ernest Gellner, *Plough, Sword and Book*, Chicago: The University of Chicago Press 1989, 209-210.

8 All names from my field notes used in this article are pseudonyms.

amongst the others, such as the so called “primitives”, but these are definitely not “religion” or “magic” amongst “civilized” and “modern” people. According to Mary Douglas we furthermore tend to believe that while we “really” do not believe in the direct material efficacy of such practices, the primitives do:

Once when a band of Kung Bushmen had performed their rain rituals, a small cloud appeared on the horizon, grew and darkened. Then rain fell. But the anthropologists who asked if the Bushmen reckoned the rite had produced the rain, were laughed out of court. How naïve can we get about the beliefs of others? Old anthropological sources are full of the notion that primitive people expect rites to produce an immediate intervention in their affairs, and they poke kindly fun at those who supplement their rituals of healing with European medicine, as if it testified to lack of faith.<sup>9</sup>

Due to this asymmetry in treating ourselves and them, difficulties arise in seeing the pragmatic rationality in such performances and their importance for our own coping with uncertainty. However, even more important is that we are not able to see what Mary Douglas points out clearly here: the pragmatic relevance and efficacy of such practices depends on being used alongside, and in interplay with, numerous other techniques. In dance improvisation this means numerous practical techniques employed as means to assess and evaluate the situation on stage, the performance of other dancers, the development of the performance, the mood, activity, satisfaction and the harmony among performers as well as the mood, engagement and enjoyment of the public, etc. All these techniques are based on the experience and sensitivity of the dancers and can hardly be described in terms of instrumentally rational control over the process. Yet, their employment is perfectly, pragmatically rational, justified and its consequences are helpful.

The reason why I went through the discussion above is to show that there are two asymmetries at once inscribed in the concept “religion”. The one, relatively widely discussed, is the political asymmetry, or more precisely, the asymmetry based on what Bruno Latour calls the Great Divide (between *us*, moderns, and *them*, non-moderns).<sup>10</sup> The political significance of this asymmetry consists in the politically silencing effect of the label “religion” and, consequently, the inherent function of creating a qualitative difference between *us* and *them*, while this qualitative difference is being conceived as an objective difference between the civilized, rational and progressive *us*, and less civilized, traditional and backward

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<sup>9</sup> Mary Douglas, *Purity and Danger: An Analysis of the Concepts of Pollution and Taboo*, London–New York: Routledge 1992 (first ed. 1966), 58.

<sup>10</sup> Bruno Latour, *We Have Never Been Modern*, trans. Catherine Porter, Cambridge: Harvard University Press 1993, 10-12.

*them*. The obstacle such conceptualization constitutes, to the possibility of unprejudiced comparative exploration, is clear. This obstacle makes numerous empirically commensurable activities of *us* and *them* non-comparable; they cannot get side by side in order to be studied at once through mutual comparison due to their classification into mutually exclusive categories.<sup>11</sup>

David Bloor formulates this problem within a more general frame:

Our everyday attitudes are practical and evaluative, and evaluations are by their nature asymmetrical. Similarly with our curiosity. Typically things which are unusual or threatening attract our attention. Ultimately this is rooted in the physiology of habituation, the process by which our brains rapidly adapt to background conditions and preserve their information processing capacity for whatever breaks the local routine. Because much of our background consists of social regularities, this alone is sufficient to ensure that our curiosity is socially structured. The symmetry requirement is the call to overcome these tendencies, and to restructure our curiosity. Fortunately, it doesn't require us to transcend the physiological laws of our own nervous tissue, but it does require us to reconstruct the local social background to which our curiosity is adapted. We can do this by creating new, specialist groups with their own taken-for-granted, professional perspective.<sup>12</sup>

However, there is one more aspect of the above-mentioned asymmetry, one related to the previously highlighted tendency to mix insecure practices of pragmatic management of unpredictability and instrumentally rational practices of pragmatic problem solving. The modern political project is aimed at eliminating such tendencies by excluding religion from the field of legitimate sources for argumentation and problem solving in the public space. However, as a result of these efforts, risks, which could not have been imagined a few decades ago, have come to take up a dominant position on the global stage. The establishment of the academic studying religion(s) supported the tendency to eliminate crossovers by viewing "religion" as a special domain of human/social activity and scholarly inquiry.<sup>13</sup> However, this led to a big paradox: while the actors inevitably mix up instrumentally rational and insecure approaches to problems in their

11 A more detailed treatment of this problem can be found in M. Fujda, "Proč nestudovat náboženství...", 36. Timothy Fitzgerald analyses this issue with reference to David Chidester's idea that the boundary between religious and non-religious shifts in scholarly and lay narrations permanently according to the context. Fitzgerald demonstrates it then on the case of treating conformity with sharia law, unlike the conformity with Western civic laws, as an act of *piety* (see Timothy Fitzgerald, "Playing Language Games and Performing Rituals: Religious Studies as Ideological State Apparatus", *Method and Theory in the Study of Religion* 15/3, 2003, 209-254: 240-241).

12 D. Bloor, "Afterword...", 175-176.

13 See M. Fujda, "Proč nestudovat náboženství..."; T. Fitzgerald, "Playing Language Games..."; Talal Asad, "What Might an Anthropology of Secularism Look Like?", in:

ordinary practices, and their ability to handle ordinary problems of life is dependent on their virtuosity in blending these approaches, scholars who study these actors work according to the logic of separation of the practices these actors employ to handle their issues. This is one of the reasons why the study of religion systematically fails to explain how religion influences behaviour, culture and society. The logic of establishing a domain for the study of religions prevents the discipline from understanding its working principles because these principles are inextricably intertwined with blending insecure and instrumentally rational practices, while simultaneously (1) excluding the instrumental rational practices from the studied domain, and (2) excluding a host of insecure practices on the basis of their perceived non-religious character. The situation is even more complicated because, by demarcating its field of interest, the discipline inevitably sides with the established political order – not as an order of working principles but as a social contract or, in the words of Bruno Latour, the “modern constitution”.<sup>14</sup> Politically neutral and objective study is impossible in these settings. This has important implications: paradoxically and quite contrary to modern scientific presumptions, setting the conditions for generating more neutral and objective theories through research becomes dependent on a set of political and moral choices. Respect for the actors’ logic of acting is one of those considerations, and this respect implies giving up “religion” as a starting point for our inquiries.

### **The symmetrical approach and postcolonial social science: Re-establishing the relation with neighbouring disciplines**

Within the study of religion(s) there is a long tradition of postcolonial and postmodern criticism inscribed in the discussions concerning the concept “religion”.<sup>15</sup> However, methodological reflection on these discus-

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id., *Formations of the Secular: Christianity, Islam, Modernity*, Stanford: Stanford University Press 2003, 21-66.

14 B. Latour, *We Have Never Been Modern...*, 13-15.

15 T. Asad, “What Might an Anthropology...”; S. N. Balagangadhara, *The Heathen in His Blindness: Asia, the West and the Dynamic of Religion*, New Delhi: Manohar 2005 (first ed. Leiden–New York: Brill 1994); Timothy Fitzgerald, “A Critique of ‘Religion’ as a Cross-Cultural Category”, *Method and Theory in the Study of Religion* 9/2, 1997, 91-110; id., “Playing Language Games...”; id. (ed.), *Religion and the Secular: Historical and Colonial Formations*, London: Equinox 2007; Richard King, *Orientalism and Religion: Postcolonial Theory, India and “the Mystic East”*, London: Routledge 1999; Russell T. McCutcheon, *Manufacturing Religion: The Discourse on Sui Generis Religion and the Politics of Nostalgia*, Oxford–New York: Oxford University Press 2003; Tomoko Masuzawa, *The Invention of World Religions: Or, How European Universalism Was Preserved in the Language of Pluralism*, Chicago–London: The University of Chicago Press 2005.



sions, as seen in postmodern and postcolonial criticism in neighbouring social sciences like anthropology and sociology since 1970s, is very rare. In these disciplines such discussions resulted in the reinterpretation of reflexivity, the nature of truth and objectivity through a textual turn<sup>16</sup> and in an emphasis on the power inequalities embedded in the scientific practice of writing texts. Writing, in this interpretation, becomes a practice which is considered to be a part of the studied social processes.<sup>17</sup> Moreover, the following issues were deeply discussed in sociological and anthropological reflections on qualitative research: the position of the researcher within the field, as a person with body<sup>18</sup> and a human identity,<sup>19</sup> who has relationships with research participants, and the implications of these relations for the quality of the assembled data,<sup>20</sup> the transcendence of the limits of discourse analysis (so common in the study of religion) by enriching it through the ethno-methodological emphasis on studying non-discursive ordering practices,<sup>21</sup> the approach to mess and the acceptance of it being messy as much as is appropriate in a given situation,<sup>22</sup> and of course the ethics and politics of social scientific inquiry. Moreover, the analysis of ethical questions was not limited to the exploitation of the “weaker” in the

16 See Zdeněk Konopásek, “Text a textualita v sociálních vědách I: Teoretické motivace”, *Biograf* 7, 1996, 33-45; id., “Text a textualita v sociálních vědách II: Metodologické motivace”, *Biograf* 8, 1996, 9-23; id., “Text a textualita v sociálních vědách III: Reflexivní impuls”, *Biograf* 9, 1996, 7-15. In English, see for example Zdeněk Konopásek, “Making Thinking Visible with Atlas.ti: Computer Assisted Qualitative Analysis as Textual Practices” [online], *Forum: Qualitative Sozialforschung / Forum: Qualitative Social Research* 9/2, 2008, <<http://www.qualitative-research.net/index.php/fqs/article/view/420/910>>, [21 November 2014].

17 James Clifford, “Introduction: Partial Truths”, in: James Clifford–George E. Marcus (eds.), *Writing Culture: The Poetics and Politics of Ethnography*, Berkeley: University of California Press 1986, 1-26; Paul Rabinow, “Representations Are Social Facts: Modernity and Post-Modernity in Anthropology”, in: James Clifford–George E. Marcus (eds.), *Writing Culture: The Poetics and Politics of Ethnography*, Berkeley: University of California Press 1986, 234-261.

18 See Elizabeth E. Wheatley, “Risk, Reflexivity and an Elusory Body: Transformations in Studying Illness”, *Journal of Contemporary Ethnography* 34/1, 2005, 68-100.

19 Roschanack Shaery-Eisenlohr, “Fixing and Negotiating Identities in the Field: The Case of Lebanese Shiites”, in: Antonius C. G. M. Robben–Jeffrey A. Sluka (eds.), *Ethnographic Fieldwork: An Anthropological Reader*, Malden: Wiley-Blackwell <sup>2</sup>2012, 103-113.

20 Gerald D. Berreman, “Behind Many Masks: Ethnography and Impression Management”, in: Antonius C. G. M. Robben–Jeffrey A. Sluka (eds.), *Ethnographic Fieldwork: An Anthropological Reader*, Malden: Wiley-Blackwell <sup>2</sup>2012, 153-174.

21 James A. Holstein–Jaber E. Gubrium, “Interpretative Practice and Social Action”, in: Norman K. Denzin–Yvonna S. Lincoln (eds.), *The SAGE Handbook of Qualitative Research*, Thousand Oaks: SAGE <sup>3</sup>2005, 483-506.

22 John Law, *After Method: Mess in Social Science Research*, New York–Abingdon: Routledge 2004.



name of scientific objectivity and truth,<sup>23</sup> and further developed practical ways through which to approach the contradictory demands for cultural relativism as a fundamental principle of anthropological research and the witnessed violence, exploitation and suffering within the studied societies and cultures.<sup>24</sup>

While in anthropology and sociology (with the exception of sociology of religion) these discussions culminated in far-reaching changes in methodological and theoretical thinking and in the development of practical ways to overcome the troubles highlighted by the postmodern and postcolonial critiques of scientific practices, the study of religion(s), it seems to me, has not yet found a systematic response to these challenges. Either its discussions are still embedded in modernist scientific sensitivity, or the postmodern and postcolonial approaches have not led beyond the deconstructive efforts inspired by Jonathan Z. Smith.<sup>25</sup> Despite the brilliance of Smith's analyses, his work remained more deconstructive than constructive. His powerful example led to the creation of an epistemic culture<sup>26</sup> in which the deconstruction of scholarly practice became an end in itself. Even though the importance of this deconstructive effort cannot be denied, within the study of religion(s) the necessary next step to reconstruct a basis on which to do research has not yet been made.

I believe that subscribing to the methodological principle of symmetry<sup>27</sup> is a step in this direction. Simultaneously, I must point out that the logic producing dominance, marginalization, and exploitation, i.e. problems addressed in postcolonial critique, is quite analogical to the logic of producing risks in the risk society. That means that the standard modern procedures ignore the existence of such risks, trivialize them, make them invisible and complicate the formulation of appropriate solutions for them. The problematic role of science in the production of risks in the risk society, and its tendency to complicate the identification of these risks and their consequent solutions, has been described in detail by Ulrich Beck.<sup>28</sup>

23 Diane Lewis, "Anthropology and Colonialism", *Current Anthropology* 14/5, 1973, 581-602.

24 Nancy Scheper-Hughes, "The Primacy of the Ethical: Propositions for a Militant Anthropology", *Current Anthropology* 36/3, 1995, 409-440.

25 Mainly Jonathan Z. Smith, *Imagining Religion: From Babylon to Jonestown*, Chicago – London: The University of Chicago Press 1982.

26 Karin Knorr Cetina, *Epistemic Cultures: How the Sciences Make Knowledge*, Cambridge–London: Harvard University Press 1999.

27 It may be useful to highlight that at this point we can speak only about what David Bloor calls methodological symmetry which, according to him, can be subscribed to while at the same leaving optionally (not necessarily) what he calls psychological and logical asymmetries at play (see D. Bloor, "Afterword...", 176).

28 An outline of the role of knowledge and science in risk production is provided in U. Beck, *Risikogesellschaft...*, 76-95, a more detailed analysis including also the role of

I am drawing this analogy in order to emphasise that postmodern and postcolonial criticism cannot be overcome by the trivialization of these criticisms and by simply reaffirming the belief in the power of naturalism and the early modern scientific project. In this view, “postmodernism” and “postcolonialism” are represented as enemies of scientific knowledge.<sup>29</sup>

While trivialising the relevance of the postmodernist and postcolonialist criticism of scientific practices and invoking the epistemological dominance and exclusivity of the early modern scientific project,<sup>30</sup> this critique and the project it supports has been accompanied by the establishment of highly asymmetrical relations between “research subjects”, who subscribe to erroneous images of the world, and clever “scientists” who know what the world consists of. Such a project aims at explaining why ordinary people are naturally predestined to err in their religious beliefs and in the connected practices.<sup>31</sup>

Contrarily, I want to show in the following part, that the problems posed by postmodern and postcolonial criticism can be effectively addressed by

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reflexivity in the risk production and management is presented *ibid.*, 254-299.

29 An example of such trivialization can be seen in Donald Wiebe, “It’s Never Been Better: Comments on the Current State of the Science of Religion”, *Religio: Revue pro religionistiku* 20/2, 2012, 173-192.

30 Apart from Wiebe’s article, see also the pamphlet by Luther H. Martin and Donald Wiebe: Luther H. Martin–Donald Wiebe, “Religious Studies as a Scientific Discipline: The Persistence of a Delusion”, *Religio: Revue pro religionistiku* 20/1, 2012, 9-18, and the responses in the same issue. I have borrowed the word “pamphlet” to characterize this text from the sophisticated reply by Hubert Seiwert: Hubert Seiwert, “The Study of Religion as a Scientific Discipline: A Comment on Luther Martin and Donald Wiebe’s Paper”, *Religio: Revue pro religionistiku* 20/1, 2012, 27-38.

31 This aspect of the issue is clearly stated in the assumptions presented by Martin and Wiebe in the above-mentioned paper: “Our *first assumption* is that the modern western research university is a purpose-designed institution for obtaining knowledge about the world. The pursuit of this knowledge is successful only when it is not in service of ideological, theological and religious agendas. Rather, its primary objective is scientific, that is, to gain public (intersubjectively available) knowledge of public (intersubjectively available) facts. Our *second assumption* is that the study of religion is the study of human behaviors that are engaged in because of, or somehow related to, a belief in agents that are beyond identification by way of the senses or scientific metric. Our *third assumption* is that religions are intersubjectively available for analysis and that, as Max Weber put it, no incalculable forces need come into play in explaining these phenomena. In other words, a scientifically respectable knowledge of religion and religions is logically possible. Our *fourth assumption* is that the current anti-theoretical and anti-science posturings of postmodernism have not undermined the credibility of modern science as a peculiarly successful instrument of inquiry into the character of the world, either natural or social. Our *fifth and final assumption* is that comprehensive scientific study of religion is not likely to be achieved by scattered scientific studies of one or another aspect of religious thought and behavior by those individual scholars who are committed to scientific research on religious thought and behavior” (L. H. Martin–D. Wiebe, “Religious Studies as a Scientific Discipline...”, 9-10).

subscribing to the principle of symmetry. This does, however, imply that questions of political and moral engagement might be related, in a surprising way, to questions of scientific objectivity.

### **Introducing the principle of symmetry**

For David Bloor and his “strong programme” in the sociology of knowledge, the principle of symmetry “seems to fly in the face of common sense”,<sup>32</sup> yet its embodiment in analytical practice provoked harsh reactions. Bloor deals with these reactions in the afterword to the second edition of his book *Knowledge and Social Imagery*. The general formulation of the principle of symmetry reads as follows: “The symmetry postulate ... enjoins us to seek the same kind of causes for both true and false, rational and irrational beliefs ...”<sup>33</sup> Subscribing to this programme has far-reaching consequences. With such an approach, the sociological study of scientific knowledge is possible – and can be conducted – in the same manner as sociological study of knowledge practices of “ordinary” people in ordinary situations. For students of religion an interesting point might be that Bloor’s critical analysis of the assumption, hitherto taken for granted, that scientific knowledge lies outside the sphere of legitimate scientific – sociological – reflection, was intimately related to the study of religion.

The very question at the outset of Bloor’s strong programme was: “Can sociology of knowledge investigate and explain the very content and nature of scientific knowledge?” Bloor supposed that, if sociology is capable of studying primitive cosmologies scientifically, than the study of “cosmologies” of our own culture might be the next natural step. Unfortunately, sociologists were reluctant to make that step. Sociologists of knowledge, he says, “have been only too eager to limit their concern with science to its institutional framework and external factors relating to its rate of growth or direction. This leaves untouched the nature of knowledge thus created.”<sup>34</sup> This reluctance took place despite the hints in the direction of the strong programme made by Émile Durkheim’s study *The Elementary Forms of the Religious Life* where he shows, according to Bloor, “how a sociologist can penetrate to the very depth of a form of knowledge ... [and drops] a number of hints as to how his findings might relate to the study of scientific knowledge. The hints have fallen on deaf ears”.<sup>35</sup>

Another aspect relating the strong programme to studying religion rises out of the accidental (sic!) analogy between two debates: one provoked by

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32 D. Bloor, “Afterword...”, 175.

33 *Ibid.*

34 D. Bloor, *Knowledge and Social Imagery...*, 3.

35 *Ibid.*, 4.

the publication of Bloor's book, the other provoked a century earlier by theologians at the Tübingen school and their approach to the historical study of the Church's dogma. In Bloor's words, the approach of Christian Baur aimed at overcoming "supernaturalism", which means that dogma had two parts and each of them was studied different way. "One part is the record of authentically apostolic truth. This flows from divine sources, and needs no other explanation beyond its divinity. The other part is the record of heresy and doctrinal deviation. This is to be accounted for by everything that can cloud the vision of the faithful and lead them astray. Here explanation is in terms of ambition, greed, ignorance, superstition and evil."<sup>36</sup> Consequently, the argument used against Baur may be formulated as follows: "When a Christian believes what is orthodox, we need enquire no further into the causes of his belief, whereas when he believes what is in fact heretical – even if he believes it to be orthodox – we require some further explanation."<sup>37</sup>

Bloor has further demonstrated this strange analogy by quoting one of his critics: "*When a thinker does what is rational to do, we need enquire no further into the cause of his action*, whereas when he does what is in fact irrational – even if he believes it to be rational – we require some further explanation."<sup>38</sup> In this context Bloor admits that "[w]hen I argued in chapter 3 that we protect science from sociological scrutiny by treating it as sacred, I spoke more truly than I knew. The strong programme first emerged in connection with sacred rather than scientific beliefs, and the arguments used against it then were exactly those used now."<sup>39</sup>

This is precisely the reason why Bloor emphasises the aspect of seeking the same kind of causes for both, true and false, rational and irrational beliefs.<sup>40</sup> For the same reason John Law emphasizes that:

the principle of symmetry suggests that there is no privilege – that everything can be analysed, and that it can (or should) be analysed in the same terms. So it erodes distinctions that are said to be given in the nature of things, and instead asks how it is that they got to be that way. ... It says, in effect, that we shouldn't take orders at face value. Rather we should treat them as the outcome of ordering.<sup>41</sup>

However, this element of subverting the established order "invariably subverts the dominant view and strengthens the side of the weak and the

36 D. Bloor, "Afterword...", 184.

37 *Ibid.*, 184-185.

38 Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth*, London: Routledge–Kegan Paul 1977, 188-189 (quoted from D. Bloor, "Afterword...", 184).

39 D. Bloor, "Afterword...", 184.

40 A more detailed argument with reference to symmetrical study of religion can be found in my article: M. Fujda, "Proč nestudovat náboženství...", 35-37.

41 John Law, *Organizing Modernity*, Oxford–Cambridge: Blackwell 1994, 12.

marginal ... [It is a] way of [methodologically motivated] siding with the oppressed”, adds Dick Pels.<sup>42</sup> This is the core of the previously mentioned political decision, which is necessary to practice the symmetrical approach as a way to facilitate objectivity. As will become clear from my further argumentation, making such a decision is easier than turning it into practice.<sup>43</sup>

However, one more ethical and theoretical consideration is required. This consideration involves the choice to study scientific research as one would study any other human activity. This concept, which was inspired by ethno-methodology, I will introduce through an account of my own research into dance improvisation. Doing dance improvisation is definitely something different than doing ethnography. However, during dance improvisation, the actors do need to use a whole range of practices related to assessing facts, interpreting and evaluating the ongoing events, generating general knowledge out of their engagement and reactions of the other actors, while simultaneously transforming it into a working experience inscribed into the body, written into a notebook or a textbook or into school curricula. In other words, they do quite the same thing as I do as a researcher and they – like me – do it all the time and systematically.<sup>44</sup> So there is a difference, but this difference is not qualitative, there is no Latourian “Great Divide”. As Zdeněk Konopásek pointed out, the only empirically available difference between scientific and ordinary practices is a quantitative difference. What scientists make use of, and what makes their analyses more detailed, accurate, better justified, more thoroughly elaborated etc. is their access to, and actual employment of, tools from a much vaster pool of resources. These resources range from the richness of the tradition of theorising (on which scientists build their own work and which they access through libraries and scientific databases), the sophisticated and long-time development of analytical tools such as analytical software, a richer scope of human resources at universities and research institutes, expensive and specialized equipment serving their needs, financial resources, and of course the available work time.<sup>45</sup> Indeed one can only be surprised by seeing the epistemic precision with which people (in

42 Dick Pels, “The Politics of Symmetry”, *Social Studies of Science* 26/2, 1996, 277-304: 282.

43 A number of problems concerning following the principle of symmetry were analysed also by D. Pels, “The Politics of Symmetry...”, 277-304.

44 Concerning this issue see Harold Garfinkel, “Common Sense Knowledge of Social Structures: The Documentary Method of Interpretation in Lay and Professional Fact Finding”, in: id., *Studies in Ethnomethodology*, Engelwood Cliffs: Prentice-Hall 1967, 76-103.

45 See Zdeněk Konopásek, “Sociologie jako *power play*”, *Sociológia* 28/2, 1996, 99-126: 113-116, 119-120.

this case dancers) deal with the issues they are faced with. They really need to understand these issues clearly for practical reasons and are experts in doing so. In addition, there is no better guide for scholars who try to understand a problem than the actors themselves and the theories they teach him/her so that he/she can become oriented. That is one of the reasons why social researchers may need to develop good and close relations with the “subjects” in their research. Good and close relations have something to do with symmetrical relations. Practical reasons for doing so will be seen in the demonstration presented in the section “The story of a tiny explosion” which will follow after opening the problem of relations among politics, ethics, and objectivity in the following part.

### **Moral and political engagement and objectivity**

There seems to be a paradox in social scientific research. There is a widespread intuition that, when moral values enter the analytical process, the theoretical quality of its outcome, treated mostly through the concept of objectivity, becomes threatened. On the other hand, as Max Weber has pointed out a century ago, without subscribing to particular ethical values or a political stance, we are unable to formulate our research problems.<sup>46</sup> Weber’s solution for this paradox is twofold: on the one hand, Weber does not take the concept of “objectivity” in its naïve positivist sense all that serious, which is why he consistently mentions it in quotation marks. On the other hand, he is very serious about the scientific quality of research outcomes and the difference between analysis and moral evaluation of the subject.<sup>47</sup> Furthermore, he divides the scientific analysis itself, and the sphere of politics that uses scientific results.<sup>48</sup> With reference to this issue he draws a sharp distinction between moral and political engagements as a necessary condition for the set up of any research on the one hand and methodological objectivity which, for him, meant the value-free and technically controlled process of concept construction on the other. The notion of “ideal types” was developed within this theoretical framework. Furthermore, Weber demonstrated that, using ideal types as technical analytical tools warrants objectivity, while improper handling of ideal types changes them into theoretically useless tools of moral evaluation. Taking Weber’s statements seriously and clinging to the idea that the research can

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46 Max Weber, “‘Objectivity’ in Social Science and Social Policy”, in: id., *The Methodology of the Social Sciences*, trans. Edward A. Shils–Henry A. Finch, Illinois: The Freepress of Glencoe 1949, 50-112: 61-81.

47 *Ibid.*, 96-99.

48 *Ibid.*, 54-57.

be done in a better or a worse way,<sup>49</sup> just as any other craft,<sup>50</sup> I would like to complicate the issue somewhat more. While Weber makes some general points which can still inspire us a century later, he completely omitted the problem of the researcher as a person whose work is greatly dependent on his/her moral relations with the people, texts, artefacts etc. he/she “connects [with] through some research protocol”,<sup>51</sup> and on the moral qualities of this research protocol itself. Of course, he was not doing ethnographical research, and this problem was therefore not so apparent to him. This does however not infer that researchers dealing with archival documents, are exempt from this problem. It only means that, for historians and quantitative or theoretical sociologists, the problem is less visible. The reasons for this, will be presented in the final part of this paper, and have to do with the fact that the problem of dominance of a scientist over the participants

49 I am mentioning this because of serious misunderstandings based on the lack of knowledge of postmodern discussions and on misidentifying postmodernism with the idea that “anything goes”, interpreted in the way that postmodernists would deny any means of distinguishing the quality of different research outcomes. Putting it metaphorically, the critics, horrified by the expression “anything goes”, think that postmodernists say that we do not have any means to distinguish between tasty and nutritious food on the one hand, and poor or spoiled one on the other, as well as between good and careless cooks. But postmodernists are quite normal people with a sound sense of taste and limited digestive capacities. What they say is only that one does not need to prefer German cuisine (for example because he or she may be French, Italian or Indian), but this does not imply that German cuisine does not produce food, but only crap. German food is nutritious and even tasty, at least for numerous Germans and many non-Germans. The same applies to systems of knowledge production. All of them, for the people who actively use the knowledge thus produced, provide usable knowledge. And of course, the fact that knowledge is used – otherwise there would be no reason to produce it – means that it has some effects, it solves problems as well as creates troubles. The knowledge we produce in modern societies make us able to produce nice cars together with traffic jams and air pollution, refrigerators together with well-preserved food and the diminishing ozone layer, Hinduism together with the Indian movement for *svarajya* as well as Hindu nationalism and Muslim-Hindu riots. If somebody sees only cars, but not traffic jams and polluted air, or if he or she conceives a nice result as a true one, while those undesirable as mere temporary drawbacks along the way to perfection, than this might have something to do with the belief in linear progress, which is a kind of material theology of salvation. Postmodernists are sceptical about instant ways to salvation, which is one of the things distinguishing them from modernists.

50 The metaphor of scientific research as a craft is borrowed here from Charles W. Mills, “On Intellectual Craftsmanship”, in: id., *The Sociological Imagination*, Oxford–New York: Oxford University Press 2000 (first ed. 1959), 195-226.

51 Bruno Latour, “On Recalling ANT”, in: John Law–John Hasard (eds.), *Actor Network Theory and After*, Oxford–Malden: Blackwell – The Sociological Review 1999, 15-24: 20. I am quoting this Latour’s wording to avoid speaking about studying someone. I want to highlight a point that will soon become clearer: that the relation in question is much more complex than just to study or to be studied (see Z. Konopásek, “Text a textualita v sociálních vědách III...”; id., “Text a textualita v sociálních vědách II...”).



in his or her research is not so urgent in the field itself (where power relations can be quite the opposite)<sup>52</sup> as it is in the phase of data analysis and in the process of writing.<sup>53</sup>

The distinction between setting the research objectives (the procedure in which ethical and political choices are inevitably present) and methodology (the set of procedures which should be void of moral values) is accepted by the social scientific mainstream as a standard and sufficient solution to the problem. We have, however, learned from postcolonial authors such as Edward Said,<sup>54</sup> Dipesh Chakrabarty,<sup>55</sup> Gayatri Chakravorty Spivak,<sup>56</sup> Ranajit Guha,<sup>57</sup> Morny Joy,<sup>58</sup> Immanuel Wallerstein<sup>59</sup> and others that moral and political engagement on the level of research objectives, combined with “objective” methodological procedures provides a powerful tool for domination over studied peoples. It facilitates their marginalization and the legitimization of neglect towards their needs and interests, as well as the establishment of rule over them. Timothy Fitzgerald has shown how “Religious Studies departments and journals are, or are similar to, ritual institutions that reproduce a dominant concept of the social order, in this case liberal capitalism”.<sup>60</sup> As such they are doing something not usually referred to when stressing the theoretical objectivity of the scientific endeavour. Postcolonial theorists thus challenge claims to objectivity by showing how it often serves, in various ways, as a pretext for domination and exploitation. This does however mean that methodology itself can apparently not be impartial in situations where it is not able to neutralize the partiality of the research objective. Consequently, this casts doubts

52 See Z. Konopásek, “Sociologie jako *power play*...”; Markéta Vaňková, “Výzkumník lapený: Úvahy k (a)symetrii vztahů mezi výzkumníkem a zkoumaným”, *Biograf* 52-53, 2010, 103-118; Andrea Beláňová, “S kým povedu dialog? Vyjednávání o rolích v terénním výzkumu”, *Biograf* 57, 2012-2013, 83-100; Kamila Velkoborská, “Reflecting on Reflecting in the Study of Contemporary Pagan and Magical Ritual”, *Religio: Revue pro religionistiku* 22/2, 2014, 211-232.

53 P. Rabinow, “Representations Are Social Facts...”.

54 Edward Said, *Orientalism*, London–New York: Penguin 1995 (first ed. 1978).

55 Dipesh Chakrabarty, *Provincializing Europe: Postcolonial Thought and Political Difference*, Princeton–Oxford: Princeton University Press 2008.

56 Gayatri Chakravorty Spivak, “Can the Subaltern Speak?”, in: Cary Nelson–Lawrence Grossberg (eds.), *Marxism and the Interpretation of Culture*, Urbana–Chicago: University of Illinois Press 1988, 271-313.

57 Ranajit Guha, “Dominance Without Hegemony and Its Historiography”, in: id. (ed.), *Subaltern Studies VI: Writings on South Asian History and Society*, New Delhi: Oxford University Press 1989, 210-309.

58 Morny Joy, “Postcolonial Reflections: Challenges for Religious Studies”, *Method and Theory in the Study of Religions* 13/2, 2001, 177-195.

59 Immanuel Wallerstein, *European Universalism: The Rhetoric of Power*, New York – London: The New Press 2006.

60 T. Fitzgerald, “Playing Language Games...”, 212.

upon any claims of “objectivity” of a theory or method and leads to the suspicion that such claims are nothing more than instruments for the legitimization of cruelty and immoral treatment of the oppressed and marginalized. The necessary partiality and moral engagement related to formulating the research problem seem to dominate over the impartiality and disengagement of the research procedures.

This might suggest that the Weberian framework does not provide tools sensitive enough to deal with the complex problems related to ethics, politics, objectivity, and the quality of theory in social inquiry. In the following story of problems with asymmetry in my analysis of instructed action I would like to demonstrate some of the relations between ethics, politics, and theory, and to show how bringing clear political and ethical decisions into a research process can enhance the quality of theory, including its objectivity. The meaning of objectivity, however, will shift a little bit. By focusing on the relations between ethics, politics, and theory, I will explore how to engage with the other side of the postcolonial coin. If moral failures in the process of theory-building make the theory persuasive only to those who share the same political objectives as the scholars, then the particular political, theoretical and moral decisions, with respect to the people we engage with in research, might be more relevant and fruitful to a much more heterogeneous collective. In other words, morally fair treatment might result in a fairer theory, i.e. one more in accordance with the classical liberal notion of maximum utility for a maximum of citizens. This formulation indicates that scientific truth has much to do with democracy. To claim objectivity and universality, for a theory backing and legitimizing the fulfilment of partial interests of the privileged few at the expense of the many who must bear the consequences, does not make much sense. But then, resistance and siding with the weaker<sup>61</sup> seem to be a theoretically more appropriate starting point than siding with those currently profiting from the established order. This is very important because it emphasizes the intermingling of “truth” and society.

The belief that “other” societies have only “cultures”, while we, the Moderns, have “Culture” and “Nature” at once, defines what Latour calls the Great Divide<sup>62</sup> we have created between *Us* and *Them* – the primitive (and oriental) others. The “Nature” on our side justifies our truth claims, as was shown above when explaining Bloor’s definition of, and reasons for, subscribing to the principle of symmetry. As a silent witness, nature attests, by its own virtue, to our truth, and makes us believe that, that which is “rational” needs no further inquiry while the “irrational” claims need

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61 See N. Scheper-Hughes, “The Primacy of the Ethical...”, 420.

62 B. Latour, *We Have Never Been Modern...*, 24-48, 91-129.

a “social explanation”.<sup>63</sup> The same Great Divide makes us oblivious to our own mixing of “rational” and “irrational” approaches to handle problems and uncertainty.<sup>64</sup> This is one important feature of what Latour calls the European critical stance. This strategy of the moderns to perpetuate their dominance, as Latour shows, is based on a rigorous separation of the constant work of purification of hybrids (of nature and culture) and the constant work of the mediation of these hybrids. These two practices, according to Latour “must remain distinct if they are to remain effective”.<sup>65</sup> Their separation defines for him what it means to “be modern”.<sup>66</sup> In this sense, the modern social scientific practice is helplessly asymmetrical and, far from any chance of being objective.

### **The story of a tiny explosion: On the difficulties of practising the symmetrical approach**

The story I am going to narrate relates to my research into contemporary dancers during their work on a performance based on improvisation. In order to build an improvisation on a particular theme, the dancers experimented with different rules by which to define particular situations. The sets of these rules would eventually form the basic structure of the improvised performance. In addition, the whole project served as a dance research of human interactions; the experiments during dance trainings were mainly about exploring what will happen in interactions among dancers if their possibilities of action were limited through a particular set of instructions. The ultimate goal of the project was to explore and represent on a stage, what is going on in a situation where people have to carry out relatively common tasks with no *a priori* assigned roles to perform. Accordingly, the performance ought to mostly be about negotiation and the establishment of order within a group which is forced to cooperate on a common task.

The problem I focus on in this particular part of my analysis of instructed action, concerns the difficulties in achieving a shared understanding of an instruction. My analysis shows which uncontrolled and unpredictable factors influence the way the instruction is interpreted and carried out, and the way all the participants, especially the instructing choreogra-

63 See above the paragraphs on “Introducing the principle of symmetry” and the second half of the chapter “Insecure and instrumentally rational practices of problem solving: Towards postcolonial study of religion(s) without ‘religion’”.

64 See the section “Insecure and instrumentally rational practices of problem solving: Towards postcolonial study of religion(s) without ‘religion’” above.

65 B. Latour, *We Have Never Been Modern...*, 10.

66 *Ibid.*, 10-12.

pher, are forced to improvise almost every second of the instructed activity. This improvisation occurred time and again despite the fact that the two choreographers spent many hours planning the instructions in order to make the dancers develop particular kinds of interactions and create particular kinds of situations.

Once, during one activity called “the tree”, a dancer called Lenka diverted relatively consistently, as I have learned, from most of the other dancers, and especially from the choreographer’s idea. In doing so, she made Šárka, the choreographer, adjust her instructions more often than the other dancers did. In one case “the tree” activity was even more problematic than usual. The task was twofold: first the dancers were instructed to imagine a tree strongly rooted in a rock and to explore and represent its shape and movement through the movement of their bodies. Lenka’s expression of the tree was particularly dissatisfying to Šárka, who formulated her dissatisfaction in terms of not believing Lenka “to feel it like that”, and suspecting her of reproducing a merely “empty form”. Second, the dancers were to set themselves under the tree and after getting ready to move out into space, locate something they desperately need, catch it, and return back to the protected spot under the tree. While locating the “thing they need”, the dancers were expected, by the choreographer, to get into interactions. However, they did not and the activity – from the point of view of the choreographer – had completely failed.

Subsequent discussions clarified that the problem lied in the understanding of particular concepts used in the instruction and thus in divergent definitions of the situation/activity by each participant. Some of the dancers reported to have got perplexed by Šárka’s expression “getting into contact” used in her instruction. During most of the previous trainings “getting into contact” meant a very general instruction, which asked of the dancers to enter into whatever kind of contact. However, due to the fact that some of the dancers were trained in classical and modern dance rather than contemporary partnering and contact improvisation, Šárka had begun to introduce more activities aimed at contact in the technical sense of sharing weight. This form of improvisation was inspired by Aikido and its principles of utilizing the energy of the opponent as well as the self. Therefore, most dancers understood the instruction to mean that they should start performing some kind of contact improvisation. Such instructions, however, seemed incompatible with the situation they experienced when establishing an intimate connection with the tree in their imagination and then moving away from it and returning to it. They did not feel any possibility, nor for that matter necessity, to interact with anybody else, especially in the above-mentioned technical sense. Such a possibility seemed quite incompatible with their experience in the given situation.

At first I understood the problem with Lenka's tree as an instance of an analogical lack of consensus on the meaning of an instruction. As she mentioned in a discussion in the cafeteria, the problem presumably derived from the fact that she interpreted the instruction for "the tree" activity through her experience with the same activity as it was carried out seven and half months earlier, at a weekend workshop during which the choreographers experimented with some interested dancers and the work style they planned to apply in this project. However, during that workshop the activity was not guided by either of the two choreographers but by Alena, a person who initiated the project but participated in it in a different role: as a visual artist responsible for the scene design. This workshop had taken place before the intensive project with a selected group of dancers started. Consequently, the problems seemed to be caused by a lack of consensus on the meaning of the instructions due to different presuppositions of various participants concerning the definition of situation. The lack of consensus seemed to result from the divergent ways of integrating shared experiences (with the instruction and the activity) in their individual biographies.

Yet, there was something that dissatisfied me when I was rereading my account of the situation in the paper I presented at a conference in Stockholm. Lenka was definitely an important actor who caused interesting things to happen. That observation led me to create the code "Lenka" with an automated coding function in the qualitative data analysis application I was using, in order to get deeper insight into what was happening around this dancer. I felt that something had to be wrong if my account made Lenka look like a trouble-maker who does things wrong all the time. So, I started to ask myself what she might tell me after reading my text. I had good reason for doing so. Once I made her angry by asking her some questions I needed to confront her with in order to test some of my provisional interpretations of what was going on in the field. I innocently told her that it seemed to me that she suffered a lot during the project and that I thought she had found a very respectable way to finally withdraw from the project as a dancer, and I asked her what she thought of this interpretation. She harshly replied to my question by saying that she was not ready to read anything about her suffering in my articles, and refused to discuss this issue any further.

This moment was very important because, at the onset of the research, I made clear to all participants that they could refrain from participation at any time. Moreover, it would imply that I would delete all passages, which mentioned this particular participant, from my field notes. As a result, "what she might tell me" turned into a pressing personal question for me as a researcher who is interested in having as rich a dataset as possible. My

ethical commitment to the participants required me to consider the issues of symmetry in our relationships more carefully. It empowered Lenka politically in relation to my research.

While editing the text, I had to consider what Lenka might say, if she were sitting next to me. I realized that, over the course of my analysis of “the tree” exercise, I had deleted two important quotes from my field notes from the particular “network view” in the analytical software I was using. This deletion was an important decision running against the automatic feature of the software to simply load all the quotes associated with a particular code into the network view. The decision I made was not completely justified. I felt that the quotes said something relevant to the problem I was analysing in this network view. These quotes were interlinked with other quotes in the view by further relations, not just by the code alone. However, I repeatedly decided that these quotes were essential with reference to a different aspect of the problem, and that I would come back to them later.

These quotes dealt with a discussion between Šárka and Lenka concerning the aesthetic quality of dance. While Šárka maintained a theory which emphasised the ability of the dancers to express their feelings and emotions through movement, Lenka emphasised the spatial and figurative aspects of the movements, and therefore its visual quality. In relation to this, I have also found quotes in which Alena, the visual artist responsible for scene design who guided the tree activity during the weekend workshop, expressed a similar aesthetic theory of dance. Alena explained to me that this aesthetic theory drove her activities in dance and land-art. The crucial aspect of the discussion between Lenka and Šárka was that they did not reach a consensus on their aesthetic theory. The problem was closed by their mutual agreement concerning their right to have divergent opinions.

This discovery led to further discoveries, especially to the recognition of the importance of power relations between the dancers and the choreographers, and the dependency of these relations on a particular setting. While a world-famous choreographer like Akram Khan can choose those dancers who fit well into his idea of the planned opening ceremony at the Olympics in London from a virtually unlimited pool of dancers, Šárka, working in the context of a local amateur dance project with a significantly smaller pool of possible participants, often expressed the necessity to work with the people she had at her disposal. However, while expressing this, she also repeatedly mentioned that she preferred the working process to its result. She was certainly becoming less steady in this approach as the date of the public performance was approaching, and yet it was an important aspect of the whole project. For the choreographers as well as the dancers, the project was a kind of experiment. However this

experiment included a commitment towards the audience. The audience should grasp and enjoy the final outcome (whatever that might be). The dancers and choreographers repeatedly asked the question, whether they should make the performance more audience-friendly, or whether they should let the audience cope with a difficult piece, if necessary. This question was not easy to answer with assurance, but the dancers and choreographers tended to prefer not oversimplifying the task for the audience.

Let us return to the passages I have repeatedly neglected, and which finally shed new light on the problems under scrutiny. My commitment to find a proper symmetrical treatment of the participants, especially with regard to Lenka in my narrative, helped me to realize the importance of power relations for the course of instructed action. Lenka, as my notes justify, could not carry out some activities in a way that contradicted her aesthetic theory; not because she would be so dogmatic about this theory, but because it was inscribed in her body, her senses and feelings, and her identity. Doing things differently would run strongly against her *habitus*.<sup>67</sup> It would of course be possible, but it would mean a huge investment of effort. Such an investment, on the other hand, demands an acceptance of the view that things should really be changed for one's own sake. This did not happen, given that the controversy between Lenka and Šárka over their aesthetic theories had not reached a consensus. They have discussed the issue at length but, as mentioned above, the outcome was a formulation of their mutual right to a divergent view. As a result, there was no pressure for change nor for any sufficient (from Lenka's perspective) justification for pressure.

In addition, this aspect shows one of the demarcating lines between professional and part-time settings. In a professional setting the relations between choreographers and dancers are more unambiguous and the dependence on salary related to the project or company, provides a strong incentive to engage in hard work on personal development, which is a crucial aspect of the artistic craft. When, on the other hand, one is dedicating her or his free time to a project and has not much more than fun and friendship as a reward for her or his engagement, breaking the bodily and discursive habits (two things which work rather as one in the given situation) becomes much more demanding, and thus based on different ways of reasoning and management than in professional settings.

Through the above narrated story I aimed to demonstrate how increased sensitivity to asymmetries can help identify certain levels of complexity

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67 Marcel Mauss, "Les techniques du corps", *Journal de Psychologie* 32/3-4, 1936, 271-293. Quoted from an electronic version at *Les classiques des sciences sociales*, <<http://dx.doi.org/doi:10.15222/cla.mam.tec>>, [16 September 2015], 7.



which otherwise would not be visible. Emphasising respect towards Lenka together with setting conditions in which she was able to significantly influence which data would be preserved and which would have to be deleted, helped to increase the theoretical solidity of the analysis. The question is thus: why was I not able to see the role of the body and identity, power relations and habitus from the onset?

The answer is relatively simple. I spent much more time with the choreographers than with the dancers. I was present during all their discussions concerning the ongoing project and the planning of training situations. As a result, I was effectively socialized into their perspective on what was going on during the trainings – so effectively that my field notes expressed a kind of joy related to my ability to see what dancers were doing *wrong*. This ability seemed to be empirically verified by the choreographers' comments, which I diligently noted down while observing the dancers in training. This way I was easily persuaded to take the power relations in the field for granted. I have subscribed to the notion that the choreographers are right while the dancers might be either right or wrong, I learned what satisfied the choreographers, and interpreted potential incompatibilities in actions of others and their disagreements from the choreographers' perspective. I needed Lenka to "strike back"<sup>68</sup> at my comment on her suffering, to realize this. This act of "striking back" thus proved to work as an ethically – and, due to the openly stated right of any participant to withdraw from the research at any time, also politically – enabled interaction between the participant and the researcher; it worked out as the tool of empirical falsification of an emerging theory.

This does however not mean that researchers should not accept the roles and hierarchies in the field. The roles and hierarchies are part of the ongoing interaction and play a crucial role in achieving the aim of the cooperation. The asymmetry lies in the fact that the researcher adopts views from the perspective of particular roles and their place in the social hierarchies as a cornerstone of his/her analysis.

In terms used in the preceding parts: she/he accepts a particular order as an *a priori*, and privileges it over analysing how the order came into being or how it is practised. This way, researchers are likely to become blind to the "good reasons" of the less privileged actors, and consequently to be unable to grasp the ongoing interactions with respect to all the crucial factors involved. Despite the fact that we might prefer the view of a particular party, the analysis should not be founded on the idea that those who think

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68 The concept of "striking back" is based on Bruno Latour's work. See Bruno Latour, "When Things Strike Back", *British Journal of Sociology* 51/1, 2000, 107-123. I will come back to "striking back" below.

and act otherwise are simply wrong, “trouble makers”, stupid or irrational. Quite to the contrary, ascribing normality and rationality (in the sense of having “good reasons”, however contradictory to the “good reasons” of others, or even contradictory to the desired implications of one’s own action) to all the actors, and taking their reasons into account, can help us deepen the analysis. This forces us to understand social action through the chaotic interplay between actors in which divergent definitions of the situation, misunderstandings, clarifications of each other’s stance, etc., strongly influence the outcome. This is consistent with the statement by William and Dorothy Thomas that:

Very often it is the wide discrepancy between the situation as it seems to others and the situation as it seems to the individual that brings about the overt behavior difficulty. To take an extreme example, the warden of Dannemora prison recently refused to honor the order of the court to send an inmate outside the prison walls for some specific purpose. He excused himself on the ground that the man was too dangerous. He had killed several persons who had the unfortunate habit of talking to themselves on the street. From the movement of their lips he imagined that they were calling him vile names, and he behaved as if this were true. If men define situations as real, they are real in their consequences.<sup>69</sup>

Therefore, the definition of a situation by one particular actor is not sufficient to properly understand the course of action. To analyse the situation demands all the actors involved to be taken serious and to let their differences and agreements shed light on what is actually happening and why. In fact the researcher has no other tool to develop an empirically grounded understanding than to follow these interplays of interactions and their temporary outcomes. The only other possibility to develop this understanding is by speculating on the basis of one’s own experiences. Some speculation can free the imagination, foster sensitivity and orient oneself amidst an overkill of textual material to be analysed. However, speculation which is not directly related to mapping of the scene by mutual interactions, negotiations and controversies and their outcomes<sup>70</sup> can hardly result in more than subjective fantasy.

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69 William I. Thomas–Dorothy S. Thomas, “The Methodology of Behavior Study”, in: *ibid.*, *The Child in America: Behavior Problems and Programs*, New York: Alfred A. Knopf 1928, 553-576: 572.

70 This idea of controversies as maps to be followed by the analyst is taken from Bruno Latour, *Reassembling the Social: An Introduction to Actor-Network-Theory*, Oxford: Oxford University Press 2005, 31-32. Latour furthermore relates this strategy of mapping with constructing the “social context”. He argues that it is not the analyst’s task to add the context by himself to the analysis since the actors themselves are engaged all the time in mapping their position in which they operate and the nature of their ties, and they leave traces of these activities.

### “Things striking back”

The space for “striking back” which I described above as a result of Lenka’s ethical and political empowerment brings us to Bruno Latour’s reformulation of the concept of scientific *objectivity*. This reformulation is, furthermore, based on subscribing to the principle of symmetry. As I will show, the idea of “striking back” enables us to find more sensitive tools to grasp the relationships between ethics, politics, and theory. It enables us to add moral relationships with participants in research into the analysis, and to grasp the research problem in a way that respects that our results are produced by ordinary practices, however quantitatively different<sup>71</sup> from everyday ordinary practices of non-experts. When speaking of “striking back”, Latour returns to his experience with natural scientists, who objected to his accounts of the social construction of scientific facts. This situation became decisive for him and led him to the reconstruction of the notion of social construction. This achievement is related to Latour’s criticism of the practice of social explanation:

After two centuries of easily explaining away the behavior and beliefs of farmers, the poor, fetishists, fanatics, priests, lawyers, and businessmen whose anger was rarely registered ..., we were going to finally see whether or not the social could explain anything else. Chemists, rocket scientists, and physicists are used to seeing their laboratories explode, but it had been quite a while before the sociologist’s office could run an experiment risky enough even to have a chance to fail! And, this time, it did explode. After a week in Roger Guillemin’s laboratory thirty years ago, I remember how inescapable I found the conclusion: the social cannot be substituted for the tiniest polypeptide, the smallest rock, the most innocuous electron, the tamest baboon. Objects of science may explain the social, not the other way around. No experience was more striking than what I saw with my own eyes: the social explanation had vanished into thin air.<sup>72</sup>

This failure of social explanation led Latour to reformulate the notion of explanation itself and to remodel the concept of objectivity.

While the traditional notion of objectivity constructs the value-free realm of scientific research by demanding an unrealistic ability of the scientist to assume God’s view, the notion elaborated by Latour, presumes a particular position among, and engagement of the scientist with those with whom he “connects through some research protocol”.<sup>73</sup> Such a notion of objectivity does not rely on godly impartiality but on a relatively simple principle of letting the studied objects object to what the scientists say

71 The notion of purely quantitative – not qualitative – difference between ordinary and scientific practices of the production of knowledge and order is dealt with in the last paragraphs of the section “Introducing the principle of symmetry”.

72 B. Latour, *Reassembling the Social...*, 98-99.

73 B. Latour, “On Recalling ANT...”, 20.

about them.<sup>74</sup> While natural scientists do not need to care much about the ability of their objects to object to what they have to say about them, the people studied by social scientists are much more careful and polite.<sup>75</sup>

If many more precautions have to be taken with human subjects, it is not because humans should not be treated like ‘mere things’ devoid of intentionality, consciousness and reflexivity, as interpretative schools would have it; nor is it, as the quantitative schools think, because they would influence the result, but, on the contrary, because they would quickly lose their recalcitrance by complying with what scientists expect of them. Contrary to microbes and electrons who never abandon their capacity to object since they are not easily influenced by the interest of experiments, too remote from their own conatus (not to say interest), humans are so easily subjected to influence that they play the role of an idiotic object perfectly well, as soon as white coats ask them to sacrifice their recalcitrance in the name of higher scientific goals (this is what happened in Milgram’s lab whose experiment proves nothing more than that a psychologist can indeed be the torturer of his students!).<sup>76</sup>

Therefore, the social scientist is in a more difficult position when preparing the conditions in which his “laboratory” might “explode”. In order for it to “explode”, Bruno Latour says,

social scientists had to study something that was higher, harder, and stronger than them. For the first time, the explanandum resisted and grinded the teeth of the explanans’ cogs to mere stumps. Not only that, but the screams of those being studied could be heard loud and clear – and they were not coming from Bali, the ghettos, TV studios, corporate board rooms, or the US Senate, but from departments next door, from colleagues in the very same hiring and grant committees.

Now, at last, it was time to carry out in the social sciences the experiment which had never been carried out before: What proof do we have that a social explanation holds when we study up? When the reactions of those studied cannot be ignored? When the ‘cultural capital’ of those studied is infinitely higher than those doing the study? When the objects to be replaced by ‘social force’ are obviously much stronger, varied, longer lasting than this very social force that is supposed to explain them? When the truths to be explained are equally valued by those who study and by those who are studied as the only treasure on earth worth fighting for?<sup>77</sup>

According to me these criticisms, reformulations, and principles by Latour need to be taken very serious if we value the quality of knowledge produced by our scientific practice. However, incorporating them is far more difficult than just making a decision to subscribe to them. What I wanted to add in this article is that the means for silencing the “objecting objects” can be quite discreet. Despite consciously and actively adopting a very symmetrical approach during field work; being respectful and em-

74 B. Latour, “When Things Strike Back...”, 114-115.

75 *Ibid.*, 115-116.

76 *Ibid.*, 116.

77 B. Latour, *Reassembling the Social...*, 98-99.

pathic to the participants and making notes as respectfully as possible, we are in the end alone with the data we have produced. It is us who interpret the data during the analytical process. In addition, it is hard to introduce the same symmetry one encounters in the field in settings where one is alone with her/his data. This is due to the fact that the analytical process is devoid of the feedback the researcher gets through interaction with the subjects. The possibilities to silence the objections are almost unlimited even if we subscribe to the principle of symmetry and try to be reflexive in our endeavour. Consequently, this becomes even harder when not subscribing to these principles at all. As Latour shows, the political power of the “objects” is important. As was shown in the case of Lenka, it was the open confrontation in the field and the specific conditions that gave her political power over my research and made me think more carefully of what I said about her in my analysis.

### Conclusion

Usually, journal papers in social sciences comprise of a collection of descriptions of social interactions. For this paper I have focussed on one minor aspect of such behaviour and how my description exploded. When everything goes right and nothing explodes, then such articles may be filled with harmful descriptions about the people who have entered the account. All this violence passes unnoticed and bad theory comes as a result: bad in the sense that it (1) fails to grasp the problem in its full complexity, (2) fails to take into account some crucial data and (3) sides with particular parties. By “bad theory”, I do not simply mean morally bad because of its violence; I rather mean “blind” to crucial information present in our data set. Simply put, such theories are bad in that they lack depth, transparency, and objectivity.

The case in question has thus shown that: (1) it is difficult to set up proper conditions, even for tiny explosions, and (2) such conditions come as a result of ethical and political decisions. These ethical considerations consist of the choice to refrain from using the power, given to us by the authority of the expert system we represent, over the participants in our research or the people represented in our accounts. The political decision consists of refraining from taking any privilege or dominance for granted,<sup>78</sup> i.e. a decision not to subscribe to, and to support, an established order. In this respect it is far from being conservative. In addition, this ethical and political decision can do more for the development of good and “objective” theory than the iron cage of any methodological canon or the naïve

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<sup>78</sup> See J. Law, *Organizing Modernity...*, 12.

positivist notion of “objectivity”, which, first of all, prevents us effectively from experiencing any kind of explosions and secondly, systematically veils our research practices by representing methodology as a means of impartiality and ethical and political disengagement.

In this context a statement, made by my friend and colleague Tomáš Kobes, comes to mind. During a lecture on participant observation he emphasised that ethnography, as a method, is the tool for doing real *hard science*. The argument he gave was that, during ethnographic fieldwork, the researcher gets constant feedback on her/his provisional theories through interactions with the participants. This statement is important. It is however not yet complete. Being in the field and making notes is only one part of the researchers’ work. And, moreover, it is not in the field where the researcher has the most power on her/his side. It often happens that she/he is dominated or even exploited in many ways by the participants.<sup>79</sup> Her/his power in this setting lies only in the routine of taking notes. Notes are something she/he can control, behind what she/he can hide, by means of what she/he can deal with the problems she/he faces in the field.<sup>80</sup> The field notes help the researcher to handle the asymmetries related to her/his status as an outsider or newcomer. The real analytical work, however, starts when the researcher returns home, transcribes the notes, feeds the data into analytical software, and starts re-reading, commenting on, coding, comparing, memo-ing etc. Only at this point the researcher starts to play a “power play”, as Zdeněk Konopásek expressed it.<sup>81</sup> Here she/he turns to the rich resources of technology and knowledge which have been amassed by generations of her/his colleagues and the opportunity for letting “things strike back” decreases significantly. Therefore, at this point political and ethical considerations, which may enhance the quality of an emerging theory, need special attention. When face-to-face interaction with the participants is over, and so the relevance of political and ethical commitments seems to diminish, it is particularly important to bring politics and ethics into the research process again. This matters more than the procedures involved in concept construction, so precious to Weber, because this can once again enable “things” to strike back.

Weber should be taken serious, no doubt, but what he offers is not sufficient. The risk of producing harmful analytical accounts cannot be avoided by any purely technical objective tool. As shown above, the solution must be moral and political. Basically it includes simply maximal

79 M. Vaňková, “Výzkumník lapený: Úvahy k (a)symetrii...”. See also A. Beláňová, “S kým povedu dialog...”.

80 See A. Beláňová, “S kým povedu dialog...”, and E. E. Wheatley, “Risk, Reflexivity and an Elusory Body...”.

81 Z. Konopásek, “Sociologie jako power play...”.

honesty, respect, carefulness, and humbleness. It is very tempting to: replace what a participant means by something different but fitting the researchers' preconceptions, ignore or misinterpret data which does not conform with the emerging theory or, to miss crucial information because we took certain structures and power relations for granted. On the other hand, no precaution is as efficient as explicit, transparent and thorough note-taking, not only in the field, but also during the analytical process. This can be facilitated by the usage of an analytical tool which records each step of the analytical process and keeps track of every reason for a particular decision. As Zdeňek Konopásek explained,<sup>82</sup> and Elizabeth Wheatley clearly demonstrated,<sup>83</sup> reflexivity has nothing to do with a kind of meditative study of our mental processes. As a practice related to the research process it is based on our ability to analyse our own analytical work the same way as we analyse all other data presented in the form of a text which we can see, read, comment, code etc.

This type of study needs to be based on a strong ethical and political decision not to neglect anybody's "good reasons" whatever her/his position and role might be. In fact this approach is self-consciously democratic in the sense that it attempts to let every actor's voice sound with the same strength.

However, this does mean methodological siding with the weaker as those who are in whatever given order marginalized. The researcher cannot neglect that privileges exist and cannot neglect the perspective of the privileged. He cannot, however, produce an analytical account from the perspective of a privileged and say that it is "objective". As affirmative action in politics is undertaken in order to establish equal conditions for all, including the underprivileged and marginalized, so methodological siding with the weaker establishes conditions for objectivity, or more exactly, it helps to increase the quality of a theory. The decision to neglect the concept of religion is an important part of this attempt to make research better, it serves to safeguard symmetry. The term's inherent connection to the particular asymmetrical order inscribed in what Latour calls Great Divide makes the term "religion" too strongly rooted in modernity with all its asymmetries. This inscribed asymmetry cannot be methodologically controlled. It carries in itself too many privileges.

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82 Z. Konopásek, "Text a textualita v sociálních vědách III..."

83 E. E. Wheatley, "Risk, Reflexivity and an Elusory Body..."



## SUMMARY

**What Would an Informant Tell Me after Reading My Paper? On the Theoretical Significance of Ethical Commitment and Political Transparency in the Symmetrical Practice of Studying Religion(s)**

This article introduces the methodological principle of symmetry as developed in science and technology studies (STS). However simple in its formulation, this principle is unexpectedly complex in practice and has far-reaching consequences. Adherence to the principle of symmetry in the study of religion(s) is one promising avenue through which practical solutions to many theoretical and methodological problems may be formulated in response to postmodern and postcolonial critique of the study of religion(s) as well as of the modern scientific practice in general.

The aim of this article is to demonstrate the promises of the symmetrical approach, the ways of its practical enactment, the complications of this enactment, and some solutions for these complications. The arguments and demonstrations provided here show that: (1) demarcating the research field of the study of religion(s) through the concept “religion” in itself creates asymmetries which contradict the aims of the discipline as an objective empirical and theoretical study; (2) the practical enactment of the principle of symmetry demands taking important ethical and political decisions in order to prepare the practical conditions for research and analysis, analogical to the practical conditions actively fostering democratic negotiation and handling inequalities through affirmative action; (3) making these decisions and maintaining these conditions is necessary in order to achieve theoretical objectivity and build good and transparent theories; and (4) being faithful to these decisions during the process of data analysis is extremely complicated and can only be more or less successfully achieved on the basis of (a) high degree of ethical commitments towards the participants in research which empower them politically in relation to the researcher and the research itself, and (b) by analysing the process of data collection and analysis in exactly the same way (i.e. symmetrically) as the activities of all other actors engaged in the research. In this sense modernist ideas concerning moral and political disengagement as the condition for neutrality, objectivity and truth are reversed with reference to postcolonial criticism and show how proclaimed neutrality, objectivity and truth justifications are based on and foster power domination, suffering and exploitation of the marginalized.

**Keywords:** study of religion(s); research methodology; research ethics; politics of research; reflexivity; principle of symmetry; symmetrical approach; ethnography; postcolonialism; insecure practices of pragmatic management of uncertainty; instrumentally rational practices of pragmatic problem solving; democracy; risk society.

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