

Saana Jukola

On the Conditions  
for Objectivity  
How to Avoid Bias in  
Socially Relevant Research



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# On the Conditions for Objectivity

How to Avoid Bias in  
Socially Relevant Research

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UNIVERSITY OF JYVÄSKYLÄ

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## ABSTRACT

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The present study focuses on the conditions required for ensuring objectivity in those fields of scientific research that are expected to produce socially relevant outcomes. It is comprised of four individual articles and an introductory chapter. The introductory chapter begins with a discussion on the concept of 'objectivity'. A short introduction to the history of the concept is given, the complexity of its current use is explained, and the connection between the objectivity of research and public trust in science is indicated. Next, the theoretical background of the dissertation is articulated: 1) criticism of individualistic views on objectivity of science, 2) criticism of value-freedom of science, and 3) acknowledgement of the importance of the context of scientific inquiry. The introductory chapter ends with an overview of the individual articles and a discussion on the implications of this study. The first article examines the conditions for objectivity in the context of commercialized research. A traditional, individualistic conception of the objectivity of science is criticized, and it is argued that certain features of commercialized research culture are epistemically harmful. The second article focuses on Helen Longino's (1990; 2002) theory on the objectivity of science. The focus is on the criteria Longino offers as tools for evaluating research communities. By introducing two cases in biomedical sciences, it is argued that her theory can be complemented by taking notice of extra-scientific factors. In the third article, different ideals of objectivity are discussed in the context of medical research and conducting meta-analyses in particular. With criticism of an article by Stegenga (2011), the article demonstrates that the so-called procedural ideal of objectivity does not manage to capture some of the most central features of medical knowledge production. A case of research on selective serotonin reuptake inhibitors is introduced, and it is argued that adopting the so-called social view on objectivity can help us to better evaluate medical science. The fourth article examines journal peer review and the effects of different individual-level biases from a social-epistemological perspective. It is argued that the institutional context of peer review can have a significant effect on how biases influence the pool of published literature.

Keywords: philosophy of science, objectivity, social epistemology, bias, Helen Longino

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## LIST OF ORIGINAL PAPERS

- I Jukola, S. (2014). The Commercialization of Research and the Quest for the Objectivity of Research. *Foundations of Science*. DOI 10.1007/s10699-014-9377-8
- II Jukola, S. (2015). Longino's Theory of Objectivity and Commercialized Research. In Wagenknecht, S., Nersessian, N. & Andersen, H. (eds.): *Empirical Philosophy of Science. Introducing Qualitative Methods into Philosophy of Science*. *Studies in Applied Philosophy, Ethics and Rational Ethics*, 21. pp. 127-143.
- III Jukola, S. [forthcoming]. Meta-analysis, Ideals of Objectivity, and the Reliability of Medical Knowledge. *Science and Technology Studies*.
- IV Jukola, S. [under review]. A Social Epistemological Inquiry into Biases in Journal Peer Review. *Perspectives on Science*.

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ABSTRACT

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# 1 INTRODUCTION

In this dissertation, I investigate the different dimensions of scientific objectivity and its preconditions. I am particularly interested in the ways in which the institutional context of research may cause biases or make the detection of biases more difficult. Objectivity is thought to be one of the central ideals for science. Thus, ensuring it in an environment where researchers are facing the pressure to produce results that are socially and financially utilizable is important. The institutional context of science has been changing (e.g., Slaughter and Leslie, 1999), and the notion of objectivity deserves to be investigated from a perspective that considers the effects of this development. In this dissertation, I argue that securing the objectivity of research is in a significant way dependent on the institutional structures and conventions of science.

Objectivity is a multifaceted concept. It is possible to talk about the objectivity of a person, of a process, of a method, or of a result. Objectivity of a person can refer to, for example, her ability to base her reasoning on facts and logic (e.g., Smith, 2004: 153) or her willingness to participate in critical discussions (e.g., Longino, 1990: 79). By an objective method we can for instance refer to a method being guided by rules that exclude individual judgments (e.g., Sismondo, 2004: 115; Stegenga, 2011), or to a critical exchange of opinions (e.g., Longino, 1990: 76). Objective results, in turn, can mean knowledge that is independent of human thought, a “view from nowhere” - as Thomas Nagel (1986) famously put it - or results that are produced by using an objective method. Objectivity can be taken to mean being unbiased, value-free or free from subjectivity. Objectivity is something else than truth, and even an objective study may fail to produce true results for various reasons. For instance, because the available methods are not advanced enough to accurately depict the object of interest. Thus, even objective results may later be disqualified.

According to Heather Douglas (2009: 44), philosophers of science have tended to focus on issues such as the status of scientific theories and the nature of explanation, while devoting less time for investigating the interconnections between science and society and the way in which societal values and assumptions influence what is eventually taken to be knowledge. Scientific knowledge

production has been seen as a process that is and should be isolated from the other areas of society. Even though there have been prominent figures in the philosophical canon, such as Karl Popper and Robert Merton, who have discussed the social aspects of research, objectivity has been associated with the integrity of individual scientists and the rigorous testing of claims, which is expected to purify the produced knowledge from improper, i.e., non-epistemic<sup>1</sup>, values. Recently, however, this conception has been questioned by the work of several philosophers (e.g., Longino, 1990; 2002; Biddle, 2007; Brown, 2010) who have investigated precisely how the outcomes of research are influenced by the social context in which science operates. This is the area of research to which I wish to contribute with this doctoral study. From this perspective, the objectivity of science should not be confounded solely with the ability of ideally impartial individuals to restrain their own biases from interfering with reasoning, and the social aspects of research should not be seen as a source of mere confusion and error. Instead, objectivity should be investigated as being sensitive to community-level processes.

The dissertation consists of four papers, in which I investigate the question of how to best secure objectivity in a context where science is expected to produce applicable results. I aim at scrutinizing the conditions for objectivity and providing insight that could help to eliminate factors which undermine these conditions in current scientific practice. The main outcome of the dissertation is that when the objectivity of science is discussed, it is not sufficient to examine merely individual scientists and their attitudes and actions. Neither is it sufficient to reduce objectivity to procedures that aim at excluding judgments or values from the process, as this is both impossible and may result in epistemically or ethically unwanted outcomes. Instead, when the practical goal-orientedness of scientific inquiry is taken into account and the non-epistemic nature of some of those goals is acknowledged, we can start developing a new understanding of what objectivity, as a necessary condition for producing trustworthy applicable knowledge, could be. In addition, comprehending how factors such as publication practices, different funding structures of academic fields, or widespread ways of measuring academic merit may create mechanisms that are detrimental, can all help us to reconsider how the institutional context could be rearranged to improve the chances that research could achieve its epistemic and practical goals.

I do not intend to offer an analysis that would apply to all disciplines. As fields of science are methodologically diverse and aim at different goals, searching for the general conditions that would secure the objectivity of research in all disciplines is not realistic. Researchers, for example, in mathematics, theoretical physics, social sciences, or pharmacological biomedical research each have separate ways of being involved in the examination of the world around us and, thus, face distinct epistemic challenges. In order to keep the analysis within certain reasonable limits, I focus on such fields of study that aim at applicable, so-

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<sup>1</sup> By non-epistemic values I here refer to social, political and ethical values. Epistemic values, in turn, refer to simplicity, scope, explanatory power etc.

cially relevant outcomes. The epitome of research of this type is biomedical research, the goal of which is to produce results that can be utilized to make practical decisions that concern the wellbeing of both individuals and populations. Examples of other disciplines of this type, yet not discussed in this dissertation, are nutrition and climate research. In employing this focus, I do not claim that the issues concerning the objectivity and reliability of, for example, mathematics or other purely theoretical fields are either entirely different or the same as the ones central to more directly applicable branches of science, as establishing this would require another study. Furthermore, I do not intend to claim that I have covered all factors that could possibly threaten the objectivity of research in those fields of study that I have been interested in. Instead, I have aimed at pinpointing some central problematic practices and mechanisms that empirical research on current scientific inquiry has uncovered. I recognize the possibility that the examples from empirical studies could have been chosen differently, which in turn might have resulted in theoretical developments of another kind.

The structure of this introductory chapter is as follows. In section 1.1., I begin with discussing the different dimensions of the most central concept of the dissertation, 'objectivity', first from the historical perspective and then in terms of contemporary usage. The aim of this section is to give the reader an understanding of the complex ways in which this concept is used, and show why the discussions concerning objectivity can be so convoluted. In addition, I show why the objectivity of science is an important topic from the non-academic perspective. In section 1.2., I shall outline the theoretical background of this dissertation. In subsection 1.2.1., I begin with questioning a view I call 'the individualistic view on objectivity' and argue that objectivity should be examined at the community-level. Next, I move on to discussing the problem of value-freedom of science. I end the section describing the theoretical background of this work by arguing for the importance of taking into account the contexts of different stages of the research process. In section 1.3., I give an outline of the articles that this dissertation is comprised of. Finally, in section 1.4., I conclude by offering an overview of the results of the dissertation, its implications and some recommendations for future research.

## **1.1 The concept of objectivity**

Objectivity is often used as a watchword in discussions concerning science: As Heather Douglas stated, this term has a lot of rhetorical force of "I endorse this and so should you" (Douglas, 2004: 453). Objectivity is something positive, and something we should strive for if we are to deliver knowledge of the world that we live in. Because of this rhetorical force, what we understand as being objective can influence the way science is practiced and how the results of research are received.

Despite the importance and centrality assigned to the term ‘objective’<sup>2</sup>, its use is not straightforward. It can be used in different contexts: we ponder if a person is objective in his reasoning, try to define what the objective methodology is, and ask for objective information on certain topics. ‘Objectivity’ can refer to criteria, a researcher's way of acquiring data and drawing conclusions from it or to independence of some piece of knowledge from human thought. It is usually thought that the objectivity of a research outcome is a consequence of the objectivity of the method that produced that outcome (cf. Longino, 1990: 62–63). Not only can the word ‘objective’ be used for describing diverse objects or processes, but the criteria for something to be named as objective can also be diverse. Objectivity is a complex concept that has been allocated several meanings throughout the course of history, and still continues to be used in various ways. In what follows, I shall give a brief overview of the history of objectivity and after that discuss some of the varied ways in which it is used today.

### 1.1.1 On the history of ‘objectivity’

Historical analyses of objectivity (e.g., Daston, 1992; Daston & Galison, 1992 & 2010; Dear, 1992; Porter, 1992) have revealed how the use of this concept has changed during the last few hundred years. As Daston (1992) has stated, in the context of acquiring knowledge, different virtues have been attached to the concept and, thus, the methods that have been taken to secure objectivity have been distinct at different periods of time. For instance, in the late medieval scholastic terminology that Descartes inherited, the term ‘objective’ was nearly the opposite of what it means today: objective being (*esse objective*) was ascribed to those things that are objects of the mind as opposed to real being (*esse reale*) (Alanen, 2003: 122–126; Reuter, 2014: 282).

According to Daston (1992: 598), the way we nowadays use the word ‘objectivity’ combines metaphysical, moral, and methodological meanings, which all have a history of their own. In her article, she focuses on the history of so called *aperspectival* objectivity: objectivity that denotes the elimination of individual or group idiosyncrasies for gaining universal knowledge (Daston, 1992: 599). For example, in the context of peer review practices, striving for objectivity in this sense would mean trying to find ways of curtailing the different evaluation styles of different nationalities, genders, more or less experienced reviewers and so on. The idea of *aperspectival* objectivity is to decontextualize the evaluator. Daston’s main claim is that this ideal originates from moral and aesthetic philosophy of the 18th century, and became part of the ethos of science only after the scientific community grew and it became impossible to personally know the people on whose work one had to rely (Daston, 1992: 600).

As disinterestedness, detachment, and impartiality are often seen as virtues in scientific research, in the 18th century they were considered to be characteristics that someone evaluating art or the suffering of a fellow human being should possess (Daston, 1992: 603–604). In the next century the same qualities

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<sup>2</sup> Objectivity is here discussed in the context of producing scientific knowledge.

became important in the scientific context. Daston sees this as a part of the endeavor to build a community that could acquire true knowledge. Overcoming idiosyncrasies was a precondition for efficient communication in a community that had just started to grow and involve more people with different backgrounds. When articles were circulated among colleagues working in different countries, it became essential to find ways of making sure that discussants were talking about the same thing. Part of this development was the standardization of units and categories, as well as the calibration of instruments. (Daston, 1992: 608.) Striving for aperspectival objectivity, i.e., the disposing of national and other characteristics, became a means of building trust between scholars working in different parts of the world: "Subjectivity became synonymous with the individual and solitude; objectivity, with the collective and conviviality" (Daston, 1992: 609).

Aperspectival objectivity became an ideal in the context where researchers no longer were able to cooperate only with those colleagues they knew personally. Appearance of subjectivity would have given distant colleagues difficulties in evaluating the value of work. According to Daston (1992: 610), this ideal of interchangeable observer would not have been appealing in the research communities in the 18th century, as at the time the personal qualities and qualifications of a scientist were taken to be relevant for evaluating their work. In this context, trying to efface these subjective features from scientific work would not have helped effective communication between the members of the community.

A noteworthy issue that emerges from Daston's study is that the pursuit for aperspectival objectivity was not without its costs. In the scientific community of the 18th century, the skill to perform complicated experiments was a respected virtue. When the interchangeability of observer became the ideal, evaluating the detailed skills of peers lost its importance. (Daston, 1992: 610-612.) Performing the agreed steps of the process was more important than showing expertise or originality in investigating the phenomena of interest.

In the same vein, the development of mechanical objectivity, the ideal of letting nature speak for itself without interfering human judgment (Daston & Galison, 1992: 81; 2010: 120), can be taken to demonstrate the pursuit of one epistemic virtue at the expense of another. Daston and Galison (1992; 2010) study the emergence of this ideal in scientific image-making in the late nineteenth and early twentieth century. During this period, the appreciation that was previously expressed to artists who skillfully employed their expertise to portray nature started to vanish. It was replaced by the quest for image-making free from human interference by developing instruments that could register the features of the objects of interests. Through using mechanical procedures and protocols, nature could be pictured objectively, in other words, without human intervention. (Daston & Galison, 2010: 119-120.) Self-surveillance was part of the ideal of mechanical objectivity. According to Daston & Galison (2010: 174), scientists began to control their own judgments and the influence of their individual proclivities on results:



“Self-restraint not only dictated the order of epistemic virtues but also governed the hierarchy of epistemic vices. Active, interventionist, speculative insertions were the worst.” (Daston & Galison, 2010: 175.)

Thus, this ideal was in conflict with the aim of trying to be “true to the nature”, i.e., trying to portray even the most minor characteristics of the individual case as accurately as possible - which had been the goal of earlier atlas makers (Daston & Galison, 1992: 87). According to Daston and Gallison (2010: 179), scientists at the time were aware of the cost that came along with the new ideal: Photographs produced in a way that was in line with the mechanical ideal did not depict the spatial depth, or the color, of the object that was portrayed as well as earlier methods, such as drawings of expert artists. This hampered their diagnostic utility in education, as the student trying to study how to distinguish, for example, different bacteria, did not learn all the details of the objects of interest.

Another manifestation of mechanical ideal can be found in Theodore Porter’s discussion on the aim at describing reality in numerical form. According to Porter, the use of rigid guidelines and presenting the results in numbers is a way of building trust by giving an impression of the absence of judgments. Especially in fields that are under constant evaluation and suspicion, such as accounting, it is essential to maintain the appearance of impartiality, and “[...] reaching agreement by following rules provided their most powerful defence [...]” (Porter, 1992: 639). Unlike appealing to expert judgment, reference to communal rules was seen as a way of highlighting the integrity of the members of the trade. As Porter (1992: 640) points out, the strength of referring to rigid quantitative rules is that it gives the process the impression of publicity. If the rules, by which the outcome has been achieved, are the same for all and possible to explicate, the possible trust issues with regard the person conducting the process diminish. It is enough that the rules can be trusted.

As Porter (1992: 645) states, “Quantification is a powerful agency of standardization because it imposes some order on hazy thinking, but this depends on the licence it provides to leave out much of what is difficult of obscure”. This tradeoff between epistemic virtues resembles a much more recent debate on how knowledge should be acquired. In medical research, the evidence-based medicine movement has advocated the use of mechanical procedures and protocols for producing knowledge on the efficacy of different treatments (e.g., Howick, 2011). Traditionally the expert opinion of accomplished doctors was considered to be invaluable in making decisions concerning the treatment of individual patients. The ideal of evidence-based medicine, in turn, takes as its starting point the fallibility of individuals and aims at eradicating it by basing the knowledge production on the foundation of strict rules for conducting studies. Some critics have criticized this movement for losing sight of the relevant individual differences of patients and their situations (e.g., Kenny, 1997). According to them, by trying to de-contextualize medical knowledge something substantial is lost.

As these historical analyses demonstrate, objectivity in its different forms has been a means for enabling communal actions. Adopting standard techniques, modes of communication, and instruments made it possible to achieve consensus on complicated topics (Porter, 1992: 646). Scientific communities worked together towards establishing guidelines and rules that enabled efficient communication. Striving for objectivity has been a way for securing trust between individuals and communities.

It also becomes clear from this short historical excursion that the different ways of using the term 'objective' have been related to the different aims of practices. Answers to the question: What factors have been found to be the most damaging with respect to the goals of the given activity? have given discussants reasons for appealing to different ideals of objectivity. One of the main goals of this dissertation is to show that we also need to ponder on the reasons for conducting research, its aims, and the values involved, as we make decisions on which ideal of objectivity to follow and what methods to choose to achieve this ideal. For example, as I argue in article III, the so called procedural ideal of objectivity - which I shall discuss later in the next subsection - is not a recommendable ideal for medical research, as it does not capture some of the practices that have turned out to be harmful to this discipline. Following the procedural ideal turns one's focus to evaluating actions of individuals conducting the studies by following a certain set of rules, while ignoring the possibility that the rules themselves might systematically bias research, or that there might be contextual factors causing research outcomes to be systematically biased. For example, with this ideal of objectivity, it is difficult to capture how funding decisions may skew the picture that research paints of reality.

### 1.1.2 On the current use of 'objectivity'

In order to highlight the complexity of the current uses of the term 'objectivity', I introduce an article by Heather Douglas (2004), who distinguishes eight different senses in which the term 'objective' has been used while describing the results of procedures. She focuses only on those senses of the term that are operationally accessible, which means that we know how to discern whether a claim is objective in a given sense. For instance, objectivity in the sense of being independent of human thought is left out of the analysis because in practice it would be impossible to tell whether there is knowledge that is objective in this sense.

By drawing on philosophical and historical work, Douglas discerns three modes of objectivity that describe different processes by which we can acquire knowledge of the world surrounding us. In each of these modes, there are multiple senses of objectivity that come in degrees. This means that objectivity is not an all or nothing quality: for instance, an individual can be more or less objective in the sense of letting her non-epistemic values interfere in her reasoning.

Douglas's first mode of objectivity, Objectivity<sub>1</sub>, can be used for describing the results of processes in which humans interact with the world, for exam-

ple, via experiments. In this mode, there are two different senses of objectivity. *Manipulable objectivity<sub>1</sub>*, can be used to refer to results that are reliable and robust enough to be used in additional experimental contexts. For example, if a theory or a concept can be used as a tool for examining some new phenomena, it can be called objective. The second sense in Objectivity<sub>1</sub>, *Convergent objectivity<sub>1</sub>*, refers to results that can be achieved by using multiple independent methods. When different types of evidence point to the same direction, we should be able to trust the result. (Douglas, 2004: 456–458.) Both senses of Objectivity<sub>1</sub> are achievable by an individual researcher conducting experiments by him or herself.

Objectivity<sub>2</sub> focuses on individuals' thought processes and their outcomes. What do we mean when we say that someone has conducted an objective analysis? What does it mean to give an objective answer to a question? Under this mode Douglas finds three different senses, which, she states, are often conflated. The first sense, *Detached objectivity<sub>2</sub>*, forbids using values in the place of evidence. Objectivity in this sense is relatively simple: one's wishes should not determine what is taken to be true while describing reality. This sense should not be confused with *Value-free objectivity<sub>2</sub>*, which would demand keeping all values apart from reasoning processes. (Douglas, 2004: 458–460.) Douglas, among other philosophers (e.g., Longino, 1990 & 2002; Intemann & de Melo-Martin, 2010; Wilholt, 2013) has argued that this ideal of objectivity should be abandoned<sup>3</sup>. Values can have a justified role to play in reasoning and, thus, this sense of objectivity should not be pursued. Likewise, I have opposed this view in the articles that comprise this dissertation; this ideal of objectivity is both unattainable and unable to capture what the role of science in today's society is.

The third sense of Objectivity<sub>2</sub>, *Value-neutral*, is also sometimes confused with value-freedom. This sense requires one to be balanced with respect to a set of values and to consider what consequences an outcome would have with respect to different positions. For example, an overview of literature is objective in this sense if it presents the relevant literature in a reflectively balanced way. (Douglas, 2004: 460.)

The third mode, Objectivity<sub>3</sub>, is used when evaluating social processes for knowledge production. Being objective in the sense of *Procedural objectivity<sub>3</sub>* means that a procedure produces the same results regardless of who is performing it. For example, multiple-choice tests can be objective in this sense: the result of a test should be the same independently of who is doing the grading (Douglas, 2004: 461–462). As Douglas notes, understanding objectivity in this sense is not unproblematic. Particularly, it should not be taken to entail freedom from values: in the case of multiple-choice test, values can be reflected in how the questions and possible answers are chosen (e.g., Schiebinger, 1999: 175 on criticism of SAT-tests). In article III of this dissertation, I discuss how the procedural ideal has influenced discussions concerning the evidential status of meta-analyses. There I show how this ideal does not capture some of the important

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<sup>3</sup> I will later discuss in section 1.2.2. the value-free ideal and its criticism.

features of the medical research and should, thus, not be used as the only criterion for evaluating the production of medical knowledge.

The second sense of objectivity<sub>3</sub>, *Concordant objectivity*<sub>3</sub>, takes the criterion of objectivity to be that a group of individuals agree on a result. In other words, the objectivity of a statement could be determined by interviewing individuals and checking whether their views on the issue are in unison. In article IV, I discuss the problems that are related to this ideal in assessing the objectivity of peer review processes. The major issue is that, according to this sense of objectivity, we could call such a result 'objective' in which a group of biased individuals agree. Thus, as Douglas (2004: 463) notes, this sense does not guarantee that the group has "gotten at something real".

The last sense of Objectivity<sub>3</sub>, *Interactive objectivity*<sub>3</sub>, in turn, requires that a result is reached through a process that involves discussions between the members of the group. It is not enough that individuals agree but they have to have critically debated the issue among themselves. (Douglas, 2004: 462-464). This latter sense of objectivity<sub>3</sub> is what Helen Longino (1990; 2002), among several other social epistemologists, has advocated. But this sense of objectivity faces problems, as well. It leaves open the questions of how the community should be structured, what rules of discussion should be followed, and in what kind of institutional context the discussion should take place. This last question is what this dissertation focuses on examining.

The crux of introducing different senses of 'objective' is to explicate that the various senses of objectivity are not reducible to one another. Consequently, "[t]he bases for epistemic endorsement and trust are varied" (Douglas, 2004: 455). I have referred to Douglas in three of the articles of this dissertation, as her analysis conveniently shows how different ideals of objectivity justify utilizing different methods and bring out different outcomes. Thus, it offers a great tool for analyzing both philosophical and methodological discussions on how to best acquire knowledge. Her inquiry on the irreducible complexity of the term helps to explicate why discussions on how to best prevent the effect of biases in knowledge production often seem insolvable: if the discussants do not agree on which ideal to follow or do not even recognize that they are referring to different senses of objectivity, it is difficult to find a satisfying solution to the debate. Differences in ways of understanding what this concept denotes and what practices should be labelled as objective, often underlie debates about which practices should be promoted in research. Likewise, debates concerning objectivity are often at least partly based on misconceptions about the objectivity of what - results, procedures, people - is actually being discussed.

Talking about the concept objectivity would be incomplete without mentioning its antonyms: what is non-objectivity? Subjective is often indicated as the antonym of objectivity. The notion of subjectivity is of course complex as well: It can refer to something that is only available for an individual to experience and impossible to share, like a feeling. Used in this sense, subjectivity does not have derogatory connotations. This is often not the case when subjectivity of a method or an outcome is hinted at in a scientific context, in which objec-

tive tends to refer to something that is idiosyncratic. (Douglas, 2004: 469.) Objectivity has often been depicted as wiping out subjectivity from the process (e.g., Stegenga, 2011: 498). However, as with its opposite, the meaning of subjectivity seems to be at least partly context-dependent and historical (Daston & Galison, 2010: 5).

Instead of being subjective, we can also talk about a non-objective result being biased. This is the approach I have taken in this dissertation. Unlike subjectivity, the notion of bias captures the idea that research can fail to be objective because of systematic factors that skew the outcome of studies. Bias can refer to both systematic errors (such as in the case of measuring instruments) or preference biases, which “occur[---] when a research result unduly reflects the researchers’ preference for it over other possible results” (Wilholt, 2009: 92). Preference biases can be caused by either blatant promotion of some extraneous causes or they can be the result of implicit preference for or against some views or goals. Thus, the concept of bias captures the idea that non-objectivity can be caused by structural or systematic factors, not only by the involvement of individuals’ idiosyncrasies. In the same way that calling something objective can raise disagreements, it is possible to disagree on whether a result, person or a method should be called non-objective.

The quest for objectivity in science has raised concerns; these can be roughly divided into two categories. First of all, some authors state that objectivity is an unachievable goal and that the knowledge we produce is bound to be inherently subjective and value-laden. Secondly, others have claimed that the whole ideal is detrimental. For instance, some feminists have stated that it is attached to masculine ideals (cf. Harding, 1986; Barwell, 2002), with Catherine MacKinnon (1987: 50) stating “[o]bjectivity is the epistemological stance of which objectification is the social process, of which male dominance is the politics, the acted out social practice”. Pursuit for objectivity, according to MacKinnon, is a pursuit for a “non-situated distanced standpoint”.

These criticisms, however, can be taken as criticism of a certain narrow understanding of what the objectivity of science is: “Whether understood as a view from nowhere or as algorithmic rule-following, whether praised as a soul of scientific integrity or blamed as soulless detachment from all that is human, objectivity is assumed to be abstract, timeless, and monolithic”, write Daston and Galison (2010: 51) when they criticize those who take part in debates over objectivity for not leaving the conceptual level and not taking notice of how the concept is used in practice. In this dissertation, I take objectivity to be contextual in the same way as Daston and Galison do. I have taken the stance that objectivity needs to be evaluated as something that is important for achieving usable knowledge. And also, what the specific criteria for usable knowledge in a given context, also provide guidelines on how we should understand objectivity in that context.

One could ask if it even makes sense to use the term objectivity because of its varying meanings and interpretations. During the process of writing the articles for this doctoral dissertation, I did at times play with the idea of dis-

carding the concept and talking instead about the reliability or trustworthiness of research. However, I decided to retain this concept. As Douglas states (2004: 453), objectivity is a central concept in philosophy of science and also in discussions concerning science among members of the general public. Because of this, shying away from using the concept seemed like an action based on idiosyncratic timidity rather than academic prudence. Also, the complexity of the concept made it possible to investigate how different authors understood it and whether these understandings had influenced their own analyses. For example, in article III I argue that because of his commitment to the procedural understanding of objectivity, Stegenga (2011) does not succeed in analyzing the way in which the results of meta-analyses can be biased by the way medical research is organized. In a similar way, using the ideal of concordant objectivity for evaluating peer review practices overlooks how agreement between reviewers does not yet suffice to insure the high quality of published literature, as I show in article IV.

In the articles that comprise this dissertation, I have not given a conclusive definition for objectivity. I have taken the stance that the search for an objective method is a quest for a method that would best ensure the epistemic trustworthiness of results, as in the sense as Wilholt (2013) uses the term: Trustworthy results are the best ones we can acquire in the given situation, using the means that are available. The trustworthiness of results in this sense, in turn, is partly dependent on how the goals of the inquiry are comprehended. In this way, objectivity is attached to the practical needs that the inquiry is expected to satisfy and thus context-dependent. In this sense, objectivity is not monolithic: different methods can be taken to be objective depending on which kind of knowledge they are expected to produce, and what the context in which the process takes place is. As we have no way of resolving how the world could be viewed independently of the limitations that human cognition and preferences pose, this is the best we can hope for.

### **1.1.3 On objectivity and the public's trust in science**

The objectivity - or apparent objectivity - of research is taken to be an essential condition for maintaining the public's trust in science, and consequently discussing the conditions for achieving objectivity is significant also from the non-academic perspective. Discussing the question of how to secure the objectivity of research is crucial in the current context in which science is expected to produce applicable results. The public's trust in science is largely based on the assumption that the knowledge that is produced is not tarnished by extraneous interests. As science - particularly academic science - is dependent on public funds and resources, maintaining the public's support is vitally important. In addition, many researchers believe that the results of their work could help members of the public lead better lives, whether it be due to increased health or improved chances of controlling their environment, which means that they wish their work to be taken seriously. An example of this scientific desire for public respect, and the possible break down of the required civic trust, is

demonstrated by the case of the recent outbreaks of vaccine preventable diseases, which have been associated with the rise of an anti-vaccine movement resulting from diminished trust in relevant medical research (e.g., Poland & Jacobson, 2011).

What is typical of medicine and many other research fields with applicable results is that they raise intense emotions among non-scientists. There are at least two reasons for this: 1) the research outcomes from these disciplines are used by doctors and policy makers when decisions affecting people's daily lives are made, and 2) claims of improper conduct and questionable research ethics have been reported by media, both of which greatly increase the focus of public attention and interest in these areas of research. Examples of recent scandals in biomedical research include the apparent concealing of the possible connection between the use of selective serotonin reuptake inhibitors and suicidal tendencies (e.g., Healy, 2012) and the link between the painkiller Vioxx and cardiovascular events (e.g., Biddle, 2007). In climate research, so-called climate skeptics accused scientists of fabricating data for political reasons just before the Copenhagen summit on climate change in 2009 (e.g., Climate scientists accused of 'manipulating global warming data', 2009). As the story reached the headlines, the reliability of climate research became increasingly under a threat, and public confidence was eroded. The reactions to these events show that one of the most crucial challenges for the contemporary scientific community is securing the public's trust in research. As one of the conditions for maintaining the trust in science is that research is conducted in a way that is thought to be objective, searching for the means that best support objectivity is one of the central tasks for philosophers of science.

In medical research, many of the scandals have been connected to actual or apparent financial interests of the researchers conducting studies or agencies which fund the studies. It is appropriate to be noted here in the dissertation introduction that despite the critical tone in which many of the practices typical of commercialized research are discussed, the aim of this dissertation is not to denounce commercialization as such, rather, discuss its effects and acknowledge that commercialization has been one of the most central institutional changes affecting academia and the scientific community in recent decades. The goals of scientific research and business are different, which, as my analysis demonstrates, can lead to conflicts between epistemic or ethical and financial aims. How to best identify and counteract these conflicts is one of the central questions in this dissertation.

## **1.2 Theoretical background**

The four articles that compose this dissertation engage in discussions concerning science, values, and the role of science in society. This area of philosophy

has become more popular since the so-called Science Wars<sup>4</sup> took place in the 1990s (Douglas, 2009: 45, see Brown, 2001 for an assessment of science wars). In these discussions, the previously dominant assumption that by the proper use of scientific methods non-epistemic ( i.e., political, ethical, and social) values can be prevented from influencing the rejection and acceptance of hypotheses has been questioned (Biddle, 2013).

In this section, I outline the theoretical background of this work. I begin by discussing the criticism of individualistic views on the objectivity of science. After this, I move on to examining the question of value-free ideal in science, and finally I end the section by investigating the role of extra-scientific factors in moulding the conditions for objectivity. I intend to show that when we examine socially relevant research, it is necessary to assess it as an activity that is practiced by communities that work towards reaching certain epistemic and practical goals and that these communities are not fully self-sufficient in regulating their actions.

### 1.2.1 Against an individualistic view on objectivity

According to a traditional conception, which I have called ‘the individualistic view on objectivity’, the objectivity of scientific research can be secured if individuals keep their undue preferences in check and base their reasoning on relevant facts and logic. Tara Smith (2004) crystallizes this view in the following way: “Objectivity is a method for human beings [---] to employ to discipline our thinking to help us gain an accurate understanding of the world” (Smith, 2004: 153). In other words, according to this view, objectivity is a quality that is attached to individuals and the objectivity of research either flourishes or fails depending on how individual scientists behave. In this dissertation, I argue that this understanding fails to capture how socially relevant research is and can be conducted.

Focusing on individuals alone is insufficient for two reasons. First of all, studies on implicit biases show that people do not recognize whether their own reasoning has been biased (Pronin, Gilovitch & Ross, 2004; Uhlmann & Cohen, 2007). Thus, it is not realistic to assume that humans are capable of acting in a way demanded by the individualistic view. Second, in addition to this practical difficulty of achieving the ideal demanded by the individualistic view, there are more theoretical reasons for approaching the objectivity of science as a community-level virtue.

Miriam Solomon’s (2001) social empiricism is an example of recent criticism of individualistic conceptions of scientific knowledge production. By drawing on studies in cognitive science and investigating historical case studies, Solomon states that when evaluating research, instead of asking whether individual scientists reason rationally, we should ask whether the context in which

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<sup>4</sup> In addition to philosophers, sociologists, historians and scientists were active in this debate. In this dissertation, I focus on the philosophical discussions concerning objectivity. Consequently, questions that have been central in science and technology studies are omitted.



they conduct research is such that the combined outcome of individual efforts in science are successful, i.e., getting closer to the truth and empirical success - were it observational, predictive, retrodictive, experimental, explanatory, or technical. (Solomon, 2001: ch. 2). According to Solomon, evaluating individuals' rationality is not necessary because sometimes even non-rational actions of individuals can be beneficial to the goals of research. Even those reasons for choosing between theories that are usually considered biased, such as ideology or the search for fame, can under the right conditions be beneficial to science. This is because they can motivate researchers to distribute their efforts in ways that eventually lead to empirical success. Solomon continues, individual scientists should strive for developing theories with empirical success (Solomon 2001, 150), but there is no better or worse reason for them to choose between empirically successful theories, for it is the duty of those who make science policy and funding decisions to make sure that all empirically successful theories get the attention they deserve (Solomon, 2001: 117-118).

Even though Solomon successfully identifies the importance of evaluating science as a communal enterprise, her view does not capture some central features of research. The only requirement Solomon directs at individuals is to aim at developing empirically successful theories, and because of this, her account is not sensitive to the actual practices of science that involve the give and take of criticism: According to her theory, if researchers are working with empirically successful theories, they are not responsible for answering further questions concerning their theory choice. This seems to conflict with what is generally expected from scientists. As Rolin and Wray (2008: 76) note, a community in which individuals were not willing to engage in discussions concerning their reasons for choosing a given theory would be in the danger of becoming "fragmented and stagnated". In this dissertation, I defend a stance in which the critical discussions between scientists, including the asking for and presenting of evidence for adopted views, are necessary for objectivity. The importance of this kind of interaction between individuals and communities is highlighted in Helen Longino's critical contextual empiricism.

Longino (1990; 2002) has forcefully argued against individualistic views on knowledge production. According to Longino (1990: 63), if we are to find an objective method that "provides means of assessing hypotheses and theories in an unbiased manner", we cannot demand that individual researchers should gather data and reason without biases. Instead, she claims that the objectivity of science is achievable because research is a social enterprise (Longino, 1990: 67). According to Longino, an appropriate social process is necessary for achieving objectivity since individuals alone cannot ensure that their reasoning is sound. Her account is based on the fact that data do not fully dictate which hypothesis should be accepted and, likewise, hypotheses alone do not specify which data would confirm them (Longino, 2002: 126). Because of this underdetermination, different background assumptions can enable inferences from certain data to different hypotheses. These assumptions may concern, for example, the relevance of certain types of evidence or research questions, or they may be princi-

ples, such as simplicity (Longino, 2002: 126). In addition, background assumptions are often context-dependent and unrecognized by those who hold them. Different notions of the goals of science and the characteristics of "good research" bring forth different ideas concerning what kind of evidence is sufficient to justify the acceptance of a hypothesis.

Sometimes assumptions working as premises in reasoning may involve social or political, i.e., contextual, values. Unlike those who support the current ideal of value-free science<sup>5</sup> (e.g., Kitcher, 2001), Longino does not denounce the involvement of contextual values in the scientific reasoning: according to her, science is value-laden but can still be objective because the effects of values and preferences can be managed. This is feasible because knowledge production is a social process where the outcome is the product of critical interaction within a community. Science is an ongoing process of evaluation and re-evaluation of data and background assumptions that influence research. Since some of the assumptions involved in reasoning cannot be justified by empirical means alone, metaphysical and normative considerations are an indispensable part of scientific discussions. (Longino, 1990: 75.) In other words, unlike for Solomon, for Longino it is essential that researchers engage in discussions concerning their reasons, including non-epistemic reasons, for choosing between empirically successful theories. Central to Longino's theory is that in addition to the traditionally required elements of knowledge production, namely that decisions on whether to reject or accept a hypothesis need to be based on sound data, a further social mechanism is needed to make sure that the background assumptions justifying inferences from data to hypotheses are acceptable (Longino, 1990: 75). Thus, achieving objectivity is a community-level task.

According to Longino, the objectivity of research cannot be obtained in a context where the social mechanism of criticism is prevented from functioning. Two of the necessary conditions for objectivity are the publicity of research and the availability of different perspectives. Withholding data or results is harmful to science because it makes it more difficult to evaluate the factors that have contributed to the acceptance of views. Widely shared assumptions may be invisible until they are questioned by someone not holding them, and thus, according to critical contextual empiricism, pluralism of background assumptions and perspectives is epistemically beneficial. (Longino, 1990: 80; Longino, 2002: 131.)

This dissertation, particularly the first two articles (*The Commercialization of Research and the Quest for the Objectivity of Science*, and *Longino's Theory of Objectivity and Commercialized Research*), draws on Longino's work. Based on her view on the social nature of knowledge production, I have aimed at further explicating the conditions that are needed for enabling the detection of different biases that may skew the picture that studies paint of their objects of interests. I have also been interested in examining the implications of the criteria that, according to Longino, need to be fulfilled by communities that strive to produce objective science.

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<sup>5</sup> This ideal is discussed in the next subsection.

Even though Longino convincingly shows that the objectivity of research needs to be seen as a social enterprise, her theory on the conditions for securing objectivity is very general, and abstract, i.e., meant to be applicable for all fields of research (see also Biddle, 2007: 23–24). It is not clear what is meant, for instance, by stating that a community should “cultivate all relevant perspectives” or that economic power should not determine what views are taken seriously (Longino, 2002: 131)<sup>6</sup>. Addressing former point is made more challenging by the fact that the boundaries of scientific communities are not clear and it is not obvious who should be counted as members of scientific communities, and thus, whose criticism should be taken to be relevant<sup>7</sup>. The latter point is particularly pressing in the current context in which cooperation between academia and industry is becoming more common and the importance of private research funding is growing. In addition, her account does not fully take into account the way in which commercialization is changing academic publishing, for instance, asking what is the role of private peer review companies in providing venues for criticism (Fernández Pinto, 2014)?

Consequently, contrary what Longino aims at, it may be difficult to see how scientists or science policy makers could use her theory as a guide for organizing research. This dissertation aims at filling some of the gaps that are left open by Longino. I have approached the problem of securing objectivity from the perspective of a naturalistic, empirically informed, philosophy of science: I have analyzed scientific practices as they exist, examined how different institutional factors influence knowledge production, investigated how science could best succeed in fulfilling its goals, and aimed at delivering normative insights based on empirical material - instead of being interested in what science could achieve under certain ideal conditions. Analyzing empirical studies on science (e.g., Healy & Whitaker, 2003; Sismondo, 2008; Pigott et al., 2010), makes it possible to investigate the harmful effects of the cooperation between academia and private industries without denouncing all commercial science as biased. Based on this material, it can be argued that commercial interests are linked to practices and mechanisms that can limit either the publicity or the diversity of points of view. For instance, the examples I discuss in the articles show how the funding structure of an academic field can steer research in a way that leaves some essential areas understudied and, thus, disturb debates in the field in question. Likewise, the material I have studied shows how commercial interests may encourage actors to leave some results unpublished. The resulting publication bias makes the criticism of published results more difficult and gives a distorted picture of the object of study. Thus, I have identified mechanisms that pose threats to objectivity in Longino’s sense. In this way, this dissertation fleshes out Longino’s ideas about the conditions for sound knowledge production and assesses which factors need to be accounted for if her theory is to be applied to current scientific practices.

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<sup>6</sup> See also Borgerson (2011).

<sup>7</sup> I wish to thank Heather Douglas for advising me to stress this point.

Different understandings of objectivity steer our attention to evaluating different parts of phenomena. Basing an account of objectivity on some unattainable ideal of human cognition is not acceptable. Paying attention to the role that science plays in today's society shows that the individualistic view on objectivity does not manage to capture some of the relevant aspects of knowledge production because it tells us to focus on the actions of individuals only and thus neglects the possibility of systematic biases. Assessing objectivity at the community-level is particularly important when examining research that is used for informing practical decision making and public policy. When scientific evidence is needed for this cause, evidence from numerous individual studies is often amalgamated and the guidelines are developed on the basis of the combined results. Because of this, it is necessary to consider the possibility that there might be biasing factors that systematically skew the results of research in a way that cannot be corrected by the means of testing hypotheses only. Later, in the subsection 1.2.3., I discuss the funding structure of research as a potential mechanism that steers research towards producing certain types of results. Focusing on the way in which researchers conduct individual studies neglects this source of bias.

### 1.2.2 Against the value-free ideal

As mentioned, philosophers of the Anglo-American tradition<sup>8</sup> have recently started to direct more attention to the involvement of non-epistemic values in knowledge production. The current ideal concerning values in science does not hold that science is completely free of values, but demands that the internal stages of science, i.e., evaluation of hypotheses, should be free of non-epistemic values<sup>9</sup>. By contrast, setting the research agenda, avoiding methods involving human or animal subjects and deciding how to use research results, can be motivated by social and ethical values. Despite the young age of this ideal, its impact in philosophy of science has been profound.

Philip Kitcher's *Science, Truth and Democracy* (2001) is an example of a project that operates according to this ideal<sup>10</sup>. He has taken up the task of describing the role that science should have in today's society and calls it "well-ordered science". The particular focus of this work is on examining how the research agenda should be set: Which are the questions that scientists should ask? According to him, we need a democratic, public control of what questions

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<sup>8</sup> It has to be acknowledged that the interplay between social factors and science has been recognized for decades outside Anglo-American philosophy of science by authors such as Foucault (e.g., 1970; 1972), Habermas (1971) and several feminist and postcolonial scholars (e.g., Harding, 1998; Keller, 1985). I thank Hans Radder for pointing this out to me.

<sup>9</sup> See chapter 3 of Douglas's book (2009) for an introduction to the history of the value-free ideal.

<sup>10</sup> In his later book *Science in a Democratic Society* (2011), Kitcher holds that non-epistemic values can legitimately influence also the certification of scientific claims, i.e., on what kind of outcomes should practical decision-making and discussion be based.

research should try to answer and how the results of this research should be applied in practice. (Kitcher, 2001: ch. 10, see Biddle, 2007 and Leuschner, 2012 for criticism of Kitcher.) For instance, the recent situation in biomedical research is not acceptable according to the ideal of well-ordered science: it is unlikely that in an ideal decision making process, participants would decide that the significant majority of research funding should be directed at projects that benefit the affluent populations while leaving the diseases that afflict the poor majority understudied (Reiss & Kitcher, 2009).

Kitcher correctly argues for the importance of discussing what goals we wish research to serve. However, the view I defend in this dissertation diverges from the one Kitcher advocates in his 2001 book in that according to him, non-epistemic values can and should influence science, but only in setting its agenda and the application of the results. The internal stages of research, e.g., testing of theories, should still follow the value-free ideal. This view can be criticized for not taking sufficiently seriously the possible non-epistemic consequences of research and the moral responsibilities that scientists have regarding their work.

A prominent argument against the value-free ideal can be found in Heather Douglas's (2000) work. In Douglas's account, the fact that pragmatic aspects are involved in the acceptance or rejection of hypotheses entails the need for value judgments. Not acknowledging that science is not practiced for purely epistemic reasons and the possible non-epistemic risks that follow, results in inadequate accounts of science. Her argument begins with Hempel's (1965) concept of inductive risk, which refers to the possibility that one is mistaken in accepting or rejecting a scientific statement. Inductive risks can occur throughout the research process: when research methods are chosen, when data is gathered, characterized and interpreted. When research has non-epistemic consequences, for example, when the aim of research is to develop a drug, researchers need to weigh the potential outcomes of their choices. Douglas demonstrates the potential risks by referring to dioxin-studies. For example, the choice of the appropriate statistical significance level in toxicological studies should involve taking into account what kind of errors are the most tolerable: false negatives or false positives. Lowering the standard of statistical significance decreases the risk of false negatives and vice versa. In certain borderline cases, the decisions on the appropriate standard can have an impact on whether a given chemical is taken to be harmful, and thus, whether it is accepted for use or not. This, in turn, has both financial and public health consequences. Consequently, when decisions are made on where to set the level of statistical significance, researchers should take into consideration these non-epistemic results of action. Likewise, at other stages of research, researchers need to take into account the apparent magnitude of the inductive risk and the possible consequences that might follow. This means that non-epistemic values should play a role in the internal stages of science. (Douglas, 2000.)

This account does not entail relativism or the view that all arguments should be taken as equally valuable: values do not dictate which account is true. Rather, the account boils down to the claim that the way in which the quality of

arguments are evaluated has to be changed. Scientists have the same responsibility for considering the consequences of their actions as other human beings have. Thus, it is not laudable for them to avoid weighing the potential risks that are involved in research. (Douglas, 2000: 560.)

The argument against the value-freedom of science has been developed further by Torsten Wilholt (2013). While Douglas shows how values should be involved in scientific reasoning, Wilholt discusses how we can discern between acceptable and unacceptable value choices. He makes a distinction between epistemic reliance and epistemic trust. As mentioned in the subsection 1.1.2., I take the quest for objective methods to be the same as the quest for methods and practices that would secure the epistemic trustworthiness in Wilholt's sense. Epistemic trust means that in addition to believing that scientists do their best in following the communally accepted rules of how to conduct studies, which constitutes epistemic reliance (Wilholt, 2013: 235), the members of the public and scientists themselves assume that when methodological rules leave space for judgments, the decisions that are made during research are "based on the presumption of shared ideas about the values of true results and the dangers inherent in errors" (Wilholt, 2013: 248). In other words, acceptable value choices are choices that are not in conflict with the goals of the community.

Thus, in order to evaluate whether the choices of individuals are warranted, science has to be understood as a collective enterprise with certain shared goals and values. First of all, communally agreed methodological standards facilitate epistemic reliance by offering some constancy to research activities. Secondly, individuals are expected to conduct their activities in a way that supports the goals of the community, and their judgments and choices should be critically evaluated by the community.

For instance, in the case of conducting meta-analyses, epistemic reliance consists in the assumption that researchers are aware of the communally accepted guidelines and conventions for conducting analyses (for instance, *The Handbook of Cochrane Collaborations*) and do their best in obeying them. However, because methodological conventions do not fully determine how research should be carried out, the need for epistemic trust emerges. When researchers need to apply the rules, they can be expected to make judgments and decisions that do not conflict with the goals of inquiry. In the case of medical research, if we accept that improving the health outcomes of different populations is a goal of research, from the perspective of epistemic trustworthiness it is necessary to take into account how the judgments (e.g., methodological choices) made in the course of research support this goal. For instance, it is possible to criticize the use of methods that make the detection of severe side effects more difficult. In this way, this approach can capture the fact that researchers have to make various judgments in the course of the research process, and that sometimes non-epistemic interests play a part in decision making. Still, it offers us tools to analyze why some ways of proceeding with research seem reprehensible.

In this dissertation, I use both Douglas's and Wilholt's ideas as a background for developing an account on how we should discuss objectivity in the context of socially relevant research. Their arguments for the inherent value-ladenness of research have shown that complete value-freedom is not a laudable goal, but that this does not have to denote that we should give up objectivity. Objectivity just has to be reconceptualized to accommodate non-epistemic values and their involvement in reasoning as well as the critical evaluation of the values involved. This, in turn, requires that the community and the institutional context in which it operates are free of mechanisms that might limit the exchange of the information concerning these values and principles governing research. Examining what these mechanisms might be in the current scientific world has been one of the main aims of this dissertation. Next, I shall move on to discussing in what way the extra-scientific context of research activities should be heeded.

### **1.2.3 For the importance of the context of scientific activities**

A traditional distinction in philosophy of science has been between the so-called contexts of discovery and justification. In the context of discovery, ideas and hypotheses are developed, and what happens in this context is, according to the traditional understanding, negligible with respect to objectivity because the validity of these ideas and hypotheses is tested in the context of discovery. (cf., Brown, 2010.) The choice of research questions is generally seen as belonging to the context of discovery, and biases entering the process in this phase are thought to be removable later when hypotheses are tested. Ideas for research can come from any possible source: based on dreams, questions left open by earlier research, or the needs of sponsors of the studies. In a similar way, discussions concerning the objectivity of science typically focus on scientists and their actions. Actors that operate outside research communities are usually considered irrelevant to achieving objective knowledge.

One of the aims of this dissertation is to show that the traditional understanding is incorrect. The so-called discovery side of science should not be disregarded if we are interested in what factors influence knowledge production. The way in which research projects and questions are framed has an important effect on the way in which science portrays the world. Because of this, examinations of the conditions for objectivity should not focus solely on the conditions for justification procedures (for example, in the case of medical research, on how trials are designed and carried out) while disregarding the ways in which research can be biased by defects that stem from extra-scientific factors.

The importance of paying attention to the way in which research questions are chosen and to the factors influencing those choices becomes clear through examining current medical research. Brown (2010: 91; 101) and Musschenga, van der Steen & Ho (2010: 112), among others, have expressed concerns over medical research being guided towards producing partial knowledge of health issues. Since the field of study is predominantly funded by actors with

commercial interests<sup>11</sup>, it is profitable for researchers who wish to enhance their chances of receiving grants to focus on specific research questions. This may result in a situation where certain research topics (e.g., illnesses typical of poor areas, or non-patentable cures) are neglected, and research may be excessively steered towards solving only certain types of problems, i.e., problems with possible profitable solutions (Brown, 2008: 761).

By choosing a certain approach to a phenomenon, it is possible to highlight or understate some of the features of the object of study. A recent study by Kearns, Glantz & Schmidt (2015) describes how the sugar industry has had a significant effect on the research agenda on dental health: it offered funding for research on how to prevent the negative consequences of sugar consumption and developed a close relationship with the National Institute of Dental Research (NIDR). As a result, research that was potentially harmful to the industry (i.e., research that recommended reducing sugar consumption) was omitted from the priorities of NIDR. This demonstrates how the way in which questions are framed influences what the eventual pool of results tells us about the world. Studies can paint a skewed picture of their object even if they were conducted soundly if parts of the phenomena are systematically omitted from the examinations.

Different policies for controlling the harmful steering effects of extra-scientific factors have been suggested. For instance, full disclosure of conflicts of interest is one common suggestion for dealing with the problems caused by commercial interests in medical research. However, disclosure alone may not be enough (e.g., de Melo-Martín & Intemann, 2009). Hence, more revolutionary solutions have been offered. While discussing pro-industry-bias in medical research, Sismondo & Doucet (2010: 279) make a radical suggestion: either drug development should be done by public agencies or generic companies, or “governments [---] could force the division of pharmaceutical companies into entities that do research and development and entities that produce and distribute drugs[---]”. The rationale for this is to make sure that drug research is done with a “genuine epistemic interest” (Carrier, 2010), not for marketing purposes alone. Brown (2008: 763) also argues for socializing medical research: the solution is to “eliminate intellectual property rights [and] [m]ake all funding public”. A similar, albeit less radical suggestion has been offered by Carrier (2010: 181) and Lesser et al. (2007: 0046) according to whom, if commercial funding fails to cultivate the plurality of viewpoints in some fields of study, public funding is needed to sustain alternative approaches. The aforementioned authors want to correct the epistemic problem of research through political solutions.

As research activities are dependent on financial resources, the allocation of funding has a steering effect on how questions are framed, which, in turn has an impact on what kinds of outcomes are produced. Thus, the conditions of objective research can be influenced by factors that we could call extra-scientific. Usually, the duty to guard objectivity of research is taken to be the task of scien-

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<sup>11</sup> In 2012, 58% of biomedical research in the US was funded by private sources (Moses et al., 2015).



tists. However, even though researchers in the roles of reviewers and panel members participate in decision making concerning how resources are allocated, the outcomes are influenced by many actors who would not generally be counted as members of “the scientific community”. Among these groups are, for instance, employees of funding agencies (Mallard, Lamont & Guetzkow, 2009) and those who make decisions on national and even international policy (Slaughter and Leslie, 1999, see also Solomon, 2001 for a view highlighting the importance of science policy makers and Rolin & Wray 2004, for a criticism of Solomon). Examining the empirical material (e.g., Slaughter & Leslie, 1999; Sismondo, 2008; Lamont, 2009) on the ways in which financial interests influence research activities and how funding decisions are made, offers philosophers of science new insights on which factors and actors are relevant when the conditions of knowledge production are discussed.

The influence that the institutional context can have on scientific practices can be seen also in journal peer review. When the functioning of this mechanism is evaluated, one should not simply focus on assessing reviewers and the effects of their possible biases, without taking notice of the wider context in which publication records play a crucial role in academic career development. As I argue in article IV, the publish or perish culture that forces researchers to submit papers that are likely to be accepted, can increase the effects of certain content-based biases: The success of peer review is not only dependent on the integrity of individual reviewers but also on the institutional context in which peer review operates. Securing the high quality of published work can be made more difficult by institutional practices that at the first sight are irrelevant, such as the methods of evaluating scientific merit.

### 1.3 Overview of the articles

In addition to the present introductory chapter, this dissertation consists of four articles, which all examine the conditions for objectivity from different perspectives. The first two of these articles (*The Commercialization of Research and the Quest for the Objectivity of Science*, and *Longino’s Theory of Objectivity and Commercialized Research*) discuss the issue at a more general level and develop a view on the conditions for objectivity. This view is then applied in the last two articles that have their focus on more particular questions: Article III (*Meta-analysis, ideals of objectivity, and the reliability of medical knowledge*) focuses on meta-analyses and the production of medical knowledge and article IV (*A Social Epistemological Inquiry into Biases in Journal Peer Review*) focuses on one of the most central mechanisms of science, namely peer review.

In article I, *The Commercialization of Research and the Quest for the Objectivity of Science* (published in *Foundations of Science*, 2014, DOI 10.1007/s10699-014-9377-8) I discuss the conditions for objectivity in the context of socially relevant, commercialized research. The article has two aims. First of all, I argue against a traditional, individualistic conception of objectivity and

show that this view does not capture the challenges of current knowledge production. Secondly, I show that certain features of the commercialized research culture are epistemically detrimental.

In the first part of the article, I defend a social view on objectivity. According to a view which I call the individualistic view on objectivity, the objectivity of science is dependent on individuals and their ability to reason logically and follow the facts (e.g., Smith, 2004). Contra to this stance, I argue that there are two reasons why focusing on individuals alone is insufficient for ensuring that the results do not reflect undue preferences. First of all, studies on implicit biases show that people do not always recognize biases in their own reasoning (Babcock et al., 1995; Katz et al., 2003). Because of this, we cannot expect individuals to be able to act in a way that is expected by the individualistic view on objectivity. Second, there is a more theoretical argument against the individualistic view. By referring to Longino's (1990) argument concerning underdetermination and Douglas's (2000) view on the role that non-epistemic values necessarily play in socially relevant research, I show that there can be multiple acceptable ways of carrying out research, which, in turn, means that we need a way of discerning acceptable judgments from unacceptable ones. According to the understanding I am advocating in the article, objectivity of research means that the judgments that are made are in line with the communally accepted goals of inquiry. This, consequently, requires that the actions of individuals can be supervised and critically evaluated by other members of the community. Therefore, factors that limit the possibility of criticism should be considered as harmful to objectivity.

In the second part of the article I argue, by referring to examples from biomedical research, that the commercialization of research has encouraged practices that limit the possibility of criticism and which should be taken to be detrimental to objectivity. The examples demonstrate how commercial interests can interfere in a harmful fashion at different stages of research. I show that the problematic nature of these practices cannot be fully analyzed from the perspective of the individualistic view on objectivity.

This first article contributes to the debates on the objectivity of science by showing how the individualistic view on objectivity is insufficient for analyzing and resolving some of the central issues in knowledge production. In addition, the article accentuates the need to pay attention to the potential epistemically problematic consequences of conflicts between commercial and scientific interests.

In article II, Longino's Theory of Objectivity and Commercialized Research (published in In Wagenknecht, S., Nersessian, N. & Andersen, H. (eds.): *Empirical Philosophy of Science. Introducing Qualitative Methods into Philosophy of Science*. pp. 127-143. Springer international publishing.), I examine Longino's (1990; 2002) theory on scientific knowledge production and focus particularly on the criteria she offers as tools for evaluating communities<sup>12</sup>. By examining

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<sup>12</sup> Fernández Pinto (2014) offers another critical evaluation of the criteria with respect to commercialized research. Fernández Pinto's project is very similar to the one I pro-

two cases in current biomedical sciences, I show how her view can be complemented by taking notice of factors that can be called extra-scientific. Focusing only on scientists and their actions is not sufficient for achieving objectivity in Longino's sense.

In the article, I contrast Longino's view on the objectivity of science with two cases of commercialized research. First of all, I discuss the way in which financial conflicts of interests delayed the official recognition of the possible risks of selective serotonin reuptake inhibitors (SSRIs), drugs that are used for treating depression and several other ailments. According to Longino's theory, the objectivity should be evaluated at the community level and, thus, individual conflicts of interest are not directly banned. However, the example on SSRIs shows how individual-level conflicts of interest can constitute a threat to objectivity if they steer research activities in such a way that certain questions are left unexamined. Thus, individual-level conflicts of interest can hinder objectivity in Longino's sense unless there are mechanisms that control them.

The second case concerns the research agenda of psychiatry. I present arguments by Wyatt and Midkiff (2006) and Musschenga, Van der Steen and Ho (2010) who state that the research on mental illnesses has suffered because the financial interests of the sponsors of the studies have had an excessive impact on what questions are examined. The authors argue that the field is unduly steered towards searching for biological causes for disorders because sponsors are not interested in funding research that is unlikely to result in patentable outcomes. The possible effect of dietary and other environmental factors is neglected, which, in turn has been shown to be both theoretically and practically disadvantageous. (Wyatt & Midkiff, 2006; Musschenga, Van der Steen and Ho, 2010.) This example demonstrates how the development of critical points of view, which is a necessary condition for objectivity in Longino's sense, can be dependent on the availability of financial resources.

I draw two conclusions from the discussion on the influence that the allocation of research funding has on the critical mechanisms of science. Firstly, following Brown (2010), I argue that when the conditions for objective research practices are examined, the so-called discovery side of science should not be disregarded. In addition to being interested in how hypotheses and theories can be assessed in an unbiased manner, we ought to be interested in how they are generated. Funding arrangements and the potential conflicts of interests can have a harmful effect on which approaches have resources to develop and which questions are seen as worth investigating. As conducting research requires resources, the allocation of those resources essentially influences what questions can be investigated, and thus, what hypotheses can be tested. Secondly, how the diversity of points of view is realized in research communities is not

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pose in the article: According to her, Longino's theory "does not take into account recent changes in the organization of scientific research" (Fernández Pinto, 2014: 14) and in order to practice effective normative philosophy of science, we need to "start paying attention to the epistemological problems emerging from current science, using this input to revise or restructure our normative views" (Fernández Pinto, 2014: 16).

dependent only on the scientists: extra-scientific mechanisms have an impact on whether the critical scrutiny of stated views can take place as it should. Paying attention to these issues helps us to understand the “material, intellectual, and social context” of knowledge production, in other words, to practice that kind of philosophy of science Longino calls for. In this way I offer further support for Longino’s view of science as context-dependent.

Article III, Meta-analysis, ideals of objectivity, and the reliability of medical knowledge (forthcoming in *Science and Technology Studies*), investigates the ideals of scientific objectivity in discussions concerning medical knowledge and meta-analyses in particular. I begin by introducing an article by Jacob Stegenga (2011) who has argued that, contra to what its promoters claim, meta-analysis fails to be an objective method for amalgamating evidence because diverse judgments are needed in order to conduct a meta-analysis. By showing how there are multiple ways of choosing the primary studies, evaluating the validity of studies, and reporting methodological choices, Stegenga demonstrates how contradictory outcomes can be reached even if the same pool of primary evidence is used.

I argue that Stegenga’s discussion on the weaknesses and strengths of meta-analysis is based on the unattainable ideal of the so-called procedural view on objectivity (Douglas, 2004), according to which a method is objective if it can be conducted without judgments. In addition, by referring to a case of research on SSRIs and their potential side-effects, I show that this ideal could not help us evaluate some of the most problematic features of the current medical research. Even if meta-analyses did satisfy the conditions of being objective in the sense of excluding the need for judgments, some biases detrimental to medical research could not be removed. I demonstrate that the procedural ideal of objectivity fails to take into account the way in which the use of narrow range of evidence and publication bias, i.e., the nonappearance of papers showing null or negative results, threaten the applicability of the results of analyses.

The article contributes to the earlier discussion by offering a constructive criticism of Stegenga’s article. Doing so, it presents an analysis of how different ideals of objectivity can direct philosophical discussion on methodological questions. The article shows why the procedural view on objectivity is a bad ideal for evaluating medical research: It is unattainable in practice, and using the ideal of procedural objectivity as the criterion for evaluating medical research is not fruitful because it manages to capture only some of the features of the process that are needed for ensuring that the outcomes of research are not biased. The concept of objectivity that we adopt should be capable of recognizing that the requirements that a research process must fulfil in order to qualify as objective necessarily concern not only the researchers conducting the analyses but the institutional context of research. I argue that the social view on objectivity, which I defend in article I, better captures the problematic features of medical research. This view does not denounce all judgments as detrimental, but makes it possible to discern between acceptable and unacceptable judg-

ments. It also takes into account the possibility of publication bias and other systematic biases.

In article IV, *A Social Epistemological Inquiry into Biases in Journal Peer Review* (currently under review in *Perspectives on Science*), I examine journal peer review, one of the most central processes of academic practice. The use of peer review for allocating resources and making publication decisions is commonly justified by referring to the assumption that the criteria of “good research” are interpreted and applied impartially, i.e., that all reviewers would evaluate submissions in the same way (Lee et al., 2013). However, studies (e.g., Bornmann & Daniel, 2008) have shown that this is not the case: the inter-rater reliability in peer review is low. This has raised concerns about reviewers being biased in different ways.

The aim of the paper is to open a new, philosophical perspective on empirical studies on peer review practices. I examine what kind of individual-level biases can be taken to affect journal peer review processes and to analyze the topic from the point of view of social epistemology. The main question is; which factors have an influence on whether the published literature is of high quality? I argue that there can be systematic reasons for peer review not achieving its goals. More particularly, first of all, I show that the so-called ideal of concordant objectivity (Douglas, 2004), which seems to underlie much of empirical research on peer review, is an unfit ideal for guiding the evaluations of peer review practices: not every case of reviewer disagreement is detrimental, nor is consensus between reviewers a sign of an unbiased process. Second, I argue that the institutional context in which peer review operates can both amplify and weaken the effects of individual-level biases.

An important contribution of the paper is to argue for the need of studying peer review from a philosophical point of view. Even though peer review has been a popular subject in empirical studies (e.g., Cole, 1992; Lamont, 2009), there are only few philosophical analyses on the topic (e.g., Fitzpatrick, 2010; Lee, 2012; Lee et al., 2013). This lacuna in the philosophy of science literature is baffling as peer review is often mentioned as one of the mechanisms through which the critical discussions so essential to acquiring scientific knowledge are thought to operate. Examining empirical work on peer review renders it possible to make a new contribution contrasting philosophical theories with actual practices of science. The analysis of the practice of peer review highlights the social nature of scientific enquiry and the quest for objectivity, i.e., issues that are widely discussed in social epistemology and philosophy of science.

## 1.4 Discussion

With this study, I aim at contributing to fulfilling a gap in research. Issues that have been positioned in the domain of research ethics, such as conflicts of interests and publication practices have not, according to Douglas (2009: 44, n. 1), received sufficient attention in philosophy of science: philosophers having been

interested in the epistemic problems of science, such as the character of scientific laws and the logic of explanation, they have paid less attention to the significant role science and its results play in society and the epistemic questions that follow from this.

By showing how the institutional context of research activities, such as the funding structure and the ranking methods that are used for evaluating researchers, can either contribute to or hinder the production of unbiased results, the dissertation urges other philosophers to pay attention to the practices and effects of actors who are traditionally not considered in philosophy of science analyses. First of all, issues that are usually taken to be part of the expertise of ethicists can be epistemically significant and should be of interest to philosophers of science as well. For example, properly functioning conflicts of interest policies of scientific journals may not only guard against unethical practices but also can encourage practices that are epistemically beneficial because they protect the publicity of research. In addition, avoiding biases in peer review is important not only for ethical reasons, but also for achieving the epistemic goals of science. Further, philosophy of science analyses should not be limited to scrutinizing practices in the so-called context of justification. This study, particularly articles II and III, contributes to previous discussions by showing how the so-called discovery side of science needs to be evaluated as well when we look for ways of preventing biases in research. Scientists are not the only actors who are relevant with regards to the objectivity of science. This is because people working outside academia, for instance policy makers, are responsible for some of the decisions that affect the mechanisms that enable the detection of biases. Thus, academia is not self-sufficient in cultivating the conditions for objectivity.

By showing how the institutional context of research is relevant for objectivity, the study has demonstrated that philosophy of science can have relevance for science policy. Decisions concerning funding and building infrastructures for the dissemination of results should be evaluated from the perspective that considers how they influence the conditions of detecting different biases. Thus, the dissertation elucidates how philosophy of science can have significance also outside academia.

To conclude, I want to mention some possible topics for future research that emerge from this study. First of all, the mechanisms that influence the possibilities of critical interactions within and between scientific communities merit further study. In particular, a closer scrutiny of the peer review practices, both in journals and funding institutions, would be relevant for evaluating the preconditions for developing the diversity of perspectives that I have also taken as a necessary condition for objectivity following Helen Longino's analysis. Longino's (2002) view on the workings of local epistemologies could be fleshed out with elements taken from the work of Michèle Lamont (2009) who has investigated multidisciplinary expert panels that evaluate funding applications: Lamont's work describes a situation in which different "epistemological styles" (i.e., views on how knowledge should be built, whether theories can be proven and if so, which methods can be used for proving them) clash, but an agree-

ment on the strengths of applications should still be found. Thus, it could be used as a case for studying how Longino's idea of the benefits of critical discussions between different research cultures works in the context of the allocation of research funding.

Another possible research question with respect to the peer review practices would be examining in detail how different modes of journal peer review (i.e., blind, double-blind, open, hybrid systems) are sensitive to individual-level biases, and whether different systems are likely to produce different outcomes. Comparing peer review systems would be an exercise in which a social epistemological theory could demonstrate its practical significance: now that journal peer review in its common forms has received criticism and alternative systems have been suggested (e.g., Young, Ioannidis & Al-Ubaydli, 2008), evaluating the advantages of different arrangements is topical.

Further, it would be worthwhile to extend the theoretical ideas developed in this dissertation to the field of nutrition research. This field has attracted very little interest from philosophers of science. This is puzzling, taking into account the common discussions concerning obesity problems and the public interest that diet guidelines and advice raise.

The field abounds with pertinent questions with respect to which health policy concerns and cultural customs intersect with questions that are typical of traditional philosophy of science - For example, do the interests of industry have an impact on what kind of knowledge is produced? How should potential conflicts of interests be guarded? What kind of evidence should be regarded? By what means should the evidence be combined for producing dietary guidelines? How should cultural and social values be regarded when population-level guidelines are combined? A research project into these questions could rather naturally take the form of a case study comparing, for example, two or more dietary guidelines and the process of their production, such as the new Nordic Nutrition Recommendations, published in 2012, and the Brazilian dietary guidelines, released in 2014. One possible research question could be to analyse to what extent differences in dietary guidelines reflect different cultural conventions related to eating and how, or whether, such conventions affect the interpretation of scientific data.

## YHTEENVETO

Väitöskirjani tutkii tieteen objektiivisuutta keskittyen tarkastelemaan luotettavan tiedon tuottamisen ehtoja erityisesti niillä tieteenaloilla, joilta odotetaan sosiaalisesti relevantteja tuloksia. Esimerkkejä tällaisista aloista ovat lääketiede ja ilmastotiede. Tutkimus pyrkii määrittämään, mitä objektiivisuudella tarkoitetaan eri konteksteissa, tutkimaan objektiivisuuden toteutumisen ehtoja ja kriittisesti arvioimaan, mitkä tekijät vaikuttavat tutkimuksen objektiivisuuteen nykyisessä tiedemaailmassa.

Tutkimus koostuu johdantoluvusta sekä neljästä artikkelista. Kahdessa ensimmäisessä artikkelissa tarkastelen tutkimuksen objektiivisuutta yleisellä tasolla ja kehitän näkemystä sen saavuttamisen edellytyksistä. Kolmannessa artikkelissa tutkin objektiivisuuden käsitteen käyttötapoja lääketieteellisen tiedon tuottamisen kontekstissa keskittyen erityisesti meta-analyysien suorittamista koskevaan keskusteluun. Osoitan, ettei niin kutsutun proseduraalisen ideaalin tavoittelu riitä turvaamaan lääketieteellisen tiedon luotettavuutta. Neljännessä artikkelissa analysoin tieteellisten lehtien vertaisarviokäytäntöihin liittyviä näkemyksiä. Keskityn tarkastelemaan sitä, kuinka akateemisen maailman julkaisemiseen liittyvät tavat, esimerkiksi julkaisumäärien painotus tutkijoita arvioitaessa, vaikuttavat vertaisarviojärjestelmän toimintaan. Argumentoin, että vertaisarvioinnin tavoitteiden saavuttaminen on riippuvaista siitä kontekstista, jossa vertaisarvio tapahtuu.

Tutkimukseni tuloksena on ensinnäkin se, että tarkasteltaessa sosiaalisesti relevantteja tuloksia tuottavia tieteenaloja meidän on hyväksyttävä, ettei tutkimus voi olla riippumatonta sosiaalisista, eettisistä ja poliittisista arvoista. Toiseksi osoitan, että niin sanottu perinteinen näkemys, jonka mukaan tieteen objektiivisuus on riippuvaista tutkijoiden asenteesta ja kyvystä pysyä neutraaleina, ei kykene analysoimaan tai tarjoamaan vastauksia moniin nykyisiin, esimerkiksi kaupallisuuden mukanaan tuomiin, ongelmiin. Objektiivisuutta ei siis tule ymmärtää yksilötason ilmiönä, sillä yhteisön toiminta ja institutionaaliset rakenteet ovat sen saavuttamisen kannalta oleellisia. Kolmanneksi argumentoin, että objektiivisuutta tarkasteltaessa on kiinnitettävä huomiota tutkimuksen kontekstiin ja tekijöihin, joita ei perinteisesti ole pidetty objektiivisuuden saavuttamisen kannalta relevantteina. Esimerkiksi tiedepoliittiset ratkaisut, tutkimusrahoituksen jakoa koskevat päätökset ja julkaisukäytännöt voivat osaltaan joko poistaa objektiivisuutta uhkaavia tekijöitä tai vinouttaa tieteen tuloksia. Väitöskirjani osoittaa näin, että teoreettinen tieteenfilosofinen tutkimus voi olla relevanttia esimerkiksi käytännön tiedepolitiikan tai tutkimushallinnon kannalta.



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**ORIGINAL PAPERS**

**I**

**COMMERCIALIZATION OF RESEARCH AND THE QUEST  
FOR THE OBJECTIVITY OF SCIENCE**

by

Jukola, S, 2014

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## II

### LONGINO'S THEORY OF OBJECTIVITY AND COMMERCIALIZED RESEARCH

by

Jukola, S, 2015

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### **III**

## **META-ANALYSIS, IDEALS OF OBJECTIVITY, AND THE RELIABILITY OF MEDICAL KNOWLEDGE**

by

Jukola, S, [forthcoming] *Science and Technology Studies*

## IV

### A SOCIAL EPISTEMOLOGICAL INQUIRY INTO BIASES IN JOURNAL PEER REVIEW

by

Jukola, S, [under review]