Science and Values: A Philosophical Perspective on the Justifiability of Evidence based Policymaking

O. Çağlar Dede
Dede, .O. Ç.

*Science and Values: A philosophical perspective on the justifiability of evidence based policymaking*

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Science and Values:
A philosophical perspective on the justifiability of evidence based policymaking

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Een filosofisch perspectief op de rechtvaardiging van evidence based policy

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by

Osman Çağlar Dede
born in Istanbul, Turkey
Doctoral Committee:

Promotor: Prof. dr. J.J. Vromen

Other members: Prof. dr. R.E. Backhouse
Prof. dr. I.P. van Staveren
Prof. dr. G. Irzik

Copromotor: Dr. H.C.K. Heilmann
To Deniz, Sema, Seda, Rabia and Fikret
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Preface

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Introduction
Science is widely regarded as the most reliable epistemic source of providing knowledge about the world. Policymakers intend to make purposeful changes in the world. The practice of policymakers relying on scientific experts to make informed decisions about which policies to implement is called Evidence Based Policymaking. This thesis provides a perspective from the philosophy of science in order to discuss the justifiability of Evidence Based Policymaking (EBP) with respect to broadly democratic and liberal values.

Justifying EBP with broadly democratic and liberal values entails that the practices of EBP promote, or at least are in harmony with, values such as democratic governance and enhancement of people’s freedom and autonomy. Identifying the conditions under which practices of EBP meet such desiderata minimally requires an understanding of how sciences and scientific experts are instrumental in realizing the public’s values, needs, interests, and pursuit of freedom. In order to approach this project, the thesis adopts a philosophical perspective to conceptualize how sciences are supposed to be guided by or promote society’s values, needs, and interests. Specifically, it adopts a perspective from the philosophy of science that focuses on the relationship between science and (societal) values.

The kind of philosophy of science perspective on “values in science” that this thesis adopts here has two overarching pursuits relevant for the project of the thesis. Firstly, it seeks to inform the debates about which values and non-epistemic considerations are supposed to inform scientific research. For instance, it discusses the proper sources/owners of the non-epistemic desiderata that inform scientific research and the proper social mechanisms to identify these non-epistemic desiderata (e.g. Kourany, 2010; Kitcher, 2011). Second, it offers theories of the non-epistemic values’ proper roles in scientific reasoning and research that specify how their involvement in science does not unduly compromise the epistemic pursuits of science. The values-in-science perspective thereby seeks to balance the instrumental value of science (i.e., its use to pursue certain societal projects and values) with its epistemic authority (i.e., its objectivity, non-dogmatism, and reliability). The thesis advances an understanding of EBP from the perspective of the values in science by addressing issues that come to
the fore when EBP is acknowledged as a value-laden practice of informed decision-making.

The thesis is structured in two parts which consist of self-standing research papers addressing the philosophical issues pertaining to both the theme of the respective part and the overall thesis. In Part I (entitled *Values in Science: Beyond the “Value Free Ideal” of Science*), the thesis engages with the philosophy of science literature on the values in science, illuminates how this literature relates to EBP, and discusses how the contemporary philosophical approaches describe the roles of values in scientific reasoning and how these values may change in relation to the social contexts within which they arise. In Part II (entitled *Philosophy of Evidence Based Policy from the Perspective of Values in Science*), the thesis focuses on the issues regarding the justifiability of EBP from the values-in-science perspective.

In the remainder of the introduction, I will describe and motivate the two parts of the thesis in further detail and offer a preview of the chapters constituting the thesis.

**Part I: Values in Science: Beyond the “Value Free Ideal” of Science**

The traditional and the most commonsensical view on the values in science is the so-called “value free ideal of science” (VFI) (see Douglas, 2009 for an extensive review). The VFI suggests that scientists should not consider non-epistemic aspects of their research (e.g., values, societal consequences) when reaching scientific conclusions. Non-epistemic considerations could play plausible roles in science in “the context of discovery” (e.g., when choosing research questions and identifying which hypotheses deserve to be assessed scientifically), but not in “the context of justification.” The latter includes methodological steps such as data characterization and assessment of claims/hypotheses based on evidence. In the face of the problems such as evidential uncertainty or underdetermination, values could play an acceptable role in making inferences, but these values are supposed by the proponents of the VFI to be “epistemic values” such as scope, simplicity, unificatory power, consistency, and
fruitfulness (as formulated in Kuhn, 1977). While epistemic values could legitimately shape scientists’ conclusions, the involvement of the non-epistemic values such as ethical and moral values may corrupt the core phases of scientific thinking, or so the proponents of the VFI argue. As this brief description of the VFI suggests, the VFI provides a clear view of the proper limits to science’s promotion of the public’s certain values, needs, and interests.

The VFI is not just an account of values’ legitimacy in science. The VFI also motivates a view on science’s place in a democratic society. According to this view, science best contributes to society by seeking objective knowledge independently from societal concerns and assembling facts to inform normative policy deliberations. This view not only emphasizes the pragmatic significance of the public’s reliance on scientific experts to make the right policy decisions but also promotes the idea that the judgments regarding normative matters (e.g., what is acceptable, what values, and whose should be promoted) should be made by democratically legitimate political actors and be engaged in the political domain (see Betz, 2013 for a review). Accordingly, the VFI also offers an approach to how scientists should properly identify the social values or needs that guide their research pursuits.

The VFI is a cogent regulative ideal, especially in the context where we reflect on how sciences contribute to policymaking, even though it may otherwise be an unattainable ideal in the actual practices of science. Indeed, the VFI has represented the dominant philosophical approach to the science-policy interface during the 20th century (Douglas, 2009). Nevertheless, the normative appeal of the VFI in the context of informed decision making has been challenged by prominent philosophers of science in recent decades (e.g. Anderson, 2004; Douglas, 2009, Kitcher, 2011). The main argument against the VFI emphasizes that science does not provide the decisionmakers with certainty. Even though science is the most reliable epistemic source into the state of affairs in the world (and how to make predictable changes in it), scientific judgments always face the risk of being wrong, and they are supposed to change over time through the discovery of new pieces of evidence and theories. Such non-dogmatic character of science is indeed one of the most important reasons
behind science’s epistemic success, hence its reliability for making decisions (Douglas, 2009, Chapter 1). In the face of inductive risks, scientists who inform policy decisions are in a position to make morally consequential judgments (see Douglas 2009 for an extensive argument from the “inductive risk” to the VFI and Biddle & Kukla, 2017 for a review of “epistemic risks” that generalizes the arguments regarding inductive risk in science). For instance, when available evidence does not fully confirm a policy-relevant hypothesis, scientists could reach a judgment about the acceptability of the hypothesis only after they make a choice on what kind of evidence is relevant and how much of evidence is sufficient to accept or reject the hypothesis. Making this choice requires considering the non-epistemic context of the decision (e.g., how severe the moral and social consequences of the decision is, whether there is more time to work further on the question). Suppose the judgment is going to justify an urgent policy decision, and the consequences of rejecting the hypothesis are too severe and morally unacceptable. In that case, one may plausibly want to accept the hypothesis based on relatively lower evidential standards (Douglas, 2000). Consequently, in the face of such less-than-ideal circumstances for scientifically informed decision making, presupposing that the scientific experts provide the society with value free and universally true statements does not unambiguously lend itself to the intelligent use of science for policy making.

In the light of these kinds of philosophical considerations, it seems plausible to reject or at least transcend the VFI. However, transcending the VFI and adopting a less ideal (and a more practice-oriented) conception of science in society raises two main philosophical challenges for a discussion on the justifiability of the EBP in a democratic context. Firstly, the observation that scientific input for policymaking is uncertain and laden with (or incomplete without) making non-epistemic value judgments seems to challenge the very idea of the public’s deference to scientific experts to make informed decisions. From the perspective of the so-called “science wars,” for instance, going beyond the VFI may be understood as a challenge to the epistemic authority of science, but that is not a premise upon which one could justify the practices of EBP. By definition, the latter rests on the idea that scientific evidence should guide policy decisions because science is the most reliable epistemic resource
to make informed decisions and the value-ladenness of science may corrupt the reliability of science (e.g. makes it more like politics). Nevertheless, the philosophical approaches to values-in-science do not aim to challenge the epistemic authority of science for decision-making when they question the plausibility of the VFI. Instead, just like the VFI, they aim to define the proper roles of values in science (and for EBP) such that scientific judgments are epistemically authoritative (i.e., they are objective in a meaningful sense and reliable and trustworthy).

The second challenge of the alternative accounts of values in science concerns the appropriate content and the sources of the value judgments that inform scientific research. The scientific input for decision-making may presuppose certain value judgments, but it is unclear where these value judgments (should) come from. Yet, in a democratic context, it is essential to identify who the proper owners of the values that inform scientific research are and how the content of value judgments are determined, or why they change over time. The sources and the contents of the values that direct scientific research should be identifiable and transparent so that the society and the researchers could critically evaluate them and explore alternative social mechanisms, procedures, principles, and institutions for determining the values that shape EBP.

These two challenges are conceptualized in the thesis through a distinction between the legitimacy problem of values and the authority problem of values in science. The former problem is about defining the legitimate roles value presuppositions play in making and communicating value-laden scientific judgments. The latter problem is about defining the cogent sources and the content of value presuppositions.

The first part of the thesis is dedicated to introducing the particular philosophical perspective (i.e., the values in science perspective) to approach and understand the justifiability of the EBP in a democratic context. It introduces the values in science perspective to approach issues in EBP, reviews the philosophical landscape on the subject, and identifies the challenges and potentials of the contemporary philosophical approaches to the values in science in the light of the debate on the VFI (see below for a preview of Chapter 1). It also investigates how our best accounts of values can
apply to describe the values that shape paradigmatic evidence based policy practices (specifically through which social mechanisms and procedures scientists’ evaluations of the inductive risks change over time) (see below for a preview of Chapter 2).

Part 2: Philosophy of evidence based policy from the perspective of values in science

The second part of the thesis is devoted to analyzing philosophical issues in Evidence Based Policymaking (EBP) from the perspective of the values in science. Here, I briefly discuss how the values-in-science perspective contributes to the philosophical approaches to EBP and describe the philosophical issues the thesis addresses from this perspective.

The extant philosophical literature on the EBP mainly focuses on the question of “what works” in the light of a critical analysis of how scientific evidence should be interpreted and used in the right way for policymaking purposes (e.g., Cartwright & Hardie, 2012). Philosophers often conceptualize this as discovering causal relations applicable to policymaking and adequately applying them through extrapolation (i.e., “hunting causes and using them” in Cartwright’s terms). The focus of this philosophical approach, therefore, is on the “external validity” of the scientific findings and the kind of evidence required for making externally valid (policy warranting) inferences. In the past decade, philosophers extensively questioned the limitations of the dominant methods used in the practices of evidence based medicine and social policies (see, for instance, Deaton & Cartwright, 2018 for an exhaustive analysis of the limitations of the randomized controlled trials for justifying policy claims). This philosophical perspective on EBP helps us understand how to produce and use scientific evidence more effectively (so that evidence based policies work).

The thesis complements and goes beyond the extant philosophical approach to EBP by offering an understanding of how to produce and use scientific evidence in ways that are more responsive to the values, interests, and needs of the members of the
public who are at the receiving end of the evidence based policies. This change of perspective shifts the focus of the philosophical discussion from the issue of “how to properly apply science for policy making purposes” to that of “how to properly inform and guide science for policy making purposes.”

The second part of the thesis is dedicated to understand and address some of the philosophical issues that come to the fore when one asks “how to properly inform and guide science for policy making purposes.” It discusses how to select the appropriate methods for evidence based policy analysis to be responsive to specific non-epistemic purposes (see below for a preview of Chapter 3). It reflects on how to treat value-laden scientific judgments in the context of informed decision-making such that these judgments are considered objective, reliable, trustworthy, and congruent with the requirements of democratic policymaking (see below for a preview of Chapter 4). Finally, the second part of the thesis explores how value judgments that inform EBP can be properly identified. It applies philosophical insights on the determination of values that guide scientific research, and discusses how the philosophical perspective offered can advance the normative justifiability of evidence based policy practices (see below for a preview of Chapter 5).

Preview of the chapters

Five chapters of the thesis are written as self-standing research articles and meant as timely contributions to the relevant specialized discussions in philosophy of science. The arguments developed in each chapter are informed by socially relevant and philosophically interesting cases, historical episodes, or illustrative examples drawn from the scientific practice. This also reflects the thesis’ ambition to contribute to the practice-oriented and socially engaging approaches to philosophy of science. In the following, the chapters are previewed for the reader who is conversant on the relevant literature.
In the first part of the dissertation (entitled “Values in Science: Beyond the ‘Value Free Ideal of Science’”), the thesis investigates the consequences of abandoning the value free ideal of science. Chapter 1 reviews the arguments for and against the value free ideal of science and illustrates these arguments by reference to the example of regulatory toxicology. The chapter describes the risks of abandoning the value free ideal. It argues that the prominent extant philosophical approaches that transcend the value free ideal of science do not reject but preserve the meta-values about science that the proponents of the value free ideal deem important. These values include the epistemic authority of science, scientific impartiality, and the avoidance of non-democratic (technocratic) policymaking.

Chapter 2 reflects on how the contemporary philosophical frameworks that advance the most prominent value-laden conceptions of science describe and evaluate the actual episodes of scientific practice. Specifically, it focuses on Heather Douglas’s inductive risk framework and discusses how the framework applies to a socially relevant and philosophically significant historical episode of regulatory toxicology. The chapter offers an empirically informed description of how regulatory toxicologists make methodological judgments about what the relevant evidence is for their purposes and choose between alternative evidence-gathering methods. It highlights some theoretically interesting aspects of this case (e.g., the roles of social and institutional processes in changing toxicologists’ perceptions of inductive risks) that are puzzling or novel from the perspective of the inductive risk framework. It shows how the inductive risk framework can be extended to reflect on these aspects of the case.

In the second part, (entitled “Philosophy of Evidence Based Policy from the Perspective of Values in Science”), the thesis reflects on the philosophy of evidence based policy making from the perspective of the values in science).

Chapter 3 investigates how an instance of behavioral interventions (Incentivized Smoking Cessation Policies) is evaluated by scientists. It demonstrates that the practice of evaluating these policies is attended by a plurality of researchers making use of different evidence gathering methods, representing different scientific
disciplines and policy perspectives regarding smoking as a public health issue. It argues that an empirical evaluation of whether the behavioral smoking policies are effective in reducing smoking-related health inequities (which is cited as the most pressing desideratum for many smoking control policies in Europe and the UK) requires integrating different methods (e.g., randomized controlled trials with qualitative methods used in social epidemiology). It discusses the implications of this pluralist perspective for the philosophical discussions over the evidential requirements of behavioral interventions.

**Chapter 4** focuses on the special cases where the claims made by social scientists include concepts that rest on unwarranted value presuppositions. Anna Alexandrova defines such claims as "mixed claims" because they mix normative content with empirical ones. Mixed claims are especially controversial when they are used for policymaking. Unless they are properly scrutinized, they implicitly impose some undetectably value-laden discursive perspectives on the policy deliberation. The chapter analyzes the contrast between Nagel and Alexandrova and offers a reconciliation that is argued to be agreeable to the both sides of the controversy. Specifically, based on the distinction between the authority and the legitimacy problem of values, the chapter demonstrates that Alexandrova’s 2017 critique of Nagel (1961) is confined to the authority problem of values and leaves Nagelian conditionalization of mixed claims intact qua a strategy that addresses the legitimacy problem of values. It also shows that the contemporary Neo-Nagelian accounts (such as E. Anderson’s and H. Douglas’s accounts of objectivity) support the reconciliation the chapter proposes, and are thus compatible with both Nagel’s and Alexandrova’s insights regarding mixed claims.

**Chapter 5** focuses on an important instance of evidence based behavioral policies; nudges. Nudge policies are prominent and extensively applied examples of evidence based behavioral policies and raise significant ethical and political problems relevant to the philosophical issues explored in the thesis. Drawing on the literature on the democratization of science (particularly on Philip Kitcher’s approach), the chapter proposes a conception of nudging, called well ordered nudge, that seeks justification
of these kinds of policies through deliberative democratic practices. It thereby offers a particular approach to address the authority problem of values in science. It seeks to identify practical and conceptual implications of this approach for an important instance of evidence based policy. The chapter compares the well ordered conception of nudges with the extant conceptions of nudges and elaborates on how the philosophical perspective offered can advance the ongoing discussions on the democratic justifiability of nudges and nudge-like evidence based policies.
References


Part I

Values in Science: Beyond the “Value Free Ideal” of Science
Chapter 1

Debating the Value Free Ideal of Science: the case of regulatory toxicology

1.1. Introduction

To be able to answer complex and technical questions, we defer to the judgment of scientific experts. And, doing so is wise. We need scientists’ advice to be able to take high-stake policy actions. More generally, complicated and morally consequential decisions should better be relied on empirical evidence and scientifically supported arguments rather than uninformed opinions, doxastic preferences of a handful of decision-makers, or blind ideological commitments.

As the problems of our advanced post-industrial societies and globalized affairs are becoming more complicated, common governance and regulation mechanisms are ever more dependent on scientific advisory. But, with great power comes great responsibility. Further reliance on scientific advisory to manage the current affairs leads to more pressure on scientific experts. Concerned citizens and students of science expect ever more from scientific experts. We demand scientists to be as unbiased as possible in their assessments, but we also wish them to be responsive to,
and sometimes, protective of our values and ideals when they inform policies that will affect us. We expect scientific experts to be specialists. Yet, we also ask them to consider multiple dimensions of a decision problem and to take the interests of all the relevant stakeholders into account in doing so.

All these growing expectations concerning scientific advisers are in tension with our commonsensical understanding of scientists. The conventional image of a scientific expert is someone who is a specialist, policy-neutral and autonomous from societal affairs. If we need to abandon this old image of scientific experts and transcend it, we have to build a new conception of scientific expertise on solid foundations. These foundations will have to make sure that our reliance on expert judgments in the management of the current affairs is not spurious. We have to carefully and systematically ponder the grounds on which scientific experts can fulfil our pressing expectations. Social studies of scientific expertise provide us with helpful insights towards reaching this goal by conceptualizing the accountability of scientific experts and examine the cutting-edge cases where the role of scientific advisors is salient in policymaking. The present chapter contributes to these scholarship by offering a perspective from the philosophy of science.

In the following, I will review the debate about the most prominent account of science in society developed by the philosophers of science in the 20th century, which underpins the commonsensical image of scientists depicted above. This account is known as the Value Free Ideal of Science. The Value Free Ideal (VFI) suggests that non-epistemic (moral, political, environmental, economic and similar) values, commitments and aspirations have no legitimate role to play in scientific judgment-formation. This standard is quite stringent as it substantially limits what we could reasonably expect from scientific experts in the context of regulation and law. Yet, prominent contemporary philosophers of science have also proposed to abandon, or else amend, the VFI. I will introduce some of the key arguments for and against the VFI and discuss the implications of these arguments for conceptualizing the accountability of scientific experts in the context of evidence-based policy and
Debating the Value Free Ideal of Science

regulation. To make the discussion more accessible to the socio-legal scholars, I will illustrate these arguments by referring to regulatory toxicology and regulation of agricultural pesticides.

My aim is to demonstrate that the recent philosophical insights that contest the VFI do not debunk but rather revive and reconstruct the value of scientific expertise. Specifically, I argue that philosophers of science such as Heather Douglas, Philip Kitcher and Helen Longino appreciate and share the main worries of the proponents of the VFI, such as the importance of objectivity and neutrality, the need for rational argumentation in policy-making and the avoidance of technocracy. Nevertheless, they disagree with the proponents of the VFI about the premise that a genuine reliance on scientific expertise is sustainable within the boundaries the VFI defines. In the face of this problem, those philosophers who propose to transcend the VFI work towards developing an understanding of science as a reliable epistemic source for making complex and challenging (policy) decisions about the pressing and history-old problems. This emerging conception of science is not only responsive to urgent and significant non-epistemic values but also epistemically robust. Accordingly, I suggest, what contemporary philosophers of science have to say about the relationship between science and values is vital to develop any projects that attempt to critically assess the accountability of scientific experts in the contemporary contexts of law, governance and decision-making. My task in this chapter is to offer the reader a survey of some of the key recent philosophical insights for those socio-legal scholars who seek to strike the right balance between epistemic and non-epistemic dimensions of scientific expertise in developing a conception of the ‘accountability of experts’ in governance and law.

The rest of the chapter is structured as follows. In Section 2, I introduce an example concerning toxicological assessments of agricultural pesticides that I will use throughout the chapter. Based on this example, I review a number of contexts where scientific experts make non-epistemic value judgments in their assessments. In these contexts, the permeation of value judgments into their analysis is seen uncontroversial
by the philosophers of science, regardless of their position on the debate about the VFI. In Section 3, I describe the contexts where the inclusion of non-epistemic value judgments in science is impermissible according to the proponents of the VFI and discuss the main arguments for and against the VFI. In Section 4, I illustrate some of the key concepts in the contemporary philosophy of science that challenge the VFI and transcend it by allowing non-epistemic values a more expanded role to play in scientific judgment than the VFI permits. In Section 5, I conclude by elaborating on how the debate on the VFI and the recent insights from philosophy of science contribute to the socio-legal scholars’ attempts to reconceptualize the accountability of scientific experts in law and governance.

1.2. Revisiting the Value Free Ideal of Science when examining the role of regulatory toxicologists in protecting environmental safety

1.2.1. The safety of pesticides and toxicological expertise

Consider environmental safety regulations in agriculture. Various kinds of pesticides are used for maximizing the productive capacities of the agricultural businesses, from which we all benefit as consumers. The overuse of toxic chemicals has adverse consequences for the environment and public health. The safety of the agricultural pesticides is, therefore, systematically regulated by institutions such as the US Environmental Protection Agency (EPA) and EFSA (European Food and Safety Authority). The processes of decision making in the regulatory institutions are far from being simple as it is not always a clear-cut judgment to decide whether a type of pesticide should be regulated more or less stringently. Making consequential regulatory decisions requires an estimation of relative costs and benefits of a particular regulatory action over a number of desiderata such as ecological safety, economic efficiency and the public interest.
It is, therefore, useful to conceive regulatory decision making about a type of pesticide as a multidimensional balancing process that goes beyond the binary judgment of banning or allowing the industrial use of a toxic substance. For instance, questions of the following type need to be answered, and the pertinent trade-offs must be drawn by the regulatory decision makers. What is the threshold (expressed in terms of dosages) on which a pesticide is safe to use in agricultural production? To which produces should the pesticide in question be applied to? Does the adversity of the pesticide depend on its proper application? If so, how should the substance be applied by its users? What is the severity of the costs on health and the environment? What are the consequences of imposing more or less stringent regulatory measures on the agricultural sector and the producers of the pesticides?

Answering such questions is likely to demand some technical knowledge and skills. Accordingly, regulators must rely on toxicological risk assessment to answer them as accurately as possible. Toxicological experts who perform required testing and who run the pertinent risk models, therefore, play some key roles in the making of regulatory decisions for environmental safety. Overall, it is desirable that the environmental safety regulations are evidence-based and informed by science.

The need to examine the accountability of scientific experts in regulatory decision-making processes arises in controversial cases. Regulatory mechanisms sometimes fail to ensure the safety of people’s health and the environment and even lead to disastrous consequences. For instance, the use of clothianidin in agricultural production has led to the death of millions of honey bees in Germany (Benjamin, 2008). Such cases are controversial not only because of the high environmental costs involved but also due to the severe uncertainties about what counts as the right course of regulatory action. In the case of the clothianidin, the details and the stringency of regulations have changed across time, and only after five years, its outdoor uses have been suspended in the EU. The gaps and incompleteness of the scientific advice over

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1 Clothianidin is a member of the class of insecticides known as neonicotinoids. Neonicotinoids are chemically similar to nicotine but more effective as a pesticide. It is applied on plants and attacks the nervous system of the targeted insects. It is first developed by Bayer BV.
the malignancy of neonicotinoids for the environment (the health of bees in particular) seem to play some role in the progression of the debate over the suspension of the neonicotinoids such as clothianidin (McDonald-Gibson, 2013).

In the face of such environmental failures caused by mis-regulation or under-regulation, it is appealing to question the role and the responsibilities of toxicological experts, and the regulatory scientists in general. More specifically, one feels compelled to ask whether regulatory toxicologists could have done better in preventing the environmental harms caused by the industrial use of the clothianidin. Perhaps, in the face of evidential gaps and uncertainties, the bits of advice for regulation could have been produced and communicated in a more precautionary manner, prioritizing the environmental safety over other concerns. For instance, regulatory toxicologists could have determined the parameters of their risk assessment models in a way that is more protective of environmental health.

But, to what extent is it justifiable to expect toxicological experts to be protective of values such as environmental safety in conducting their assessments? Should scientific advisors qua scientists endorse non-epistemic values or commitments and then let those values influence their analyses and conclusions? I suggest that philosophers of science have a lot of useful insights about these kinds of questions, and the VFI is a good place to start engaging with the philosophical perspectives on issues pertaining to the accountability of experts in governance and regulation.

The VFI is a deeply rooted analytical-philosophical view of how science relates to society. It defines clear boundaries under which non-epistemic values such as environmental health can legitimately influence scientific judgment. Specifically, according to the VFI, value judgments such as a commitment to maximizing environmental safety, may play some permissible roles in conducting scientific research. However, the VFI states, non-epistemic values should not influence scientists’ inferences from the evidence, hence the results of their analysis (see, for instance, McMullin, 1982). In the context of science-based regulation, a proponent of the VFI would suggest that expecting scientific experts to let certain value
considerations to influence their results undermines the *raison d’être* of expert advice for making regulatory decisions. So, this view goes, the main reason for seeking scientific advice is to be informed about the facts of the world that are relevant for making morally consequential decisions; scientific advisors qua scientists are *not* regulators; hence scientists’ judgments should not reflect any value judgments.

Accordingly, the VFI provides us with a clear benchmark that defines principles under which scientific experts can legitimately employ values such as environmental safety in their analysis. This ideal can, therefore, help us examine what we could justifiably expect from toxicologists to do in order to prevent regulatory failures, which may be partly made based on their judgments. I will now review a bit more closely what boundaries the VFI draws for the inclusion of values in science. We will then ask ourselves how those boundaries are justifiable and engage with the philosophical debates over whether and how the VFI should be transcended.

### 1.2.2. Values in regulatory toxicology according to the Value Free Ideal

In the face of regulatory failures in science-based regulation of agricultural production (manifested in the cases such as the regulation of clothianidin), we are interested in knowing whether non-epistemic values and commitments, such as maximizing the interest of public health or the environment, should have a more expanded role to play in the formation of scientific judgments made for regulatory action. I have suggested so far that the philosophical *Value Free Ideal of Science* provides us with a sound starting point to determine plausible ways in which values can play more or less expanded roles in science. My aim now is to offer a clear understanding of the principles of the VFI so as to proceed with a structured discussion about how regulatory toxicologists should employ value judgments in their analysis.

#### 1.2.2.1. Scientific integrity and fraud

It is agreed by all the philosophers of science, irrespective of their position in the VFI debate, that scientists should avoid fraud. Scientific integrity is the most commonly
accepted (though also sometimes violated) moral principle in scientific practice. For instance, it is strictly forbidden to make up data for one’s convenience. Similarly, scientists are not expected to cherry-pick a piece of evidence from a wide range of available evidential sources in a way that would favour the results they wish to obtain through their analysis. When regulatory toxicologists engage in these kinds of practices, they commit to *scientific fraud*.

Scientific experts who engage in fraudulent scientific practices causally contribute to the regulatory failures when the policy outcomes partly result from their untruthful judgments. The corruption of policy-informing experts is a genuine issue that needs to be discussed thoroughly. Indeed, many commonly known failures of expert-based regulatory advisory in environmental safety result from the violation of basic scientific integrity (see, for instance, Lacey, 2005; Gøtzsche, 2017). As extensively documented by the socio-legal scholars, the problem of fraud is most commonly observed among the so-called ‘industry scientists’ who are pressured to produce research results that serve the interests of large, profit-driven pharmaceutical and biomedical companies. The sources and the dynamics of indecent scientific practices need to be carefully studied by the socio-legal scholars and philosophers of science (see Bright, 2017 for a philosophical discussion).

### 1.2.2.2. Non-epistemic values in the selection of research questions and tools

The VFI does not suggest that non-epistemic values have no legitimate function in science. There are two contexts where value judgments are needed, and it is legitimate for scientists to employ value judgments in their reasoning, according to the VFI. Firstly, the context in which scientists choose a research question to investigate and decide how to investigate that question. It is permissible that scientists attend to certain subjective value judgments, commitments or interests when they make decisions in such initial phases of their inquiry.
For instance, a toxicologist may want to study the effects of pesticides on the population of bees because she might deem it valuable to conserve the bee population’s health. Her employment of values in this way does not conflict with the VFI, as values (ecological safety of bees) do not determine the results of her inquiry. Similarly, she can attend to certain (communal or personal) value considerations in determining the methods through which she will conduct her investigation. She may, for example, want to work only with molecular-genetic tools instead of exposed animal bio-assays because she may not prefer to harm or kill animals to conduct her scientific research. Again, the VFI does allow a role for non-epistemic values in this particular sense.

1.2.2.3. No values in scientific inference

The VFI does limit the permeation of values into science when value judgments influence results obtained by an investigation or when values change scientists’ judgments. Specifically, values should not influence certain methodological decisions in science. These are decisions such as how evidence is gathered, how hypotheses are justified and accepted, and how the results are interpreted. These methodological contexts are referred to by the proponents of the VFI as the core stages of the scientific reasoning; also known as the context of justification. The context of justification is commonly contrasted with the ‘context of discovery’ where the research questions and methods are chosen, in which values play a permissible role. According to the VFI, the context of justification should, in principle, be free from non-epistemic value considerations.

So, for instance, regulatory toxicologists could endorse the value of protecting environmental safety and choose to examine those hypotheses that would be protective of the environment (if their judgments will then be used for justifying regulatory processes). The regulatory toxicologists’ endorsement of a non-epistemic value in this particular sense complies with the standard of the VFI. However, a commitment to protect environmental safety should not influence how the same
toxicologists justify the hypotheses they are assessing. In other words, values should not determine which judgments are accepted.

Moreover, scientific experts’ role *qua scientist* must be distinguished from scientific experts *qua policy advisors* (see Steele, 2012; de Melo-Martín and Intemann, 2016 for elaborations). Qua policy advisors, scientists might make use of values in determining how they communicate their results (see John, 2015 for a philosophical analysis of this context). For instance, they could permissibly emphasize certain aspects of their results to motivate a particular regulatory response. However, qua scientists, policy advisors should not allow non-epistemic value judgments to influence how they obtain their results. In short, values should be ignored in scientific interference, according to the VFI.

### 1.2.2.4. Except epistemic values

The so-called epistemic values are exceptional to the principle discussed above. The proponents of the VFI find it benign when various epistemic values of a hypothesis, such as its simplicity, explanatory or predictive power, accuracy, robustness and fruitfulness are used by scientists to assess that hypothesis. Thomas Kuhn, in particular, thought that scientific reasoning or hypothesis-confirmation should better be conceptualized as a process that appeals to some shared standards of epistemic values (which may differ across different communities of scientists) (Kuhn, 1977). Since Kuhn, it is commonly thought that the involvement of *epistemic* values in scientific inference does not harm the justification of scientific results or the objectivity of scientific judgment in general. The controversy has been over the status of the so-called ‘non-epistemic values’ in scientific justification. Kuhn’s ideas about epistemic values in relation to scientific disagreement and objectivity were thought to be protective of the VFI (Douglas 2009, Chapter 3).

The VFI thereby provides us with a clear benchmark regarding how far we should expect scientific experts to be protective of certain non-epistemic values in producing their judgments for regulatory procedures. The principles of the VFI discussed so far
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can be summarized as follows. Non-epistemic values such as environmental safety are permissible in scientific experts’ reasoning only in certain contexts that are considered to be separate from the core stages of science, such as collecting evidence and making inferences from them. Only the so-called epistemic values can influence scientists’ judgments over the confirmation of a hypothesis.

These principles of the VFI are supposed to be protective of scientific objectivity, hence scientific authority for informing the processes of governance and regulation. Given this conceptual background, we now need to ask ourselves: should the socio-legal scholars adopt or abandon the VFI? In the context of specific cases such as the assessment of clothianidin, what are the limits of scientific activism of the sort that is protective of values such as environmental safety and public health? I will now review arguments for and against the VFI in order to motivate relevant philosophical insights regarding whether and how the VFI could be transcended.

1.3. Debating the Value Free Ideal of Science

As I have discussed in Section 2, the VFI does not leave room for any policy desiderata to play a detrimental role during the formation of expert advice for regulatory processes. This implies that if the socio-legal scholars endorse the VFI as a benchmark model of how scientific experts make judgments about policy-relevant issues, they should not expect from scientific experts to adjust their findings or judgments in order to serve for the protection of certain values. For instance, toxicologists assessing clothianidin should not be expected to produce results that are protective of the financial interests of a patent-holder company or the economic efficiency of the agricultural industry. Similarly, if the VFI is endorsed, the dangers of clothianidin-use for ecological health should not influence the expert’s assessment of clothianidin’s toxicity. However, the VFI is not free from criticisms and challenges. At least starting from the 1980s, philosophers of science have questioned the descriptive adequacy and the normative appeal of the VFI. I will now review some of the influential arguments that are used for contesting or defending the VFI.
1.3.1. **Contesting the Value Free Ideal**

An important type of challenge against the VFI is directed at the attainability of it as an ideal (Douglas, 2009). Specifically, these criticisms are about the nature of scientific reasoning and methods and the core concepts of the scientific method, such as evidence, theories and explanation. Helen Longino, for instance, argues that inferences from the evidence to hypothesis often rely on background assumptions which may encode ontological assumptions about the world, value presuppositions about what deserves to be examined, or methodological assumptions about what types of evidence are relevant to given hypotheses (Longino, 1990). The problem, according to Longino, is that scientific communities’ shared background assumptions may often be implicit, insufficiently examined, or not be subjected to critical scrutiny of alternative perspectives. This work has later motivated more recent studies in philosophy of science, which conceive of scientists as members of social groups and knowledge-production as a social activity characterized by a plurality of epistemic communities with varying values, interests and methods to pursue research. Given such an understanding of scientific judgment that is undetermined by evidence and laden with disagreements about background presuppositions, the VFI seems to be an unattainable ideal, if not an illusion (Kincaid, Dupré, & Wylie, 2009). In Section 4, I will also discuss how philosophers such as Longino make constructive suggestions about scientific objectivity while holding a descriptive view of science as a deeply pluralistic social practice.

However, the descriptive problems of the VFI are not of direct relevance to the problems examined in this edited volume. We are not interested in grand questions regarding the nature of science, scientific knowledge and objectivity. We are interested in the rules and principles that guide scientific experts when they need to attend to value judgments in their analysis for regulatory purposes. The critiques of the VFI that focus on the normative appeal of its principles are of more direct relevance to discuss the accountability of regulatory expertise. Some philosophers of science, such as Heather Douglas – whom we can see as a contemporary successor of Richard Rudner’s approach to scientific reasoning – have argued against the VFI as
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an ideal, irrespective of its feasibility (Rudner, 1953; Douglas, 2000, 2009). Heather Douglas’ work has been particularly influential in recent years in pushing the argument against the VFI forward (Douglas, 2009).

Douglas argues against the normative appeal of the VFI on two grounds, which, when taken together, leads her to reject the VFI normatively. Firstly, Douglas emphasizes that scientific judgment often involves the risk of being wrong. That is to say, when a scientist makes an inference about a hypothesis, the available evidence does not fully confirm the hypothesis being true or false. And so, she argues, scientific confirmation actually takes place in degrees and commits to an error akin to the errors in basic statistical inferences: a false positive judgment or a false negative one (i.e. accepting $p$, when $p$ is actually false; or rejecting $p$ when $p$ is actually true). This idea is known as the ‘inductive risk’ argument (see Brown, 2013 for a review and Elliot and Richards, 2017 for a collection of recent philosophical debates on the inductive risk argument). This is the first building block of Douglas’s framework.

Douglas’s second argument relies on the recognition of the pragmatic function of science in the last century. Scientific experts are increasingly playing a more authoritative role in resolving regulatory conflicts, making policies and legitimizing interventions to societies and people’s lives (Douglas, 2009). Douglas suggests that it is better to conceive of scientific judgment-formation as a process that is not autonomous from the management of the current affairs and hence should not be governed by its own rules only.

The combination of these two ideas (the inductive risk argument and the [partial] causal dependence of policy outcomes to scientific expert judgments) compels Douglas to reject the normative appeal of the VFI as an ideal. Specifically, she claims that it is plausible for scientists to take into account the risks of being wrong in their judgments in the face of the inductive risk problem. Moreover, she argues, if the judgment made by a scientific expert influences policy outcomes (e.g. as it happens in the policy-relevant areas such as regulatory toxicology), the risks of being wrong bear on non-epistemic consequences of the policy action. For instance, as the
clothianidin case suggests, a regulatory toxicologist’s erroneous judgment may have consequences for the health of the bee population and the economic productivity of the pesticide-producing sector. Accordingly, Douglas suggests, it should be normatively desirable that scientists make an evaluation of such non-epistemic consequences in order to decide how much evidence is sufficient to confirm a hypothesis given that there are inductive risks in scientific judgment-formation.

Douglas’s conclusion goes against the normative appeal of the VFI. It does so because it leaves room for non-epistemic values to play a permissible role in the core stages of scientific inference. If we agree with Douglas that non-epistemic values can plausibly influence the results of scientists’ judgments, we would then expect regulatory toxicologists to let values such as environmental protectionism to play a more prominent role during the formation of regulatory judgments. This idea has evaluative consequences for a case like the regulation of the clothianidin.

Suppose that a regulatory toxicologist collects data from the pathological examination of bee tissues, which are exposed to varying dosages of clothianidin in laboratory conditions. Based on the collected experimental observations, she chooses a model that describes how tissues respond to the different doses of clothianidin (typically in the form of a dose-response curve). Then, based on the predictions of the model, she makes a judgment about the hazardousness of clothianidin over a given bee tissue-type under varying dosages. This judgment is a probabilistic one such as ‘clothianidin is likely/highly likely/unlikely to be malignant for the exposed tissue’ (but typically with precise numbers) (Douglas, 2000).

Now, as discussed above, Douglas’s idea is that because the available evidence is not fully confirmatory of the hypothesis that the clothianidin is harmful, the toxicologist’s judgment will be an inductively risky one, consisting of one of the following errors. Error I is reporting clothianidin as benign when it is actually malignant. Error II is reporting clothianidin as malignant when it is actually benign. But, both errors have non-epistemic consequences because they can be used for legitimizing different regulatory agendas. Committing Error I could motivate regulatory actions that do not
strongly discourage the use of the clothianidin even though it should really be discouraged. This contributes to the development of adverse ecological or public health outcomes such as the collapse of the bee population. On the other hand, committing Error II provides justification for unnecessary over-regulation of clothianidin, leading to unwanted economic outcomes.

Douglas suggests that it should be legitimate when toxicologists consider the severity of these errors and pertinent outcomes in non-epistemic terms to decide how much evidence is sufficient to make one of the risky inferences. If failing to detect the malignancy of clothianidin has too severe environmental costs, it should be permissible for the toxicologist to confirm its malignancy based on relatively weak evidential support. In other words, non-epistemic values such as ecological health should be allowed in scientific reasoning to help scientists to determine how much evidence is sufficient to confirm a given hypothesis. If Douglas is right, then the ecological concerns should play a more extensive role in regulatory toxicologists’ evaluation of clothianidin and similar toxic substances.

Douglas’s normative argument against the VFI provides us with some reasons to depart from the VFI as a benchmark for evaluating experts’ accountability in evidence-based law and governance. Perhaps, non-epistemic values or interests of the public or the environment should, after all, influence scientific experts’ judgments more wittingly. But, notice that Douglas’s argument only criticizes the VFI but does not offer an alternative benchmark that regulates values in science. If we do not know how to replace the ideal, isn’t abandoning it too costly?

1.3.2. Why is the VFI normatively appealing?

Without a new ideal that is as useful and convincing as the VFI, the permeation of values to scientific inference may be undesirable because the scientific authority could be conceived as unduly politicized when there is no normative benchmark that regulates the permeation of values into scientific reasoning. Some philosophers of science have argued that there are good reasons for not abandoning the VFI, at least
until a better benchmark is proposed (see, for instance, Betz, 2013, 2018; Bright, 2018). The defences of the VFI are based on two main concerns.

The first and the most pressing worry against the idea of abandoning the VFI is the potential loss of scientific objectivity in the context of scientific advisory. Betz, for instance, argues that the best use of scientific knowledge is to hold it detached from social and political processes, as that is the best way to keep the reliability of science intact. Disbelief in the objectivity of the scientific advisory would undermine the very reason to defer to the experts in making complex regulatory decisions.

The inclusion of non-epistemic values into scientific reasoning may lead to dogmatism; that is, unwillingness to update or replace one’s predispositions, values and judgment (see Anderson, 2004 for a compelling argument). Non-dogmatism is a characteristic commonly ascribed to scientists. Scientific experts are supposed to report what available evidence indicates regardless of whether her evidence-based inferences are in clash with her personal wishes, viewpoint or causes. A significant departure from the VFI depiction of science seems to spoil this basic feature of scientific reasoning.

Consider a regulatory toxicologist who endorses environmentalist values. She might be worried that the industrial use of clothianidin may harm biodiversity. She accordingly would wish that her assessment of clothianidin would conclude that clothianidin is extremely harmful to wild-life even when exposed in very small dosages. One could reasonably suspect that an expert endowed with such an activist mindset is likely to be more subjective in her judgments, and her conclusions are unreliable for informing regulatory processes. More generally, when a scientist makes a judgment that she prefers to be true even though there is no clear evidence supporting that judgment, she would commit to wishful thinking. Wishful thinking has no legitimate role to play in genuine scientific reasoning. When scientific experts engage in wishful thinking, they will lose their reliability for informing regulatory processes.
Accordingly, the VFI could be considered as protective of the public trust in scientific authority in regulatory processes. Philosophers who propose to transcend the VFI should then have an account of how scientific inference can remain non-dogmatic and trustworthy while being influenced by non-epistemic value judgments. As I will review in Section 4, philosophers such as Heather Douglas or Elizabeth Anderson have addressed this worry, but the controversy remains up-to-date (Betz, 2018).

The second argument for the normative appeal of the VFI emphasizes the inconvenience of scientists as agents of ethical and political decision-making (Betz, 2013). Letting scientific results be influenced by certain non-epistemic value considerations may lead to the expansion of experts’ arbitrary political power in regulatory processes. Experts can influence the policy processes by adjusting the results in accordance with their preferred policy desiderata. However, scientific experts are not considered to be legitimate political actors, at least in a democratic context. In a democratic governance structure, regulatory decisions are not supposed to be made by experts but by democratically legitimized political subjects. The acceptance of values into the production of scientific judgments is in tension with the liberal-democratic values.

This argument is underpinned by an understanding of policy-making in which the roles of democratically legitimzed decisionmakers and the scientific advisors are clearly divided. According to this understanding, scientific experts are tasked with a politically neutral, advisory role which is to report the available evidence as accurately as possible. The democratically and legally legitimate decisionmakers then bear the responsibility of making any politically and morally binding value judgment. This ideal resembles the traditional model of risk management in regulatory sciences, which was critically discussed by the science studies scholars such as Sheila Jasanoff (1990) as well as legal scholars who contributed to this volume (Arcuri and Simoncini, 2015; see also Arcuri 2021, Chapter 3 and Kanetake, 2021, Chapter 8). The model distinguishes scientists as risk-assessors and decisionmakers as risk-managers. While the decisionmakers are responsible for managing risks and taking risky decisions, the
scientific experts are tasked with assessing the risks without taking a stance on how those risks should be managed or letting that stance influence her assessment.

For instance, toxicologists conduct research on the nature of clothianidin’s toxicity and assess the scale of environmental risks resulting from the use of clothianidin. Then, they communicate their estimations of risks to the decision-making agents. They make clear when the evidence is inconclusive or in how far certain policy actions are supported by the available evidence. Only then, the decisionmakers (e.g. board of directors of private companies or regulators) make a decision about how to regulate clothianidin use (e.g. selling it, regulating it, banning it) by taking into account the risks estimated by the experts and valuation of potential adverse or desirable consequences of their decisions (Jeffrey, 1956). If this model of evidence-based regulation would be transcended, the replacing accounts of expertise in governance should also be able to respond to the democratic defence of the VFI, either by effectively arguing against it or by arguing that the new alternatives to the VFI do not harm democratic legitimacy of regulatory decision-making processes.

In this section, I have reviewed some of the key arguments for and against the VFI. A complete and widely accepted philosophical alternative to the VFI is not yet offered, and how the VFI should be replaced is still discussed by philosophers of science (for a very recent attempt to replace the VFI, see Brown, 2020). In the rest of this chapter, I will review a number of prominent approaches in the contemporary philosophy of science which transcend the VFI. The conceptual insights from these approaches can be insightful for the socio-legal scholars’ search for the right balance between epistemic and non-epistemic dimensions of scientific expertise in developing a conception of the accountability of experts in governance and law.
1.4. **Going beyond the Value Free Ideal while keeping scientific objectivity and experts’ reliability intact**

The aim of this chapter has so far been to offer a perspective from the philosophy of science on the debates about the accountability of scientific experts in the context of regulation. I have questioned how far scientific experts can and should make non-epistemic value judgments in their assessments for regulatory purposes. In response to this question, I have introduced the philosophical notion of the VFI, which permits values in science in some restricted ways. I have then discussed why we may want to move beyond or else protect the VFI. Now, in this section, I will introduce some of the key ideas in the recent philosophy of science that transcend the VFI but nevertheless respect the normative reasons for endorsing the VFI such as objectivity, the authority of the scientific judgments, and democratic governance.

1.4.1. **Does abandoning the VFI undermine science?**

In contrast to the VFI depiction of value-free scientific experts who are distant from societal issues and neutral towards policy outcomes, the philosophers who are unconvinced by the VFI (such as Douglas, Longino, Anderson, Brown, Kitcher) portray a regulatory expert who is concerned with the policy outcomes and who is protective of relevant value judgments and commitments in producing her analysis. For some commentators, replacing the VFI with such a ‘non-neutral’ portrayal of scientific advisory might come across as a dangerous turn in the philosophy of science.

One could, for instance, forcefully argue that the VFI is too important to be dismissed and too risky to abandon because of its normative appeals. The VFI, the skeptic might argue, is an ideal that appreciates the authority of science and effectively regulates the science-society relationship in a regulatory context. Acknowledging and demanding a more expanded role to the non-epistemic considerations in scientific experts’ reasoning opens the door to a lot of practical problems and may harm rational deference to experts in regulatory decision making. Similarly, one might be worried
that the quest for including general public’s expectations and values into expert-based regulatory decision-making may lead to a ‘politicization of expertise’, (Douglas, 2009, p. 113, p. 134) ‘tranny of ignorance or vulgar democracy’ (Kitcher 2011, p. 113, p. 126, p. 140, p. 177), or democratically unjustifiable ‘epistocracy’ in governance structures (see Reiss, 2019 for a discussion). Moreover, leaving the VFI may trigger the erosion of the authority of science (see Longino, 2002; Kitcher, 2011 for elaborations of these worries), motivating unsubstantiated versions of post-truth politics and unconvincing adoptions of Paul Feyerabend’s ‘anything-goes’ argument (see Russell, 1983 for a discussion). After all, given its broad normative appeal (reviewed in Section 3), the VFI could still be the best normative benchmark for evaluating experts’ role and responsibilities in evidence-based decision-making (however untenable it might be as a descriptive account of science). If we allow the permeation of non-epistemic considerations into scientific reasoning and open the Pandora’s box, so to say, how can we make sure that scientific experts are objective when they inform policy decisions? Does an expert who is driven to preserve values such as health-provision, environmentalism or inequality-aversion almost unavoidably lose her reliability as a neutral policy assessor? Is it not too dangerous to abandon the VFI as a normative benchmark for scientific reasoning? So, even if it might be an untenable ideal, why not keep the VFI as a guiding principle?

These important concerns need to be properly addressed if we are to fully replace the VFI. I do not claim that these concerns are already satisfactorily addressed by the philosophers of science. However, all I want to clarify is that the recent philosophical critics of the VFI, who seek to provide us with the foundations to go beyond it, endorse similar concerns but nevertheless believe that the best way to address these concerns, at least in the context of regulatory sciences such as regulatory toxicology, is to transcend the VFI. In their arguments against the VFI, philosophers of science such as Philip Kitcher, Helen Longino and Heather Douglas, have explicitly addressed these concerns.
In the remainder of this section, I will introduce some of the key concepts in the literature on the values in science that move beyond the boundaries of the VFI, which nevertheless are protective of the meta-values that the VFI subscribes to such as scientific objectivity, the importance of scientific authority, democratic quality of science-based decision-making, and the avoidance of imposing values to the citizenry.

1.4.2. Transcending the Value Free Ideal of Science

I will review four recent concepts in the contemporary philosophy of science that go beyond the VFI while keeping the authority of science for democratic governance intact. These are Douglas’s objectivity as impartiality or detachment (Douglas, 2004), Steel’s notion of epistemic priority (Steel, 2018), right values for science (see for example among others Hicks, 2014; Kourany, 2010) and Kitcher’s account for the democratization of scientific expertise (Kitcher, 2011, pp. 86-113).

1.4.2.1. Objectivity as impartiality as opposed to value freedom

Heather Douglas argues that the exclusion of value judgments from scientific inference is not the only plausible rule to protect the objectivity of scientific experts’ reasoning (Douglas, 2004, Douglas, 2009, pp. 115-132). She claims that values may, in principle, legitimately influence regulatory scientists’ decisions about key methodological choices, which would then influence their results. However, she argues, permitting values in scientific inference does not necessarily imply that scientific objectivity qua impartiality is harmed. According to Douglas (2004, pp. 458-461) and Elizabeth Anderson (2004) the key to achieve objectivity in scientific reasoning while permitting non-epistemic values is that value judgments do not override scientists’ evidence-based inferences, but rather supplement them. So, Douglas suggests, rather than banning the non-epistemic values in science as the VFI advises, we need to an account that distinguishes permissible and impermissible permeations of values into scientific reasoning. She, in turn, offers one such account.
Douglas introduces a distinction between the direct and the indirect use of values in scientific inference.

When values influence scientific judgment indirectly, they supplement evidential reasoning by helping scientists decide how much evidence is sufficient to confirm a given hypothesis, under inductive risks. The scientific reasoning is in the following form: if the consequences of me failing to accept p when, in fact, p is true would be very bad (ethically, socially), then I will accept that p on the basis of weaker evidence. When the role of values is direct, values go beyond this supplementary function (helping assess the sufficiency of evidence), and instead value considerations trump over evidential ones or replace them. It would then be in this form: if the consequences of me failing to accept p would be very bad (ethically, socially), I then will accept p (irrespective of the sufficiency of the evidence).

Now, Douglas argues that there is an important difference between the two uses of value judgments: when employing values indirectly, a scientist is prepared to be non-dogmatic. That is to say, she would reject p when evidential support against p would increase (as the severity of non-epistemic consequences of errors would diminish when there is more certain evidence). The same is not the case for the direct employment of value judgments in scientific inference: in that case, the scientists’ reasoning disregards evidence. Therefore, only the indirect role of non-epistemic values should be permissible in scientific reasoning, according to Douglas.

The reader might be wondering: How would this rather conceptual-theoretical distinction work in practice such that it can inform our analysis of the real cases of expert accountability? Although the individual scientist’s right reasoning attitudes, social norms of transparency and peer-review are supposed to filter out the impermissible, direct uses of values in scientific reasoning, the idea of detached objectivity should be conceived as a regulative ideal that may not be fully attainable in practice, just as the VFI.
The concept of detached objectivity transcends the VFI in that it allows values to play some (indirect) role at the core of scientific reasoning. It is, however, protective of the value of objectivity as impartiality or avoidance of dogmatism, which is at the central to the VFI.

1.4.2.2. Epistemic priority

As noted in Section 2, the employment of epistemic values such as simplicity, internal consistency in making inferences in science does not conflict with the VFI but rather supports it. However, some philosophers of science, such as Rooney (2018), argue that the definitions and instances of epistemic values in science may sometimes be affected by hidden non-epistemic considerations as well. This kind of scepticism about the VFI leads to new approaches in philosophy of science that rethink the nature of the epistemic values in science and accept the idea that epistemic values may sometimes be context-dependent or not always serve for the attainment of truth (see, for instance, Longino, 1996).

Recently, Daniel Steel suggests that some of the epistemic values that help with scientific inference (such as simplicity, consistency with what is already known, openness to criticism, following agreed-upon methodological procedures) sometimes do not serve the attainment of true beliefs (Steel, 2010; 2018). Whether such epistemic values hinder or promote the attainment of truth depends on the context and the case at hand. Steel suggests that non-epistemic values may similarly help with the attainment of truth, so in some cases, their role is similar to the role of the benign employment of epistemic values. The studies on the values in regulatory sciences can make use of Steel’s account to judge whether experts’ employment of certain non-epistemic values harms the quality of scientific advisory in a certain case. For instance, the standardization of policy-relevant scientific inquiry (e.g. through establishing agreed-upon protocols and mechanical procedures) may minimize any particular community of experts’ arbitrary influence on political decision-making in some contexts but could also lead to biases in different cases, depending on the structure and the quality of the procedures.
Steel’s argument is another prominent example of a recent approach in philosophy of science that goes beyond the VFI, which nevertheless is clearly protective of the core meta-values that the proponents of the VFI have subscribed to. Notice that Steel’s criterion for allowing non-epistemic values in scientific reasoning prioritizes the attainment of truth as the guiding principle to decide whether the permeation of non-epistemic values in science leads to any harm in the quality of scientific judgment in a given context. Similar to the idea of detached objectivity, Steel’s argument for the inclusion of non-epistemic values in science is protective of the scientific authority in an important sense.

1.4.2.3. Pluralism and the right values in science

Yet another remarkable and prominent philosophical approach to the role of values draws on feminism and the work of feminist philosophers of science who have grappled with the question: Is a feminist science possible? (see for instance Haraway, 1988; Anderson, 2004; Longino, 1987; Wylie, 2007). In advancing replies to this question, philosophers such as Alison Wylie, Elizabeth Anderson and Helen Longino, have offered compelling theories according to which scientific judgment can be value-laden but nevertheless deserve the label objective.

Famously, Helen Longino (1990) argues that the plurality of views, values and background assumptions in science is not a weakness but rather constitutive of scientific objectivity and rationality. For instance, Longino has established the idea that avenues for criticism and peer review, openness to new perspectives and participants, and similar social institutions of science that facilitate rational argumentation help science progress and establish itself as an objective inquiry. Longino’s account, therefore, invites us to think that non-epistemic values in scientific reasoning are not to be refrained from but to be embraced, reflected upon and to be regulated through the critical social interactions among scientists.

While Longino’s account is a general one, more recent approaches in the feminist philosophy of science, such as Janet Kourany’s Philosophy of Science after Feminism
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(2010), have addressed the problem in more specific terms by asking which non-epistemic values are conducive of good and responsible scientific practices. Note that venturing into an account of ‘right values’ is a controversial subject and may conflict with the ideals of democracy and neutrality. This kind of work is ongoing. However, Kourany’s project is similar to Longino’s in that she argues for a more extended role of non-epistemic values in science because she believes that cogent and responsible science cannot be realized without genuine engagement with values.

The feminist accounts of science, such as Longino’s and Kourany’s, invite us to assess the quality of the social dimensions of the regulatory sciences used for legitimizing policies. The relevant examples of these dimensions are pluralism, diversity of perspectives, transparency of non-epistemic values. Assessing these criteria for the quality of regulatory expertise can help socio-legal scholars detect whether and how failing to fulfil such feminist ideals in regulatory sciences hinders or obstructs the attainment of good regulatory outcomes (that are acceptable by the stakeholders and are protective of environmental safety and the public health). We can also use this kind of analysis to identify whether acceptable non-epistemic values are employed in regulatory sciences such as toxicology and pharmacology (see Hicks, 2014 for an exploration).

1.4.2.4. Democratization and citizen participation in science

Some philosophers of science, notably Philip Kitcher (2011) and Stephen Turner (2001) have explored these issues from a different angle by asking under what conditions citizens’ and governments’ deference to expertise to manage current affairs is reconcilable with liberal and democratic values. In response to this theoretical problem, the science studies scholars and philosophers of science have explored to what extent and how scientific expertise can be democratized by establishing institutionalized avenues that facilitate a deep and meaningful interaction between experts and non-expert citizens. In turn, various models of inserting citizens into science, or more generally models of democratization of expertise, have been
proposed or investigated by the philosophers of science and science studies scholars (see, for a collection of contributions, Maasen & Weingart, 2005). This literature is also known as the democratization of science literature.

Some of the commentators in this literature, such as Kitcher and Douglas, have explicitly abandoned the VFI. But again, the point has not been to criticize the meta-normative values the proponents of the VFI subscribe to but to transcend them. Specifically, Kitcher addresses the second argument in favour of the VFI noted in Section 3. He proposes that the involvement of non-epistemic values and directions of scientific research should be decided by the informed citizens and deliberated in ideal settings. Kitcher’s ideal demands that the heterogeneity of values in the public should be represented in scientific research projects. In order to avoid the arbitrary negative influence of value-laden experts over citizens, Kitcher argues in favour of reforming scientific institutions in such a way that science is properly situated in an advanced deliberative democratic settings. Trying to come closer to Kitcher’s ideal of science (the Well Ordered Science), he argues, is not only a requirement of democracy but also a vehicle to fulfil liberal values such as people’s freedom to pursue meaningful life projects and equality in doing so.

Together with Kitcher’s Well Ordered Science, the four concepts reviewed above show us that the worries of the proponents of the VFI can, in principle, be properly addressed (see Table 10.1 for a summary). For the examination of the accountability of regulatory experts, these philosophical insights highlight that departing from the VFI does not necessarily imply an unguarded politicization of scientific expertise in regulatory governance processes. Moreover, they form a helpful pool of conceptual resources that can be used for evaluating the quality of how non-epistemic issues are addressed by scientific experts across diverse regulatory contexts in practice.
Table 1.1: Four key concepts from the recent philosophy of science that transcend the VFI while reclaiming the value of scientific authority in democratic governance

<table>
<thead>
<tr>
<th>Concept</th>
<th>Description</th>
<th>Key Text</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Objectivity As Impartiality</strong></td>
<td>The permeation of pragmatic considerations into the core of scientific reasoning violates the sense of scientific objectivity as value-freedom. However, the violation of value-freedom does not imply that the sense of objectivity as ‘detachment from values and evidence’ is also violated.</td>
<td><em>Science, Policy and the Value-Free Ideal</em> by Heather Douglas</td>
</tr>
<tr>
<td><strong>Objectivity through Pluralism and Dissent</strong></td>
<td>Proper social institutions of science (e.g. an inclusive and transparent peer-review community) may guard against the arbitrary dominance of any particular community of experts’ subjective biases or hidden values over practical decision-making. Scientific objectivity is partly established inter-subjectively through learning from each other and discovering one’s biases through critical peer-reviewing.</td>
<td><em>Science as social knowledge: Values and objectivity in scientific inquiry</em> by Helen Longino</td>
</tr>
<tr>
<td><strong>Epistemic Priority</strong></td>
<td>Non-epistemic values can be included in scientific inference if we take the epistemic value of doing so as a guiding principle.</td>
<td><em>Qualified Epistemic Priority</em> by Daniel Steel</td>
</tr>
<tr>
<td><strong>Well Ordered Science</strong></td>
<td>In order to keep the authority of science intact, we need to make sure that the heterogeneity of values in public is well represented in science. This would fulfill the instrumental value of science, which is to maximize societies’ freedom to pursue meaningful life projects. In order to avoid the arbitrary negative influence of value-laden experts over citizens, we need to design the avenues for expert-citizen interaction in such a way that science is properly situated in an advanced democracy.</td>
<td><em>Science in a Democratic Society</em> by Philip Kitcher</td>
</tr>
</tbody>
</table>

1.5. Conclusion

As reviewed, the philosophical debate over the cogency of the VFI provides the socio-legal scholars who are interested in the accountability of regulatory scientific expertise with useful conceptual insights. These insights can be used for examining individual cases such as the case of the regulation of clothianidin. Based on the relevant conceptual grounds reviewed here, we could ask, for instance, whether the VFI is followed by the regulatory toxicologists or justifiably violated.
Recognizing the fact that value-free and objective scientific judgment is, at best, an ideal should not readily pave the way for the politicization of scientific expertise in the context of expert-based governance. After all, multi-level regulation and governance need fair amount evidence as opposed to opinions; rational arguments as opposed to politics-driven wishes. This however does not imply that we must stick to an old-fashioned positivist ideal that is hardly applicable to the complex contemporary multi-level governance mechanisms in which experts’ judgments play crucial roles. Scientific judgments must be produced with the awareness that their conclusions might be misinterpreted, misused by the policymaking agents. It is also important that regulatory experts genuinely serve for public’s interest. What is required is an understanding of science as a reliable epistemic source that does not refrain from attending to the relevant non-epistemic issues.

In this chapter, I have considered different ways in which scientific experts can be permissibly expected to let non-epistemic value judgments to influence their assessment. In particular, I have offered that the arguments for and against the philosophical Value Free Ideal of Science provide us with a suitable conceptual ground to examine the accountability of scientific experts in addressing the relevant value considerations such as economic interest and environmental safety in their analysis. I have emphasized that the contemporary philosophical debates on the role of values in science urge us to consider ways in which values may play roles in regulatory scientists’ analysis, but within certain limits that hopefully protect the epistemic and democratic legitimacy of our reliance on scientific experts in regulatory decision-making.
References


Debating the Value Free Ideal of Science


2.1. Introduction

Contemporary philosophers of science are increasingly interested in evidence-based policy assessment and the role of value considerations in scientific analysis. This has revitalized some of the traditional philosophical debates about the proper role of values in science, the norms of scientific reasoning, and the cogency of the traditional ‘value-free ideal’ (Betz, 2013; de Melo-Martín & Intemann, 2016; Douglas, 2009; Elliott, 2011; Hicks, 2018; Jeffrey, 1956; John, 2015; Longino, 1990; Rudner, 1953). Heather Douglas contributes to this debate by offering a normative account that distinguishes legitimate from illegitimate value-permeations in scientific reasoning (Douglas, 2000, 2015; Douglas, 2009).

Douglas conveys that scientific reasoning can legitimately be value-laden while its objectivity stays intact. This is possible, Douglas argues, if the role of non-epistemic
values is constrained to the assessment of evidential sufficiency in the face of uncertainty. More specifically, it is legitimate that non-epistemic values inform an evaluation of not-fully-confirmed hypotheses by providing researchers with information about pertinent risks of being wrong in one’s inferences when the available evidence is inconclusive (i.e., inductive risks). Douglas argues that allowing non-epistemic values in science in such an “indirect” manner does not violate the objectivity of science, at least in a significant sense of objectivity that appeals to the virtue of assessing evidence in a detached way (Douglas, 2004). Douglas further suggests that this understanding helps us assess when scientific judgments get dogmatic or irrationally politicized and therefore untrustworthy (2009, pp. 112–114).

The most prominent aspiration of Douglas’ project is to replace the value-free ideal, which strictly prohibits value considerations in evidential reasoning during the so-called core (internal) stages of scientific justification.

Douglas’ project has been found promising by philosophers of science who are interested in the role of values in science (see, for instance, Elliott & Richards, 2017; Kitcher, 2011 for prominent reviews and reflections). Many commentators involved in science-based policy discussions accept that scientific inputs should be sensitive to relevant moral and political considerations without losing their rationality, objectivity, and trustworthiness in some meaningful sense. I, too, am sympathetic to this pragmatic aim. Therefore I am interested in demonstrating in detail how Heather Douglas’ rational account of scientific reasoning in the face of ethical and pragmatic considerations, and factual uncertainty, is and can be considered consistent with real-world contexts of evidential decision-making that scientists typically face.

In this chapter, I examine how Heather Douglas’ inductive risk framework applies to a specific and carefully selected case of scientific practice. I analyze toxicologists’ evidential judgments during a societally important historical episode of toxicological practice, the so-called molecularization of policy-relevant toxicology. Policy-relevant toxicology, which is also often aptly referred to as “regulatory toxicology”, is practiced for the purposes of regulation of the industrial use of toxic components. In the U.S., the major research institutions in this area are the National Institute of
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Environmental Health Sciences (NIEHS), which is also home to the National Toxicology Program (NTP), the Environmental Protection Agency (EPA), and the National Center for Toxicological Research (NCTR) based in the Food and Drug Administration (FDA). These institutions are tasked with protecting the public and environmental health through supplying scientific risk assessments about the potential adverse effects of the use of various toxic substances.

I shall focus on regulatory toxicologists’ judgments about the acceptability of molecular evidential sources and evidence-gathering methods for their purposes. In analyzing this context, I draw on the evidence documented in the relevant historical studies of toxicology (such as (Frickel, 2004; Shostak, 2005, 2013) to offer a descriptive account of how regulatory toxicologists actually make and update methodological decisions about evidence. Based on my analysis, I demonstrate that toxicologists evaluate different kinds of evidence by assessing their epistemic advantages (such as further accuracy and precision achieved through the use of different kinds of evidence) and by considering relevant non-epistemic consequences of their inductively risky methodological decisions (such as the convenience of the use of different kinds of evidence for regulatory use, and for the broader purpose of protecting the public and environmental health). I highlight some philosophically interesting aspects of the case, such as the initial rigidity of toxicologists’ inductive risk judgments and the role of social and institutional processes in the formation of their evidential decisions, which Douglas’ account, and the broader inductive risk framework, is not designed to address. I argue that Douglas’ account of scientific reasoning can be integrated with a suitable conception of evidence, such as Helen Longino’s contextual empiricist one, in order to account for the highlighted aspects of the case. I then introduce such an integrated account and demonstrate how Longino’s contextual empiricism and Douglas’ inductive risk framework fruitfully complement each other in describing the case.

The chapter proceeds as follows. In section 2, I illustrate Douglas’ theory for scientific reasoning, and how it can be used for representing and assessing the actual cases of scientific judgment-formation in the face of value considerations and uncertainty.
Chapter 2

about facts. In section 3, I examine how the inductive risk framework can be applied to understand toxicologists’ acceptance of molecular methods and evidential sources over the conventional ones for the purposes of regulatory toxicology, and I highlight the exceptional and theoretically novel aspects of the toxicology case which the inductive risk framework is not designed to address. In section 4, I show how Douglas’ inductive risk framework works in harmony with Longino’s contextual empiricism to address the highlighted problems such as toxicologists’ initial rigidity of the inductive risk judgments and the role of social context in mediating scientists’ evidential judgments. In section 5, I conclude with a brief summary of the chapter.

2.2. Heather Douglas’ Account as a Norm of Scientific Judgment in the Face of Inductive Risks

The so-called “value-free ideal” suggests that scientists’ assessment of evidence should be free from non-epistemic values. As a norm of scientific reasoning, this suggestion prohibits appealing to contextual pragmatic or ethical considerations in making evidence-based judgments. The value-free ideal has been widely debated in the modern philosophy of science (Betz, 2013; Bright, 2018; de Melo-Martín & Intemann, 2016; Douglas, 2009; Jeffrey, 1956; Kincaid, Duprè, & Wylie, 2009; Kitcher, 2011; Rudner, 1953). Heather Douglas has contributed to the debate by re-examining the role of inductive risk in scientific reasoning in her Science, Policy and the Value-Free Ideal (Douglas, 2009). Once we acknowledge the extended pragmatic role of scientific knowledge in contemporary societies, such as the increasing prominence of scientific expert advisory in risk management and regulation. Douglas proposes an alternative norm for scientific reasoning that preserves the objectivity and rationality of science in a meaningful sense. In this section, I present a summary of Douglas’s account, and motivate the project and the case I am going to analyze.
2.2.1. Distinguishing Roles of Values to Conceptualize an Alternative to the Value-Free Ideal

Heather Douglas proposes that scientists' judgments are often used for informing policy decisions, and thereby scientists may contribute to morally and politically significant outcomes in the world. In such cases, Douglas argues, scientists cannot and should not resist appealing to non-epistemic considerations in their decision-making. She argues that in many methodological decisions, scientists inescapably need to choose thresholds of evidential sufficiency, asking “how much evidence is sufficient for me to accept or reject a hypothesis?” because scientific inference and judgment often involve ineliminable uncertainties about what is actually true or right (Douglas, 2000, p. 559). Accordingly, Douglas argues that it is rational and morally desirable when scientists consider the non-epistemic consequences of their decisions in contexts where they accept a hypothesis, make methodological choices, or provide support for a course of policy action. She concludes that the value-free scientific reasoning is therefore “flawed” and “incomplete” as a normative and descriptive account of policy-relevant sciences because it precludes scientists from making decisions regarding evidential sufficiency in the face of factual uncertainty and the pertinent need to consider the moral and pragmatic consequences of scientific judgments (2009).

Douglas proposes an alternative norm for scientific reasoning, which conceives of non-epistemic considerations as information used for determining the seriousness of making inductive errors in evidence-based reasoning:

The scientist will need to consider both the quantity of evidence or degree of confirmation to estimate the magnitude of inductive risk and the valuation of the consequences that would result from error to estimate the seriousness or desirability of the consequences. The weighing of these consequences, in combination with the perceived magnitude of the inductive risk (i.e., how likely one is to be wrong), determines which choice is more acceptable. Where inductive risks are involved and non-epistemic consequences follow from error, non-epistemic values are essential for
deciding which inductive risks we should accept, or which choice we should make (2000, p. 565).

This suggests that non-epistemic values help researchers determine how much evidence is sufficient to accept or reject a hypothesis or to make a potentially harmful methodological decision. Douglas contrasts this restricted, “indirect” role of values in scientific reasoning with what she calls the “direct” role of values in scientific reasoning. When the role of values is direct, their use goes beyond this supplementary function (that is, helping assess the sufficiency of evidence), and instead, trumps evidential ones or replace them. Only the indirect role of non-epistemic values is permissible in scientific reasoning, she claims, as this indirect role does not harm scientific objectivity in an important and meaningful sense of the term objectivity.

To support this argument, Douglas compares the cases of indirect permeation of values with the cases of direct permeation of values during the internal stages of the scientific method, which consists of the methodological steps that concern scientific justification such as selection and interpretation of data, modeling, and confirmation of hypotheses. The latter involves “wishful thinking”, “cherry-picking evidence from a wide variety of evidential sources”, and “constructing a methodology that will give results serving [one’s] own liking” (2009, pp. 150-52). These kinds of epistemic practices would harm scientific objectivity and invite dogmatism, corruption, or radical politicization of science. In contrast, so Douglas argues, the indirect permeation of values in scientific reasoning is different from these cases and does not lead to an erosion of scientific objectivity. She emphasizes that scientists’ consideration of non-epistemic values in the indirect manner preserves detachment and non-dogmatism, which she takes to be one of the essential aspects and the core virtues of scientific reasoning (2009, pp. 112-114). Her proposed alternative to the value free ideal preserves these core virtues of scientific reasoning (Douglas, 2004).

Douglas’s argument from inductive risk is therefore promising to conceptualize a new normative benchmark for scientific reasoning (although there are important skeptical arguments such as Betz (2013) who questions whether the value free ideal should be abandoned, de Melo-Martin and Intemann (2016) who question whether Douglas’s
account really rejects the value free ideal). Douglas’s account is indeed interpreted in different ways: as part of an alternative to the traditional value-free ideal, as a tool for policing scientists’ reasoning, and as a benchmark to distinguish permissible value-ladenness in science from corrupt or politized use of values in science (see, Elliott 2011, for an extensive critical review of these distinct pursuits of Douglas’s account).

At the same time, various prominent commentators have discussed the scope of the inductive risk arguments such as Douglas’, debate to what extent the inductive risk framework can serve as a full-fledged account of values in science, analyze how Douglas’ account and the broader inductive risk approach to values in science relate to the other established descriptive and normative arguments about the role and nature of values in science, and demonstrate how the inductive risk framework applies to different cases of scientific practice (Biddle, 2016; Biddle & Kukla, 2017; Brown, 2013; de Melo-Martín & Intemann, 2016; Elliott & Richards, 2017; Hicks, 2014, 2018; Steel & Whyte, 2012).

In this vein, a growing body of philosophical literature contributes to our understanding of the proper place of values in science by applying the inductive risk framework to the assessment of the actual contexts of scientific decision-making. Here, I aim to contribute to these debates by focusing on an episode of scientific practice that instantiates some aspects of scientific reasoning, which are less commonly examined from the perspective of the inductive risk framework. I will analyze the context in which scientists choose between different kinds of evidential sources and evidence-gathering methods and make judgments about the (inductive) risks pertaining to their decisions. I will focus on the case of regulatory toxicologists’ gradual adoption of molecular tools. Specifically, I will examine precisely how the inductive risk framework applies to this case, and how Douglas’ account can account for some interesting aspects of toxicologists’ reasoning such as the graduality of the changes in their evidential judgments and the role of contextual social and institutional processes in the formation of their methodological decisions. Based on my analysis, I will argue that these aspects of the case can be described by Douglas’s inductive risk framework when it is considered to be in synchrony with suitable neighboring philosophical approaches, specifically Helen Longino’s contextual empiricism. I aim
to demonstrate that these two accounts complement each other in fruitful ways. Let’s then focus on the case and discuss how Douglas’s inductive risk framework applies to it.

2.3. Analyzing the Case of Molecularization from the Perspective of Douglas’ Inductive Risk Framework

Applied toxicologists aim to assemble scientific assessments concerning the toxicity of chemical substances to inform regulatory decisions taken by public health institutions such as the National Institute of Environmental Health Sciences (NIEHS), the Environmental Protection Agency (EPA), and the Food and Drug Administration (FDA) in the U.S. context. Because applied toxicology is a “regulatory science” (as initially termed by Jasanoff, 1990), toxicological practice constitutes a perfect case study for Douglas (2000; 2009) and any philosophical discussion regarding the issues arising in the context of policy-relevant sciences and evidence-based policies. In this section, I will investigate the extent to which Douglas’ account successfully evaluates various types of decisions that toxicologists make. Specifically, I will focus on the acceptance of molecular evidential sources for conducting policy-relevant toxicological research—the so-called molecularization of regulatory toxicology.

2.3.1. Douglas’ Norm applied to Toxicologists’ Judgments

The practice of regulatory toxicologists is a very suitable case for illuminating Heather Douglas’ account of values, as it is a rigorous scientific practice that is not autonomous from society or policy-related processes. In her reflections on regulatory toxicology, Douglas debunks the division of labor that has been widely assumed to hold between “toxicologists as value-free risk assessors” and “regulators as risk managers” (2009, p. 140). Douglas claims that the depiction of toxicologists as value-free risk assessors, who operate in isolation from society and who serve the decision-making processes of value-laden policymakers, is descriptively incomplete. She also
argues that trying to achieve such an ideal in practice is normatively undesirable due to the inductive risks involved in toxicological risk assessment. Instead, Douglas proposes that toxicologists should consciously and transparently take responsibility for triggering environmental risk management by adjusting their thresholds for determining the sufficiency of the evidential grounds in response to potential societal consequences of their decisions. In line with the account illustrated in Section 2, Douglas considers this (“indirect”) role of values in toxicology-based judgment formation legitimate.

Douglas applies her account to analyze various critical methodological stages of toxicological science (2000):

- Making a judgment about the severity of the changes observed in exposed tissues (Douglas, 2000, sec. 4);
- Choosing an adequate data-analysis method among alternative dose-response curves to fit the available data (Douglas, 2000, sec. 5);
- Interpreting the results in a particular way (e.g., stating whether a study allows scientists to conclude that the substance under consideration is toxic irrespective of its dosage; or that the toxicity of the substance is contingent on the dosage). (Douglas, 200, sec. 6).

How does Douglas’ framework represent the structure of scientific reasoning in these contexts? Take the context in which toxicologists interpret the results. Imagine, for instance, two representative toxicologists (say “T1” and “T2”) who take ‘environmental health’ into account in their reasoning when they assess the hazardousness of chemical substances. T1 reports that “the substance is highly toxic” even though she does not unambiguously detect any significant malignant changes in the exposed tissues observed. T1 nevertheless confirms the toxicity of the substance because T1 refrains from making judgments that might lead to undesirable environmental health outcomes. T2 also reports that “the substance is highly toxic”. But, different from T1, T2 detects some malignancies in the exposed tissues she observes. Even though available evidence does not fully confirm the hypothesis that “the substance is toxic”, T2 lowers the threshold of evidential sufficiency by
considering the undesirability of environmental risks involved and accepts the hypothesis. It is clear that the non-epistemic value (the protection of environmental health) trumps the evidential considerations in T1’s judgment. For T2, however, the concern for environmental health only plays a supplementary role in the reasoning. If we follow Douglas’ account, it is then pretty straightforward to deem T1 as biased, dogmatic, and unjustifiably subjective. Similarly, it is pretty simple to judge that T2 preserves “objectivity”, as some metaphorical distance from evidence is kept in the assessment while allowing non-epistemic considerations to influence the judgment.

The suitability of Douglas’ account to evaluate the toxicologists’ judgments in cases like the above bears little controversy. There are some pieces of objects, such as slides of the exposed animal tissues, which have an evidential relationship with toxicologists’ hypotheses and, more generally, their judgments. These pieces of evidence confirm toxicologists’ judgments, such as concerning the toxicity of a substance, to different degrees. Inductive risks are therefore present, and hence Douglas’ norms could readily be applied to describe and assess the permissibility of the ways in which non-epistemic values permeate into scientists’ judgments in these cases.

The case of molecularization is about a different methodological context than the one I described above. Unlike the sufficiency of evidence for confirmation, the acceptance of molecular data and methods is a methodological issue about the relevance of evidence for the aim of regulatory toxicology. This context (i.e., the choice of evidence-types and how scientists make inductive risk judgments about them) is less commonly examined in the literature on inductive risk (see Biddle, 2016; de Melo-Martín & Intemann, 2016; Biddle & Kukla, 2017; Hicks, 2018 for a discussion of the conceptual and theoretical issues regarding how the inductive risk framework applies to this context).

In the following, I will illustrate that Douglas’ inductive risk framework properly applies to toxicologists’ judgments about the kinds of evidence, too. My aim is not only to demonstrate how Douglas’ account applies to this context but also to highlight
some important aspects of the case that the inductive risk framework is not designed or purported to address.

Before I focus on this context in the next subsection, a disclaimer is in order. In my analysis of the case of molecularization, I will specifically refer to some science studies scholars’ detailed examination of toxicologists’ judgments concerning molecular evidence (such as Frickel, 2004 and Shostak, 2013) with the aim of providing a comprehensive description of the case. Some readers might anticipate that I will pit the science studies scholars’ arguments about toxicology against Douglas’ arguments. However, that is not my aim. I will not refer to any philosophical claims made by these scholars. Rather, I will rely solely on these authors' characterization of the historical facts regarding the case. I will thereafter defend the philosophical significance of the historical details of the case for the inductive risk framework.

I will now start with a brief explanation of the so-called “molecularization of regulatory toxicology”. Then, I will offer a description of how regulatory toxicologists choose between molecular evidential sources and conventional pathological ones and examine how the inductive risk framework captures their reasoning. I will then discuss why it is a good idea to integrate the inductive risk framework with a suitable neighboring approach to analyze the specifics of the case.

### 2.3.2. Choosing between the Molecular and Conventional Methods in Regulatory Toxicology

Classical policy-relevant toxicology used to be predominantly based on pathological examination of exposed animals in laboratory conditions. In this method, laboratory observations of exposed tissues, which are then statistically analyzed by toxicologists, are used for making predictions about the hazardousness of toxic substances under changing dosages based on different dose-response models. This primarily pathology-based, observational method was referred to as the gold standard for

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1 Notice that the examples in previous section, which Douglas examines in her work, pertain to classical toxicology, which is based primarily on pathological studies.
performing policy-relevant toxicological analysis until the 2000s (see, for instance, National Toxicology Programs’ seminal report (2004), where a vision for a change is introduced).

The received methodology of policy-relevant toxicological science has changed dramatically toward the adoption of molecular methods, especially in the United States. Contemporary toxicological science, at least in the context of the United States, including toxicology practiced for regulatory purposes, has been systematically moving towards becoming a genomic and molecular science (see, a prominent consensus report published by the National Academy of Sciences (2017) that reviews the developments in the last two decades). Though some controversy remains and the widespread application of new methods to the regulatory realm is still a project in progress, the relevance and the usefulness of molecular methods for informing environmental health policies are widely accepted by the major institutions of regulatory toxicology in the U.S. The research outputs of NTP at NIEHS and NCTR at FDA or the activities of ToX21 collaboration attest to this change in major institutions’ methodological judgments about molecular methods (see EPA 2019 for a review).\(^2\) Regulatory toxicologists nowadays increasingly rely on data gathered through advanced micro array technologies and advance quantitative tools such as high-throughput screening, which affords a comprehensive and ever more precise measurement of the simultaneous effects of multiple toxic chemicals across genetically different populations and individuals.

Nevertheless, the shift towards the molecular methods in applied toxicology, which is labeled “the molecularization of toxicology” by science studies scholars (Shostak,

\(^2\) The debate over the acceptability of molecular evidence and tools in regulatory toxicology may still persist among different groups of toxicologists (see, for instance, Hicks (2018) analysis about the acceptability of a particular molecular model for endocrinal risk assessment). In the light of my analysis, I will also reflect on how the inductive risk framework may interpret such disagreements among different communities of toxicologists (such as the one described by Hicks) later in Section 4. However, it is beyond dispute that a substantial shift has been taking place toward the adoption of the molecular methods, and that the major institutions of toxicology no longer consider the traditional methods as the gold standard for conducting regulatory risk assessments, and their strategical aim is to move further towards the adoption of molecular tools for conducting regulatory risk assessments.
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2005; 2013), has been highly controversial among toxicologists and relevant stakeholders such as regulators, industrial organizations, and environmental advocacy groups. As we will see, toxicologists’ reasoning about the adoption of molecular methods was influenced by epistemic considerations such as the high precision of the molecular evidential sources as well as non-epistemic ones such as how regulatory toxicologists should protect public health and reduce ecological and environmental health risks.

Broadly construed, the controversy regarding the choice of the molecular over the pathological method revolves around the following. Many applied toxicologists and relevant stakeholders such as research-based environmentalist advocacy groups initially proposed that molecular evidence-gathering methods and evidential sources were ill-suited for the pragmatic and regulatory roles of toxicology. Among other things, these roles include informing legislators about how the industrial production of chemical substances should be regulated and performing evaluations of ecological and health risks associated with various toxicants so as to conserve environmental health. The traditional pathological evidence-gathering methods and evidential sources were deemed entirely sufficient for serving these purposes, as they were conducive to assessing hazards of one chemical substance in isolation from other causally relevant chemical and genetic factors. Furthermore, to venture into comprehensive analyses of the complex molecular and genetic mechanisms of disease causation was thought to be impractical and inefficient for regulatory toxicology, however interesting it might be for purely scientific purposes. Some toxicologists working at prominent regulatory institutions in the U.S. (e.g., NTP-NIEHS, NCTR-FDA, and EPA) or researchers from environmental advocacy and justice groups (e.g., West Harlem Environmental Action, WEACT) thought that the epistemic benefits of molecular methods, such as greater precision and explanatory power, could only be achieved at the expense of making environmental regulation more complicated and less effective (Shostak 2013, 64-70). Hence, so their view went, these methods would potentially serve the interests of profit-seeking industries, which aim to minimize the financial costs of toxicological regulation. Here are some excerpts from the interviews that a prominent science studies scholar, Sara Shostak, conducted with applied
toxicologists working at NTP, which exemplify these kinds of worries about the increasing use of molecular methods in regulatory toxicology:

[at NTP] we need some people with practicality. We need some people with skills in toxicology . . . empirical descriptive toxicology. [If] you find out something causes cancer, then let somebody else mess around with the mechanism…. I don’t want to know how it does it . . . I want to know, “Is this safe?” (Shostak, 2013, p. 64).

My interest is in what can we change to make people healthier? We can change exposures. . . You can’t change your gene pool (Shostak, 2013, p. 65).

Any new technology, it’s always a good delaying tactic for environmental health risk assessments (Shostak, 2013, p. 66).

While the molecular kinds of evidence are conducive to more accurate and precise toxicological risk assessment, toxicologists based in prominent regulatory institutions initially did not accept the relevance of molecular kinds of evidence for their inquiry. Their reasoning was that molecular evidence-gathering methods complicated toxicological risk assessment as they are conducive to more detailed results, the significance of which is ever more difficult to translate into judgments that can conveniently be used for regulatory purposes. Working with molecular evidence-gathering methods thereby slows down and renders ambiguous the regulatory process, hence harming their mission of protecting environmental health.

It seems that these toxicologists took the relevant inductive risks into account when making pertinent negative judgments about the acceptability of molecular kinds of evidence. Given the risk of making a wrong methodological decision for the pragmatic purposes of regulatory toxicology, some toxicologists such as the ones quoted believed that they should keep low standards for evidential sufficiency (i.e., the level of evidential precision and certainty they deem sufficient for the regulatory purposes of toxicology). This belief was shaped by taking into account the environmental health risks associated with the purported impracticality of the molecular methods for serving policy-relevant toxicological assessment.

While we can explain toxicologists’ reasoning by reference to the language of Douglas’ inductive risk framework, the content of toxicologists’ inductive risk
judgment in this instance is also puzzling from the perspective of Douglas’ account. Note that those toxicologists referred above who were skeptical toward the molecularization did not argue that the adoption of molecular kinds of evidence would worsen the precision, accuracy, and the overall quality of evidence used for conducting toxicological risk assessment. On the contrary, it seems that the toxicologists acknowledged that the molecular kinds of evidence would enable them to make more precise and accurate assessments than they could do through the classical kinds of evidence (Shostak, 2013, pp. 48-64). They nevertheless resisted adopting molecular kinds of evidence by considering the magnitude of inductive risks regarding the protection of public health and the ease of regulation. This kind of reasoning is not fully in line with Douglas’ account because more precise and accurate evidence is supposed to decrease inductive risks, according to her theory. In other words, when the available evidence is more certain, the chance of making wrong decisions becomes lower; hence the magnitude of inductive risks is supposed to be lower, too (Douglas, 2009, p. 96). Yet, puzzlingly, in this case, if the toxicologists’ assessment were right, then additional or better evidence would lead to more uncertainty, not less.

The same issue has been recently highlighted by Hicks through a case study about the practice of policy-relevant molecular toxicology (Hicks, 2018). Hicks also observes that additional evidence, of the kind achieved using molecular models in the studies of endocrine disruptors, does not decrease uncertainty. They note that “uncertainty here is not the result of limited evidence but of more accurate or precise evidence” (2018, p. 170). Furthermore, they label this as “an important counterexample or exception” to Douglas’ norm (p. 170). For now, I flag this aspect of the case as an important issue to be addressed. I will respond to this problem later, in Section 4.

An equally interesting—in fact the main—aspect of the case of molecularization is that the prominent communities of toxicologists who were initially skeptical about the molecular approaches to toxicological risk assessment later updated their judgments and gradually accepted the use of molecular evidential sources and evidence-gathering methods (Shostak, 2013). In so far as this change in toxicologists’ judgment
is described in terms of the language of the inductive risk framework, it must be the case that the content of toxicologists’ inductive risk judgments changed. The same toxicologists must have concluded that the use of molecular evidential sources did not increase inductive risks, unlike what they had initially thought.

I will now describe how this change in toxicologists’ judgment happened, drawing on the relevant historical studies. Thereafter, I will discuss why and how the historical details of the ‘molecularization’ can suitably be addressed by an analysis of the case from the perspective of the inductive risk framework.

2.3.3. The Transition to Molecular Regulatory Toxicology: The Role of Social and Institutional Processes

Let’s review the relevant historical facts about how major communities of toxicologists in the U.S. gradually adopted the molecular approaches to toxicological risk assessment, and then ask ourselves whether and how we should fit the details of the case with Douglas’ inductive risk framework.

Prominent historical studies of contemporary toxicology (such as Frickel, 2004; Shostak, 2005, 2013) describe the process of molecularization as one that is mediated by a set of sociological and institutional processes. According to these studies, a significant macro-sociological process that contributed to the molecularization of regulatory toxicology was decreasing financial and institutional support for regulatory toxicology. Although not adopting molecular methods would not have practically prevented toxicologists from performing effective policy-relevant analyses, regulatory toxicologists increasingly met criticism for not using what were considered cutting-edge scientific methods (Shostak, 2013, Chapter 3). In particular, these criticisms were raised by commercial scientists working in profit-seeking industries (2013, p. 6). In addition, the popularity of genomic studies had increased in the general public, and the neighboring disciplines had already adopted molecular methods. These macro-scale developments led regulatory toxicologists to worry about losing the financial and social support they received from governmental and non-governmental
organizations. In the 2013 book, Sara Shostak extensively documents how leading communities of applied toxicology in the U.S. interpreted these macro-sociological changes as threats to the scientific authority of toxicology for regulatory purposes. Shostak explains that such financial and institutional considerations boosted the toxicologists’ gradual adoption of molecular methods.

Shostak also describes that the major communities of regulatory toxicologists initiated a set of strategic micro-institutional processes in response to these macro challenges to their inquiry. Those developments, too, contributed to the gradual adoption of the molecular evidence-gathering methods over pathological ones in regulatory toxicology. Here are some examples of these institutional processes toxicologists deliberately engaged in:

- Initiating new institutional ties with research centers, which were formerly thought to be only remotely related to applied-toxicological practice (such as the National Human Genome Research Institute, NHGRI) (Shostak, 2013, pp. 141-143).

- Forming new research centers that aim to raise funds for promoting policy-relevant molecular research in toxicology, including providing infrastructure for training new toxicologists who are experts in both classical toxicology and molecular methods (Shostak, 2013, pp. 143-150).

- Defining a research agenda for exploring applications of gene expression profiling specific to regulatory toxicology. This includes encouraging the use of adequate tools (such as ToXChips) for translating the new type of data into the old forms of regulatory procedures (Shostak, 2013, p. 147).

- Establishing research initiatives such as the Toxicogenomics Research Consortium to develop new methodological procedures and standards relevant for the use of new molecular risk assessment models, and to advance tools so as to allow the transparent and standardized use of new kinds of evidential sources (Shostak, 2013, pp. 156-157).
Organizing workshops such as consensus-building forums that aim to convince the relevant stakeholders (such as governmental agencies, funding agencies, private sector participants, environmental justice groups, and practicing scientists), explain the need for molecular methods in performing regulatory toxicological practice (such as the large-scale information forum “the Committee on Emerging Issues and Data on Environmental Contaminants”), and shape users’ and producers’ expectations of toxicological practice (Shostak, 2013, pp. 160-166).

Now, these are important descriptive details regarding how major communities of regulatory toxicologists gradually changed their judgments in favor of the molecularization of regulatory toxicology. I want to highlight two aspects of the case based on this description. First, according to the historical details of the case, the updating of toxicologists’ evidential judgments was mediated by a variety of social and institutional processes working in the background such as disciplinary politics, economic and cultural pressure, and toxicologists’ own attempts to build a new consensus through academic and technical innovations. Second, and by implication of the first, the updating of toxicologists’ evidential judgments was gradual and followed the social and institutional processes I have briefly described.

My contention is that these highlighted observations about the historical details of the case are philosophically significant, and an analysis of the case from the perspective of the inductive risk framework would get conceptually stronger by properly addressing and accounting for them. More generally, I suggest, these aspects of the case are important for our understanding of the nature of toxicologists’ inductive risk judgments. In the following, I will first explain why this is the case and then offer a promising way through which Douglas’ inductive risk framework can address these details.
2.3.4. The Need to Account for the Role of Social and Institutional Processes in the Formation of Toxicologists’ Inductive Risk Judgments

As we have seen, toxicologists consider the pertinent inductive risks in judging the acceptability of the molecular evidential sources in regulatory toxicology. They do so by weighing the epistemic quality of molecular evidence (e.g., additional precision and accuracy in toxicological risk assessment) and the perceived non-epistemic consequences of adopting molecular methods (e.g., making toxicology-based regulation more complicated, slowing the pace of risk assessment, and ambiguating the evidence that is supposed to trigger policies which preserve environmental health). Moreover, we have seen that the content of toxicologists’ inductive risk judgments pertaining to the adoption of molecular approaches has changed over time. Furthermore, the historical details of the case suggest that social and institutional processes have mediated this change.

The role that sociological and institutional processes may play in the formation of scientists’ various decisions are not referenced by the standard presentations of the inductive risk framework such as Douglas’. This is natural because the inductive risk framework is not designed to address the role of social context in the formation of scientists’ judgments. Our question is then the following: How should an analysis of the molecularization of toxicology from the perspective of the inductive risk framework account for the relevant historical facts reviewed above? In other words, what would be a plausible interpretation of the relevant historical details of the molecularization from the perspective of Douglas’ account?

A plausible response would be to deny the philosophical significance of these historical details for an inductive-risk-analysis of the case. It could be that the documented historical facts, which describe how some toxicologists came to accept the relevance of molecular methods and evidential sources, are some nitty-gritty details of precisely how the toxicologists updated their inductive risk judgments. The role of sociological mediating factors, so this response goes, is redundant in an analysis of the case from the perspective of inductive risk. I will ultimately reject this
response. To do so, I will present how the standard presentation of the inductive risk framework can describe the change in toxicologists’ judgments, excluding the details about the role of social and institutional context. Then, I will discuss why the standard reconstruction of the case would be incomplete.

Recall the logic of the inductive risk judgments that I summarized in Section 2. Douglas suggests that scientists consider the inductive risks of their methodological choices based on an assessment of the magnitude of the non-epistemic consequences of making a risky decision and an assessment of the evidential uncertainty (that is, an estimation of how accurate and confirmatory the available evidence is). According to this standard presentation of scientists’ decision-making, a change in the content of scientists’ inductive risk judgments may result from a change in one or two of these variables that enter into the inductive risk calculation. That is, the content of scientists’ inductive risk judgments could follow a change in scientists’ assessment of the magnitude of the adverse consequences stemming from error, i.e., a change in their evaluation of relevant non-epistemic considerations. The change could also follow an update in the confirmatory power of available evidence (for example, by producing more precise and accurate evidence, possibly through technological advancement).

Now, in describing how toxicologists first rejected the molecularization and then later updated this judgment and adopted the molecular types of evidence, we cannot reasonably attribute this to a change in their evaluation of the importance of public and environmental health. Nor can we cogently claim that the major advisory institutions of regulatory toxicology abandoned their pragmatic mission of producing evidence that can be used for regulatory action. Hence, the acceptance of molecular methods is not reasonably attributable to a shift in toxicologists’ assessment of the relevant non-epistemic consequences. Accordingly, if we want to stick to the standard model of the inductive risk framework, we would expect that the shift in toxicologists’ judgment should have resulted from some improvement in the perceived confirmatory power (and epistemic quality) of the molecular tools and evidential sources for the purposes of regulatory toxicology. For instance, toxicologists might have reasoned that accepting molecular kinds of evidence would not only improve the accuracy and
the precision of the toxicological risk assessment but also do so without bearing additional costs for the pragmatic, regulatory function of toxicology. Toxicologists might have, therefore, chosen to raise their standards for evidential quality, as that decision no longer posed serious inductive risks – contrary to what they initially thought.

If this rational reconstruction of the case of molecularization is persuasive, then toxicologists’ acceptance of molecular kinds of evidence can be explained purely in terms of Douglas’ standard presentation of the inductive risk model. The historical details of the case that I highlighted above (that is, how the social and institutional processes mediated the shifts in toxicologists’ judgments) would then be philosophically uninteresting details of precisely how toxicologists made their judgments about the relevance of molecular kinds of evidence for their inquiry. However, I argue that the reconstruction of the case I have just offered is incomplete in an important respect, and that the role of the social context would better be addressed in an analysis of the case in terms of the inductive risk framework. What is then missing in the presented reconstruction of the case of molecularization?

Admittedly, it is true that the molecular risk assessment models in contemporary regulatory toxicology are highly sophisticated such that they effectively fulfill the regulatory and societal purposes of contemporary toxicology. Nevertheless, this sophistication is not directly followed by some drastic advances in molecular technologies. On the contrary, the molecular methods used in contemporary toxicological research were already being used by scholars in neighboring disciplines such as bio-medicine, biology, and even in less applied parts of toxicology (such as the genetic branch of toxicology that deals with the mechanisms of toxicity irrespective of policy-relevant questions) decades before the adoption of these technologies in regulatory toxicology. Applied (regulatory) toxicology appears to be the last scientific field among its neighbors that adopted the molecular methods and made use of molecular evidential sources (see Frickel, 2004 for an exhaustive historical account). Indeed, this fact is precisely why the molecularization of toxicology, or researchers’ initial resistance to it, is such an interesting case to
investigate in the eyes of science studies scholars in the first place (Frickel, 2004; Shostak, 2013). Hence, it is not as straightforward as to say that the molecular methods in toxicology had advanced so much that adopting these new methods no longer bore the risk of making policy-relevant toxicological analysis less practical.

The reconstruction of toxicologists’ reasoning offered in the standard presentation of the inductive risk framework is, thus, not compelling because it does not explain why toxicologists did not adopt the advanced technologies earlier. In my reading, the change in toxicologists’ judgments and the graduality of the change can be explained by reference to institutional and sociological processes that mediate regulatory toxicologists’ acceptance of molecular kinds of evidence. In other words, in the absence of these social and institutional processes, toxicologists’ acknowledgment of epistemic virtues of molecular methods over the pathological methods, and only that, would not be enough to change their inductive risk judgments. Only after these contextual changes took place did regulatory toxicologists gradually update the content of their inductive risk judgments and accept the relevance and aptness of molecular approaches for their purposes. The social and institutional processes (including broad sociological processes or the institutional activities of the communities of toxicologists) therefore played a philosophically significant role in mediating the changes in toxicologists’ inductive risk judgments.

This argument supports the need to account for the social processes for an analysis of the case like the molecularization through the language of the inductive risk framework. It also motivates my proposal to integrate Douglas’ inductive risk analysis with neighboring philosophical accounts that can accommodate the idea that social contextual factors may sometimes scaffold the formation of scientists’ evidential judgments. Discussing how this can be done will be my next task.

In order to accomplish this task, I propose that we do not need to radically transform Douglas’ inductive risk framework such that it also accounts for the relevant social dimensions of scientific reasoning. Such a modification would be unnecessary as Douglas’ account and the broader inductive risk framework are not designed to examine the role of social and institutional factors in scientists’ reasoning, and more
importantly, doing so is not their main philosophical function. The main function of the inductive risk framework is that it gives us a plausible rational account of scientific reasoning in the face of factual uncertainty and the pertinent need to consider pragmatic and moral consequences of scientists’ decisions. In line with Douglas’ compatibilist reading of the literature on values in science (2015), my strategy will be to show that the inductive risk framework is compatible and works in harmony with the relevant philosophical accounts that accommodate the constitutive role the social and institutional processes may play in mediating scientists’ research decisions. I will then demonstrate how such an integrated understanding of the inductive risk framework helps us address the novel aspects of the case I have identified so far.

2.4. Integrating Douglas’ Account with Longino’s Contextual Empiricism

I have so far argued that it is desirable for Douglas’ account and the inductive risk framework to account for the social and institutional processes that mediate scientists’ evidential judgments when examining cases like the molecularization of regulatory toxicology. I have also proposed that a reasonable way to do so is to show how Douglas’ account can be complemented with a relevant philosophical framework that gives special attention to the contribution of social and institutional surroundings to individual scientists’ research decisions. What immediately comes to mind is Helen Longino’s contextual empiricist conception of scientific evidence (1979; 1990; 2008). Longino’s framework is suitable to integrate with Douglas’ account of values in science in the context of our discussion, not only because the former entails a social perspective on scientists’ decisions about evidence (or, more precisely, on the question of why certain objects and states of affairs are considered by scientists as relevant evidence), but also because Douglas (2004; 2009, p. 18) considers Longino’s broader framework as compatible and complementary to her own project. In this section, I will discuss how Longino’s conception of evidence can be integrated with Douglas’s inductive risk framework and how this integrated account works in the analysis of the molecularization case.
2.4.1. Integrating Longino’s and Douglas’ Accounts

Helen Longino has an explicit conception of evidence and evidential reasoning that describes how objects and states of affairs acquire evidential status (1979). Longino famously argues that there is no intrinsic feature of any states of affairs or objects that make them evidential for a given hypothesis, and that states of affairs or objects acquire evidential status for hypotheses or theories only in light of some contextual background assumptions (1990). These background assumptions may sometimes be products of the broader social and cultural context within which scientists work, and the micro-sociological institutional context such as issues like disciplinary politics, methodological conventions, financial and technological constraints, institutional interactions, disciplinary preferences and interests (2008).

Unlike the pessimistic philosophical views about the social dimensions of science, Longino famously put forward that the social character of science is key to understanding the rationality and trustworthiness of scientific inquiries. Most prominently, for instance, she argues that social institutions of science that facilitate critical interactions among the members of a scientific community or members of different communities would eliminate the potential dominance of biased viewpoints over others (1990). In the same vein, in her more recent work (2002), Longino has argued that the social and institutional background context of science, which historical studies of science examine thoroughly, can in principle contribute to the rational progress of individual scientists’ research decisions rather than deteriorate or bias them. According to Longino’s framework, then, toxicologists’ acceptance of new, molecular kinds of data as relevant evidence for their inquiry can plausibly be mediated by the

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3 Longino (2002) contrasts her account with the approaches that consider “the social” harmful to rational decision-making in science. While prominent scholars of sociology of science interpret the documented influence of the social context in scientists’ decisions as evidence of the irrationality of science, prominent philosophers of science have developed normative accounts of science which consider the social factors as deteriorating or irrelevant for the rationality in science. Longino (2002) subscribes to neither of these traditions and instead seeks a cogent reconciliation between these two canonical perspectives.
sociological and institutional processes such as the ones I have reviewed here. In line with Longino’s ideas, these processes can, in principle, play a constitutive role in the sense that they scaffold toxicologists’ inductive risk considerations about the kinds of evidence. I suggest that this theoretical possibility provides us with a basis for integrating Longino’s and Douglas’ accounts.

While Longino’s account supplies a prominent theory of how the social processes in science can plausibly change scientists’ judgments about evidence and methods, Douglas’ account supplies a theory of the principles that make scientists’ inductive reasoning reliable and legitimate in the face of factual uncertainty and the pertinent need to consider moral and pragmatic consequences of scientific judgments.

My suggestion is that these two theories complement each other in describing the case of molecularization I have examined here. Specifically, Longino’s theory is suitable and helpful in describing the case of molecularization because it accounts for the philosophically significant role that social processes played in changing toxicologists’ inductive risk judgments. At the same time, Longino’s framework is complemented by the inductive risk framework, as her own account of values in science does not put forward a theory of individual scientific decision-making under inferential risk.

Now, let’s put this integrative understanding to use and provide an example of how Douglas’ inductive risk framework and Longino's contextual empiricism complement each other in analyzing the case.

2.4.2. Back to the Case of Molecularization

Armed with the inductive risk framework integrated with Longino’s contextual empiricism, we can address some of the puzzling aspects of the case again. Specifically, we can account for why some toxicologists only gradually accepted molecular methods about which they were initially skeptical in a way that is exceptional to Douglas’ theory of scientific reasoning. Moreover, we can supply a plausible explanation of some persisting disagreement between different communities of toxicologists about the acceptability of the molecular approaches to regulatory toxicology.
Remember that regulatory toxicologists were initially skeptical toward molecular kinds of evidence and thought that molecular methods increased inductive risks even though these methods were conducive to a more precise and accurate measurement of environmental health risks. This is, however, puzzling from the perspective of the inductive risk framework, which has been noted by Hicks (2018) as an important counterexample to Douglas’ norm of reasoning under inductive risk, as I have flagged earlier in Section 3.2. While Hicks gives us a plausible explanation of this apparent exception by suggesting that molecular models increase evidential uncertainty at another level (that is, in relation to the question of what ranges of potency are relevant for toxicological risk assessment (Hicks 2018, p. 170), the integrated account proposed here complements Hicks’ interpretation by supplying an explanation of why this kind of apparent exception may have arisen and how toxicologists’ reasoning is still in line with the inductive risk framework. It goes as the following.

Individual toxicologists’ evidential judgments can be intransigent at first but change gradually in response to a set of social and institutional changes, as the case of molecularization manifests. While the observed judgment of toxicologists who were initially skeptical about the adoption of molecular tools seems exceptional to Douglas’ theory of scientific reasoning, the toxicologists working at the major regulatory institutions did gradually update the content of their inductive risk judgments in line with Douglas’s norms, followed by a set of social changes (i.e., active institutional attempts to re-evaluate the contribution of molecular sources to regulatory toxicology and to establish the relevance of molecular tools for regulatory toxicologists). In other words, toxicologists came to accept that adopting molecular approaches does not necessarily increase inductive risks, but this change in their judgment was mediated at least partly by a set of social and institutional processes that helped major communities of toxicologists establish the new methodological standards. Accordingly, once we acknowledge the mediating role of the social and institutional processes in the formation of toxicologists’ judgments, and accept that the updating of toxicologists’ judgments can be a gradual process, then the toxicologists’ observed reasoning does not conflict with Douglas’ inductive risk framework. Douglas’ account of scientists’ reasoning is still descriptively adequate in this context, but the structure
of reasoning described in Douglas’ account seems to be instantiated gradually in practice and scaffolded by a set of institutional and social factors in line with Longino’s constitutive understanding of the social dimensions of scientific judgment-formation.

Consequently, by showing how Douglas’ account is complemented by a suitable philosophical account such as Longino’s contextual empiricism, we can plausibly explain an interesting aspect of the case of the molecularization and do so from the perspective of the inductive risk framework.

The integrated account can also provide us with an explanation of why there are still ongoing disagreements between different communities of toxicologists about the acceptability of molecular approaches to regulatory toxicology. To make this point, I will refer to a concrete example from Hicks’ 2018 study. Hicks (2018) focuses on a recent controversy in regulatory toxicology in which toxicologists assess a set of chemicals as potential endocrine disruptors by using a molecular data-generating model referred to as the ER model. Hicks observes that different toxicologists’ evaluations of inductive risks attached to the use of certain molecular evidence-gathering methods for toxicological risk assessment are inconsistent. Moreover, they report, this is reflected in conflicting estimations of risks calculated by different groups of regulatory toxicologists who made use of the same methods in calculating the risks. Specifically, the NRDC (Natural Resources Defense Council), an environmental advocacy organization, has argued against the use of the molecular model in question because NRDC claims that the method generates more inconclusive data-points than the conventional methods do, however precise and accurate those results might be. Given their purpose of environmental protection, NRDC does not favor the use of the method because, when used for regulatory assessments, it motivates less aggressive regulations of the industrial use of the chemicals in question. Hicks contrasts NRDC’s judgment with that of the EPA-NIEHS. The researchers and regulators at the EPA-NIEHS collaboration are in favor of adopting the molecular method (the ER model). Unlike NRDC’s judgment, EPA-NIEHS’s use of the same model generates less inconclusive evidence and motivates a more aggressive
regulatory action when used for justifying regulatory decisions. Hicks reports that the
difference between the two judgments results from “different ways of handling
inconclusive chemicals” that score low-range response rates. While EPA-NIEHS’s
methodological approach allows researchers to interpret such data as evidence of
toxicity, NRDC’s methodology interprets the same results as “inconclusive” (Hicks,

Hicks hypothesizes that the discrepancy between the views of two groups of
toxicologists might be the result of a difference in how much they value the protection
of the environment: “it is highly plausible that [NRDC’s] calculations are directly
motivated by their concern to protect human health and the environment” (p. 170).
But, at the same time, they rightly observe that NRDC’s resistance to EPA-NIEHS’s
approach is notable because “EPA-NIEHS’s interpretation of inconclusive chemicals
would be much more protective of these values” (p.170).

Since Hicks is interested in another aspect of the case he focuses on, they stop there
and do not question the persistence of this disagreement. But, the disagreement
between these groups of toxicologists about the content of inductive risks is hardly
attributable to differences in evaluations of relevant non-epistemic values. Indeed,
EPA and NIEHS are also explicitly motivated by the concern to protect environmental
health.

The integrated account I have proposed here supplies another plausible hypothesis
that explains the disagreement between these two groups of toxicologists, which again
complements Hicks’ analysis. In my reading of the example, it is plausible that the
two groups of toxicologists have gone through different institutional processes. EPA
and NIEHS have reached the judgment that the use of molecular methods is
effectively conducive to the protection of health, and they have built a new set of
shared methodological and technical standards regarding how to use molecular tools
for the regulatory purposes of toxicology effectively. (Recall the social and
institutional processes that mediated the formation of this consensus, such as those
described by the historical studies of toxicology or the activities such as the formation
of the ToX21 collaboration). In the context of Hicks’ case study, EPA-NIEHS’s
judgment is about how to interpret low-range response rates that are generated through the use of molecular methods. While the EPA-NIEHS consensus advises to count low-range potency rates as conclusive evidence of toxicity in the context of endocrinal disruption studies, NRDC does not seem to accept this new methodological standard even though doing so would be more protective of the non-epistemic values that they aim to protect.4

Accordingly, it is a plausible hypothesis that the researchers at NRDC have not participated in those social and institutional processes that helped the researchers at EPA-NIEHS update their inductive risk judgments about the acceptability of the molecular approaches. Recall, for instance, the consensus-building forums that aimed to introduce and promote the use of new methods in regulatory toxicology, which prominent institutions of regulatory toxicology such as NIEHS initiated (Shostak, 2013, pp. 160-164; see also p. 176 for an example of such forums with the relevant environmental justice groups). From the perspective of the integrated account of the inductive risk framework presented here, such forums are good examples of institutional processes that could help the researchers from environmental advocacy groups such as NRDC share or update their methodological judgments.5

4 In this specific example, it seems rational for NRDC to subscribe to EPA-NIESH’s acceptance of the molecular method (the ER model) and the pertinent methodological standards. Note, however, that my account does not purport to make a normative assessment of the success of regulatory toxicologists’ changing judgments or the quality of the institutional and social processes they have attended to. Engaging in such an evaluation would require one to qualify the integrated account proposed here in normative terms; specifying the conditions under which the mediating social processes are conducive to reaching the right or desirable content of inductive risk judgments. Obviously, one could ask, for instance, how far Longino’s normative criteria for social interaction between scientific communities (such as enhanced inclusiveness) are fulfilled in the case of molecularization (Longino, 1990); or to what extent the financial/industrial interests have permeated into the social and institutional processes similar to the ones described here, as it would rightly be questioned by many philosophers of science (e.g. Elliot, 2014). I thank an anonymous referee of this journal for encouraging me to highlight these important questions that I do not address in this article.

5 It is also intriguing to analyze how and why the mediatory role of institutional and social processes becomes dysfunctional or fails to generate (desirable) forms of agreement between different communities of scientific researchers in this case or in similar cases. This is another issue I do not attempt to analyze in this article.
This example is yet another demonstration of why the mediating role of social and institutional processes is philosophically significant for our understanding of toxicologists’ inductive risk judgments, and why integrating Douglas’ account with suitable philosophical frameworks, such as Longino’s contextual empiricism, is useful to reflect on the cases such as the molecularization of toxicology.

2.5. Conclusion

We have seen that the prominent regulatory toxicologists’ choice of molecular kinds of evidence over traditional ones is informed by some assessment of how suitable the different methods are for their mission of protecting public health and the environment. In particular, that assessment involves accounting for non-epistemic considerations such as the seriousness of potentially harmful consequences that could follow from toxicologists’ choices over different kinds of evidence (e.g., slowing the pace of producing regulation-related analysis or making toxicological assessments less intelligible for the regulatory action). The case I have presented here, therefore, instantiates a context in which the inductive risk framework applies to scientists’ choices across different types of evidence.

In this context, I have offered a description of how and why the content of regulatory toxicologists’ inductive risk judgments changed over time, drawing on the relevant historical studies of contemporary regulatory toxicology. In doing so, I have focused on some aspects of toxicologists’ judgments that Douglas’ inductive risk framework is not purported to address, namely the initial rigidity of their evidential judgments, and the graduality of the updating of these judgments, and the role of institutional and social processes in mediating toxicologists’ judgments. I have argued for the philosophical significance of these aspects of the case and suggested that Douglas’ account, and the broader inductive risk framework, should be able to suitably address them.

I have proposed an account that integrates Douglas’ inductive risk account and Longino’s contextual empiricism as a suitable philosophical account of evidence that can accommodate the idea that social contextual factors may sometimes plausibly
mediate scientists’ evidential judgments. I have then shown how Longino’s contextual empiricism and the inductive risk framework fruitfully complement each other in analyzing the specific questions the toxicology case raises, which are also of interest to the specialized philosophical literature.

The case of the molecularization of regulatory toxicology motivates the need to consider how our sophisticated philosophical and historical accounts of scientific judgment relate to each other. And, the integrated account proposed here instantiates an exploration where such connections between Douglas’ account of values in scientific reasoning and Longino’s contextual empiricism are drawn and then put into use to describe and understand toxicologists’ acceptance of molecular approaches to regulatory toxicology.
References


Integrating H. Douglas' Framework with an Account of Scientific Evidence


Chapter 2


Part II
Philosophy of Evidence Based Policy from the Perspective of Values in Science
Behavioral Policies and Inequities: The Case of Incentivized Smoking Cessation Policies

3.1. Introduction

Behavioral policies aim to change people’s behavior through interventions to choice contexts and psychological mechanisms of decision-making. Some well-known examples of behavioral policies are nudges (Thaler & Sunstein, 2008), boosts (Hertwig & Grüne-Yanoff, 2017), and behavioral-economics-informed incentivized programs for behavior change (Loewenstein & Chater, 2017). A growing body of research investigates the justifiability of behavioral policies. Behavioral economists, law scholars, public policy specialists, and those invested in evidence-based policy aim to identify the ethical, scientific, and institutional grounds for putting behavioral public policies into practice.

There is also a distinct philosophical literature on behavioral policies. Philosophers raise ethical worries about behavioral policies by, for instance, discussing the
desirability of the normative principles these policies aim to instantiate and the attainability of these goals by different types and tokens of behavioral policies (e.g. Bovens, 2010; Hausman and Welch, 2010). Philosophers also raise epistemic worries about behavioral policies, asking “how do we know that a behavioral policy is successful in achieving a desideratum in a certain environment?” (Heilmann, 2014; Barton & Grüne-Yanoff 2015; Grüne-Yanoff, 2016; Grüne-Yanoff et al., 2018). The arguments focusing on this methodological question have been informed by the theories of human action underlying behavioral policies, such as the dual systems approach (Kahneman, 2011), the methodological accounts of experimental social science (e.g. Guala 2005; Steel, 2008), and the philosophical literature investigating the epistemic and methodological requirements for the evaluation of evidence-based bio-medical and social policies (Clarke et al., 2014; Hardie & Cartwright, 2012; Cartwright, 2012).

In this chapter, I contribute to this growing body of specialized methodological literature that assesses how empirical researchers predict and evaluate the success of behavioral policies (see Grüne-Yanoff, 2016 for a paradigmatic account). A common worry raised by the commentators in this literature is that the methodology used for evaluating behavioral policies is not conducive to a comprehensive analysis of these policies, as the evidence typically gathered establishes that these policies work, but without indicating how they do. Here, the focus is on the adequacy of Randomized Controlled Trials (RCTs) in assessing behavioral public policies. The use of RCTs, so the critics argue, does not serve researchers in investigating long-term, unintentional or distributional consequences of behavioral policies without evidence regarding how they work.

I address this literature through an analysis of how Incentivized Smoking Cessation Programs (*henceforth*, ISCP) are evaluated. ISCP are prominent examples of evidence-based behavioral health policies (Bhargava & Loewenstein, 2015, p. 400; Loewenstein & Chater, 2017; Cabinet Office UK, 2011; Volpp et al., 2011). ISCP are thoroughly investigated or implemented in the UK and US in the last ten years. My analysis of ISCP is illuminating for the methodological literature for several reasons.
First of all, ISCP are instances of a prominent type of behavioral public policies, so called “incentivized behavioral policies”, that so far have not been investigated by the specialized methodological literature. Secondly, and more importantly, the evidence-based evaluation of ISCP, as it is practiced in the UK, offers us rich resources for investigating specific methodological issues. Specifically, the evaluative perspectives and questions engaged in the evaluations of ISCP tend to be more comprehensive, unlike the evaluations of other commonly known behavioral policies, such as nudges. This includes the alternative methodological approaches to the use of RCTs.

I focus on the evaluation of ISCP's impact on health inequities, hence the assessment of ISCP’s long-term effectiveness across specific sub-groups in the population. I argue that RCTs, when combined and synchronized with different evidence gathering methods, have distinct advantages in delivering inequity-relevant evidence over primarily RCT-based evaluations. More generally, I contend that this example gives us a reason to believe that a more pluralist evaluative methodology for behavioral public policies rectifies some of the commonly stated methodological limitations of primarily RCT-based extant behavioral policy evaluations.

The chapter proceeds as follows. In section 2, I introduce and explain what Incentivized Smoking Cessation Policies are and argue that assessing how ISCP are evaluated affords promising insights that are relevant for the methodological literature on the evaluations of behavioral public policies. In section 3, I explicate what it means to evaluate a policy’s impact on health-inequities, and how ISCP are evaluated in this respect. In section 4, I assess the extant evaluative practices for ISCP’s impact on health-inequities and argue for a pluralist methodology. In section 5, I reflect on the implications of this analysis for the philosophical literature on the evaluations of behavioral public policies. In section 6, I conclude by emphasizing the importance of pluralism of evidence gathering methods and the community of researchers’ capacity to synchronize diverse methods to achieve more comprehensive evaluations of behavioral public policies.
3.2. Incentivized Smoking Cessation Programs (ISCP)

3.2.1. What are ISCP?

It is well documented that smoking contributes to the development of serious non-communicable diseases such as type 2 diabetes, respiratory and cardiovascular diseases, and lung cancer (WHO, 2011). Most smokers acknowledge such malignant health consequences of smoking. Yet, they often fail in their attempts to quit. There are various policy instruments to influence people’s smoking behavior. These include taxation of tobacco consumption, health-information campaigns, promotion of anti-smoking culture, and more coercive forms of regulation such as limiting the supply of tobacco and mandating smoking-free zones in cities. However, smoking remains a significant public health problem (e.g. NHS, 2017). Governments, therefore, actively seek new approaches to smoking cessation policies (e.g. Department of Health UK 2010, 2011; Commission on Social Determinants of Health [CDSH, WHO], 2008).

Behavioral economics offers a novel approach to smoking cessation (Loewenstein et al., 2007; Dolan et al., 2010; Volpp et al., 2011; Loewenstein et al., 2012; Cabinet Office UK, 2004; 2011). A well-studied example of the behavioral-economics-inspired smoking policies is that of Incentivized Smoking Cessation Policies (henceforth, ISCP) (e.g. Volpp et al., 2009; Halpern et al. 2015; Sunstein, 2015). ISCP promote quitting through monetary or non-monetary rewards. The supporters of ISCP (such as the Behavioral Insights Team in the UK, and authors such as Kevin Volpp, George Loewenstein, and Cass Sunstein) consider them applicable to small-scale environments such as firms or neighborhoods. They also consider ISCP as decentralized policies: implementers of ISCP could be a regional health service agency, or a non-governmental organization, or a private company. ISCP have been found attractive mostly in the US and in the UK. In the context of the US, large private

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1 The numbers cited by the WHO report about the non-communicable diseases around the globe are striking: “Almost 6 million people die from tobacco use each year, both from direct tobacco use and second-hand smoke […] Smoking is estimated to cause about 71% of lung cancer, 42% of chronic respiratory disease and nearly 10% of cardiovascular disease” (WHO, 2011, p.1).
companies have an interest in smoking cessation among their employees, as it reduces the insurance costs (Strickland, 2014). In the context of the UK, governments find it more desirable to approach smoking cessation at the regional, rather than national scale (Department of Health UK, 2011).

Some unsophisticated variants of ISCP were already implemented in the UK. For instance, the Quit4U program in Scotland (Ormston et al., 2014) and the Give It Up For Baby program in the UK (Ballard & Radley, 2009; Radley et al., 2013) used financial rewards (such as food and shopping vouchers) to reduce smoking in socio-economically disadvantaged smokers. Based on results from behavioral economics, the proponents of ISCP suggest that the way in which incentives are presented can make a difference in changing smoking behavior (Lowenstein et al., 2012; Adams et al., 2013). It is, therefore, important not to conflate those standard policy-interventions that alter financial incentives with the behavioral incentivized policies that alter incentives in sophisticated ways, based on evidence regarding the cognitive or psychological models of decision-making.

Consider, for instance, the finding that explicit financial rewards, such as direct cash payments or holiday vouchers, are more likely to lead to behavior change than ‘relatively more invisible incentives’ of the same magnitude, such as costs tied to insurance premiums (Strickland, 2014; Volpp et al., 2009). The more salient the incentives are, the more likely the behavior change will be, or so the proponents of ISCP argue. Similar to the salience or visibility of incentives, there are other and much more complicated aspects of incentive-provision for behaviour change that the proponents of ISCP deem helpful for designing and implementing ISCP (see Tversky & Kahneman 1974; Congdon et al., 2011; Bhargava & Loewenstein, 2015 for the relevant behavioral economic literature). Importantly, behavioral economic studies suggest that people who engage in non-volitional health behaviors (such as smoking, binge-eating, and excessive gambling) might suffer from the effects of ‘cognitive biases’ that most humans have,2 and that policy makers can make use of these biases

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2 For example, the so-called ‘peanut effect’ consists in underestimation of cumulated long-term effects of minor negative health behavior (Weber and Chapman, 2005). ‘Present bias’ is the
for incentivized behavior change by harnessing them through offering incentives in varying immediacy, duration, frequency, or timing. Accordingly, the proponents of ISCP contend, for instance, that increasing the immediacy and frequency of incentives may render the incentivized quitting more likely to be effective, as it harnesses certain known cognitive biases of smokers (such as ‘choice-bracketing’ and ‘present bias’; see Loewenstein et al., 2012 for a review).

At first glance, it seems that ISCP are promising evidence-based instruments for smoking policy that should perhaps be used much more widely. Should, for instance, governments encourage private companies and regional health services to adopt ISCP? In the rest of the paper, I will examine the methodological dimension of this question, leaving important ethical and political issues regarding ISCP aside (see Bovens, 2016; Schmitt, 2016; and Kelly, 2016 for a discussion of these matters). Indeed, I am solely interested in the question of whether ISCP should be adopted on evidential grounds; more specifically, which evidential sources warrant a positive evidential assessment of ISCP. It is also important to note that by “an evidential assessment of ISCP” or “an evaluation of ISCP’ impact”, I mainly refer to an ex-ante evaluation of evidence-base for the adoption of ISCP.3 To this end, I will critically examine the methodologies researchers currently employ to evaluate ISCP. I will then discuss whether the employed evaluative methods are entirely adequate to assess these policies.

3.2.2. The importance of ISCP evaluations

While philosophers have analyzed the evaluations of well-studied types of behavioral public policies such as boosts and nudges (Grüne-Yanoff, 2016; Grüne-Yanoff et al., forthcoming), ISCP can neither be defined as boosts nor nudges. Nudges and boosts

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3As I will explain in section 3, it is possible that an ex-post evaluation of evidence-base for an already-implemented ISCP is considered as an evidential source in an ex-ante evaluation of ISCP (e.g. the use of systematic reviews in the evaluations of ISCP).
alter people’s cognitive biases and heuristics to change behavior while leaving the incentives intact. ISCP and other incentivized behavioral policies do not fit this definition because they alter people’s cognitive biases and heuristics in order to increase the effectiveness of an incentive-altering intervention or an incentive-structure existing in the target environment.

The categorical difference between nudge/boost and incentivized behavioral policies does not render the latter less of an important type of behavioral public policy, for two reasons. Firstly, incentivized behavioral policies are categorically different than the traditional use of incentives in policy making, because the former make use of behavioral economic insights about how people respond to different ways of presenting incentives and information. Secondly, and more importantly, the proponents of the nudge agenda regard incentivized behavioral policies as a prominent type of behavioral public policies. Loewenstein and Chater (2017), for example, argue that behavioral public policy making should not be equated with nudging because the majority of available tokens of behavioral-economics-inspired policies are not instances of nudge (and nor of boosts for that matter), but rather of incentivized behavioral policies (see also Chetty, 2015; Bhargava and Loewenstein, 2016 for similar arguments).

How incentivized behavioral policies are evaluated has not yet been investigated in the methodological literature focusing on the evaluation of behavioral public policies. ISCP certainly are prominent and well-referenced examples of this latter category of behavioral policies (see, for instance, Sunstein 2015, Bhargava and Loewenstein 2016). An investigation of how ISCP are evaluated is, therefore, an important addition to the methodological literature in its own right. Beyond these, there are two more aspects of ISCP and their evaluation that render them interesting.

4 Loewenstein and Chater (2017) also argue that incentivized behavioral policies have a wider range of applicability than nudging because the conditions for the proper applications of ideally defined nudges are harder to instantiate in practice than that of incentivized behavioral policies.
Firstly, ISCP are evaluated by a wider range of evaluators than the range of evaluators corresponding to nudges or boosts. Nudges (and boosts, for that matter) have so far been evidentially assessed primarily by behavioral economists or behavioral scientists. As I will demonstrate in the next section, incentivized health policies such as ISCP, on the other hand, are evaluated not only by behavioral economists (e.g. Loewenstein et al. 2007; Giné et al., 2010), but also by public policy scholars specialized in smoking cessation (e.g. Halpern et al., 2015), social epidemiologists (e.g. Adams, 2009), scholars from bio-medicine and preventive medicine (e.g. Bickel et al., 2016; Majestko et al., 2016), as well as prominent systematic reviewers of evidence-based public health interventions such as the Cochrane Collaboration, Campbell Collaboration, King’s Fund, and NICE (e.g. Cahill et al., 2015; Jochelson, 2007).\(^5\) Importantly, the plurality of evidential evaluators of ISCP also implies a plurality of policy desiderata with respect to which behavioral public policies can and should be evaluated. For instance, the evidential evaluations of nudges have so far primarily focused on the question of whether nudges are effective, or the extent to which they are. The methodological literature thereafter questioned the extent to which the evaluations of nudging realizes the evaluative goal of short or long term effectiveness (e.g. Grüne-Yanoff, 2016). But the evaluators of ISCP, as we will see, are focusing on further evaluative goals of ISCP, such as the effectiveness of these policies in specific subgroups in a population, or in reducing smoking-related health inequities, or the minimization of unintended consequences (side effects).

Secondly, the evaluations of ISCP, as they are practiced in the UK, are based on a *plurality of methods*, which makes the case of evaluating ISCP different from the evaluation of nudges.\(^6\) The evaluation of ISCP is primarily based on evidence gathered through randomized controlled trials (RCTs) and systematic reviews of

\(^5\) At least, this is the case in the UK where the comprehensive evaluation of ISCP is mostly practiced.

\(^6\) Grüne-Yanoff (2016) argues that the evaluations of nudges are primarily based on lab and field experiments.
RCTs.\footnote{See, for example, Halpern et al. (2015) as a typical example of an evaluation of ISCP based on RCTs and Cahill et al. (2015) for a prominent example of a systematic review which evaluates the Halpern et al.’s study along with other empirical investigations of ISCP.} Systematic reviews of ISCP typically excludes or gives lower grades to non-randomized trials. Yet, importantly, some reviews consist of studies that make use of non-experimental and observational evidence gathered such as through in-depth interviews.\footnote{See Morgan et al. (2015), Thomson et al. (2014), Adams et al. (2013), Graham et al. (2012), for an examples of systematic reviews of ISCP that make use of non-experimental evidence.} Just as the extant methodological studies questioned in how far the evaluation of nudging is practiced on sound methodological grounds, we may also ask in how far the evaluation of ISCP is based on the adequate use of the evidence gathering methods available. As the background research regarding the evaluation of public health policies tends to be comprehensive and well-documented in the UK, the evaluations of ISCP help us to focus on specific methodological problems regarding the use of experiments in the evaluation of behavioral policies discussed in the literature.

I have so far argued that we have good reasons to investigate the evaluation of ISCP from a methodological perspective. In the rest of the article, I will focus on the evaluation of ISCP’s \emph{impact on health inequities}. The evaluation of ISCP’s impact on health inequities helps to further analyze the interesting and novel characteristics of the evaluation of incentivized behavioral policies that I listed above. More broadly, the health inequity focus will allow me to reflect on the question of whether behavioral public policies, as a new type of public policy, alter existing inequities and how this aspect of behavioral public policies should be evaluated.

In the next section, I will first explain what it means to evaluate a public policy’s impact on health-inequities. I will then review what we know about the evaluation of ISCP’s impact on health-inequities, introducing diverse perspectives of different kinds of researchers who provide us with relevant information for the assessment of how ISCP are evaluated.
3.3. The Evaluation of ISCP with respect to Health Inequity

3.3.1. What Does It Mean to Evaluate a Policy’s Impact on Health Inequity?

What does the term “health-inequity” mean? To start with, inequities in health outcomes should not be conflated with inequalities in health outcomes. Health inequalities are measurable differences in health outcomes across different populations and individuals. Health inequities, on the other hand, are health inequalities that result from people’s unequal access to health services and capabilities to sustain healthier lives. For instance, health inequities in health outcomes in a country may result from its citizens’ differential access or ownership of health services, nutritional sources, or health-related social capital. Being in worse health condition due to these disadvantaging and contextual factors is different than being worse-off due to unchangeable biological factors or volitional preferences. Hence, health inequities are commonly regarded as unnecessary, avoidable, unfair, and unjustifiable inequalities in health outcomes, which should be addressed by public policy interventions (O’Neill et al., 2014; WHO 2012; Tugwell et al., 2006; Whitehead, 1992).

An effective health policy intervention may not be successful in reducing the inequalities in health outcomes between the most and the least disadvantaged individuals. For instance, it might be that an intervention is effective overall but ineffective or less effective for disadvantaged people. Similarly, a specific way to offer a public health intervention may discourage certain group of individuals from taking up the treatment, although that was not intended by the policy designers. The resulting inequities may result directly from the intervention itself or may appear as one of the already existing inequities prior to the intervention but exacerbated by the intervention (Lorenc et al., 2014). Health inequities may, therefore, remain intact or exacerbate due to the mistakes in the implementation and design of public health policies as well as the knowledge gaps in the evaluation of these policies.
Accordingly, when I talk about “evidence-based evaluation of policies with respect to inequities”, I refer to the empirical investigation of the potential ways in which a policy may generate inequities once it is implemented or exacerbate a known inequity. These kinds of investigations, then, aim at detecting if there are any inequity-relevant mistakes in the implementation of the policies in question, or if there are knowledge gaps related to that (see O’Neill et al., 2014 for a more detailed description).

Our question is: what sort of methodological practices lend us the evidential basis for making this sort of judgment about ISCP and similar behavioral public policies? I now turn to answering it.

3.3.2. A Plurality of Perspectives for the Evaluation of the ISCP’s Impact on Health-Inequities

We now have fixed an understanding of what it means to evaluate a policy’s impact on health inequity. I will now present an overview of how ISCP are evaluated in this regard. As we will see, it would be misleading to characterize the assessment of ISCP as a scientific activity that is governed by a homogeneous set of methodological principles, advising the use of a single method such as RCTs. As I articulate in this section, it is more appropriate to describe ISCP’s assessment as a scientific activity that involves different Types of Researchers who have different evaluative goals and who make use of different evidence gathering methods in evaluating ISCP. Each type of researcher delivers some evidential output that is relevant for the assessment of ISCP with respect to inequities. It is precisely this plurality of evaluative and methodological perspectives that makes the investigation of ISCP interesting for informing the methodological debates regarding the evaluation of behavioral public policies. In the following, I will first describe the evaluation of ISCP with respect to inequities, demonstrating how the plurality of perspectives plays an important role. I will thereafter focus on the methodological lesson we should draw from this practice.

Table 3.1 offers a structured summary of my characterization of the specialized literature evaluating ISCP. I list different Types of Researchers who contribute to the evaluation of ISCP. I also give a few Representative Examples for the respective type
of researchers. I indicate *Primary Evaluative Goals* that the different types of researchers aim for. I specify *Evidence Gathering Methods* the different types of researchers commonly use to gather the evidence in question. Finally, I mention some *References* from the literature that report or exemplify the kind of research in question.

**Table 3.1:** The characterization of the specialized literature evaluating incentivized smoking cessation policies: a plurality of evaluative and methodological perspectives.

<table>
<thead>
<tr>
<th>Type of Researchers</th>
<th>Representative Researcher</th>
<th>Primary Evaluative Goal</th>
<th>Evidence-Gathering Methods</th>
<th>References</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Decision-Makers</strong></td>
<td>National Health Services (NHS) in the UK</td>
<td>Specify evidence required for policy making (e.g. ISCP’s effect on health inequities)</td>
<td>Behavioral economic theory, experimental evidence, RCTs</td>
<td>CSDH (2008), Petticrew (2004), Department of Health (2011)</td>
</tr>
<tr>
<td><em>The Proponents of Behavioral Policies</em></td>
<td>Behavioral Insights Team in the UK</td>
<td>Defend behavioral policies such as ISCP based on evidence and propose new tokens of behavioral policies</td>
<td></td>
<td>Haynes et al. (2012), Dolan et al. (2010)</td>
</tr>
<tr>
<td><strong>Evidence-based Policy Specialists</strong></td>
<td>(a) RCT specialist Policy Evaluators (e.g. Kevin Volpp, Scott Halpern) (b) Public Health Evaluators (e.g. Jean Adams, Heather Morgan, Gill Thomson NICE in the UK, Higgins)</td>
<td>Measure the ISCP’s (and similar incentivized policies’) impacts such as effectiveness, persistence, cost-effectiveness, unintended consequences, effects on specific populations</td>
<td>RCTs, Observational Methods, Systematic reviews, Mixed methods</td>
<td>Halpern et al. (2015), Volpp et al. (2009), Morgan et al. (2015), Higgins and Solomon (2016)</td>
</tr>
<tr>
<td><strong>Behavioral Economists</strong></td>
<td>George Loewenstein, Paul Slovic</td>
<td>Pursuing empirical and theoretical knowledge about the psycho-cognitive factors that determine the differential effects of incentives and information across different types of smokers.</td>
<td>Theory, lab and field experiments (methodologically individualist orientation).</td>
<td></td>
</tr>
<tr>
<td><strong>Social Epidemiologists</strong></td>
<td>Micheal Kelly, Hilary Graham, Jennie Popay, Stanley Blue</td>
<td>Empirical and theoretical knowledge about the structural factors that determine various aspects of smoking behavior, the health inequalities and differential effects of public health policies.</td>
<td>Theory, observational methods including econometrics descriptive statistics and qualitative methods (with a holistic methodological orientation)</td>
<td>Kelly (2010), Graham (2011), Popay (2008), Blue et al. (2016)</td>
</tr>
<tr>
<td><strong>Systematic Reviewers</strong></td>
<td>Cochrane Collaboration, Campbell Collaboration, King’s Fund</td>
<td>(i) Review, rate, and report available evidence on various impacts of ISCP; (ii) inform the design of new evidence-based policy analysis (e.g. by generating new hypothesis, defining gaps in evidence)</td>
<td>All of above and methods of structured review</td>
<td>Cahill and Perera (2011, 2015), Jochelson (2007)</td>
</tr>
</tbody>
</table>
Table 3. 1 characterizes a number of categories for each of the Type of Researchers listed on the left. Let’s start with Decision-Makers. Decision-Makers are policy makers or public policy agents who seek information to determine the justifiability of implementing ISCP in a certain context. Although Decision-Makers are, strictly speaking, not types of scientific researchers, they search for scientific consultants, commission reports of available evidence, and seek evidence-based arguments for making particular types of policy making. Two good examples of Decision-Makers in the context of ISCP are the NHS or the UK Department of Health, who not only consider political, ethical, economic or other, pragmatic, concerns for implementing ISCP, but also seek information about available evidence concerning the impact of ISCP (UK Department of Health, 2011). As policy makers, Decision-Makers demand certain types of evidential output, which then indirectly determines the kinds of evaluative goals the other Types of Researchers seek to deliver. For instance, evidence regarding health inequities is undoubtedly very important for Decision-Makers all around the world (Petticrew et al. 2004, CSDH 2008, UK Department of Health 2011). More specifically, smoking control is one of those areas in public health where the evidence on policies’ impact on specific disadvantaged groups and inequities is highly important.9 Because there are widely accepted smoking-related inequities in health inequities across various socio-economic-demographic strata, health policy’s success in reducing health inequities is crucial to investigate.

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9 Statistical data show that although overall smoking has decreased over time in many countries, smoking prevalence is unevenly distributed across different socio-economic classes in all countries (European Commission, 2015; NHS, 2017). For instance, according to the most recent statistics in the UK, where overall adult smoking prevalence is lower than in many other European countries, socioeconomically disadvantaged citizens are more likely to be a smoker than affluent ones (classified as those with lower income, less education, routine and manual jobs; as those with higher income, higher education, managerial and professional jobs; respectively) (NHS, 2017, part 3). In the same way, the influence of being an unemployed person or a member of an ethnic minority in the UK increases the likelihood of being a smoker. Moreover, smoking during pregnancy is highest in the economically poorest regions of the country. Also, smoking during pregnancy is more common among young mothers who are likely to experience financial and social distress. Similar facts about the socio-economic distribution of tobacco use also hold for other West-European countries where overall smoking prevalence is very low in comparison to the rest of the world (Verdurmen et al., 2015; Baha et al., 2016).
Who then are the *Types of Researchers* that are supposed to assemble the evidence relevant for the assessment of ISCP with respect to health inequities? Firstly, there are the *Proponents of Behavioral Public Policies* who advocate ISCP. Secondly, we have the *Evidence-based Policy Specialists* who assess ISCP and similar policy proposals based on evidence. Thirdly, we have *Systematic Reviewers* who review, rate, and report available evidence-based assessment of ISCP. Finally, there are different kinds of scientists associated with different research disciplines such as *Behavioral Economists and Social Epidemiologists* who provide all the other *Types of Researchers* with the relevant theoretical and empirical input gathered through the practicing so called “primary science” regarding smoking behavior and health inequities. Let’s first focus on *Proponents of Behavioral Policies*.

The *Proponents of Behavioral Policies* are policy researchers who advocate a particular policy approach, that is the behavioral approach. They aim to inform or else convince *Decision-Makers* based on scientific evidence regarding the performance of behavioral policies. In the context of behavioral public policies and ISCP, the researchers in the Behavioral Insights Team (BIT) of the UK are a good example of this category of researchers. BIT aims to justify behavioral public policies based on evidence. Hence, its main evaluative goal is to gather or report evidence that supports various tokens of behavioral public policies such as ISCP. Now, it is important for our discussion to note that BIT in the UK, and similar so-called Nudge Units around the world, adopt a particular methodological strategy to evaluate behavioral policies. That strategy prioritizes RCTs as *the* evidence gathering method that should be used for evaluating behavioral policies. This strategy is often explicitly stated. For instance, in one of BIT’s methodological reports, the researchers claim that “Randomized Controlled Trials are at the heart of the Behavioral Insights Team’s methodology’ and that ‘RCTs are the best way of determining whether a policy is working’” (Haynes et al., 2012, p. 4). We now have a more detailed characterization of the *Proponents of Behavioral Policies* in the context of UK, and their methodological strategy. Let’s focus on the next type of researchers engaged in the evaluation of ISCP: *The Evidence-based Policy Specialists.*
The *Evidence-based Policy Specialists*’ primary aim is to measure ISCP’s (and similar Incentivized Policies’) success with respect to various policy desiderata. Effectiveness is their most commonly presupposed policy desideratum; however, other evaluative goals such as investigating a policy’s cost-effectiveness, persistence, unintended consequences, and impacts on specific populations or inequities are also pursued. I consider all advanced researchers who pursue this kind of evaluative goal as evidence-based policy specialists. However, for the purpose of characterizing diverse methodological lines in the ISCP’s evaluation with respect to inequities, I focus on two qualitatively different examples of *Evidence-based Policy Specialists*: (i) RCT-specialists and (ii) Public Health Evaluators.

Similar to BIT’s methodological approach, the empirical literature evaluating ISCP is marked by the prominence of randomized trials comparing the effectiveness of various ISCP (e.g. Halpern et al., 2015). *RCT-specialists* evaluating ISCP are primarily interested in determining whether a token of ISCP is effective in an environment for a particular target. As it is well-known, RCTs work perfectly in pursuing that particular evidential output. Hence, it is no surprise that *Evidence-based Policy Specialists* who investigate the effectiveness of ISCP for smoking cessation base their research primarily on RCTs.

*Public Health Evaluators*, on the other hand, are akin to social policy scholars who assess health policies from the perspective of public health. Hence, their primary research interest and evidential output goes beyond the specification of ISCP’s effectiveness and includes the specification of ISCP’s effectiveness in certain groups in a population; for instance, ISCP’s impact on inequities, or ISCP’s cost-effectiveness, or other pieces of evidential output relevant for justifying health policies. *Public Health Evaluators* are therefore not a homogenous category of researchers. Rather, they occupy roles in different disciplines such as epidemiology, social epidemiology, social policy, economics, sociology, preventive medicine, etc. This variety is then reflected in the different evidence gathering methods employed by Public Health Evaluators across disciplinary backgrounds. For instance, researchers in NICE tend to use qualitative and observational types of evidence.
gathering methods, which are pertinent to disciplines such as epidemiology, social epidemiology, or sociology of health (e.g. NICE, 2007). Preventive medicine scholars, on the other hand, tend to pursue their primary evaluative goal regarding ISCP through laboratory experiments (e.g. Higgins et al., 2012).

I should emphasize that these two categories of example researchers, *RCT-specialists* and *Public Health Evaluators*, are not mutually exclusive. In other words, an RCT-specialist may also be a public health evaluator (e.g. Jean Adams). I distinguish between the two in order to emphasize the following point: *Evidence-based Policy Specialists* use different evidence gathering methods for evaluating various aspects of ISCP. More generally, that is to say, *Evidence-Based Policy Specialists* as a type of researcher does not correspond to a homogenous body of researchers with respect to the primary evaluative goal and evidence gathering methods used.

The next *Type of Researchers* are scientists who deliver primary theoretical input and empirical evidence relevant to the evaluation of ISCP. Since smoking cessation is a complex and multi-faceted scientific subject, there are many such different kinds of “primary scientists” involved. As they are the most relevant ones for the assessment of ISCP’s impact on health inequity, I focus here on *Behavioral Economists* and *Social Epidemiologists*. Behavioral economists assemble empirical and theoretical knowledge on the psycho-cognitive factors that determine the differential effects of incentives and information across different types of smokers (Loewenstein et al. 2012; Gine et al., 2010). *Social epidemiologists*, on the other hand, analyze the impacts of social-structural factors on individual, population health states, health-related social practices and health behavior (Honjo, 2004). Social-structural factors are commonly referred as “wider determinants of health” by epidemiologists. Social epidemiologists generally make use of observational evidence gathering methods to investigate health inequities.

Finally, we have the *Systematic Reviewers* who review, rate, report and evaluate available evidence about various impacts of ISCP. The most prominent examples of *Systematic Reviewers’* research in the context of health interventions and ISCP are assembled by major evidence-based policy institutions in public health and
biomedicine such as the Cochrane Collaboration, Campbell Collaboration, and Kings’ Foundation in the UK. Systematic Reviewers play a major, arguably the most crucial, role in the evaluations of health interventions. The most explicit and the main contribution of Systematic Reviewers is the reporting of evidence in a way that is useful for decision-making. Hence, Systematic Reviewers’ reviews, rating and reporting of evidence directly inform Decision-Makers in the case of UK A less salient but a very important contribution of Systematic Reviewers is the evaluation of available evidence so as to inform the design and implementation of new evidence-based policy assessment, ex-ante. That is to say, Systematic Reviewers’ evidential output informs Evidence-based Policy Specialists’ research. Specifically, Systematic Reviewers do so by generating new hypotheses, determining gaps in evidence or theories provided by Primary Scientists, and communicating the relevant evidential demands on behalf of Decision-Makers.

Scientists delivering primary theoretical and empirical evidence relevant to the evaluation of ISCP (e.g. Behavioral Economists and Social Epidemiologists) may also benefit from the Systematic Reviews’ research output in the same way as Evidence-based Policy Specialists do. However, the interaction between Systematic Reviewers and Evidence-based Policy Specialists is much more direct than the one between Systematic Reviewers and Primary Scientists in practice. This is the case firstly because Evidence-Based Policy Specialists draw generally on evidence available from the systematic reviews (e.g. Evidence-based policy specialists’ research articles would generally include a ‘background’ section where the evidential output of relevant Systematic Reviews is reported). Secondly, it is often the case that an Evidence-based policy specialist is also a specialist in systematic reviews (e.g. researchers such as Jean Adams, Gill Thomson, and Heather Morgan).

What do we know about the evidence gathering methods Systematic Reviewers use in pursuing their primary evaluative goal? As I described above, Systematic Reviewers deliver two types of evidential input for the evaluation of ISCP: one that is relevant for reporting, another one that is relevant for further evidence-based policy assessment. The systematic reviews rely on observational evidence gathering methods
Chapter 3

in reviewing available evidence. However, it is also appropriate to speak of heterogeneity of ISCP’s systematic reviews in terms of the kinds of evidence selected for the reviews. Depending on the aims and the orientations of Systematic Reviewers, systematic reviews of ISCP sometimes draw only on RCT-based evaluations of ISCP (e.g. when assessing overall effectiveness, see for instance, Cahill & Perera, 2011). Yet, they also draw on other evidential output assembled by the use of alternative evidence gathering methods including the Public Health Evaluators’ assessment based on non-RCT studies, theoretical and empirical evidence delivered by Social Epidemiologists and Behavioral Economists (e.g. when assessing aspects of interventions other than the effectiveness, see for instance Thomson et al., 2014). Systematic reviews sometimes integrate different evidence gathering methods. Such reviews, therefore, significantly contribute to methodologically more integrative evaluations of ISCP. Systematic Reviewers’ research output, which may be reinforced by the multiple evidence gathering methods, also serves as observational evidence that informs Evidence-based Policy Specialists’ research (see, Section 4, for an example).

Here, I have offered a general definition of what it means to evaluate a policy’s impact on health-inequities (3.1) and have reviewed the different sources and types of available evidence relevant for judging whether ISCP reduce health inequities in diverse contexts and demonstrated that that there are different methods of evidence gathering involved (3.2).

I will now assess how ISCP’s impact on health-inequities is evaluated. Based on this analysis, I will specifically argue that the evaluation of ISCP through the combination of different evidence gathering methods has distinct advantages in delivering inequity-relevant evidence in comparison to primarily RCT-based evaluations [advocated by the proponents of behavioral public policies and some of the RCT-specialized policy evaluators represented in the table (e.g. Haynes et al., 2012)]. More generally, I contend that this example gives us a reason to believe that a more pluralist evaluative methodology for behavioral public policies rectifies some of the commonly
stated methodological limitations of extant behavioral policy evaluations which are pertinent to reliance primarily on RCTs.

### 3.4. RCTs Integrated with Different Evidence Gathering Methods for the Evaluation of ISCP’s Impact on Health Inequity

I have offered an overview of different types of evaluators of ISCP and different evidence gathering methods they use for delivering inequity-relevant evidence. I will now examine the adequacy of these evidence gathering methods in investigating ISCP’s success with respect to the reduction of health inequities. My purpose is not to propose a single best methodology for the evaluations of ISCP; however, I will argue that RCTs fare better in delivering relevant evidence when integrated with alternative evidence gathering methods. This argument, to the extent that it is an argument regarding the use of RCTs in policy evaluation, is concerned with how RCTs can be used in a better way rather than stating how limited RCTs are. In this section, I will first review how well-known limitations of RCTs arise in the context of evaluating ISCP’s impact on health inequities. I will then illustrate how some evaluative studies integrate RCTs with different evidence gathering methods and argue for a more integrated evaluative methodology for the evaluation of ISCP.

Let me first review the limitations of RCTs as an evidence-gathering method used for the evaluation of ISCP’s impact on health inequities. To do so, I consider the evidential output delivered by the RCT-specialists who are Evidence-based Policy Evaluators and the Proponents of Behavioral Policies who primarily rely on RCTs.

I will offer two examples of the kind of evidential output that is relevant for evaluating ISCP’s impact on inequities, yet not delivered by Evidence-based Policy Evaluators and the Proponents of Behavioral Policies who only use RCTs.

The first kind of evidence is concerned with sub-groups, specifically the group of socioeconomically disadvantaged smokers. Based on available systematic reviews, what we know is that primarily RCT-based assessments which report that some tokens of ISCP are significantly effective in ceasing smoking (e.g. Volpp et al., 2009;
Halpern et al., 2015) are not informative about effectiveness in specific subgroups, such as disadvantaged smokers. The following comment that appeared in a Cochrane Collaboration systematic review concerning these studies is quite telling in this respect:

“Since both trials enrolled employees of large American companies, who were predominantly white and enjoyed relatively high levels of education and income, their success may not be readily generalizable to other populations of smokers, with different regional, socio-economic and ethnic mixes” (Cahill & Perera, 2011).

The lack of evidence regarding ISCP’s impact on specific disadvantaged groups is a major limitation for making evidence-based judgments about ISCP’s impact on health inequities. For instance, without comprehensive information about the subgroups of the population under investigation, one cannot judge whether ISCP’s effectiveness is modified across smokers with different demographic characteristics, or whether some overall effective ISCP are not successful in ceasing smoking for disadvantaged groups. A possible methodological reply to this challenge might be to carefully stratify the population of the experiment prior to the experiment, thus to define the different subgroups. However, doing so in the right way in fact invites researchers to use alternative evidence gathering methods such as observational studies and descriptive statistics together with RCTs, as I will illustrate further below.

The second example I would like to put forward concerns the lack of evidence regarding the long-term effectiveness of ISCP. The long-term effectiveness is crucial for understanding ISCP’s impact on health inequities. There is well-known social epidemiological evidence, based on qualitative and quantitative observational studies, reporting on the specific challenges of long-term smoking cessation in the context of disadvantaged smokers (e.g. due to stressors associated with social and economic exclusion). Many social epidemiologists would consequently argue that post-ISCP smoking behavior would be different across groups, even if an ISCP is initially successful, anticipating that disadvantaged smokers are more likely to relapse (Popay, 2008; Blue et al., 2016). Although such social epidemiological evidence is theoretically plausible, it gives us at best an indirect or prima facie reason to believe that disadvantaged quitters are more likely to relapse months after a successful, ISCP-
generated, abstinence. But those who favor ISCP may always demand further and stronger evidence to believe in social epidemiologists’ arguments against the effectiveness of ISCP. Although evidence gathered through RCTs are generally considered stronger than observational evidence, RCT-based studies of ISCP fail to provide us with evidence confirming or disconfirming arguments pro or against ISCP’s long-term effectiveness in disadvantaged smokers. Specifically, primarily RCT-based studies of ISCP do not deliver information about the distribution of relapse behavior across different strata of smokers in the long term.

Based on the reports of systematic reviews, what we know is that the smoking abstinence generated by effective ISCP usually does not last longer than a couple of months after the incentives are withdrawn (Marteau & Mantzari, 2015; Cahill & Perera, 2011; Jochelson, 2007). Yet, based on the considerations I stated above, it would be crucial to have RCT-based evidence indicating whether the post-ISCP relapse behavior is stratified and modified by the characteristics of disadvantages, such as unemployment or social exclusion, as many social epidemiologists would anticipate. As I will illustrate further below, such information can be more easily delivered when RCTs are integrated with alternative evidence gathering methods that are more suitable for predicting the stratification or modification effects in the long-term (e.g. by studying specific mechanisms of behavior change through qualitative studies or further modelling by primary scientists).

Now, it is not surprising that the evaluations of ISCP that rely only on evidence gathered through RCTs do not deliver evidence on these two aspects. RCTs are considered more adequate tools for determining the overall effectiveness of interventions rather than the variation of effectiveness across sub-groups. Similarly, since RCTs are not supposed to give information regarding how or through which mechanisms an intervention works, they are similarly not informative about the long-term impacts. These issues have been widely discussed in the relevant philosophical literature on policy evaluation in general (e.g. Cartwright & Hardie, 2012), and the behavioral public policy evaluation in particular (e.g. Grüne-Yanoff, 2016). It is therefore well known that RCTs have limitations in delivering comprehensive
evidence regarding the effects of interventions (such as evidence characterizing the heterogeneity of subgroup in the target population and differential distribution of effects across different subgroups, longer-term effects of the intervention, or how the intervention interacts with the context of the target environment). My aim is not to advance upon on these well-known critiques of RCTs, or to offer a new one. I fully acknowledge these criticisms and point out that the same issues arise in the context of ISCP evaluation as well. I also do not suggest that RCTs are in principle unconducive to investigate health inequities. My aim is rather to make a constructive methodological claim regarding how RCTs, as they are currently employed in the case of ISCP, might be designed and harnessed better for the purpose of making judgments about how behavioral policies such as ISCP fare with inequities.

To this end, I suggest that the Evidence-based Policy Specialists’ assessments of ISCP can and do actually deliver the necessary evidence when they integrate different evidence gathering methods with RCTs. To demonstrate this and to exemplify what kinds of evidence gathering methods are needed, let me offer a closer look at those methodologically more integrative evaluations of ISCP and why they perform better.

Consider Morgan et al.’s (2015) investigation of the incentives for smoking cessation during pregnancy conducted for the NHS in the UK. This social scientific research involves multiple methodological steps, but it is possible to represent Morgan and her colleagues’ investigation in two parts for the sake of understanding how they integrate multiple evidence gathering methods. The first part of their study involves a systematic review of RCT-based evaluations of various ISCP’s effectiveness, a report of qualitative and theoretical literature about the mechanism of incentive-based behavior change, and a collection of socio-epidemiological and behavioral scientific evidence regarding the barriers and facilitators of smoking cessation during pregnancy in the context of socioeconomic disadvantage. Morgan et al.’s aim in the first part is to integrate these different pieces of available evidence so as to inform the design and the scope of the second part of their study. The second part of the study involves conducting primary qualitative studies (based on structured interviews) to understand how the target audience of ISCP trials, which were carefully pre-selected in the first
part of the study, responds to incentive provision. That is to say, Morgan and her colleagues make use of qualitative studies in order to assemble comprehensive evidence regarding the working and the consequences of RCT-based trials. In a simultaneous study based on the same data, Thomson et al. (2014) gather evidence specifically relevant for making judgments about various unintended consequences of ISCP under investigation for disadvantaged smoking pregnant women in the UK. Their results do also inform judgments about ISCP’s impact on health inequities based on empirical evidence gathered in these two simultaneous studies.

The integrated methodology, exemplified in Morgan and her colleagues’ study, extracts inequity-relevant information from the available RCT-based studies of ISCP which do not necessarily have inequity-relevant content. It does so by making use of different evidential sources such as the evidence delivered by Primary Scientists regarding the possible mechanisms, barriers and facilitators of smoking cessation, and evidence delivered by qualitative literature concerning which ISCP trials have failed, for which sub-groups, and how. In doing so, it reveals comprehensive evidence regarding subgroups, which is not readily available from RCTs. Moreover, the integrated methodology also better predicts the potential long-term consequences of selected ISCP by conducting follow up structured interview studies in order to extract information about potential modifications or unintended consequences of the interventions. It thereby succeeds in rectifying the abovementioned limitations of primarily RCT-based evaluations.

Moreover, the studies which integrate multiple methods do also a better job in inequity assessment than those studies which rely only on social epidemiological evidence or qualitative methods. While the latter provides prima facie indirect descriptive and theoretical evidence for ISCP’s various impacts, the former deliver more direct and comprehensive causal evidence, which also makes use of available social epidemiological evidence.

This kind of more integrated methods are increasingly advocated by the Evidence-based Policy Specialists and Systematic Reviewers (Petkovic et al., 2017; Welch et al., 2015; O’Neill et al. 2014; Welch et al., 2010), and the number of similar studies
is increasing, as demands for evidence-based evaluations of inequity increases (NICE, 2013). To the best of my knowledge, none of the behavioral public policies or incentivized behavioral health policies has been evaluated in this manner, but there is no principled reason against doing so.

I have suggested that the evaluations of ISCP that integrate different evidence gathering methods with RCTs fare better in delivering pieces of evidence relevant for the inequity assessment in comparison to those which rely only on RCTs, or only on social epidemiological methods. This argument gives us a nuanced view regarding what RCTs can and cannot do in evaluating behavioral public policies. I will now also discuss in what way the argument advanced so far also contributes to the following more general epistemological question: what kind of evidence does the justifiability of behavioral public policies require?

### 3.5. Discussing Evidence Gathering Methods in the Philosophy of Behavioral Public Policies

I have illustrated that there are different kinds of evidence gathering methods involved in the evaluations of ISCP. I then have claimed that the evaluation of ISCP through the integration of different evidence gathering methods has distinct advantages in delivering inequity-relevant evidence in comparison to studies relying on single methods. I will now specify how my argument relates to more general philosophical debates.

Philosophers contribute to the evidence-based policy making by specifying the kinds of ideal epistemic requirements (pertaining to the nature of causal knowledge necessary for policy-making purposes) the evidence-based policies should meet, and by determining the sources of evidential gaps exists in the practice of evidence-based public policy justification (e.g. Cartwright & Hardie, 2012). Philosophers specialized in this literature often make a distinction between two broad categories of evidence: evidence of difference-making and evidence of mechanisms. There are controversies regarding what mechanisms are (Williamson & Illari, 2012), what counts as
mechanistic or difference-making evidence (Illari, 2011), and how mechanistic evidence relates to the difference-making evidence types of evidence (Clarke et al., 2014).

A general contention of the philosophers regarding the received methodology of evidence-based policy assessment (such as the idea of “evidence hierarchies” in biomedical and public health interventions) has been that the assessment of these policies is based on a limited array of evidence, although inclusion or prioritization of different categories of evidence is sometimes necessary. Evidence-based public policy assessment is primarily based on randomized controlled trials, which is commonly seen as delivering evidence of difference making. Philosophers often demand evidence of mechanisms for enhancing the evaluation of evidence-based policies (Russo & Williamson, 2007; Clarke et al., 2014). The former denotes the kind of evidence establishing that a policy-intervention or a treatment makes a difference in a target environment. The latter denotes the kind of evidence establishing how (through which mechanisms) a policy intervention makes a difference in a target environment. Grüne-Yanoff (2016) discusses these issues in the context of behavioral public policies such as Nudges. He observes that behavioral economists typically do not gather evidence of mechanisms to assess behavioral policies and considers this an important drawback for the justification of these policies. ¹⁰

The dichotomy between evidence of difference-making and evidence of mechanisms definitely helps us understand some of the limitations of policy evaluations based primarily on RCTs (see, for instance, Russo & Williamson (2007) in the context of

¹⁰ He provides examples from the literature so as to demonstrate that a lack of sufficient mechanistic evidence regarding how these policies make a difference in people’s behavior avoids us to assess various policy desiderata attached to these policies. For instance, without mechanistic information, Grüne-Yanoff argues, one cannot determine whether a default nudge program will be effective across different target environments, and whether it will lead to robust, persistent, and welfare-improving behavioral changes. This is the evidence that is assembled by behavioral economists corresponding to their explanatory models of choice behaviors. For example, default nudges make a difference in people’s behavior because of a mechanistic model A that specifies “inertia” as the main causal entity or process that leads to the conclusion, or model B cites recommendation effect, or C loss-aversion etc.
evidence-based medicine). I am broadly supportive of this conclusion, as I too try to spell out how RCT-based evaluations of behavioral policies can be improved. Yet, the argument I advanced in favor of the pluralism of evidence gathering methods is different from the arguments made in favor of or against mechanistic evidence in at least two ways. Firstly, the former and the latter have different evaluative goals. My analysis aims to answer how behavioral policies’ impact on inequities should be analyzed; whereas the call for mechanistic evidence is motivated to answer how behavioral policies’ efficacy should be assessed in different contexts. Secondly, my argument and the argument(s) for mechanistic evidence operate on different levels. While mine is a demand for a pluralism of methods, a demand for mechanistic evidence is about the content of evidence required for the respective evaluative goals. Of course, an implication of evidential pluralism as a thesis about the content of evidence for policy evaluation might be that we need to integrate different evidence gathering methods to deliver evidence of difference making and mechanisms. To the extent that evidential pluralists are happy to endorse the pluralism of evidence gathering methods for policy evaluation, this article provides further support for evidential pluralism in the context for behavioral public policy evaluation.

I would be broadly sympathetic to draw closer and more precise connections between the demand for pluralism of evidence gathering methods and the demand for evidence of mechanisms. For instance, one might argue be that the integration of evidence gathering for inequity assessment demands only specific kinds of mechanistic evidence and specific kinds of difference-making evidence. Or one might also quite naturally contend that the demands for pluralism of evidence gathering methods and need for mechanistic evidence could imply or complement each other, depending on how these positions are formulated exactly, or to which case they apply. Spelling out an exact relationship between my analysis and the need for mechanistic evidence, however, goes beyond the purpose of the present article. My aim has been to advance a specific perspective on the evaluations of behavioral public policies, one that I hope will be a useful addition to the discussions about the need for evidence of mechanism in this broader philosophical literature as well.
3.6. Conclusion

In this chapter, I have offered new insights for evidential evaluations of behavioral policies by focusing on evaluative challenges of a prominent example of behavioral health policies (Incentivized Smoking Cessation Policies). I have contended that evaluators of behavioral policies should go beyond a primarily RCT-based methodology for more comprehensive evaluations. I have also offered a pluralist evaluative methodology in which RCTs are integrated with alternative evidence gathering methods.

I focused on the evaluation of incentivized behavioral policies; however, I also believe that my comprehensive and practice-oriented analysis of ISCP also expands our understanding of behavioral policies more broadly. My focus on a fundamental public policy problem, the evaluation of health inequity aspects, invites readers to think about my analysis also in a different way: as exploration of a salient way in which behavioral economics, the ‘run-away success story of contemporary economics’ (Angner, 2015), modifies standard public policies, conventional ‘welfare regimes’ (Esping-Andersen, 2013[1990]), and claims to rectify the standard economics approach in addressing one of the centuries-old central problems of welfare-provision, how to address inequities. The chapter, therefore, provides a rationale for why philosophers and evaluators of behavioral policies may want to adopt a more interdisciplinary toolbox and mindset in addressing the potential merits and pitfalls of behavioral policies as evidence based public policies.
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Chapter 4

Reconciling Ernest Nagel's Impartiality with Anna Alexandrova's "Mixed Claims"

<Chapter removed under temporary embargo>
Chapter 5

Nudges, Well Ordered

<Chapter removed under temporary embargo>
Conclusions
A Perspective on the Justifiability of Evidence based Policymaking

The thesis has offered a perspective from the philosophy of science to understand and approach the justifiability of Evidence Based Policymaking (EBP). This perspective focuses on identifying the proper sources and the content of (non-epistemic) values that inform and guide scientific research and the legitimate roles these values play (conceptualized in the thesis as the “authority” and the “legitimacy” problem of values in science, respectively). The thesis has consequently analyzed the justifiability of EBP from the philosophical perspective of “values in science.” Approaching the subject from this perspective, the chapters in the thesis have conceptualized EBP as a practice of scientifically informed decision-making that can be considered complete and justifiable only when properly identified non-epistemic value considerations properly inform it. Specifically, the chapters contribute to understand, highlight, clarify, conceptualize, and resolve some of the major challenges of EBP that come to the fore when the non-epistemic dimensions of EBP are explored.

We have seen that the challenges of EBP pertaining to its value-ladenness can be analyzed from the perspective of values in science, specifically, by addressing the authority and the legitimacy problems of values in EBP. Addressing the authority problem of values in science in the context of EBP helps us discuss which values, projects, interests, and needs should guide the policy-advising scientific research and how these non-epistemic desiderata can and should be identified (e.g., should “lay citizens” or “scientific experts” determine these values? Through which procedures or avenues may these values be justifiably determined?) Addressing the legitimacy problem of values in the context of EBP helps us to discuss under which conditions allowing non-epistemic values to guide scientific research does not compromise the reliability of science for making informed policy decisions. Although society may ask scientists to serve for the general public’s valuable projects, the instrumental value of science decreases when non-epistemic pursuits of science override its epistemic quality (e.g., unduly harming science’s objectivity, reliability, and trustworthiness). In short, the practices of EBP should then ideally strike the right balance between these two goods: the pragmatic and the epistemic value of science.
The thesis has provided philosophical insights on the authority and the legitimacy problems by engaging with the values-in-science literature in philosophy of science which is devoted to responding to these compelling problems. The philosophical conclusions on these problems are meant as contributions to the conceptual progress in the ongoing theoretical discussions regarding values in science, social dimensions of science, and science in a democratic society. In order to make meaningful contributions to these discussions, each chapter of the thesis has provided insights on different aspects of the relevant topical issues and done so by analyzing some of the paradigmatic and societally important examples of EBP such as toxicological risk evaluation, incentivized behavioral interventions, and nudges. In the following, the thesis will conclude with a summary of some of the general insights drawn from the preceding chapters.

The first part of the thesis has introduced the values in science perspective to approach and understand the justifiability of the EBP in a democratic context. We have reviewed the challenges and potentials of the contemporary philosophical approaches to the values in science in the light of the debates on the value free ideal of science. Chapter 1 has highlighted the normative appeal of the “value free ideal” of science and invited the scholars in the sciences to pay special attention to the consequences of transcending the value free ideal of science. It also elaborated on the need for formulating new ideals for science in society that also convincingly address the broader worries of the proponents of the value free ideal (e.g., concerning the epistemic authority of science or its place in a democratic society). The chapter has suggested that the extant accounts that transcend the value free ideal of science (such as Douglas’s, or Longino’s) indeed do, in different ways, address these worries, even though we may still lack a full-fledged theory of values in science that replaces the traditionally accepted value free ideal of science in a comprehensive manner.

Chapter 2 has motivated the idea that our philosophical theories of value-laden decision-making in science can benefit from resting on more detailed descriptions of scientific practice (such as those offered by empirical studies of science). It has done so in the context of the most prominent contemporary account of values in science
(i.e., the inductive risk framework). This philosophical exercise is not only important for the sake of the descriptive adequacy of our philosophical theories of values in science but also fruitful in exploring comprehensive and normatively appealing philosophical accounts of science. Consider, for instance, the analysis in Chapter 2 regarding how social and institutional context may shape or change regulatory toxicologists’ evaluations of the relevant inductive risks. It is in and of itself essential that our philosophical theories of values in science are able to adequately describe and assess these processes as defended in the thesis (especially concerning the societally interesting cases of EBP such as regulatory toxicology). Moreover, it would also be desirable that our philosophical approaches can advance prescriptions regarding how value judgments are determined and how they change in response to the social environment of scientists (pertaining to the authority problem of values in science). The latter may be a subject for future work in the relevant field, and the kind of extended/integrated account of inductive risks proposed in the chapter is suitable for critically analyzing the conditions under which the social interactions between different groups of scientists (or between scientists and the public, or between scientists and institutions surrounding them) lead to epistemic or non-epistemic progress.

The second part of the thesis has focused on understanding and addressing some of the philosophical issues that come to the fore when we ask how the public’s needs, values, and interests properly inform and guide scientific research for policy making purposes. The focus on the non-epistemic dimensions of EBP leads us to examine scientists’ own value presuppositions, the proper use and the communication of these values in policy-relevant contexts. The resulting philosophical analysis lends itself to a pluralistic view of science practiced by groups of researchers pursuing analytically distinct research questions, epistemic and non-epistemic projects. This view of science is consistent with the thesis’s conceptualization of EBP as a case of informed decision-making where the issues such as the fit-for-purpose of scientific evidence and the balancing of the evidence and values come to the fore. Contrast this with the philosophical conceptualization of EBP as a case of causal inference where the primary focus of analysis is the extrapolation of scientific evidence irrespective of
their fit-for-purpose or pragmatic value. An advantage of the former conceptualization of EBP is that it helps us to critically evaluate the use of scientific findings in policymaking without making universalized or strict methodological claims. This has been nicely illustrated in Chapter 3. The chapter examines the limitations of randomized controlled trials as an evidence-gathering method (in informing behavioral health policies targeting at reducing health-related inequities) not by reference to its inherent methodological problems but its fit-for-purpose: to what extent it is conducive to achieve policy desiderata in question.

The last two chapters have offered more direct insights into the values-in-science perspective on the justifiability of EBP in a broadly liberal and democratic context. Chapter 4 has instantiated the advantages of approaching the issues in EBP from the perspective of values in science by clearly introducing the distinction between the authority and the legitimacy question of values in science. The distinction helps us appreciate the idea that the claims regarding the authoritativeness of value judgments in science (e.g. those presupposed by scientists) do not necessarily put one in a position to endorse a claim about the legitimacy of value judgments in science. As illustrated, this idea could help us advance philosophical views on how to address and resolve the legitimacy and authority problems. Chapter 4, for instance, specifies the view that we could integrate the most robust responses to these problems even though they may seem conflicting. It supports the arguments of those philosophers (such as Heather Douglas and Elizabeth Anderson) who combine the novel and promising responses to the authority problem of values (e.g. feminist and democratic responses such as Alexandrova’s) with more traditional responses to the legitimacy problem of values (e.g. impartiality-centered views such as Nagel’s).

While Chapter 4 has advanced the distinction between the authority and legitimacy problems, Chapter 5 explores a specific response to the authority problem of values in the context of EBP. Following the overarching aim of the thesis (that is, analyzing the justifiability of EBP with respect to meta-values such as democratic self-government), the chapter sides with Philip Kitcher’s view on normative expertise, which rests on the procedural and interactionist accounts of normative justification.
where neither of the relevant actors (e.g. the members of the public, scientists, or moral philosophers) should count as normatively authoritative. Chapter 5 has elaborated how this promising take on the authority problem of values can fruitfully contribute to the ongoing discussions on the democratic justifiability of nudges, which is a controversial contemporary case of EBP.

I hope to have demonstrated that the preceding chapters’ socially relevant and practice-oriented philosophical insights on the use and formation of values in science can improve our understanding of how evidence based policymaking practices can be made more accountable and justifiable in a democratic context.
Appendices
Appendix A

Samenvatting (Dutch Summary)

Wetenschap wordt over het algemeen gezien als de meest betrouwbare bron van kennis. Beleidsmakers hebben als doel om beleid te ontwikkelen met effect op de maatschappij. Op wetenschap gebaseerd overheidsbeleid wordt ook wel ‘evidence based policymaking’ genoemd. Dit proefschrift biedt een wetenschapsfilosofisch perspectief op de rechtvaardiging van evidence based policy (EBP) met betrekking tot algemene democratische en liberale waarden.

Het rechtvaardigen van EBP op basis van democratische en liberale waarden houdt in dat EBP waarden zoals vrijheid en autonomie bevorderen en versterken, of in ieder geval in overeenstemming met die waarden zijn. Het identificeren van de voorwaarden waaronder EBP in overeenstemming met deze waarden is vergt begrip van de rol van wetenschap en wetenschappers bij het realiseren van publieke waarden, menselijke behoeften, interesses en het streven naar vrijheid. Dit proefschrift biedt een filosofisch perspectief met als doel om te conceptualiseren hoe wetenschap in overeenstemming met maatschappelijke waarden, behoeften en interesses kan zijn, of deze zelfs kan promoten. Meer specifiek neemt behandelt het proefschrift een wetenschapsfilosofisch perspectief dat focust op de relatie tussen wetenschap en (maatschappelijke waarden). Dit type wetenschapsfilosofische perspectief op ‘waarden in wetenschap’ heeft twee overkoepelende doelen. Ten eerste heeft het als
doel om bij te dragen aan debat over welke waarden en niet-epeistemische overwegingen wetenschappelijk onderzoek zouden moeten dienen. Het gaat bijvoorbeeld in op de vraag welke sociale mechanismen de niet-epeistemische waarden die een rol spelen in wetenschap zouden moeten blootleggen en wat adequate bronnen voor deze niet-epeistemische waarden zijn (bijv. Kourany, 2010; Kitcher, 2011). Ten tweede biedt het proefschrift theorieën over de rol van niet-epeistemische waarden in wetenschappelijke redeneringen en onderzoek. Deze theorieën laten zien dat niet-epeistemische waarden de epistemische kwaliteiten en doelen van wetenschap niet ondermijnen. Het waarden-in-wetenschap perspectief balanceert daarbij tussen de instrumentele waarde van wetenschap (i.e., het gebruik van wetenschap om bepaalde maatschappelijke doelen en waarden na te streven) met haar epistemische autoriteit (i.e., de objectiviteit, non-dogmatisme en betrouwbaarheid). Het proefschrift bevordert begrip van EBP vanuit het perspectief van waarden in wetenschap door het adresseren van problemen die naar voren treden wanneer erkend wordt dat EBP een waardegedreven vorm van geïnformeerde besluitvorming is.

Het proefschrift bestaat uit twee delen die ieder op zichzelf staande wetenschappelijke artikelen bevatten. In deel 1 (getiteld Values in Science: Beyond the “Value Free Ideal” of Science), gaat het proefschrift in op de wetenschapsfilosofische literatuur over waarden in wetenschap. Het laat zien hoe deze literatuur zich verhoudt tot EBP en gaat in op hoe hedendaagse filosofische benaderingen de rol van waarden in wetenschap zien en hoe die waarden kunnen veranderen in relatie tot de context waarin ze zich voordoen. In deel 2 (getiteld Philosophy of Evidence Based Policy from the Perspective of Values in Science) behandelt het proefschrift problemen met betrekking tot de rechtvaardiging van EBP vanuit het waarden-in-wetenschap perspectief.

In het eerste deel van het proefschrift (getiteld Values in Science: Beyond the “Value Free Ideal of Science”), worden de consequenties van het verlaten van het waardenvrije ideal van wetenschap onderzocht. Hoofdstuk 1 evalueert de argumenten voor en tegen het waardenvrije ideal van wetenschap en illustreert dit door gebruik te maken van de gereguleerde toxicologie. Het hoofdstuk beschrijft de risico’s van het verlaten
van het waardenvrije ideaalbeeld. Het beargumenteert dat de prominente filosofische perspectieven die het waardenvrije ideaal van wetenschap overstijgen het waardenvrije ideaal niet afwijzen, maar de meta-waarden van wetenschap die de voorstanders van het waardenvrije ideaal belangrijk achten juist behouden. Onder deze waarden vallen de epistemische autoriteit van wetenschap, wetenschappelijke onpartijdigheid en het vermijden van ondemocratisch (technocratisch) beleid.

_Hoofdstuk 2_ reflecteert op de vraag hoe hedendaagse filosofische frameworks die de meest prominente waarde-geladen concepties van wetenschap ontwikkelen en evalueert voorbeelden uit de wetenschappelijke praktijk. In het bijzonder wordt er aan aandacht besteed aan het inductief risico framework van Heather Douglas en bediscussieert hoe het framework van toepassing is op een maatschappelijk relevant en filosofisch significante historische episode uit de regulerende toxicologie. Het hoofdstuk biedt een empirisch geïnformeerd beschrijving van toxicologen die methodologische beslissingen maken met betrekking tot het relevante bewijs voor hun doelen en die kiezen tussen verschillende methoden van bewijsvergaring. Het hoofdstuk gaat specifiek in op interessante theoretische aspecten van deze casus (d.w.z., de rol van sociale en institutionele processen bij veranderende perspectieven van toxicologen op inductieve risico’s) die nieuw zijn voor het inductieve risico framework. Het laat zien hoe het inductieve risico framework kan worden uitgebreid om ook op deze casus van toepassing te zijn.

In het tweede deel (getiteld _Philosophy of Evidence Based Policy from the Perspective of Values in Science_), reflecteert het proefschrift op de filosofie van evidence based policy vanuit het waarden-in-wetenschap perspectief.

_Hoofdstuk 3_ onderzoekt hoe een specifieke vorm van gedragsinterventies, namelijk anti-rook programma’s, wordt geëvalueerd door wetenschappers. Het laat zien dat de evaluaties van deze programma’s in de praktijk worden gekenmerkt door een hoeveelheid aan wetenschappers die gebruik maken van verschillende bewijsvergaringsmethoden, die verschillende wetenschappelijke disciplines representeren en verschillende beleidsperspectieven op roken als een publiek gezondheidsprobleem hanteren. Het hoofdstuk beargumenteert dat een empirische
Samenvatting (Dutch Summary)

evaluatie van de vraag of anti-rookbeleid effectief is in het verminderen van rookgerelateerde gezondheidsverschillen (wat over het algemeen gezien wordt als het meest belangrijke doel voor veel anti-rookbeleid in Europa) integratie van verschillende methoden (zoals gerandomiseerde controlestudies en kwalitatieve methoden uit de sociale epidemiologie) vergt. Het hoofdstuk bespreekt de implicaties van dit pluralistische perspectief voor de filosofische discussies over de voorwaarden voor bewijs voor gedragsinterventies.


Hoofdstuk 5 richt zich op een belangrijk voorbeeld van evidence based policy, namelijk Nudges. Nudge beleid is een prominent en vaak toegepast type evidence based policy. Nudges roepen belangrijke ethische en politieke vragen op die relevant zijn in het kader van de in dit proefschrift besproken filosofische vraagstukken. Gebaseerd op de literatuur over de democratisering van wetenschap (specifiek op Philip Kitcher’s benadering), bevat dit hoofdstuk een voorstel voor een Nudge concept, oftewel een ‘well-ordered nudge’, die rechtvaardiging van dit soort
gedragsbeleid zoekt via deliberatieve democratische besluitvorming. Het hoofdstuk biedt daarmee een specifieke praktische benadering en laat de conceptuele implicaties zien voor een belangrijk type evidence based policy. In het hoofdstuk worden de well-ordered Nudges vergeleken met de reeds bestaande Nudge concepten en gaat in op de vraag hoe het filosofische perspectief dat dit proefschrift biedt de huidige discussies over de democratische rechtvaardiging van Nudges en nudge-achtige evidence based beleidsvormen kunnen verreiken.
Appendix B

Curriculum Vitae

<Section removed under embargo>