Science and the Production of Ignorance

When the Quest for Knowledge Is Thwarted

edited by Janet Kourany and Martin Carrier

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Acknowledgments

The idea for this book emerged at a conference we organized back in 2011, entitled "Agnotology: Ways of Producing, Preserving, and Dealing with Ignorance." Many years would pass before we had the opportunity seriously to act on the idea, and the book that has resulted bears little resemblance to that conference. Nonetheless, we wish to thank Bielefeld's Center for Interdisciplinary Research (Zentrum für interdisziplinäre Forschung (ZiF), Universität Bielefeld, Germany) for sponsoring the event, the participants at the conference for increasing our enthusiasm for all things agnotological, and Peter Galison for much gentle nudging at the conference to do the book and much assistance more recently to make it a reality. We also wish to thank Katie Helke and Ginny Crossman of the MIT Press for an unbelievably smooth, well-orchestrated review and production process, and Notre Dame's Institute for Scholarship in the Liberal Arts (ISLA) for indexing support.

I IGNORANCE: A NEW FOCUS OF INQUIRY

1 Introducing the Issues

Janet Kourany and Martin Carrier

1. Agnotology: The Study of Ignorance as Socially Constructed

Science has traditionally been billed as our foremost producer of knowledge. For more than a decade now, however, science has also been billed as an important source of ignorance. Indeed, historian of science Robert Proctor has coined a new term, *agnotology*, to refer to the study of ignorance, a new area of inquiry, and it turns out that much of the ignorance studied in this new area is produced by science (see, for example, Proctor and Schiebinger 2008; Gross and McGoey 2015). The examples are numerous. Whether it be global warming, the health effects of smoking or environmental pollutants, the relation of processed foods to high blood pressure, obesity, and diabetes, or the safety and efficacy of prescription drugs, in each case the public is confused, uninformed, or in some other way ignorant, and in each case the ignorance has been, to use Proctor's (2008, 8) phrase, "made, maintained, and manipulated" by science—by an increasingly politicized and commercialized science. According to this new approach, in short, ignorance is far more complex than previously thought. Indeed, ignorance is not just the void that precedes knowledge or the privation that results when attention focuses elsewhere. It is also—in fact, it is especially—something socially constructed: the confusion produced, for example, when special interests block access to information or even create misinformation.

Thus Proctor (2011) has written about the tobacco industry and the techniques it used in the past, and continues to use, to produce doubt in the public regarding the health risks of smoking. Historians of science Naomi Oreskes and Erik Conway (2010) have revealed how a small group of politically conservative scientists worked behind the scenes to stall the public recognition of such problems as acid rain, the ozone hole, and global warming. Pulitzer Prize-winning *New York Times* reporter Michael Moss (2013) has exposed the elaborate methods that food scientists have developed to con the public into preferring foods packed with salt, sugar, and fat over healthier fare. And so on.

And this is just the beginning. Ignorance as “active construct” (or “strategic ploy”) is only one type of ignorance on the research agenda of agnotology. Proctor (2008, 7–8, 20–23) has distinguished two other types of ignorance also produced by science: ignorance as “passive construct” (“lost realm” or “selective choice”)—the kind of ignorance that is the unintended by-product of choices made in the research process; and ignorance as “virtuous”—when “not knowing” is accepted in research as a consequence of adopting certain values. Unfortunately, though, Proctor has left these other two types of ignorance relatively unexplored. It is the aim of the present volume to rectify this imbalance. In addition to casting more light on the deliberate production of (harmful) ignorance, we intend to show that the passive production of ignorance as well as virtuous ignorance are at least as interesting and important as the kind of active ignorance on which Proctor has lavished so much attention. We hope to thereby exhibit the scope and potential of agnotology. At the same time, we hope to foster a fruitful collaboration between agnotology’s investigations of ignorance and philosophy’s investigations of knowledge. Thus far, surprisingly little interaction has occurred between these two mutually relevant sorts of investigations. The hope, then, is that the present series of essays by a distinguished group of philosophers and historians familiar with agnotological matters will help to remedy this deficiency.

In what follows we will provide a glimpse of what is to come. But first some preliminary reflections on what has already been accomplished in agnotology, how it relates to traditional philosophy, and what the relevant varieties of ignorance are.

2. The Relation between Agnotology and Philosophy

Let’s start with the understanding of ignorance provided by philosophy. At least in modern times philosophy has provided a very expansive conception of ignorance. Human ignorance, of course, refers to what humans fail to know, and for much of the modern philosophical tradition this ignorance has been thought to be, or at least it has been feared to be, nearly boundless. Epistemology in the past, for example, very much followed in the footsteps of René Descartes, and much of it still does, but while Descartes never thought that his evil demon had gotten the better of him, his followers have certainly thought otherwise. Indeed, neither Descartes nor any of the epistemologists after him has ever been granted a clear victory over the kinds of skeptical doubts raised by Descartes, and New York University epistemologist Peter Unger (2002), in his now classic and highly influential book *Ignorance*, has argued with great cogency that no one ever will be granted that victory. According to Unger, in fact, no one knows anything at all, and hence no one will ever be justified or reasonable in believing anything at all as well. Knowledge and rationality are simply not to be had.

The story from traditional philosophy of science has been similar. There David Hume has been the hero rather than Descartes, and Hume's problem of induction and later what might be called the problem of abduction (or inference to the best explanation) have taken the place of Descartes's demon. And although scientific realists have tried frightfully hard to justify scientists' claims to know in the face of these and related challenges, once again none has finally been granted the prize. As a result, some of the most learned figures in the philosophy of science, such as Karl Popper (1959, 1963), have candidly admitted that scientific ignorance is our fate: neither theories nor even basic statements (accepted by convention) are ever justified. And even when the learned are not quite that candid, the result seems the same. Think, for example, of Thomas Kuhn (1962), for whom the march of science is a movement away from primitive beginnings but not toward anything at all—certainly not toward any fixed end set by nature. Or think of Bas van Fraassen (1980), for whom induction but not abduction is reasonable. For him, therefore, observation statements and empirical laws are knowable, but theories are not, even though for him observation statements and empirical laws are theory laden with those same unknowable theories.

So prominent traditions in both epistemology and the philosophy of science have emphasized the paucity of human knowledge and the vastness of human ignorance. Of course, there have always been problems with these stories. For one thing, their standards for knowledge have been unreasonably high, involving as they do what John Dewey would call a misguided quest for certainty. Perhaps because of this, no one has ever really believed these stories even with all the arguments given in their defense. At least no one has ever acted as though they did. Unger, for example, after his book was written, continued to give his talks, publish his papers, and teach his classes even though he claimed to know nothing at all. And Popper, we dare say, never worried over the consequences of doing the things he did even though he claimed to have no grounds for any particular expectations. And this highlights the fact that there has never been an appropriate response to the traditional stories. At least these stories never called for any response, never demanded any remedy for the ignorance they described. How could they, when their arguments called into question even the bodies that would have been needed for a response? No matter. The upshot is that knowledge, for epistemologists and philosophers of science, has seemed a difficult—perhaps an impossible—achievement, and at any rate one that needs to be explained, to be justified, while ignorance is perfectly natural.

Agnotologists seem to be offering the opposite story. According to this new story, *knowledge* is frequently what is natural, and ignorance is frequently what needs to be explained. Think, for instance, of Proctor's tobacco study, and Oreskes and Conway's study of global warming. Cigarettes are surely harmful; that is uncontroversial and, in fact, has been uncontroversial for more than fifty years. They have been linked not only to lung cancer but also to heart disease, strokes, emphysema, diabetes, and a host of other diseases. And they kill six million people per year, more than all the world's infectious diseases combined. Proctor calls them the deadliest artifacts in the history of human civilization. Yet the belief continues to be widespread that smoking is an "adult choice" that connotes freedom, independence, and sophistication; that it is a wonderfully relaxing pastime. And so cigarettes continue to be the most widely used drug in the world, with six trillion cigarettes sold every year (Proctor 2011). Global warming has also been uncontroversial for years. If no measures are taken to deal with it, the rise in global surface temperature could be as high as eleven degrees Fahrenheit during this century, and the longer we wait to deal with it, the worse the outcome will be (EPA 2017). Yet a significant segment of the general public and even some in the highest leadership positions continue to doubt that global warming is occurring. According to Pew Research Center surveys conducted in February, March, and August 2014, for example, only 61 percent of Americans believe that global warming is occurring, and only 40 percent believe that it is human caused (Motel 2014). And in other countries the situation is even worse. Indeed, a 2007–2008 Gallup Poll that surveyed people in 128 countries found that over a third of the world's population has never even heard of global warming (Pelham 2009). As a result, serious action has been delayed. With all the information readily available about the hazards of smoking and the dangers of global warming, what needs explaining here is the enduring ignorance of so many people.

For agnotologists, then, *ignorance* is frequently what needs to be explained. What's more, agnotologists go on to explain that ignorance, and their explanations, tend to centrally involve science. In his book *Golden Holocaust*, for example, Proctor (2011) explains how in the 1950s, when the evidence for the link between smoking and lung cancer had become overwhelming, the tobacco industry opted to "fight science with science" in order to forestall regulation and protect profits. The industry devised what Proctor calls the "tobacco strategies" to do this—strategies that included funding decoy research to distract from critical questions, thereby "jamming the scientific airwaves"; organizing "friendly research" for publication in popular magazines; establishing scientific front organizations; producing divergent interpretations of evidence regarding the health effects of smoking, and even misinterpretations and suppression of such evidence; forever calling for more research and more evidence, and setting standards for proof so high that nothing could ever satisfy them; and exploiting or actually producing divergent expert opinion (see also Michaels 2008). In addition, Proctor's 1995 book *Cancer Wars* explains how trade associations were later organized to pursue some of the same strategies to shield still other industries from regulation and loss of profits—trade associations such as the Polystyrene Packaging Council, the Fertilizer Institute, the Chemical Manufacturers Association, the National Dairy Council, and the American Meat Institute. Other books that have appeared since *Cancer Wars* have offered additional details regarding the methods that these industries have developed to produce ignorance in the public. For instance, Moss's (2013) *Salt, Sugar, Fat: How the Food Giants Hooked Us* explores the strategies that the food industry (many of whose companies are owned by tobacco giants R. J. Reynolds and Philip Morris) has used to manipulate the public into increasingly less healthy food choices to generate ever more profits for the industry.

For their part, Oreskes and Conway (2010), in their book *Merchants of Doubt*, explain how four distinguished scientists—Fred Seitz, past president of the National Academy of Sciences and Rockefeller University; Robert Jastrow, founding director of the Goddard Institute for Space Studies; William Nierenberg, past director of the Scripps Institution of Oceanography; and Fred Singer, first director of the National Weather Satellite Center and founder of the Science and Environment Policy Project in his home state of Virginia—adopted the same tobacco strategies to produce doubt and confusion in the American public regarding not only global warming but also other serious problems, such as acid rain and the ozone hole. In each case, these scientists worked against a strong consensus within the international scientific community using such tactics as misrepresenting, suppressing, or attacking the results of scientific studies that supported the consensus positions; lodging personal attacks against scientists whose research was central to the development of the consensus positions; defending competing but weaker positions than the ones accepted; and in general acting to create the impression that controversy still surrounds issues that the international scientific community considers settled. This time the motivation, Oreskes and Conway tell us, was not so much the safeguarding of profits as an anti-regulation, market fundamentalist set of political commitments. Still, the activities of Seitz and the others were backed by major conservative think tanks that were, in turn, backed by the US fossil fuel industry, particularly ExxonMobil.

The upshot is that science has been a central player in the production of the public's ignorance even while it has been a central player in the production of the knowledge that the public is lacking. What's more, it has been very difficult—for the public and even sometimes for the experts—to distinguish when science is producing the one and when it is producing the other. For although agnotologists accept (indeed, *presuppose*) that science produces knowledge—the knowledge of the harms caused by smoking, for example, or the knowledge of global climate change, or the knowledge of the elements of a healthy diet—agnotologists have largely failed to explain what constitutes these results of science as knowledge. Oreskes and Conway, for example, have portrayed science's peer review system and the mechanisms it employs to generate scientific consensus as hallmarks of the production of genuine knowledge. At the same time, Proctor has challenged this account, emphasizing how economic and political interests have helped to shape these mechanisms—emphasizing how, for instance, the tobacco industry has organized conferences, established journals, supported certain lines of research while criticizing or even suppressing other lines of research, and so on. As a result, these peer review mechanisms are as involved with the production of ignorance as they are with the production of knowledge. Ironically, Oreskes and Conway have also challenged their account of knowledge with their many illustrations of how such tobacco industry strategies have been put to use in a variety of other, nontobacco-related contexts. Meanwhile, none of these individuals has offered a more adequate account of scientific knowledge, even though the distinction between such knowledge and the socially constructed ignorance on which they focus is central to their project.

Of course, what constitutes knowledge, scientific or otherwise, is just what philosophy has traditionally attempted to explain. If it has so often failed in the attempt, that may be because its methods of analysis have largely lacked the constraints on philosophical imaginings that real-life, socially important questions about knowledge and ignorance provide. But these constraints are just what the questions of agnotology now supply. In short, the way to achieve success in both enterprises—the knowledge-focused inquiries of philosophy, epistemology, and philosophy of science, as well as the ignorance-focused inquiries of agnotology—may be to combine the resources and needs of both into a new, more inclusive area of inquiry that adequately deals with knowledge *and* ignorance (*agnoepistemology*?).

3. Ignorance as Bliss

In addition to Proctor's central notion of ignorance as information intended to mislead and confuse the public, as just recapitulated, the two categories of ignorance acknowledged but then largely ignored by Proctor are *virtuous ignorance* and *ignorance as passive construct*. Virtuous ignorance is actively constructed, or at least allowed and respected. Science, for example, is required to stay away from examining certain issues. Traditionally the realm of the sacred was exempt from critical scrutiny, but Robert Merton's (1973) ethos of organized skepticism committed science to not bowing its head before the revered and venerated. Still, as Proctor has underscored, there are many things that we rightly do not want to know and rightly do not want science to explore. Virtuous ignorance is the ignorance that results when “not knowing” is deliberately accepted in research as a consequence of adopting certain values. It arises, for instance, when knowledge would be gotten by improper means (such as involving serious risks to human or animal subjects), or when knowledge would be too harmful or dangerous to pursue (such as the know-how involved with certain kinds of biological or chemical weapons). The right to privacy is a legitimate shield of ignorance, and not knowing one’s own genetic diseases is considered bliss by many (Proctor 2008). In this vein, certain kinds of research may be barred for good reasons. Doing research on human cloning is rightly outlawed worldwide. A more contentious issue is the creation of a highly efficacious flu virus. In 2011, Rotterdam biologist Ron Fouchier generated a novel H5N1 flu virus by using well-known methods to produce a series of mutations in existing virus strains. This created virus could be transmitted through the air and proved 100 percent fatal for ferrets. So the moral and legal question arose about whether this result should be published, or if it would be better left in the virtuous ignorance closet. The fear was that the finding could be abused by bioterrorists, while the response was that publishing the strain would open up opportunities for developing a vaccine that could prove vital once the dangerous sequence of mutations happened to emerge in the wild.

The Dutch government considered Fouchier’s submission to *Science* of the manuscript reporting his research as an export of a dual-use commodity and required him to seek government approval first. A Dutch court confirmed this legal point of view. Fouchier applied for an export license (under protest because he claimed that his academic freedom had been infringed) and was granted this license. So his paper appeared in *Science*, but only after a significant delay (Enserink 2013). There is no straightforward solution to this moral challenge (Resnik 2017). Of course, virtuous ignorance, like many concepts, has fuzzy boundaries. Yet it seems clear enough that we don’t want science to increase public risks and expose unsolicited information about us as individuals. It is not always evident how far these constraints should extend, however—as the virus example illustrates. Assaults to self-esteem alone should not count as a sufficient reason for abandoning research endeavors in any event. Sigmund Freud (1920) argued that humanity suffered from three such assaults—namely, Copernican heliocentrism, Darwinian evolution, and the Freudian unconscious and its subversion of rationality. Whatever is to be made of this claim, barring research out of the mere fear of feeling insulted by its results comes close to an agnotological ploy in itself.

4. Ignorance as Passive Construction

As stated at the outset, active construction, whether virtuous or vicious, is not the only way in which science produces ignorance. Proctor also calls our attention to science's passive construction of ignorance—that is, the ignorance that scientists produce as an *unintended by-product* of their research. Placing something at the forefront of attention is tantamount to leaving something else unattended. As Proctor (2008, 7) puts it, "inquiry is always selective," and research not selected at any particular time "is not just research delayed"; it may be research lost forever.

Agnotological challenges of this sort derive from the fact that the system of scientific knowledge is not dictated by nature. Scientists are free to ask certain questions while leaving others unaddressed, they may decide to follow certain principles at the expense of others, and they can choose alternative frameworks of testing and confirming scientific assumptions. Such choices can be made differently, and have been made differently in the history of science. Ian Hacking (1999, 163–185; 2000) argues that styles of scientific reasoning pave the way for problems of certain sorts, while discouraging or preventing scientists from taking up others. Hacking maintains that one obvious reason for this limiting character of styles of reasoning lies with the resources available for research. In a world of limited funds, devoting efforts to a particular issue withdraws the means for tackling a different issue. Conceptual constraints are yet another influential factor. If certain groundworks have not been laid, certain insights cannot emerge. Hacking emphasizes the choice left to scientists by advocating a contingency thesis to the effect that alternative pathways of science exist that are conceptually nonequivalent but might still enjoy similar success. And this contingency shows up regarding styles of scientific reasoning. In what Hacking calls the laboratory style, scientists intervene in nature and build and combine models of their research target as well as the apparatus they use for manipulating that target. By contrast, observation-centered approaches construct models that focus on unsolicited or spontaneous effects of nature.

The idea here is that the quest for knowledge in general and scientific knowledge in particular operates like a searchlight. Each such endeavor illuminates certain aspects of experience while it leaves other aspects in the dark. Seeking knowledge in a particular way or seeking particular forms of knowledge tends to leave other areas unexplored. As a result, choosing particular programs of research and the selective ignorance that they produce are generally not to be criticized; rather, they are inevitable. We are simply unable to place everything in the limelight. The point is to become aware of the research options in play and make our choices in such a way that the values we cherish are best served. Unfortunately, Proctor does not specify the various ways in which selective ignorance might come about, but we suggest the following four possibilities.

First, ignorance might result from the way a research inquiry is defined. Treating AIDS as simply a biomedical problem (of how to deal with the AIDS virus) rather than, more comprehensively, as a public health problem (of how to deal with those suffering from HIV/AIDS), for example, left hidden for decades the socioeconomic, cultural, and globalization aspects of the AIDS epidemic and, as a result, precluded a timely solution to it (Kourany 2010, 121–122). Similarly, the continuing emphasis in health research on disease management and biochemical processes leaves hidden how health and disease are produced by people's daily lives, access to medical care, economic standing, and relations to their community, and thus blocks important avenues of disease prevention (see, for example, Krieger and Fee 1996). Speaking more generally, the crucial choice is to focus on the social conditions of the problem or seek technological solutions; either choice produces selective ignorance (Elliott 2012). Of course, in the abstract nothing precludes pursuing all avenues simultaneously. But finite resources and the need for a particular heuristics make it impossible to follow all lines of research in parallel. Research needs specific goals, assumptions, and guidelines in order to uncover salient features and distinguish them from noise. This induces selectivity in the research pathways open to a scientific community. Such passively constructed ignorance might be called *focus-generated ignorance*. It is the product of the system of rules and incentives in place and not the result of any external interference (as in Proctor's notion of agnotology as intentional deception).

Another example of focus-generated ignorance concerns current bacteriophage research. While in the AIDS case, research priorities were set in a limited way because of social and political as well as economic factors, this new example features only economic reasons. Over the past decades, the medical treatment of bacterial infections has been jeopardized by the increasing resistance of bacteria to antibiotic substances; existing antibiotics have simply become less effective. At the same time, the pharmaceutical industry has failed to come up with new agent substances. Enter bacteriophages. They are viruses that attack and eliminate bacteria, and can in principle be used for medical treatment. And actually, knowledge of bacteriophages predated the discovery of antibiotics (Summers 2005), but was taken to be irrelevant when efficacious antibiotics were available. In fact, only in the former Soviet Union, which was not part of the Western system of medicine, was research on phages pursued (Patel et al. 2015). It seems plausible to suppose, then, that after the fall of the iron curtain and after the predicament of antibiotic treatment had become apparent, the Western pharmaceutical industry would have turned to bacteriophages and sought to tap their potential. But it did not, and it even renounced such a research program for a long time. The reason is that bacteriophages are living creatures that cannot be patented. The relevant task is to seek and identify phages that thrive on particular strains of bacteria. In other words, such research actually means searching for existing varieties of phages and matching them with the relevant bacteria. Such research is hardly profitable given the economic incentives in place in Western medicine, and as a result—except for a few publicly financed studies—this research did not happen until recently (Salmond and Fineran 2015, 782–783; see Strathdee and Patterson 2019). In short, the system of incentives for medical research covered bacteriophages with the veil of focus-generated ignorance.

A second way in which ignorance might result from research pertains to the conceptual framework with which scientists operate. Consider alternative approaches to cancer research as an example. Cancer can be tackled from a single-cell perspective or a more holistic angle. The so-called somatic mutations theory represents cancer as a series of mutations of single cells. Factors from outside, such as radiation or smoking, but also possible genomic dispositions, induce an accumulation of stepwise changes within individual cells. This cellular research program is focused on particular genes, their changes and interactions, and the ways that cells cope with such alterations. A tumor is nothing but an array of individual cells that have gone out of control (Plutynski 2013). The opposite tissue organization field theory emphasizes the interactions between a *group* of cancer cells—a tumor—and its environment—interactions without which the tumor could not survive. A tumor is an organized entity, not simply a collection of individual cells in genetic disarray. For instance, carcinoma cells manage to neutralize the regulatory system that constrains cell reproduction. They do so by changing the relevant patterns of interaction among cells. This conceptualization of cancer depicts malignant growth as a holistic process (Hanahan and Weinberg 2000; Carrier and Finzer 2011).

Cancer research takes a different form within these two programs. In the one, a tumor is regarded as an accumulation of individual cancerous cells; in the other, a tumor is characterized by a particular pattern of cellular interaction and represented as an organized whole similar, in this respect, to a bodily organ. Different factors are highlighted in each program, and different causal relations are explored. The cellular approach centers on such factors as the failure of cellular repair mechanisms, while the holistic approach targets the blood supply of the tumor and the induced suppression of immune response.

What appears especially striking from an agnotological point of view is that glaring anomalies within the somatic mutations approach, such as large-scale correlations among the genetic alterations of a given tumor, were completely overlooked (Bedessem and Ruphy 2019). Moreover, clearly visible structures were ignored, and ignored for quite some time. It was discovered only recently, for example, that the cells in a carcinoma are connected by so-called nanotubes or microtubes through which biomaterials such as RNA and proteins are exchanged. Researchers were flabbergasted that such obviously distinguishable features in a field under intensive scrutiny had completely escaped their notice. The apparent reason is that the somatic mutations theory is strongly dominant and features supporting the rival approach were simply disregarded. The physician Emil Lou, who first drew attention to the phenomenon, is quoted as saying, “It’s right in front of our face, but if that’s not what people are focusing on, they’re going to miss it” (Callier 2018).

Thus, although it is abstractly conceivable to combine the two different conceptualizations of cancer into a single unified account, this has not been feasible in the reality of the lab. Both approaches have been kept distinct and pursued separately; following the one has made the other unavailable, invisible. This way of passively constructing ignorance, or rather, the ignorance so constructed, might be called *framework-centered ignorance*.

A third way in which ignorance might result from scientists' research relates to their choice of methodology. The research scene that surrounded HIV/AIDS again forms a telling example. Consider, in particular, the first attempts to find a cure for HIV/AIDS in the early 1990s. At the time, the inclusion requirements for the corresponding clinical trials demanded that the relevant patients would not take any other medications besides the specific one at issue. The rationale behind this policy was that only if the agent substance under scrutiny was the sole factor that was different between the experimental and control groups would an unambiguous causal judgment be justified. Otherwise, interactions with other drugs might distort the results. Affected HIV patients argued, by contrast, that the groups involved in clinical trials should resemble the expected future users of the potential drug, and such actual patients, as a rule, take diverse medical drugs simultaneously (Epstein 1995). So there were two conflicting methodological frameworks at play. According to the one, a well-confirmed judgment about the effects of a drug was the goal; according to the other, the goal was the maximum benefit for later users of the drug. The conflicting goals, in short, were reliability versus relevance, and they promoted different sorts of ignorance as well as understanding.

A similar, more recent case concerns the use of sanitized mice in sterilized environments for discovering medical drugs that involve the immune system. Such breeds yield reproducible and thus reliable findings which, however, transfer badly to humans. The shift toward more variable real-life breeds that is presently underway testifies that relevance had not been given due weight as compared to reliability (Willyard 2018).

The same conflict, if in a different guise, emerges in the tension between evidence-based medicine and personalized medicine. The former emphasizes randomized controlled trials in which averages across large numbers of patients are considered. The latter is based on the observation that drugs work differently in different patients so that medication should be customized to the individual at hand. As a result, no large numbers of users are needed, and physiological mechanisms or small-scale studies are accepted as sufficient information. So again, we see the contrast between reliability and relevance. Of course, both these goals can be pursued, but not in the same research program. They require different kinds of studies, and provide different kinds of information and failures of information. The type of passively constructed ignorance produced in each case might be called *methodology-created ignorance*.

Finally, ignorance might result from the composition of the scientific community that conducts the research. Think, for instance, of the knowledge that was lost to anthropology from such traditional contributors as travelers, merchants, soldiers, missionaries, and local intelligentsia when these "amateurs" were excluded from anthropology in the process of its professionalization. Similar losses have accompanied the professionalization of other scientific disciplines as well. Likewise, consider the relationship between the gender composition of a research community and the knowledge produced by that community. Until some decades ago, the mostly male community of primatologists focused on male activities as the primary engine of a primate group's social order and reproductive success. When female primatologists entered the field in the 1970s, they revealed that female behavior is often central to the structure and development of a primate group (Anderson 1995, 72–73). A biased composition of the research community eclipsed important features of the field of study. This type of passively constructed ignorance might be called *community-based ignorance*, and it links up in interesting ways to all the other forms of passive ignorance construction already described.

There are, then, many forms of Proctor's second type of socially constructed ignorance—the type passively constructed in the course of scientific research (probably many more than we have illustrated above). As Proctor (2008, 23) says, "Access to all kinds of information is limited . . . for more reasons than the moon has craters."

5. A Preview of What Is to Come

In the essays that follow, all the foregoing forms of ignorance construction are exemplified. In particular, the book elaborates Proctor's chief notion of agnotology as the deliberate creation of ignorance, illuminates the notion of virtuous ignorance, and then takes up all four subcategories of ignorance as an unintended by-product of other sorts of choices.

In chapter 2, "Agnotology in Action: A Dialogue," Proctor and Peter Galison explain what led them to the study of ignorance. They recount the kinds of confusion, lack of information, and misinformation that they found especially alarming even in their student days—the kinds of ignorance, in fact, regularly produced in the United States by government, industry, and the media. And they go on to describe some of the most important strategies used over the years to produce this ignorance. Their dialogue provides a historical context for today's explorations of agnotology by presenting the concerns and motivations of the early pioneers. In the process, Galison and Proctor make it abundantly clear why the rest of us need to be as concerned as they are with such ignorance and the new area of its investigation.

Martin Carrier continues Galison and Proctor's focus on ignorance as active construction, although he directs our attention now to science in particular. In chapter 3, "Agnotological Challenges: How to Capture the Production of Ignorance in Science," Carrier points out that scientists do not usually resort to blatant means such as falsifying evidence or outright lying in order to deceive other scientists or the public at large. The methods employed are usually more subtle, and concern especially the biased design and interpretation of experiments or empirical studies. Further, the intention to misrepresent a situation or confuse the public is often hard to establish; agnotological moves should be identifiable by publicly accessible indicators. Carrier proposes to identify agnotological ploys by the discrepancy between the conclusions suggested by the design of a study and those actually drawn or intimated. Agnotological moves are characterized by the unacknowledged difference between those issues for which a study is sensitive and those issues that feature in its interpretations. Such a mechanism of "false advertising" is claimed to implement agnotological endeavors and also serves to document them without having to invoke the intentions of the relevant agents. Carrier illustrates this account of agnotological ploys with case studies on bisphenol A and Bt maize and Roundup.

In chapter 4, Philip Kitcher continues the focus on ignorance as active construction, although his special concern is the effect of such ignorance on democracy. In "Can We Sustain Democracy and the Planet Too?" he points to the deliberate efforts of climate change deniers to mislead the public and thereby interfere with genuinely democratic decision making. As he argues, the will of the people in democratic societies should be formed by a free and informed public debate. As a matter of fact, however, the political debate about climate change has been purposefully skewed by agnotological agents, and this structural condition serves to undermine democracy. Some social institutions manipulate public opinion in such a way that citizens' preferences diverge from their own long-term interests. The free choice of the people undercuts some of their conscious goals. Promoting democracy means exposing currently invisible forms of deception by making information available to citizens and helping them to realize where their true interests lie. Crucial preconditions of open debate need to be restored, and science needs to be granted its rightful place in this debate. Meeting this agnotological challenge is the right way to strengthen democracy and at the same time alert people to the threat of climate change.

Kitcher's argument amounts to claiming that the framework of a debate may be slanted in such a way that people may undermine their own long-term ends by pursuing their own preferences. Given certain structural biases, an allegedly free and open debate may contribute to eclipsing people's true intentions as well as undermining the means to their realization. Kitcher's argument thereby illuminates how each instance of ignorance as active construct need not be the effect of a specific intervention. As a result, driving out agnotological distortions should not necessarily focus on particular cases of interference but rather might more effectively address such structural conditions.

The chapters by Carrier and Kitcher consider the deliberate construction of ignorance for harmful purposes. The ones by Janet Kourany and Miriam Solomon, on the other hand, consider "virtuous ignorance": the deliberate construction of ignorance for *helpful* purposes. In chapter 5, "Might Scientific Ignorance Be Virtuous? The Case of Cognitive Differences Research," Kourany takes up the case of race- and gender-related cognitive differences research. As she points out, scientists have pursued this research for centuries, and much of the time scientists have concluded from it that women are intellectually inferior to men, and that blacks are inferior to whites. Of course, these conclusions have been contested and corrected over the centuries as well, but they still continue to be drawn. Meanwhile, scientists have documented the harm done to women and blacks by the publication of these claims. It would be good, therefore, if this research were finally curtailed and research efforts directed more fruitfully elsewhere. So Kourany suggests an agnotological solution. Freedom of research is universally recognized to be of first-rate importance. Yet constraints on that freedom are also universally recognized. Kourany considers three of these constraints, and argues for tighter restrictions on race- and gender-related cognitive differences research on their basis. As a result, certain forms of ignorance should be maintained; they are a virtue rather than a vice.

Of course, whether forms of ignorance *are* a virtue is sometimes a complicated question. In chapter 6, "Agnotology, Hermeneutical Injustice, and Scientific Pluralism: The Case of Asperger Syndrome," Solomon discusses the categorization of Asperger syndrome in the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-5*). The *DSM-5* abolished Asperger syndrome as a separate diagnostic category and subsumed the relevant patients, along with patients formerly diagnosed with classic autism and other disorders, under the heading of "autistic spectrum disorder." The Asperger category had been introduced in 1981, and had become an element in the self-understanding, self-identity, and more for patients (they were "Aspies"), since those with Asperger syndrome were considered only mildly autistic and cognitively high functioning. Thus, the concept served group building and social empowerment. All this was taken away by *DSM-5*. On the other hand, since Asperger syndrome was also considered a rather mild and benign form of autism of high-functioning individuals, keeping the separate category served to stigmatize the rest of autistic patients. That is, a positive identity was accorded to Asperger patients at the expense of denigrating another patient group—namely, those with classic autism. Abandoning this category therefore meant an act of emancipation regarding the rest of autistic patients. Solomon's conclusion is that the overall effect of abandoning Asperger syndrome as a separate category was committing a hermeneutical injustice, but relieving another one too—a mixed bag of epistemic as well as social gains and losses.

The remainder of the volume addresses ignorance as a passive construct—that is, ignorance as the unintended by-product of choices made in the research process. Carl F. Cranor's contribution in chapter 7 on "How the Law Promotes Ignorance: The Case of Industrial Chemicals and Their Risks" deals with focus-generated ignorance. As he contends, the regulatory procedures for releasing chemicals to the market function as an agnotological mechanism. The incentives set by the legal system encourage companies to maintain ignorance about the toxicity of such substances. In this case, social, political, or economic preferences (and probably all three) have established a system of rules and incentives that privileges certain research directions over others. This system leads companies to concentrate on the beneficial and commercial effects of a product rather than its toxic effects, especially its more subtle and long-term toxic effects. The legal framework invites and predisposes companies to remain ignorant about the product's toxicity, because when a substance enters commerce, a company need only report what it knows, if anything, about toxicity. Accordingly, the one-sidedness of the outcome is produced not through a case-by-case intervention but rather generically, as a regular result of how the system is set up. Ignorance here is systematically generated by the established rules, without explicit interference. Of course, this legal system is itself not a passive construct but is instead the product of active deliberation that includes, among other things, pressure from groups pursuing vested interests. (This does not mean the legal system has been crafted by the intention to have people poisoned! Doubtless a variety of interests yielded the present system. It simply points to the poor construction of the result.) Still, the system established in this way sets rules and incentives such that remaining ignorant of harmful side effects is ultimately favored; it is seen as just playing by the rules.

The next two chapters are devoted to framework-centered ignorance and the kinds of conceptual obstacles that make it possible. Interestingly, the place to be filled by new knowledge is sometimes blocked by existing knowledge. Under such circumstances, things supposedly known may make it impossible to ask new questions. In chapter 8, "On Knowing What One Does Not Know: Ignorance and the Aims of Research," Torsten Wilholt examines this predicament by distinguishing the more usual *conscious ignorance* from other forms of ignorance. For example, people in *deep ignorance* are aware of a knowledge gap and are able to form the pertinent questions, but have no inkling of plausible answers. By contrast, *deeply opaque ignorance* prevails when people are unable even to recognize that there is a question to be posed in the first place. They don't realize that there is something more to be known, which is why they simply cannot set out to explore what is missing. Wilholt's taxonomy of ignorance can be particularly valuable to agnotologists. For one thing, it can help them understand the ways in which scientists of the past were unable to anticipate later conceptual developments. For another, it can serve to highlight potential obstacles to remedying present-day ignorance produced by differences in worldviews, cultures, outlooks, or community membership. Wilholt shows that becoming aware of ignorance can be a complex process requiring considerable knowledge as well as competence.

In "Strong Incommensurability and Deeply Opaque Ignorance," Paul Hoyningen-Huene further develops Wilholt's taxonomy of ignorance. Especially, he suggests that the notion of deeply opaque ignorance should be explicated in terms of *semantic incommensurability*—that is, in terms of obstacles regarding translation: when certain distinctions or generalizations in one linguistic framework cannot be adequately rendered in a different framework. More specifically, Hoyningen-Huene explains that certain questions can sometimes not be asked because the required concepts are inaccessible in a given conceptual framework. So knowledge acquired using the one framework cannot be acquired using the other. In short, certain conceptual structures may eclipse other structures and thus thwart the acquisition of knowledge. In such cases, removing ignorance demands a profound cognitive reorientation, which is a truly challenging task. Hoyningen-Huene illustrates this state of affairs with three case studies drawn from the history of science. And he suggests that such deeply opaque ignorance may be the permanent fate of the human condition despite all the progress in the sciences.

In chapter 10, "A View of Scientific Methodology as a Source of Ignorance in Controversies about Genetically Modified Crops," Hugh Lacey takes up methodology-created ignorance. Lacey explores the impact of adopting what he calls "decontextualizing strategies." Such research strategies seek to understand phenomena quantitatively, in terms of underlying entities and processes, while dissociating them from their human and social contexts. Lacey examines how these methodological strategies have affected the research and development of genetically modified organisms, and shows how they hinder the understanding of relations between the agricultural and social realms. These strategies serve to eclipse the connection between research findings and human agency, value, social arrangements, and ecological embedding. Relying on decontextualizing strategies exclusively, Lacey concludes, is bound to produce ignorance of sustainable agroecosystems and the social context in which they can thrive. This ignorance regarding the wider context and impact of research, it is important to note, is not the result of a deliberate choice to create ignorance (an active construction of ignorance). Rather, it is the by-product of implementing research strategies chosen for other reasons (such as the desire for precise and reproducible results).

Finally, in chapter 11, Londa Schiebinger addresses community-based ignorance. In the past and, in fact, up until quite recently, the scientific community was dominated by white, middle- or upper-class men. As a result, the research conducted by this community exemplified a rather narrow set of interests and perspectives, and the knowledge it generated exemplified corresponding limitations. In "Expanding the Agnotological Toolbox: Methods of Sex and Gender Analysis," Schiebinger spells out the kinds of techniques we now need in order to rectify such limitations. Methods of sex and gender analysis are recommended for considering research questions and priorities as well as methodologies, concepts, and theories. Such methods not only make possible more adequate information but also serve to stimulate creativity and gender equality, and make research more responsive to society. For instance, clinical standards for recognizing and treating heart disease have traditionally been modeled on male pathophysiology. As a result, female patients were often misdiagnosed. Introducing gender analysis into this field has prompted changes in the definition and diagnosis of the disease as well as changes in preventive strategies and treatment. Once sex and gender were taken into consideration, knowledge about heart disease improved substantially—for everyone. The upshot is that an imbalance in the gender composition of the scientific community leads to one-sided research outcomes that need to be redressed on a variety of levels. The scope of Schiebinger's program, in fact, extends to all the items and levels that make up the scientific enterprise.

6. The Role of Philosophy in Exploring Agnotology

The essays brought together in this volume share a common feature: they provide the wherewithal to engage with concrete cases of the production and maintenance of ignorance, and do so from a markedly philosophical perspective. The latter is characterized by an evaluative concept of knowledge. Knowledge is not simply what is accepted in a society but instead what is accepted with good reasons. A second evaluative component concerns the appropriateness of items of knowledge from an ethical point of view. As a result, the contributions to this volume come equipped with evaluative judgments regarding epistemic and moral matters. Passing value judgments of this sort is typically considered a privilege of philosophers as well as public intellectuals, but the crucial point is that such an evaluative perspective is indispensable for a comprehensive analysis of both the active and passive construction of ignorance. Only with a rich notion of the epistemic credentials and moral worth of knowledge can the nature of ignorance, the ways it is produced and preserved, and our options for overcoming it be successfully explored.

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2 Agnotology in Action: A Dialogue

Peter Galison and Robert Proctor

1. Ignorance as a Topic of Investigation

Peter Galison: Robert, you and I have been talking about agnotology—the structure of impressed ignorance—for a long time now, or certainly since the time we were graduate students. Over the years, you have been focused quite rightly on the vast industrial apparatus that produces nonknowledge, especially in the spheres of the environment and health. I've been engaged with government secrecy—the excision of knowledge imposed on society, especially in matters of national security. Both of us see the intersection of politics and knowledge, or should I say nonknowledge, as an essential area of inquiry. Let's begin with your side: Could you say a little bit about how you got interested in what we don't know?

Robert Proctor: I remember going to Harvard as a graduate student and being surprised at the apathy of my professors toward what ordinary people know. I had come from the Midwest and Texas, where many of my relatives had been creationists and racists, and I was interested in knowing how this came to be. How could people be so ignorant? So I was asking all my Harvard professors whether they believed in God and questions like that—and they seemed to feel this was rather strange. So I started wondering, Why this scholarly disinterest? Why were scholars so uninterested in what ordinary people know? I realized there was a kind of a vanguardism among historians of science: you study the "best" knowledge not the worst, what is known and how that came to be, what "we" know and not "they." And you ignore what is unknown. So there was this unreflective, implicit presumption of the unknown being either trivial or inconsequential.

I was also teaching Biology and Social Issues (Bio 106) with Richard Lewontin and Ruth Hubbard and others from that Marxist/feminist/Jewish crew, exploring the politics of health and food, impact of military priorities on science, sexism and racism in certain lines of inquiry, and how all this shaped exclusions of one sort or another—certain kinds of people, certain kinds of topics. Key here was this sense of the radical contingency of what gets studied and what doesn't, who does science and who gets science done to them, who benefits, who suffers. I was struck by how much attention had been given to American eugenics, and how little to the "big eugenics" of Nazi Germany, so I spent several years excavating Nazi science, led by the Jewish proverb that "nonsense is nonsense, but the history of nonsense is scholarship!" I started to realize that the history of science as a discipline was overlooking some of the most important questions—as in how ignorance is created and why. So that was part of how I got involved.

RP: How did you get interested in what we don't know, Peter?

PG: Coming of age in physics (and the history of physics) during the height of the Cold War meant that secrecy was always just past the blackboard. Many of my teachers had worked on the Manhattan Project or radar; many in the 1970s were still working or had worked on postwar, classified research. It was early on very salient to me that there would be no writing of the history of modern physics without taking into account physics behind as well as outside the fence. There was another event too—one of those small things that stayed with me for many years. In 1977, I was struck by a newspaper report about a Polish censor who had defected to Sweden with his "Black Book of Censorship" that explained just what was to be censored. It was, in one compact place, a summary of what knowledge should *not* be released: disasters and toxicities, production shortfalls, and much more. These manuals struck me as essential to understand the antiepistemology of knowledge. Here were the rules of not knowing. Such volumes have remained essential to my work across many domains from then to now.

2. The Ignorance Produced by Industrial Science

PG: How did you get interested in industrial science, and in particular tobacco, and the issues surrounding it?

RP: I'd been exploring since my undergrad days the social and political causes of cancer; I'd worked to get a ban on smoking in my sophomore dorm at Indiana University (in 1973), after I realized that about a third of all cancers are caused by cigarettes. Other cancers were caused by what we eat or breathe, the radiation we're exposed to, etc.—topics generally ignored in Richard Nixon's War on Cancer, which focused on cures rather than causes and ignored cigarettes. I realized pretty early on that there were powerful industrial interests trying to shape what we know and don't know about these topics, and incorporated these into our Bio 106 topics at Harvard. I discovered there were about fifteen hundred trade associations whose business was just to protect a particular substance against claims it was causing harm. So there was the Asbestos Information Association defending asbestos, the Global Climate Coalition denying global warming, the Methyl Butyl Ether Task Force defending (guess what)—I'm talking about the late 1980s now—plus of course the Tobacco Institute defending tobacco. Most of these trade associations were denying there was "sufficient evidence" to convict a particular compound of causing harm. And this was a relatively unstudied social aspect of science, although Harvard did have a radical medical student newsletter cautioning that "Harvard may be hazardous to your health!"—in 1973, thanks to payoffs from the Tobacco Institute (*Present Illness*, October 1973). What I found remarkable about these trade associations was how they were using (or creating) science to create ignorance—partly by funding what I like to call "distraction science" or "red herring research." Science was effectively being supported as part of an effort to disguise harms.

Recall that agnotology is both the study of ignorance and the studied social production of ignorance—just as English is both a language and the disciplined study of that language. What's brilliant about the industrial production of ignorance is that when it was developed in the tobacco context in the early 1950s (following earlier efforts to defend lead and sugar, inter alia), they could actually claim to be acting in the name of science when they called for "more research." The call for more research was an effective legal-savvy form of denial, expressed in a manner that effectively captured the allegiance of universities (and the high rhetorical ground of open-mindedness) while retaining plausible deniability in court ("we never said cigarettes are safe!"). Science could also be supported in such a way as to distract from the evidence that was accumulating that cigarettes were in fact causing cancer.

PG: One of the most striking features of governmental secrecy is its extent. For fiscal year 2014, for example, the government itself estimated that the cost of secrecy runs, for the government as a whole, about $15 billion (*Report to the President* 2014). That is bigger than the fiscal year 2016 budget for the Department of Labor ($13.2 billion) or the Environmental Protection Agency ($8.6 billion) (Federal News Network Staff 2015). The effort involves tens of millions of pages of classified material that must be sorted, classified, and guarded. Recognizing that the scope of this effort is important—scale matters, and tracking the structure and effects of this system is part of what I've tried to address not only in my written work but also in my film (with Robb Moss), *Secrecy* (2008). Of course, even these staggering numbers don't include the full range of outsourced materials, much less the secrecy that has its origin and maintenance in the private sphere. Which leads me to ask, How big is the production of distraction or decoy science in the industrial realm? Can you give a sense of the amounts of money involved, or the number of people, or any measure of how widespread it is?

RP: Well, in tobacco alone, between 1954 and the 1990s, $450 million were funneled through just the Council for Tobacco Research (CTR), only one of several arms of the cigarette conspiracy. Most of that went to distraction or decoy research, mainly into basic virology, genetics, biochemistry, immunology, etc.—any kind of agent that was not nicotinic. Over seven thousand papers were published with support from the CTR, the principal arm of the cigarette conspiracy. CTR-funded scholars went on to win at least ten Nobel Prizes—so we're talking about some pretty solid research, albeit "harmless" from the industry's point of view. The problem is not really visible from looking at any one published paper; from a micro level, the conspiracy, the intent, is invisible. The pernicious intent is really only visible when you look at the research funded in the aggregate—and fortunately we have the industry's own secret documents, which talk about the

CTR as a "front," a "shield," and "a successful defensive operation." Again, the intent was to distract from cigarettes as a cause of harm, especially by focusing on proximate rather than ultimate causes. CTR research can be considered a kind of data chaff, jamming the scientific airwaves with noise. The CTR also funded a good deal of basic research, along with research looking at harms from things like carpet fumes, radon, occupational exposures, genetic predispositions—all of which are respectable topics, but which in aggregate create an impression that something other than tobacco is causing harm. And that's why you can't see the bias in any one publication. You have to look at macro bias rather than micro bias, because it turns out that if you actually control for industry funding—when looking at, say, the hazards of secondhand smoke—you get a very different result than if you just take the aggregate of all research that's published.

PG: I remember one example you once told me about, where it was discovered that miners in coal country got lung cancer or black lung disease or other diseases of the lung in large numbers if they smoked and were miners. The response was not to improve mining conditions, it was instead to ban smokers from some of the mining tasks. I might not have that story quite right, but could you say what happened, and how it fits into this idea of distraction or chaff science?

RP: Cancer hazards are often multiplicative, synergistic—which makes sense if the disease is caused by the accumulation of mutations. So if you are exposed to asbestos, you increase your risk of lung cancer by, say, fivefold, and if you smoke you increase it by fivefold. But if you smoke and are exposed to asbestos, you increase your risk by fiftyfold; there's a nonlinear, disproportionate augmentation. What's interesting about that is how different industries strategize to blame something other than themselves. Asbestos will blame tobacco, and tobacco will blame asbestos, but what's important is that each of those groups can create and then rely on particular types of science as weapons to be deployed in court. And a lot of these strategies do arise out of litigation—one of the three pillars of the cigarette conspiracy (the others being legislation and public opinion). Expertise is important in court, and cigarette makers have been able to create what they call a "stable" of expert witnesses to defend themselves and their products. And the role of science in this? As Imperial Tobacco once said, "Research must go on and on." Support for science is used as a way to prevent certain kinds of questions from ever becoming "closed." In the tobacco case this was called the "open controversy": the idea was that by continuing to support science, they could use this very fact ("we need more research") to claim that harms had not been definitively proven.

It is important to appreciate the scale and scope of this campaign. CTR research alone resulted in over seven thousand peer-reviewed publications. The collaboration with the American Medial Association produced hundreds of others—and I mentioned the ten Nobel Prizes. At least twenty-six Nobel laureates have taken money from the cigarette conspiracy. R. J. Reynolds funded almost all of Stanley Prusiner's research into prions, for which he was awarded the Nobel Prize in 1997. Again good research, but part of the cigarette makers' interest in funding "small virus" research to distract from cigarette causation. Cigarette makers helped found the field of behavioral genetics; they funded leading scholars, like Hans Selye and Ancel Keys, publishing on the role of stress or cholesterol in causing heart disease. When people today think of heart disease as caused by stress or cholesterol, that is in no small part because cigarette makers encouraged this kind of research. Keywords here are corruption and monopoly (they tried to monopolize certain kinds of expertise), but also alternative causation and open controversy.

PG: You mean that the opposition in a sense would have to take a position of illiberalism? They would have to say, "We are against further research"?

RP: That's right.

PG: And that's a bad rhetorical spot to be in; by making doubt into a product, the industry has hidden perfectly well-established science. Demonstrated health risks became uncertain topics for research; inquiry became the opposite of understanding. Here is a particularly damaging form of knowledge manipulation and an important result of focusing on how ignorance is produced—agnotology at work.

RP: That's right. Tobacco and other industries were able to use their support for science as a central pillar of their conspiracy, which was that we don't really know whether cigarettes cause cancer. They were able to use the openness of the question and their support for research as a defense of their legal stance and propaganda position. So it's a brilliant example of using the liberal rhetorics of science to defend what's essentially a criminal enterprise: a conspiracy to hide the hazards of smoking. It's really quite brilliant because it captured the authority of science and allegiance of scientists, made the tobacco industry seem open-minded, and made public health advocates seem like close-minded fanatics.

PG: And scientists who wanted to take the industry's money would then be able to say to themselves and their peers that they were involved in open-ended research, that as long as they were doing research they weren't guaranteed to produce what the tobacco industry hoped they would. They were simply adding to a general, broad and open-ended debate.

RP: That's right. I think of it as something like an army of idiot miners, paid to look for gold where the funders know there's no gold to be found. And then you (the paymaster) say, "Oh look, there's no gold." Cigarette makers loved funding research topics that posed no threat to their business. So most of the research funded by the industry had nothing to do with tobacco, and certainly nothing to do with that product causing harm (with a few exceptions, which I call "leakage"—since the conspiracy was not perfect).

PG: It's significant too that these techniques of ignorance production have multiplied over many different domains. Naomi Oreskes and Erik Conway's (2011) *Merchants of Doubt* shows how these same techniques, and indeed same public relations firms, even many of the same prominent scientists, went on to argue that climate change was "doubtful" and in need of more research. Once again, an "openness" toward scientific research could, in the event, serve short-term industrial gain, block regulatory and political action, and scramble public debate, even when the science was clear.

RP: It's really quite brilliant: if you don't like the science that's out there, create some of your own. And then claim "we need more research." And then label your opposition as a bunch of close-minded fanatics. Later this became more subtle, with the industry claiming that knowledge of cigarettes causing cancer was "common knowledge," and had been so for hundreds of years. This was part of the industry's "assumption of risk" defense: you were fully informed of the nature of the hazard, information on the hazard was "available," you have only yourself to blame for whatever harms you may have suffered. The common knowledge defense is now deployed in every tobacco trial. So first they falsify science, now they falsify history.

As for science, though, the deceit was not what we normally think about in terms of research misconduct—falsification or fabrication of data, for example. The bias was further upstream than we imagine, and the fact is that once funded, the industry generally did not influence the science funded.

PG: You mean if they accepted money for research from the Tobacco Institute or CTR?

RP: Right. The CTR would say, "Publish whatever you want." But the bias was built in to the selection of problems in the first place. And that's a general principle that historians and philosophers need to pay more attention to: problem selection and funding shape what kind of science gets done. One of the more general points about agnotology is that there are infinitely many things you might know, and that whatever in fact becomes known is only a tiny sliver of what might be known—infinitesimal really. What this means is that when you're shining a light on something, almost everything else remains in the dark. And sometimes that darkness is deliberately kept dark; the darkness itself may be created, maintained, exaggerated, inflated, and reinforced, sometimes even by the very power of the light itself (think flashy fish lures or Donald Trump). I think there's an assumption in a lot of thinking about science that there is some finite quantum of knowledge humans might acquire. Maybe we'll never get it all, but at least we're moving forward, vanquishing the darkness. But darkness has many friends, and often deep pockets as well. And darkness can easily grow as fast as (or faster than) the light. So it's much more a constructive or organic metaphor that we need.

PG: Of course, the tobacco industry employed other strategies of ignorance production as well. One technique that was often used was to mention things out of proportion. For example, 90 percent of lung cancer is caused by smoking. But it's true that people sometimes will know somebody who got lung cancer but who didn't smoke. If you obscure the fact that 90 percent of lung cancer comes from smoking by mentioning twelve other possible ways to get lung cancer—or even having in that 10 percent some that are completely of unknown origin—then you've actually created ignorance.

RP: Yes. I like to ask my World History of Science students, "Are you convinced that smoking is the leading cause of lung cancer?" only about two-thirds will say yes. If you ask, "Do you think smoking causes cancer?" a much higher proportion will answer in the affirmative. And if you ask, "Have you heard that smoking causes cancer?" an even higher proportion will say yes. So much depends on how the question is asked, and this has often been exploited by the tobacco industry in court because it'll say, "Look, in the 1950s, 90 percent of Americans had heard that smoking causes lung cancer." But they'd also heard that aliens were being held in Area 51. There is a big difference between awareness and belief.

PG: Certainly if you are aware of a debate, you could say, "Are you aware that there's an argument for increasing taxation? Are you aware there's an argument for decreasing taxation?" People would with high probability recognize both of those.

RP: Right. The industry is able to use this proximity of ideas in deception. Merely by pointing to someone who smoked and lived a long time—George Burns lived to be a hundred—the industry creates the impression that smoking doesn't always causes cancer, which of course is true. But you also have to remember that if you ask ordinary people, ordinary smokers, "Do you think smoke will kill you?" the most common answer will be, "Well, if God wants me to die, I will die." We tend to forget that most people are still monotheists and think their fate lies in God's hands. That has agnotological implications.

PG: The everyday epistemology is really strange to me, given that we don't actually have experience with absolute causality in our daily lives. Wearing a seat belt while driving does not make you invincible against any possible car crash, it just makes you more likely to survive or to survive with lesser injuries than had you not worn it. But when we're talking about a public, disputed, economically powerful area and whether something causes something else, as a society we often go back to saying, "If there is an exception, then the thing isn't true." That's strange, right? Because we know someone who lived to be a hundred and smoked, we think smoking really shouldn't cause cancer, even though most of the causal things we have any experience with are probabilistic. And in science, our most basic account of the world, quantum mechanics, tells us that when one elementary particle hits another, the outcome can be predicted only probabilistically. So whether we are deciding about whether to wear a bicycle helmet or how deep-inelastic electron scattering will proceed, we use, formally or informally, a notion of probabilistic causation. It's really stunning that the industry can get away with saying that causes inevitably either lead to a particular outcome . . . or else they are not causes at all. Strange reasoning, and stranger still that it worked (somewhat).

RP: Yeah, it's called sophistry! But it seems to work. One of the brilliant things cigarette makers were able to do was to say that unless every person who smokes gets cancer, and unless every cancer is in someone who smoked, then smoking doesn't cause cancer! By that same logic you'd say that drunk driving can't cause traffic accidents, because some people who have accidents weren't drinking and not everyone who drinks has an accident. So cigarette makers created an impossibly high bar for inferring causality. And for decades they were successful using this argument—by essentially redefining causality. They actually funded a lot of scholars, people like Alvan Feinstein at Yale, an early CTR special projects operative and one of the founders of evidence-based medicine, to create such a high bar for evidentiary proof that nothing could jump over it! So there's a long litany of actual harms he was able to deny because they didn't meet his criteria for causality. The industry was able to manipulate these philosophical ideas to its benefit because of how much money it's got.

3. Ignorance as a By-product of the Media and Publishing

PG: Back in 2004, Oreskes published an article in *Science*, "Beyond the Ivory Tower: The Scientific Consensus on Climate Change," which examined the nearly one thousand abstracts of papers in refereed scientific journals that explicitly referenced "climate change." She wanted to know whether, as the media often implied (in their crude parody of objectivity), there was a roughly even split between those who argued for human-made climate change and those who argued against. What she found was that 75 percent of these articles implicitly or explicitly agreed with human-originated climate change, and that *none* took the position that this was not so. The so-called scientific debate had effectively been created in the public sphere by what you call chaff science—all these news reports, even the nightly news version of objectivity, which is 50 percent of X and 50 percent of anti-X. If you think that way and consider that debate "makes good television," as the news announcer Ted Koppel used to say, then it's easy to mistakenly think that there are 50 percent of scientists on one side and 50 percent of scientists on the other side of the climate debate. Once there is a debate, it can be made to seem like it's a coin toss. Experts disagree, so this line of implied reasoning goes, and what's a reasonable person to do but to stay agnostic?

RP: Right. It's sometimes called the balance routine. Journalists usually have short deadlines, they don't really know the technical details, and they think it makes a better story if you give two sides to an issue. And it's one of the reasons, as you used to like to say, that [Dwight David] Eisenhower wanted a one-handed adviser because there aren't always two sides to every question! In the tobacco case, what would happen is there would be a story and then at the end of the story there would be the tobacco institute's refutation. And like the tail of a kite, such denials would be attached to every announcement of new evidence of a hazard to make for more exciting reading. We actually have some great industry documents on this, instructing writers and editors of cigarette propaganda on how to headline their stories: according to Hill+Knowlton, the public relations arm of the conspiracy, headlines in the Tobacco Institute's *Tobacco and Health Reports*, sent to every doctor in the country, were supposed to "strongly call out the point—Controversy! Contradiction! Other Factors! Unknowns!" Journalists are now starting to appreciate the danger of such an approach, realizing that not every "controversy" has two equally valid sides, and "balance" can actually misrepresent the truth.

PG: Another place where this kind of fabricated scientific debate gets deployed is around the creationism and intelligent design issues, where the creationists and intelligent design advocates would say, "Just teach the debate. That's all we're saying." And then again, somebody who said, "No, actually you should just teach evolution as best we understand it," would again be forced into that illiberal position of seemingly being against inquiry. Agnotology makes this process of ignorance production into an object of inquiry.

One of the most famous sites for these debates over teaching uncertainty—making secure scientific knowledge insecure—occurred in the small town of Dover, Pennsylvania. Schools there had wanted to teach "intelligent design" as a viable, non-Darwinian account of how the biological world came to be as it is today. The 2005 case (*Kitzmiller v. Dover Area School District*) challenged a policy that required teachers to discuss intelligent design as an alternative theory to evolution, claiming that intelligent design was a not-very-covert form of creationism that had been rejected by the courts as religious doctrine. Plaintiffs showed that the required textbook, *Of Pandas and People*, (written before but revised and published after *Edwards v. Aguillard* had barred the teaching of "creation science" in public schools in 1987), simply replaced the words "creation," "creationism," and similar terms with the phrase "intelligent design," without changing any of the actual content. Plaintiffs argued that *Pandas* was in effect a creationist textbook (with a search-and-replace toggle), and whose requirement violated the First Amendment's Establishment Clause, which prohibits the "establishment of religion" by the government. Judge John E. Jones III sided with the plaintiffs, equating intelligent design with "creation science," which "'is simply not science' because it depends on 'supernatural intervention.'"[1] The courts were pretty clear: you can't hide religious doctrine in a proxy bit of cooked-up science.

RP: Right. It's a little bit like when my ten-year-old son would ask, "What if I say my religion is two plus two equals five? Does that mean I won't be counted wrong in math class?" The strategies there have changed interestingly over time, because for many years after the Scopes trial, it was rare to find evolution being taught in US schools. But then they had that successful campaign to introduce balance laws, or what they called "equal time": if you teach Charles Darwin, you have to give equal time to what they call creation science and, more recently, intelligent design. This is one of the problems of a type of radical constructivism, the kind of symmetry of different epistemic systems that some philosophers of science like to uphold, balancing forage against garbage. It's almost like a justification of this balance routine in the media and shows the impoverishment of that point of view. Now we're seeing it powerfully with things like global warming denialism, and things that are really of life and death consequence.

As for creationism, I think that's generally a more honest form of agnotology; creationists seem to actually believe the world is six thousand years old and the Bible is the literal word of God. The irony is that intelligent design really poses an interesting, nonobvious problem: How do we know when something is the product of an intelligence? That is a key question posed all the time in archaeology and the search for extraterrestrial intelligence: How

1 *Kitzmiller v. Dover Area School District*, 400 F. Supp. 2d 707 (M.D. Pa. 2005).

do we know that an artefact is human made and not a geofact? And how do we know that a pattern of signals from outer space is the result of an alien intelligence? Those are fascinating, nontrivial questions. But intelligent design à la refurbished "creation science" ignores these genuine questions in favor of a pseudoscientific argument from ignorance—basically, "I can't imagine how natural selection could have produced this micromotor in a flagellum, so it must have been made by an intelligence, really an unmoved mover whose name I won't mention." That is just intellectual sloth, cryptocreationism, as even republic magistrates have realized (in the notorious *Dover* case in Pennsylvania).

PG: True, one can distinguish between sincere and insincere advocates of this form of doubt production. But I'm perhaps a tad less persuaded that the *Kitzmiller v. Dover* case has a solid bit of sincere objection. After all, one of the great blows to the prointelligent design case was when that side insisted that its views were utterly independent of creationism—which the courts had struck down as an offshoot of established religion. You may recall that the philosopher of science Barbara Forrest showed the court that the antievolution side had, in fact, taken a creationist manuscript and simply swapped out "creationists," replacing it by "intelligent design proponents." In the end, we may need to focus more on the structure of the argument ("there is always doubt, there is always more research to do, we cannot act when there is doubt") than whether those advocating are, in the inner recesses of their souls, convinced or cynical.

For years, the Texas school system has had disproportionate influence over textbook development because it buys textbooks as a block. So what gets incorporated into the Texas curriculum has dictated what a lot of high school textbooks look like all over the country. As tough as that may have been in the twentieth century, because there were really big battles over what would be said about evolution, we are now entering a time, I think, where we're going to have modular online units of learning and teachers will essentially compose their own textbooks. I think the future of the hardbound, expensive textbook that's been approved by the Texas Board of Education may well come to an end. We may have more openware that people will assemble in different ways. Of course, teachers always had the option of not teaching a chapter on evolution if there was one, but once it becomes modularized in this way it's going to be possible to eliminate all sorts of things that teachers or school boards or counties or states don't like. And in a way I think that battle hasn't been fully realized yet, but in the future we're going to have free shared modules that people will assemble. How that chapter of our educational history will unfold still remains to be seen.

RP: Yes, I hope you are right! That ties into the much-discussed dangers of the "echo chamber" of the internet, where personalized search (really, personally return) has led us into "filter bubbles" allowing seemingly endless confirmation bias. It is now of course easier to find whatever you want on the internet, reinforcing old forms of cultural tribalism. The spread of massive digital misinformation has become ever cheaper and easier. But again, one difference between creationism and the whole business of industrial doubtmongering is that creationists actually seem to believe what they say (with some brazen exceptions, as in the *Dover* textbook word swap). Even in the history of propaganda, however, there has always been this distributed mixture of imagined truths and outright lies. So intent is important, especially with regard to distinguishing different degrees of honesty and dishonesty (and self-deception). In the creationism case, you have strongly held convictions that seem to be genuine; in the tobacco case, there's clearly a type of duplicity—but there must also be a kind of denial in the psychological sense. As when causality is redefined so that cigarettes actually don't cause cancer!

In the climate case, there's seems to be a mixture of honesty and dishonesty, because it's sort of like the point Oreskes and Conway were making earlier about surrogacy: climate denial is often really just a disguised defense of untrammeled capitalism or God's benevolent plan for the planet. Oftentimes the force of an idea is different from its literal expression, and the true point or intent of an expression remains unexpressed (or dog whistled). Creationism is a great example because in the Scopes trial in the 1920s, the question of whether humans evolved from apes was really a surrogate for whether racial mixing is tolerable or intolerable. So "humans evolving from apes" in the 1920s was de facto understood as black men having sex with white women. If you look at the Klan literature from the 1920s, it's really associating evolution with miscegenation, whereas creationism nowadays is much more about the dangers of homosexuality and abortion—and Darwin is blamed for the Holocaust! In the climate case, climate deniers see environmentalists as watermelons: green on the outside, red on the inside. Basically socialists with a big government agenda. So it's an interesting way in which a lot of strongly held views are really surrogates for some other thing that's really at stake: creationism is about race and then feminism and homosexuality; climate denial targets environmentalists and challenges to some religious order.

4. The Ignorance Produced by Governments

PG: Government secrecy is another place where we, as a culture, are more and more accepting of big sectors of our world being blacked-out spaces.

RP: And more and more people do not even know that they're blacked-out spaces, they're so blacked out. A lot of people really don't have any idea about how many secrets there are, or how strongly they are actually organized by our government. There's an entire apparatus designed to create secrets that few of us know anything about.

PG: Yes! Understanding the mechanism, how things actually come in and out of secrecy, is a hugely important practical means toward making our world more understandable. I've been interested in laying out these procedures, in how they are established, enforced, and changed. What kind of thing can be a secret, and how that has changed over time? Probably the most important fact about the history of secrecy is that it is punctuated. Secrecy doesn't just slowly increase over time, it gets amplified in times of conflict and war, and never fully relaxes to its preconflict form—more like a ratchet than a spring. And the three big moments, I would say, over these last hundred years of secrecy have been World War I, World War II, and the terror wars. Let me expand on this a bit.

Governmental secrecy came into something recognizably similar to the system under which we live now starting with the Espionage Act of 1917. Under this act, sharing information that influenced the outcome of a military operation—whether giving advantage to an enemy or to the detriment of the United States—spreading dissent, or interfering with recruitment and enlistment practices were punishable by fines, imprisonment, or death. Under this act, the picture of a secret was one of propositions or individual objects—for example, the exact diagrammatic layout of the Springfield Armory or the statement that "General John J. Pershing will arrive in Boulogne, France on June 13, 1917." And that understanding of what kind of thing a secret could be had different ways of being manifested. It could be exhibited by a photograph or a drawing or a letter, but secrets at this time were basically what in ordinary speech we would call facts. This fact or that fact would be dangerous if other people knew it. In a kind of addendum to the original Espionage Act, there was a powerful and highly problematic addition known as the Sedition Act, under which you could be prosecuted if, for example, you interfered with the process of recruitment. And that was a much more draconian standard because it allowed prosecution if you said—for instance, as a farmer in Montana did—that the fields of France will be fertilized with the blood and bones of our young men. Just that utterance, by its demoralizing power, could be seen as interfering with recruitment. So spoken or written opinions could be considered to be violations of the Sedition Act. Even President Woodrow Wilson was uneasy with some of that language and eventually it was rescinded, whereas the bulk of the Espionage Act continued, and has been added to, modified, and updated in various ways to cope with new technologies.

In World War II, a new kind of secrecy arose: the classification of whole domains of research. At one point, just after the war, all knowledge about elements on the periodic table from uranium on up were secret; the whole domain of the physics of fission fell into the black zone. At this point domains of knowledge became classified—a far step past the mere utterances of the Great War.

Finally, in what we might call the terror wars—from the Patriot Act of 2001 forward—the reach of secrecy extends far beyond Cold War secrecy. As the notion of a target expanded, infrastructure (gas mains, water conduits, electric switching stations, and electronic and data centers) could be classified—in ways that had been excluded even as the Soviet Union and NATO faced off across the divide. Secrecy extended even beyond the extension of classified facilities to include infrastructure. Indeed, purely *symbolic* sites could be brought under the veil. It became possible to classify aspects of national monuments. To cope with this augmented sense of vulnerability in which just about all we could see around us was potentially swept into secrecy, whole new kinds of restricted (but unclassified) knowledge came into existence. There are hundreds of new kinds of bounds on what can be known—unclassified but restricted. Published documents (such as maps of floodplains beyond dams) could be withdrawn from libraries. This was a new and nearly boundless extension of a government-imposed curtain of ignorance—one that extended far beyond a nuclear weapons design secret or the silence surrounding a military operation.

RP: What do you think are the oldest secrets? Are there things that remain secret today that were originally classified over a hundred years ago?

PG: In 2011, the CIA declassified a clutch of documents that up to that point had been hidden since World War I. Several had to do with French, German, and US invisible inks, one instructed how to write with them (use a quill pen), and another, a most discreet way of opening letters surreptitiously (but don't breathe the chemical solvent).[2] Why that took so long to be released from the secret stash, I can't say.

RP: Those were still actively classified up to 2011?

PG: Yup.

RP: Could you say something about how more recently secrets are actually maintained? Because it's not just a matter of failing to reveal them, there's actually a cost to maintaining them; they're created, they're sustained, there's an army of censors responsible for administering them.

PG: Every level of secrecy has its own formula for protection and corresponding degrees of punishment for those who expose them. For example, top secret documents must sit in an approved form of container, with at least one of the following:

(A) Continuous protection by cleared guard or duty personnel;
(B) Inspection of the security container every two hours by cleared guard or duty personnel;
(C) An Intrusion Detection System (IDS) with the personnel responding to the alarm arriving within 15 minutes of the alarm annunciation . . . or
(D) Security-In-Depth conditions, provided the GSA-approved container is equipped with a lock meeting Federal Specification FF-L-2740.[3]

2 See Freedom of Information Act Electronic Reading Room, CIA, accessed May 30, 2016, https://www.cia.gov/library/readingroom/search/site/secret%20writing.

3 32 CFR Part 2004, Safeguarding Classified National Security Information; Final Rule, sec. 2004.6, Storage, accessed May 30, 2016, http://www.fas.org/sgp/isoo/safeguard.html.

Such precautions cost money—as does the process of maintaining those who create, sort, and apply the rules.

People sometimes don't realize how pyramidal this structure is. At the top is the president, the first classifier among all classifiers. The president then vests the authority to classify in the heads of different cabinet-level agencies—the secretary of defense, the secretary of energy, and so on. Each of these heads of agencies then deputizes a group of people, the smallest number of whom can actually classify as "top secret" certain things. These are the "original classifiers," the people deputized by the heads of the cabinet-level agencies to create new secrets, to say that something is a secret that wasn't a secret before. Over the years, there has been an effort across the executive branch to adhere to Executive Order 12356, Section 1.2 (d) (1): "Delegations of original classification authority shall be limited to the minimum required to administer this order." The US Information Security Oversight Office (2017) has monitored and reported on the number of classifiers at every level, and by the 2000s, the number of "authorities" able to classify new secrets at the top secret level was down to just shy of two thousand.

Below the secret creators is a much larger group of people called derivative classifiers. And they simply say, "Does the document in front of me, which I have to classify, contain information that the original classifiers have said is secret, top secret, or confidential." Often, they will even do that paragraph by paragraph: this paragraph is unclassified, this paragraph is top secret, and this paragraph is secret. Then the document as a whole gets the classification of the highest level that's assigned to its contents. Using those rules of classification set by the original classifiers there were, however, in 2017, some 9.6 million top secret "derivative" classification actions and 36 million such derivative actions at the level of secret (Information Security Oversight Office. 2017). And then there are the 5.1 million Americans who, in 2014, had security clearance (Fung 2014). For quick reference, that's more than five times the number of medical doctors—and means that about one out of every fifty adults in the United States has a clearance.

RP: Is "top secret" the highest level of formal secrecy classification?

PG: Yes, but then there can be special programs that have a need to access what is top secret. There was a big battle over this back at the height of the Cold War; every general and every colonel wanted to be head of such a special program, because having the authority to “read people into your program,” as the expression goes, was considered to be a mark of authority, importance, and status. This proliferation caused no end of problems, and over the years the number of people with the authority to create new secrets was reduced, even though the total number of classified pages grew wildly.

RP: Are there some things that are secret to everyone except a tiny number of people?

PG: Yes. Even if you have a special program, even if you have a top clearance authorization in the navy, that doesn’t say that you can go over to a nuclear submarine yard and read all its top secret documents.

RP: Because you don’t have a need to know.

PG: Yes, you don’t have a need to know.

RP: And when did that idea of a “need to know” arise? World War I?

PG: It became important in World War II. It may have existed in some antecedent form in World War I, but in World War II, in the big, multibillion-dollar weapons projects—the atomic bomb and radar—this is when the massive, bureaucratic structures of secrecy first took hold. It was only then that began an effort to limit the damage that could be done by one person with top secret clearance. In the war, the Manhattan Project leaders tried to restrict what people on the bomb project could say to each other; that was the start of large-scale compartmentalization. Scientists on the bomb hated it; Edward Teller, for example, railed against this kind of walling off of some secret holders from others.

RP: Is it true that there were certain words that were classified?

PG: Yes.

RP: I’ve heard that the word “radiation” was not used in the first two hundred reports after the Hiroshima bomb, and that the word “plutonium” was not supposed to be published.

PG: The radar laboratory was called the Radiation Laboratory (Rad Lab), and the Manhattan Project atomic bomb laboratory avoided the term “radiation” in its title. In fact, the A-bomb scientists tried spreading rumors about what they were doing at Los Alamos that had nothing to do with the atomic bomb; they tried to get false information out there.

RP: Isn't the name "health physics" a kind of disguise for the medical effects of radiation?

PG: Some of the dangerous effects of radiation were known from early on as people began dealing with X-rays back at the end of the nineteenth century. In World War II, there were a myriad of names that used to disguise work on the atomic bomb and its constituent parts—and yes, the effects of nuclear weapons components and use on bodies. The bomb itself became the gadget, plutonium-239 was replaced by "49," with the "4" standing for element 94 (plutonium) and the "9" for the fissionable isotope of atomic weight 239. Some participants have identified the origin of the name "health physics" as a similar sleight of expression—a way of avoiding saying or writing "radiation."

RP: Would there be clearance-related activities in the Environmental Protection Agency EPA or the Occupational Safety and Health Administration, or is this all military related, and therefore military and atomic?

PG: It's definitely not just military. As I said, in recent times we've gone from this sort of propositional secrecy of World War I to the scientific domain classification of World War II, where things that had to with chain reaction physics were classified, where the separation of isotopes was classified—isotopes that could be used for making nuclear weapons. You didn't classify things like power plants, dams, electric transmission cables, or telephone switching stations, but with the Patriot Act in the aftermath of the 9/11 attacks there has been a huge expansion beyond even certain areas, like the World War II classification of chain reaction physics, to include infrastructure that had been expressly forbidden to be classified during the Cold War. So you have a growing domain of what potentially can be classified, and it now includes a kind of parallel system, which I think of as a parasecrecy of controlled but unclassified information, which itself has a hundred or two hundred different subareas—for official eyes only, for official use only; there are lots of these. So something could actually be kept from you or kept from the public that isn't classified—like how often the guards change at Mount Rushmore, or what the capacity of some wires is that come out of a switching station, or something of the sort. So there has been a huge expansion in the remit of what can be kept in the dark.

RP: Do you think there are secret laws of nature?

PG: Well technically you are not supposed to classify a law of nature, but this becomes a rather subtle issue. In addition to the kind of examples we spoke about earlier—like classifying aspects of nuclear physics in the 1940s—there are even branches of mathematics that have come under pressure to classify. For years, mathematicians have argued about what their obligations are and aren't to cooperate with the National Security Agency and the British equivalent, the Government Communication Headquarters. As it turns out, there are deep issues of number theory and algebra that cross with fundamental demands of cryptography. The form of the tension has changed—from work relevant to decryption to questions that bear on backdoor access to encrypted commercial data. The questions run deep. Without a lot of the mathematical work, we wouldn't have any internet security at all; there would be no internet commerce and no privacy. Much of our banking and financial system would grind to a halt if you couldn't secure a deposit or effect a transaction without risk of identity theft or other fraud. All that stuff is encrypted with things that had been classified not many years earlier. The idea that "the purity of mathematics protects the field from classification" has gone the way of the carrier pigeon.

5. Virtuous Ignorance

RP: Well, this bridges to another topic—what I like to call virtuous ignorance. In other words, a lot of the things that could be known, you don't want to be known. You don't want it known how to take an AIDS or bird flu virus and make it airborne, you don't want it known how to make a neutron bomb in your basement, you don't want it widely publicized how explosives can be made, or what the most vulnerable parts of a city's water supply or an airplane are, or innovative ways to smuggle something. So there's a lot of ignorance that is actually good, right? I like to think about the whole notion of a right to privacy as a form of virtuous ignorance. We all have things we don't want other people to know about ourselves; we all can name examples of dangerous technology that are better not distributed. So how should we think about that?

PG: I think there's an enormous temptation for people who are shocked by the huge expansion of secrecy and the parasecrecy regime of controlled but unclassified information to worry that if too much is hidden, we are not going to be in a position to make democratically reasoned decisions. But from that shock to the idea that the procedures for making binary nerve gas should be published—that is a jump, and to me a non sequitur that could get us killed.

RP: So do you think that secrecy is inherently authoritarian at the level of the state?

PG: Secrecy always concentrates power because if you don't know, then you can't participate. As I see it, the question is not whether there should be any secrecy but rather how do we keep it from overwhelming us, how do we keep so much power from flowing to the center that democracy becomes unworkable? The most important decision our society makes is whether to go to war. In my view an overheated and overly powerful secrecy system left us, the people, along with Congress and the courts, in no position to assess the real evidence (of which there was none) that Saddam Hussein's Iraq had or was building nuclear weapons. This was a catastrophic failure—one built on a rotten foundation of callouts to secret knowledge.

RP: But still, secrecy can be a good thing or a bad thing, right? I mean, secrets about yourself allow you to maintain control over yourself and prevent others from doing you harm, no?

PG: Well sure. Without privacy we cannot have the trust and intimacy that make our lives worth living. And yet I think we have to be careful about identifying privacy and secrecy. At root there is a fundamental asymmetry: protecting the individual from the overzealous prying eyes of the state is a matter of defending the less powerful against the more powerful. So we are often tempted to make an identification that might be expressed:

Individual: privacy; state: secrecy

Indeed, more generally in my view, we have moved, in our laws and common discourse, far too quickly toward the identification of the person, the corporation, and the state. They are not equivalent, no matter what the Supreme Court decided about First Amendment corporate rights in *Citizens United*. Even now the Supreme Court does not allow a corporation (or for that matter the state) to plead the Fifth Amendment to avoid self-incrimination. And we should not think that the state deserves the same degree of protection that we aim for in making sacrosanct privacy of an individual, a family, or a couple.

RP: Yes, *Citizens United* has to be one of the worst Supreme Court decisions of all time; it basically enshrined and formalized a new political principal—one dollar, one vote—and gave "dark money" new and unprecedented powers.

PG: I want to distinguish individual privacy from corporate or state secrecy. On one side, we want to protect an individual's most intimate beliefs and relationships from prying eyes. Does that obligate us to defend a kind of privacy for the state (state secrecy)? Or conversely, if we are for transparency for governmental—that is, national security—secrecy, are we obliged to advocate a similar transparency for our personal lives? I think this often made analogy is false: the state is *not* a person writ large. Nor, for that matter, do I buy the specious reasoning that corporations are people too, with rights of expression or other such privileges. The corrosive creep, recently accelerated in the United States, of corporate personhood (or for that matter, a way of speaking and thinking that implies a state personhood) toward natural personhood strikes me as highly dangerous.

That said, like you, I think it would be a much worse world if it was easy to find out how to make weapons of mass destruction, or how to use the internet to collapse the national grid or a hydroelectric dam. So if we're opposed to the overreach of secrecy, should we take the position that we're against all secrecy? There's a kind of satisfying absolutism about saying, "Just let everything get out there." I think that we are, in all our political engagements, always going to be using judgment. There may be some things that we think are better not disclosed but whose disclosure we don't want to criminalize, and some things we think are better not disclosed whose disclosure should be criminalized. Surely we want to protect the name of somebody who is investigating a plot to steal from or reveal a nuclear or chemical weapons program in order to give us realistic estimates of the danger. That seems to me what our intelligence agencies should be doing. But there are other places where the arguments are dicier. The *New York Times* and *Washington Post* began publishing the secret history of the Vietnam War (the Pentagon Papers)—knowing perfectly well that they could be prosecuted. The Nixon administration indeed tried to exercise prior restraint and block publication. In the end, the courts sided with the paper (blocking the injunction). There is no doubt that we have had revelations in the United States and elsewhere that violated the law, sometimes for the worse, and sometimes (as in the Pentagon Papers) for the good.

RP: Of course, the law is not always good—but neither, then, is radical transparency, which can in fact be weaponized.

PG: That's right.

RP: Philip Morris, for example, was behind the passage of two data transparency laws in 1999–2000 that required that the data underlying any governmentally funded (nonclassified) research be open for public inspection. This, however, created an asymmetry in the degree of inspection or interrogation between what the tobacco industry is doing, which is entirely cloaked, versus what publicly funded research is doing. And it allows the industry to get its hands on the raw data of any governmentally funded research, and deconstruct it, challenge it, rework and generally mess with it. Cigarette makers had successfully taken over the Framingham study in the 1970s, for example, allowing them to excavate cigarette-friendly facts from the raw data—or even claim that certain things "could not be proved" from the data. So access to underlying data can be used for good as well as evil.

Of course in quite different contexts, there are times when in order to guarantee quality science, you actually need a certain type of privacy, a certain type of sequestration, confidentiality, or anonymity—which is another form of virtuous ignorance.

PG: In order for fruitful deliberation to take place?

RP: Yes, and for it to be done in an honest and, in a sense, open yet sequestered way. I mean think about something like peer review, where the anonymous nature of the reviewer is important for guaranteeing a kind of openness and honesty of evaluation.

PG: And that's true in our letters of recommendation too. I mean we both, and all our colleagues, have had the experience of writing letters that we know are available to everybody including the person we are writing about, in which case we know how to do that and the letters are anodyne.

RP: And this falls into the category of virtuous ignorance, where a certain type of sequestration of information can be empowering, can promote democracy, can promote honesty. It can promote freedom of inquiry in a sense without retribution. It's sort of the same kind of logic that's involved in something like a trial, where jurors are not supposed to have any knowledge of the facts of a case prior to sitting in judgment of it—with the theory being that pretrial ignorance creates a kind of honesty and integrity and objectivity in the procedure. You and I have both worked a lot on the history of objectivity, and there's this myth we encounter about a certain distance being required for objectivity. But there are also ways in which distance—which is a kind of ignorance—can give us a helpful lens.

6. Final Thoughts

PG: I want to come back to join the two strands of our conversation about industrial and governmental agnotology. One of the great and greatly worrying trends over the last twenty years or so is that many things that used to be handled by government agencies have been privatized. Of course, it is by now a commonplace to note the precipitous rise in private prisons, contracted mercenaries, and private charter schools. Much of the electromagnetic radio and television spectrum is now in private hands. These are just a few of the formerly public spheres. But for this conversation, it is important to note too the analogous subcontracting of intelligence functions covering a vast range—the *private* culling and processing of data, the corporate mining and analysis scooping up biometric, locational, metadata, and financial and online behavior, and the government's interest. Some of this is propelled by the more general push toward the private sector, but some is motivated because there is much that the government is restricted by law from doing that can be done by private industry. The government can legally buy what it cannot itself gather. Privatization offers a form of backdoor passage around laws of disclosure and transparency.

If I was thinking ahead to where a study of secrecy will be in the coming years, I would say that late Cold War government secrecy, classically understood and regulated (for example, through the Freedom of Information Act, or FOIA) may well come to look like the halcyon days of openness. One can see this already in the nuclear domain—where the big contractors running the weapons and power domains already show themselves vastly *less* open than the Department of Energy ever was. How will this play out across the landscape of other agencies—as the Department of Defense, three-letter agencies (CIA, NSA, FBI, and DIA), and other branches of government hand over their functions to increasingly airtight global corporations? Corporate state secrecy, walling off police, military, and intelligence domains, is driving a new and deeper black.

RP: At the end of the day I'm sort of a populist, in the sense of that term prior to its corruption to mean "nationalist." One of the things I'm interested in is the ignoring of what ordinary people think in their daily lives, and how this affects their bodies, their freedoms, and so on. I think this is one of the things that too often gets left out in epistemology. I mentioned that one reason I started trying to understand ignorance was to respond to the radical apathy so many scholars evidence toward what ordinary people think about the world. In other words, it's not just about industry fooling us, it's not just about governments allowing certain types of knowledge to be constrained by the military-industrial plus media-entertainment complex. It's also about understanding what nonelites think they know about the world, and what relationship scholars have to such knowledge. It gets back to the issue you raised about things like creationism. Why do we have a world in which there are large bodies of consensus about certain scientific facts, but millions of people seem to have no difficulty living their lives in relative oblivion? I think of something like the recent presidential election, where most of the Republican candidates in the primary confessed to not believing that humans evolved from apes. This is a staggering indictment of modern knowledge—and therefore education, and hence the accomplishments of scholars. And as historians and philosophers of science, we don't seem to be doing enough to understand its origins. So that's another thread—which you might call populist: How do we understand what ordinary people know and don't know, this radical discontinuity that exists between expert and popular knowledge? Many philosophers seem to be blissfully unconcerned with it, reflecting perhaps that vanguardism I mentioned, where attention is focused on smart new ideas, while dumb or commonplace ideas are ignored. That's one of the concerns I've had, and one reason I asked the linguist, Iain Boal, to coin the term agnotology.

PG: How we choose to regulate this corporate state secrecy could well determine the future of democracy. Imagine a world not too different from ours, where investigative print and digital media thin out beyond recognition, replaced by performative politics and anecdotal, narrative nonfiction journalism. With much of the state apparatus privatized, and therefore no longer answerable either to sunshine laws and FOIA inquiries, we will need other kinds of mechanisms to understand our world. It is now fifty years since the signing, in July 1966, of FOIA. Perhaps now is not too soon to launch a serious discussion about the information we and the coming generations will need from the corporate state if we are to have the deliberative democracy we want.

RP: Go for it! I'm particularly worried about how humanity and the nonhuman world will fare, given the rising tide of threats from climate change. But I'm also excited to see so many young philosophers start to grapple with such fateful matters; Christophe Bonneuil and Jean-Baptiste Fressoz's *Shock of the Anthropocene* (2016) has an entire chapter on the agnotocene, recognizing the crucial role of engines of ignorance in our feeble response to the climate crisis. Years ago I abandoned philosophy from a sense that the field had become too inward looking and narrowly technical, with too much focus on wordplay and trivial puzzle solving. Reality can pack a punch, however, which is probably why we're seeing more and more philosophers recognize the vital—and sometimes fatal—force of ignorance. Especially with the growth of ever-faster technologies for spreading mis- and disinformation, we're seeing renewed interest in how we've landed ourselves in this golden age of ignorance. And how we might escape from it.

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II IGNORANCE AS ACTIVE CONSTRUCTION

For Harmful Ends

3 Agnotological Challenges: How to Capture the Production of Ignorance in Science

Martin Carrier

1. Agnotology and Scientific Method

Agnotology is supposed to represent the downside of epistemology. The concept, as introduced by Robert Proctor (2008, 27–28) in 1992, denotes the active creation and preservation of ignorance. He examined the deliberate suppression or neglect of information for economic or political reasons. As Proctor (2006) argued, the danger involved in smoking tobacco had been intentionally concealed by the pertinent industry. Naomi Oreskes and Erik Conway expanded this approach to global warming. They diagnosed a systematic cover-up operation launched by right-wing political circles that was intended to hide the fact of anthropogenic climate change (Oreskes and Conway 2008; Oreskes 2015). The method used in both cases was generating doubt by placing the threshold of acceptance for unwelcome claims at such an exceedingly high level that scientists would forever be unable to overcome it. With regard to smoking, epidemiological studies were charged with not being controlled laboratory inquiries and thus untrustworthy. But laboratory experiments with rats were declared irrelevant because the effects might be different in humans. Nothing would eventually ever convince the critics; each and every finding or argument was countered by the demand for additional evidence. Doubt was created with the sole intention of preventing political bodies from taking action (Proctor 2008, 11–18; Michaels 2008, 91).

Agnotological endeavors in this pejorative sense involve the deliberate violation of established standards of judgment in science. Agnotological agents ignore facts, conceal facts known to them, dismiss objections without an argument, and fail to respond specifically to problems and difficulties pointed out to them. Moreover, in the cases studied so far, these agents dismissed information purposefully. The underlying motive of the tobacco lobby was to shield corporate interests from the economic detriment caused by public health protection (Proctor 2008; Michaels 2008). A scientific position is advocated for the sole purpose of promoting sociopolitical interests, regardless of the relevant epistemic credentials.

A glaring agnotological example concerns the alleged connection between the measles-mumps-rubella (MMR) vaccination and development of autism. A study conducted by Andrew Wakefield in 1998 suggested a causal link of this sort together with a presumed causal mechanism. The study was published in a prestigious medical journal and attracted widespread attention. Various subsequent studies, however, showed that no such connection exists. Later investigations disclosed that Wakefield had manipulated his data in various ways. For instance, children were not selected randomly for the study but rather came from families that already suspected a link between the MMR vaccine and autism. Furthermore, a third of the children reported to have autism were not in fact diagnosed with it. During the time he carried out the research, Wakefield worked as a consultant for a law firm that represented parents in MMR litigation because their children had purportedly been harmed by the vaccine. And to top it off, Wakefield had a patent application pending for an alternative measles vaccine. He claimed that his vaccine would not exhibit the side effects attributed to the MMR vaccine (DeStefano and Thompson 2004; Flaherty 2011). The British General Medical Council (2010) found Wakefield guilty of intentional and irresponsible professional misconduct, and barred him from practicing medicine. In the Wakefield case, not only methodological blunder, but intentional forgery has been established. This is what qualifies the case as agnotological in the proper sense. Damage was done too.[1] Due

1 Inmaculada de Melo-Martín and Kristen Intemann (2018) challenge this claim by arguing that Wakefield's notorious study had beneficial consequences. Although the study was deliberately rigged, Wakefield's dissentig view "has led to a strengthening of the evidence about vaccine safety" (42). Yet this only means that the detrimental impact of Wakefield's agnotological attempt has been mitigated (but in no way neutralized) by other studies that were not equally intentionally misleading. Fortunately enough, many evils can be overcome by sustained effort, but they still qualify as evil. de Melo-Martín and Intemann's account would also clear the tobacco industry and merchants of climate doubts—the two agnotological stock perpetrators—of any criticism. After all, the machinations of the latter prompted counterstudies as well and thus should be taken to have served the common good. I take this conclusion to be self-defeating.

to Wakefield's paper, vaccination rates dropped, resulting in subsequent measles outbreaks (Flaherty 2011, 1302). Agnotological endeavors are likely to have a detrimental impact.

Proctor (2008, 7–8, 18–19) introduced a variety of notions of agnotology, among them military secrets or ignorance produced by a selective choice of questions. Indeed, scientific research operates like a searchlight. Each research endeavor illuminates certain aspects of experience and thereby leaves other features in the dark. The notion of agnotology has different meanings—many of them nonderogatory. Yet I focus on the notion of agnotology as a deliberately antiepistemic strategy. That is, some kinds of scientific interaction are epistemically damaging and hurt the production of knowledge. This notion raises nontrivial epistemological problems—some of which I try to address. Here is an outline of what I aim to do.

An important, if contentious, distinction concerns the difference between epistemic and nonepistemic values. Epistemic values appreciate knowledge and understanding, while nonepistemic values refer to sociopolitical interests and utility. Many philosophers of science do not acknowledge an in-principle distinction between the two and are at the same time committed to scientific pluralism. As a result, scientific research may legitimately be shot through with sociopolitical values and a variety of different research endeavors is welcomed as enriching the cognitive landscape of science. This ushers in the predicament that there is no justification left for rejecting any such approach as being inappropriately biased. In the framework sketched, it seems natural to argue that everybody may feel free to draw on one's favorite values for conducting and interpreting a study so that no clear reason can be given for criticizing a study as being inappropriately lopsided. There is no basis for regarding a study as distorting or misleading. This seems counterintuitive, though, for the promotion of smoking, denial of human-made climate change, and rejection of the MMR vaccination. In these cases, sociopolitical values seem to have overruled epistemic ambitions.

As a result, it is challenging to characterize agnotological endeavors more distinctly, and clarify which methodological standards are violated in the active production and maintenance of ignorance. The point is that agnotological agents usually do not resort to trivial means such as falsifying evidence. Rather, one such mechanism has been sketched before: raising the threshold of acceptance beyond what can be achieved in practice. But it is not that clear what the difference is between being rightly demanding and unreasonably strict. Another agnotological scheme is to remain "deliberately ignorant": possibly harmful effects are intentionally left unexplored (Brown 2008, 199–201). I wish to draw attention to an additional strategy that has not been identified as yet: what I call "false advertising," which capitalizes on the discrepancy between the design of a study and its use.

I begin by expanding the notion of agnotology to include misleading agreement and bias due to differences in worldview. I then elaborate precisely which methodological rules are violated by agnotological machinations. In section 3, I discuss an attempt to delineate "epistemically detrimental dissent" and argue that it is not selective enough: it admits false positives and false negatives. In section 4, I present my false advertising approach and apply it to a number of cases in section 5. I propose to identify agnotological ploys by the discrepancy between the conclusions suggested by the design of a study and those actually drawn or intimated. Agnotological ploys are characterized by the unrecognized difference between those issues for which a study is sensitive and those issues that feature in its interpretation. This mechanism of false advertising serves to implement agnotological endeavors and helps identify them without having to invoke the intentions of the relevant agents. In section 6, I elaborate on suggestions for combating agnotological endeavors. Such endeavors are best neutralized by fostering transparency and plurality. Transparency means to recognize the partial character of a study, and plurality encourages conducting a different study so as to achieve a more balanced picture. The identification of agnotological moves serves to curb the diversity of contrasting assumptions that characteristically goes along with pluralism. This is achieved by weeding out accounts that look alright at first glance, but whose design makes them unfit to achieve their intimated ends. Dropping such endeavors helps transform a pluralist manifold into a manageable range of alternatives.

2. Characterizing Agnotological Maneuvers

In the above stock examples of agnotological maneuvers, such as denying tobacco risks, anthropogenic climate change, and the safety of MMR vaccination, the relevant move was launching a controversy. The underlying understanding is that agnotological conflicts were produced deliberately and without epistemic justification. Economically or politically motivated interference induces artificial controversies that do not express justified and fertile diversity but instead merely create confusion. The idea is that if the relevant scientific community had been left to its own devices, a consensus on the matter would have emerged. It is worth noting, however, that consensus is not always justified either. Perhaps cases of politics-induced harmony are even more frequent than mock controversies. In such instances, a unanimously shared bias prevails. It is not the opposition among the professional opinions of scientists but rather their deceiving agreement that betrays the impact of nonepistemic factors. Consider Vioxx, an NSAID pain reliever that had been erroneously accepted in unison as an efficacious medical drug in the early 2000s because clinical data about its side effects had been actively suppressed (Biddle 2007; Michaels 2008, 101–102). In the same vein, the drug Tamiflu had been widely thought to combat influenza effectively until it was revealed in the early 2010s on the basis of the original figures of the clinical trials that the usefulness of the drug had been grossly overstated by its manufacturer (Cochrane 2014, 2015).

The understanding is that economic and political interests have deprived scientists of their neutrality or impartiality in weighing the evidence. Such nonepistemic values are used in judging the evidence and assessing the assumptions in question. In other words, social, political, and economic values are invoked in the context of justification. The problem is not the invocation of such values in selecting research topics (the context of discovery) and applying the research results (the context of application). Rather, the assumed predicament is a biased appraisal brought about by employing nonepistemic standards that induce partisan and misleading judgments about the credibility of an assumption. Agnotological endeavors undermine the role of science as an impartial arbiter in socially contentious issues. It is granted that some nonepistemic values, such as the protection of human subjects in social or medical experiments, promote objectivity, while crafting a study in order to push a sociopolitical agenda means detracting from its objectivity (Resnik and Elliott 2016, 35).

I suggest including issues of philosophical worldview among the possibly distorting influences. Ways in which matter, the universe, and human beings are conceived may induce biased procedures that are hard to distinguish from those prompted by more worldly nonepistemic aspirations. This rather broad notion of nonepistemic interests allows us to see that the pursuit of projects in fundamental research may exhibit agnotological features too (see the "Generalizing the False Advertising Account" section).

Agnotological challenges are often merely captured in terms of mock controversies. But as the examples show, ignorance can be produced or confusion can be created in two opposite ways—namely, by fabricating dissent and manufacturing spurious consent. Groundless strife and premature unanimity are symmetrical in their deceiving impact. The common ground among all these illustrations is that judgments about justification are tainted by nonepistemic motives, mostly of political and economic origin, but at times also related to beliefs about the makeup or essence of nature or humans.

The questions are what precisely goes wrong in such instances, and how relevant cases are to be identified. What seems to be lacking here at first sight is an appropriate response to data and opposing factions in the scientific community. Karl Popper (1957) insisted that an essential element of the scientific method is taking objections seriously and trying to cope with them. In the same vein, Helen Longino (2002, 129–130) contended that the process of critical examination in science necessitates that one take up criticism and respond to objections appropriately. By contrast, agnotological agents go ahead undauntedly. It is not that clear, though, what the scientific method really demands. Thomas Kuhn and Imre Lakatos regard tenacity in the face of epistemic challenges as a scientific virtue. Scientists equipped with a theory they consider promising legitimately focus on pursuing their projects, and rightly ignore recalcitrant data and annoying opponents. Accordingly, staunchly pursuing an approach, even one-sidedly and lacking the open-mindedness often attributed to scientists, is not generally viewed as breaching scientific standards and cannot serve to identify agnotological ploys for this reason. There is no methodological rule that specifies the right amount of responsiveness or resistance to the data and opponents.

Moreover, pluralism has been defended by various authors as a suitable epistemic tool. The social notion of objectivity emphasizes the importance of reciprocal criticism and control. A multiplicity of approaches serves this critical spirit best and is suited to neutralize the impact of each person's blind spots. Scientific objectivity does not grow out of the impartiality of individual scientists but instead is the result of strife and antagonism within the scientific community. Thus, dissent and opposition are core features of the scientific method (Popper 1966, 412–413; Longino 1990, 66–80; Carrier 2013, 2548–2549). Such a social approach has been extended to include nonepistemic considerations. Philip Kitcher has famously claimed that the desire of scientists to earn a reputation and be recognized among their peers serves to divide up the scientific community into competing camps. Risk-aversive scientists will join the mainstream, while their more risk-seeking colleagues will adopt and pursue minority views. The odds of success are higher in the former instance, but the merit each scientist gets in the case of success is smaller. By contrast, the minority view is unlikely to come out victorious, but if it does, the share of glory each contributor receives is larger. This ensuing diversification of risks represents the epistemic optimum (Kitcher 2001, 112–113). Judged against this backdrop, there are no arguments against pursuing idiosyncratic research objectives and applying nonstandard procedures. Employing a variety of goals, methods, study designs, and criteria of judgment seems to strengthen the pluralism of approaches, which is a major prerequisite for the objectivity of science. Agnotology appears to be a misnomer.

It is generally understood, however, that the social notion of objectivity involves the prevalence of epistemic values. All competing accounts strive for knowledge that satisfies the shared standards of scientific judgment. By contrast, agnotological endeavors seem to be characterized by the primacy of nonepistemic, sociopolitical values (Biddle and Leuschner 2015, 264). I agree with this depiction; agnotological maneuvers lack an epistemic attitude (Carrier 2013, 2563–2564). They are dominated by sociopolitical aspirations, and marked by their determined effort to obscure facts and relations. Agnotological agents do not want to know. Consequently, agnotology is characterized by antiepistemic intentions.

This is fine as far as it goes, but three sorts of difficulties emerge. First, intentions are not always easy to fathom, and we would certainly benefit from more tangible indicators of agnotological moves. Second, diagnosing the prevalence of sociopolitical values demands the distinction between epistemic (or knowledge-oriented) values and nonepistemic (or sociopolitical) values. I accept this distinction, but it is not generally admitted. Torsten Wilholt (2009, 96) takes epistemic values to be inseparable from nonepistemic ones; Kristen Intemann (2015, 220–223) assumes epistemic and nonepistemic values to be difficult to distinguish in general, and hard to disentangle in concrete cases. Accordingly, along with coauthor Inmaculada de Melo-Martín, she suggests that the notion of epistemically appropriate dissent (or conversely, "normatively inappropriate dissent") is difficult to establish (de Melo-Martín and Intemann 2014, 602; 2018, 4). Given this inability, attempts to single out epistemically harmful dissent seem bound to fail and could be feared to backfire in that they stifle justified dissent. In view of the social notion of objectivity, dissent is essential to scientific knowledge gain. But the ambition to neutralize fake dissent may encourage scientists to silence opposing views by institutional means (such as suppressing publication) or ostracize nonconformist rebels (de Melo-Martín and Intemann 2014, 595–598; see also de Melo-Martín and Intemann 2013). More specifically, Intemann and de Melo-Martín argue that Longino's requirement to take up criticism is useful only if the debate proceeds against a background of shared standards. The broader and deeper this agreement is supposed to be, the higher the risk of discounting valuable alternatives, while the less substantial these shared standards are demanded to be, the more prima facie weird positions are required to be included in the debate. Their conclusion is that setting limits on reasonable dissent is likely to fail, and if pushed through, may easily curb epistemically valuable pluralism (Intemann and de Melo-Martín 2014). As such, transferred to the present context, the conclusion championed by Intemann and de Melo-Martín is that agnotology is not a sensible notion. There is no justified way of targeting detrimental dissent, and any attempt to do so could cause more harm than benefit. They recommend instead educating policy makers and the general public such that they are able to assess as well as deal with pluralism in science. The chief step in this direction is to point out that most political controversies are not based on scientific dissent but on disagreement about relevant values (de Melo-Martín and Intemann 2018, 130–142).

Third, the complaint that the adversary's position is tainted by nonepistemic interests is entertained by either side. Climate change deniers are charged with acting in the service of a political agenda. They are claimed to be market fundamentalists who oppose government regulation, and deny each and every environmental challenge. Their primary concern is said to obstruct certain policies based on scientific conclusions, and the disagreement they have produced is used for promoting their political ends (Oreskes 2015, 44–46; see also Kitcher 2011, 162). But this strategy works in both directions. Climate change deniers blame scientists for overstepping the legitimate bounds of scientific knowledge and advocating certain policies (Kitcher 2011, 30). Some accuse scientists of pursuing a hidden agenda of anti-industrial fanaticism and left-wing environmentalism. In addition, climate skeptics point to economic interests that are served by fighting allegedly pressing climate change. Companies involved in renewable energy and electricity grid modernization are criticized for pursuing vested interests in highlighting the risks of climate change and alerting society. The "climate-industrial-government-activist-scientist complex" seeks to drive up subsidies for self-declared green companies. Its members have a stake in attracting massive government spending (Driessen 2009).

As a result, we are faced with a predicament. The traditional notion of agnotology, as coined by Proctor, features obscurantist intentions that are nourished by sociopolitical values. This notion certainly enjoys some prima facie plausibility, yet it encounters the three serious difficulties outlined. Intentions are frequently hard to establish, epistemic values are sometimes taken to be inseparable from nonepistemic values so that epistemically harmful dissent is claimed to be indistinguishable from epistemically valuable pluralism, and many parties to a dispute can be charged with being driven by sociopolitical values. But if the distinction between epistemic standards and sociopolitical interests is denied and at the same time pluralism is welcomed, then everybody may feel entitled to draw on their particular predilections for conducting and interpreting a study. No basis exists for categorizing a study as actively distorting.

By contrast, advocates of identifying and barring agnotological endeavors point to the hazards for public policies if unjustified dissent is allowed to undermine justified scientific consensus. This applies to the two stock examples of smoking and climate change, in which interest-guided, scientifically unfounded intervention succeeded in delaying political action considerably. The same is true for the MMR vaccination fraud and antivaccination campaigns in general, which are often, though not always (see Goldenberg 2016), grounded on a metaphysical commitment to naturalness and opposition to "chemical" means. Justin Biddle and Anna Leuschner underline additional adverse epistemic consequences of agnotological maneuvers. The latter forced scientists to respond endlessly to the same worn-out objections that had already been answered, and created an intimidating atmosphere in which scientists feared to address certain topics or defend certain hypotheses (Biddle and Leuschner 2015, 268–269).

These arguments support the conclusion that agnotological machinations cause harm, and correspondingly, there is something to be gained in epistemic respect by recognizing and neutralizing them. Undermining scientific positions with nonepistemic arguments and for nonepistemic purposes might do damage to the state of knowledge, and could hurt public policies. If we succeeded in pinpointing such maneuvers, de Melo-Martín and Intemann's concern about impairing worthwhile pluralism by suppressing dissenting voices would be dispelled. Blocking agnotological machinations specifically, rather than ostracizing opposing factions in general, would help to avoid the feared side effect of narrowing the range of serious options taken into consideration.

3. The Impact-Centered Approach: Agnotology as the Shift of Inductive Risks

The conclusion I draw from these considerations is that it might be a worthwhile undertaking to discern agnotological maneuvers. The original notion of epistemically damaging dissent or consensus turns on the deliberate production as well as maintenance of knowledge gaps. Yet intentions are notoriously difficult to ascertain. Thus, the first aim is to recognize instances of agnotology without appeal to motivations. The second objective is to elaborate which epistemic requirement has been transgressed by an agnotological maneuver. Such maneuvers are usually based on subtle manipulation and otherwise would be self-defeating. The challenge concerns precisely the epistemic nature of agnotological endeavors.

In this section, I discuss an approach elaborated recently by Biddle and Leuschner that is supposed to identify epistemically detrimental dissent (which constitutes a subset of agnotological ploys). They rightly seek to identify harmful disagreement on methodological grounds alone, and without having to immerse themselves in the underlying motives and intentions. In Biddle and Leuschner's (2015, 272–273) view, epistemically detrimental dissent is identified by a deviation from "well-entrenched conventional standards" that lead to a shift in inductive risks from producer to public risks. This account is based on Richard Rudner's (1953) approach to hypothesis confirmation. Rudner argued that adopting or dropping a hypothesis on the basis of necessarily incomplete data may produce false positives or false negatives—that is, the mistaken assumption that a certain effect obtains or the erroneous supposition that the effect does not occur. This lack of secure evidence makes it necessary to decide about how much evidence of which kind is required for accepting the pertinent assumption. A high threshold level of acceptance reduces the risk of false positives, but increases the risk of false negatives, and vice versa. Rudner's suggestion is that weighing the nonepistemic consequences of these potential errors should determine where to place the threshold of acceptance (see also Douglas 2000; 2009, 103–106).

For instance, the risks involved in admitting a possibly hazardous substance to the market can be shifted by adjusting the standards for assuming safety. If we wish to definitely rule out that a harmful substance is released, we demand a lot of evidence in support of safety before we endorse its public use. Placing high standards on assuming safety reduces the risk of erroneously admitting hazardous materials (and thus diminishes the risk of false negatives). At the same time, it heightens the risk of mistakenly keeping a harmless substance from the market (and hence augments the risk of false positives). There is no general methodological rule for determining what the right choice is in each case involved, and this is why Rudner and others appeal to ethical standards for setting the threshold of acceptance.

As Biddle and Leuschner (2015, 271–272) argue (relying on Wilholt 2009, 97–98), however, some such experimental designs and the inferences drawn on their basis are judged as making use of this methodological leeway in an unjustified and misleading way. Such experiments are not regarded as merely invoking methodological and ethical choices that we might disagree with. They are rather viewed as misrepresenting the situation and actively falsifying the conclusions. Take bisphenol A, a substance used in many plastics. Bisphenol A chemically resembles estrogen and therefore is feared to interfere with human hormonal balance. Tests with this substance performed by the relevant industry employed rats, which were said to be rather insensitive to estrogen. The judgment is that the leeway left by the evidence for assessing the features of bisphenol A is abused by this experimental design.

We smell fraudulence here, but the trouble is to pinpoint why exactly such an experimental setup strikes us as being improperly biased. What precisely is methodologically inappropriate here? The first impulse is to object that the experiments were designed such that the acceptance of certain favorite claims was unduly facilitated. The design made it easier to produce supporting evidence for the hypothesis that bisphenol A had no adverse health effects, and this favored outcome was in agreement with the interests of the study's sponsors. This is good as a start, but in need of reinforcement. Namely, this condition is satisfied in instances that look markedly dissimilar from the examples presented before. Lowering the standards in order to make one's pet principles appear more agreeable is a strategy pursued by some first-rate scientists. Think of Isaac Newton's "hypotheses non fingo," intended to discredit objections to his theory of gravitation to the effect that no mechanism for the transmission of gravitational force had been specified. Newton's response was that the force of gravity could do well without a mechanism. Similarly, when James C. Maxwell realized that no mechanical model of the ether was able to produce the entirety of electromagnetic interactions as described by his equations, "Maxwell's equations," he disposed of the ether and declared electromagnetic theory to be the same thing as these equations. Finally, one of the problems of electromagnetic accounts of moving bodies around 1900 was to derive the empirical inaccessibility of uniform rectilinear motion on the basis of electromagnetic effects. Albert Einstein approached this challenge by stating this empirical inaccessibility as a principle, and then placing this principle of relativity at the top of his new special theory of relativity. Therefore, he spared himself the effort of deriving the assumption (Carrier 2006, 21–22). Furthermore, in these cases, certain worldview-related ambitions were operative. For instance, Newton sought to establish that the mechanical world picture with its emphasis on push-and-shove causation was mistaken, and Einstein was driven by an operationalist approach to scientific concepts. Yet in contrast to the examples presented in the first section, no active production of confusion is involved in either case. Thus, diminishing the demands so as to pave the way for one's favorite principles seems not to be a reliable indication of an agnotological maneuver.

Biddle and Leuschner's account serves to overcome predicaments of this sort by featuring the mechanism by which the odds of obtaining a desired result are improved. This improvement needs to be achieved by infringing on well-entrenched methodological standards to the effect that producer risks are lowered at the expense of public risks. More specifically, they suggest four conditions that are supposed to be jointly sufficient for identifying epistemically detrimental dissent. These conditions state that the "non-epistemic consequences of wrongly rejecting [hypothesis] *H* are likely to be severe" (thus bringing Rudner's argument into the scope), and the dissenting research "violates established conventional standards" and "involves intolerance for producer risks at the expense of public risks," assuming that the two "risks fall largely upon different parties" (Biddle and Leuschner 2015, 273). In short, the risks of error are shifted toward the public by violating an established methodological rule.

The aforementioned test design of bisphenol A breached the recognized methodological rule to choose animals that respond to the substance in question, and the corresponding risks were shifted from the producers to the public. Barring the substance erroneously is a producer risk, since the effort invested in its development would be invalidated without justification, releasing the substance mistakenly to the market is a public risk since unrecognized health hazards may emerge. As a result, the agnotological character of the test design can be shown by relying exclusively on the relation between the risks involved and the violation of a well-entrenched conventional standard (Biddle and Leuschner 2015, 272). Considering the impact of concocting a test design is sufficient, no need arises for speculating about the motives behind the ploy.

Plausible as this impact-centered approach may appear, it seems to include major uncertainties as to how risks are individuated and what counts as a relevant risk. Take the 2009–2010 swine flu epidemic that turned out to be mild in the end. When the figures of infected people were feared to soar, two vaccines were admitted to the European market that had merely run through a test procedure of reduced severity and whose safety had not been ascertained accordingly. The motive behind this alleviation was the fear of imminent danger that urged immediate action. At the same time, German authorities relieved the manufacturers and in some German states also medical doctors from their liability due to the possibly premature administration of new agent substances. They were liable only for the damage caused by gross negligence while any damage done by the side effects of the vaccination itself was borne by the government (Kuhlen 2009; Wolff 2011; Schlitt 2013). Put differently, the standards of accepting vaccines as safe were lowered. A shift of this kind certainly involves nonnegligible risks, and assuming government liability amounted to transferring these risks to the public. From the perspective of the relevant authorities, however, the situation looked different at the time. They believed that lowering the standards meant speeding up the test procedure and that this acceleration actually prevented risks caused by the epidemic. Yet as a matter of fact, hardly any such risk existed. The fear was largely overdrawn. But the converse risk associated with suspending the established safety protocol was real (and may have produced increased narcolepsy incidence). This means that the two first conditions of the Biddle and Leuschner account are satisfied in this example: erroneously accepting a vaccine as safe without approval by the standard protocol (this is the dissenting view in their terminology) involved incurring nonnegligible risks. And the two last conditions also apply: these risks were shifted from the producers to the public.

It is odd, though, to see an agnotological maneuver in this regulation. The reason is that the authorities acted in good faith. Their intention was to combat public risks. Thus, this is not an agnotological case in terms of intentions and aspirations, but it looks like one in terms of impact. Consequently, this example is a false positive of the impact-centered approach: it qualifies as detrimental dissent in its light, but it seems significantly different from the agnotological cases listed in first section.

The problem underlying this difficulty is that the identification of risks and their evaluation is highly malleable and subject to disparate judgment. In particular, Rudner's account on which Biddle and Leuschner rely for the identification of the relevant risks is questionable in this respect. Rudner suggested placing the threshold of acceptance of a hypothesis based on the nonepistemic consequences of erroneously adopting the hypothesis as compared to mistakenly rejecting it. Producer and public risks in the impact-centered approach are modeled on this idea. Yet the damage done by a given action can be determined in a variety of contrasting ways. Assume that in the case of bisphenol A, the relevant scientists had considered the economic damage done by an erroneous withdrawal of the substance from the market to be egregious. The company could well be ruined and hence thousands of employees would lose their jobs. The use of insensitive rats could be viewed as preventing the possibly groundless destruction of the economic basis of many people. In order to preclude this public risk, they designed the study such that it is improbable to overestimate pertinent health risks. In the same vein, authorities in the swine flu example may be credited with acting on the best estimate of risk available at the time or blamed for mistakenly assessing the objective perils posed by the disease. Rudner's account at times fails to suggest a clear conclusion as to what the relevant risks are (for more details, see the next section). In the present context, this means that since risks can be evaluated differently, their shift cannot always be determined unambiguously.

In addition to "risk," the notion of "established conventional standards" is dubious in epistemic respect. Biddle and Leuschner characterize the illegitimate agnotological shift by its deviation from well-entrenched principles that are suited to transfer risks to the public. What appears doubtful in this depiction is that the nature and credentials of these standards are left unexplained. As to the first item, Kevin Elliott (2014, 925–926; 2016, 536–537) has drawn attention to the fact that typical laboratory guidelines leave huge leeway for designing studies, such as picking laboratory animals, adjusting doses, or selecting statistical procedures (for more details, see the "Generalizing the False Advertising Account" section). Second, the mere fact that a standard is accepted only serves to make it part of a (maybe) cherished tradition, but fails to offer epistemic reasons in its favor. Departing from an established canon could make perfect epistemic sense. As Elliott has emphasized, some guidelines of standardization introduce an inappropriately narrow point of view. In testing for the risks associated with genetically modified Bt maize, the relevant standard requires using the pure Bt protein rather than the maize itself. Following this standard obscures the possible toxic effects of the maize plant produced by its genetic modification (Elliott 2016, 538). Another case of methodologically inadequate standards is revealed by the present credibility crisis of social psychology. A typical response is to adjust rules for analyzing data and drawing conclusions (such as stressing effect size, taking distorting factors such as publication bias into account, or switching to Bayesianism) (Open Science Collaboration 2015; see also Ioannidis 2005). We observe presently a transition among relevant conventional standards in this field that is supposed to improve methodological judgment. It would seem inappropriate to consider this transition a harmful dissent even if it led to a shift of risks from producers to the public. What is missing in the impact-centered approach is an argument for why the traditional rules are epistemically justified. Overstepping conventional constraints should count as illegitimate only if the latter have been established on epistemic grounds in the first place. In many cases, standards are adjusted bona fide in a transparent way and with good reason. Such cases should never count as agnotological, regardless of any possible shift of risks involved.

4. Agnotology and False Advertising

My conclusion is that shifting risks by breaching conventional standards is not plausible as a sufficient criterion for pinpointing agnotological endeavors. Thus, what else is a methodological recipe for misleadingly casting doubt on certain claims? One such recipe is arguing for elevating the threshold of acceptance regarding unwelcome hypotheses. Such a ploy could appeal to the "principle of sound science" and suggest that any science-based intervention needs to rely on secure knowledge (Hansson 2007, 265). The agnotological loophole is that there are always options left for denying that this threshold has been passed in a particular case (see the first section). This maneuver, however, has obviously not been practiced in the bisphenol A case, which looks like a stock example of agnotological machinations. The critical feature here is not the location of the threshold of acceptance but rather the biased design of the experiment. The methodological flaw involved in adopting inappropriate study designs needs to be captured differently.

This additional strategy proceeds by what I call, as noted earlier, false advertising. The corresponding methodological defect is that the pertinent studies actually avoid the issues they pretend to address. They do not tackle the questions they purport to answer. In the bisphenol A case, the issue supposedly attended to was whether human health hazards could emerge. The expectation is that the substance is established as being safe. This issue was dodged by employing a strain of rat that was rather insensitive to the health risks at hand. Due to this lack of sensitivity, the study fails to achieve this aim and instead rules out that the use of bisphenol A is prematurely barred as being unsafe. The issue purportedly addressed concerns the harmlessness of bisphenol A, but this issue is circumvented by the study design. This design is not flawed or inappropriate in general. The procedure is suited to make sure that bisphenol A is not dismissed for the mistaken reason that its use is unsafe. Employing more sensitive creatures could have made the alarm bell ring prematurely and would have caused an overestimation of the health risks involved. Using less sensitive strains of rat reduces this risk of error and eschews unjustly strict regulation. Yet the question that the study suggested to answer is the converse one—namely, to ascertain that no health risks exist. This discrepancy was not acknowledged but rather was passed over tacitly. The methodological flaw involved here is false advertising (Carrier 2013, 2560–2561; 2018, 161–162).

False advertising of this sort, I take it, underlies the illegitimate intrusion of nonepistemic interests in the outcome. In general, researchers may feel free to pursue nonepistemic research objectives, so that we might take apparently biased test designs as the legitimate expression of value commitments that we do not share ourselves. Yet in fact, we rather consider such designs as being lopsided in an unjustified fashion. But supporting such an assessment requires identifying a methodological shortcoming that I claim consists of an unrecognized discrepancy between the study design and the use made of this study. Its design makes it fit to rule out the overestimation of health risks, but it is used for intimating that health risks have not been underestimated. The discrepancy between design and use means that the setup of the experiment makes it rather insensitive to the issue it supposedly addresses, and this incongruity is concealed in the interpretation of its results.

Note that the swine flu case can be handled easily within this framework. Reducing the threshold of release of the relevant drugs to the market meant diminishing the sensitivity of the tests regarding efficacy and safety. The drugs were still tested for efficacy and safety, however, and this is what determined the conclusion. No incongruity between design and use is present here. Furthermore, one of the assets of the false advertising account is that it can deal with cases of agnotological consensus. I mentioned the examples of Vioxx and Tamiflu (see the "Characterizing Agnotological Maneuvers" section) in which the available evidence in no way justified the advertised conclusion of safety or efficacy, respectively. A striking discrepancy emerged between the claimed result and its factual basis.[2] These examples qualify as agnotological in the false advertising account.

2 It is true that the difference between remaining less sensitive than possible in a certain respect and being sensitive to a different factor is a matter of degree. Although the boundaries are fuzzy, as most conceptual boundaries are, the pertinent cases are sufficiently distinct in character to regard them as being categorically different.

5. Generalizing the False Advertising Account

In this section I try out the false advertising account by considering additional prima facie instances of agnotological maneuvering, and examining how the false advertising account and alternative impact-centered approach deal with them. The first example is the 2012 experiment of Gilles-Eric Séralini, who fed rats with genetically modified maize that is resistant to the herbicide Roundup. He reported an elevated rate of cancer in rats fed for two years with low doses of the two substances (Séralini et al. 2012). The study was heavily criticized in methodological respect, though; it was accused of having used a sample of rats that was too small for obtaining significant results, and a strain of rat that was likely to contract cancer sooner or later without any external interference (de Souza and Oda 2013). As a result, the study was retracted against the will of the authors, but republished in 2014 in a different journal in considerably altered form. In justifying the withdrawal of the paper, the editor of the journal argued that "no evidence of fraud or intentional misrepresentation of the data" had been found, but "there is a legitimate cause for concern regarding both the number of animals in each study group and the particular strain selected." This was said to cast doubt on the reality of the claimed difference between the study and control groups: "normal variability cannot be excluded as the cause of the higher mortality and incidence observed in the treated groups" (Séralini et al. 2012). The salient point in the present context is that Séralini and colleagues claim in their introduction to have conducted a study on the possible toxic effects of genetically modified maize and Roundup. The chosen group size and strain of rat used would comply with the conventional standards for toxicological studies. Yet in fact, the health effect most extensively discussed was tumor growth. Cancer development was then attributed to toxic effects (4229). The bottom line, however, was that the standard protocol for cancer studies would have required a fivefold group size and a strain of rat that was less cancer prone. In their conclusion, Séralini and colleagues recommended the careful evaluation of the substances in view of their potential toxic effects (4230). But the health damage chiefly addressed in the paper was cancer, and it was this effect that actually underlaid their recommendation. Séralini and colleagues (2014) implicitly acknowledged this objection by confining their republished study to toxic effects in a narrow sense and mentioning cancer only in passing. This restructuring of the argument confirmed that the original paper was a piece of false advertising: the survey was designed as a toxicity study, but it was interpreted as a carcinogenicity one. Its design made it rather unsuitable to estimate risks of cancer. This is why we are faced with a striking discrepancy between design and use, and consequently, an agnotological endeavor.

Evaluating this case in the impact-centered approach requires identifying the relevant risks. On the face of it, the assumed violation of methodological standards serves to keep Bt maize and Roundup out of the market, even if they did not cause harm. Given the design of the study, the outcome is likely to be alarming even if the substances at issue did not in fact cause cancer. The risk-related reasoning underlying the interpretation of the experiment might be something like the following.[3] A mistaken assessment of Bt maize and Roundup as safe could produce tremendous damage, and should be ruled out in any event. Thus, following Rudner's recommendation and seeking to keep the damage done in the event of error at a minimum requires raising the threshold for releasing genetically modified substances to the market. This framework appears to guide the interpretation of the experiment—if not its design. In this understanding, the interpretation given by Séralini and colleagues (2012) involves a shift from public to producer risks. Accordingly, this strategy does not seem to qualify as agnotological in Biddle and Leuschner's lights. I take this to be a false negative of their account.

It is true, their account is intended to specify a sufficient criterion only, so that this case is, strictly speaking, not valid as a counterexample. Nevertheless, this irrelevance makes the predicament even worse since it means to ignore the common pattern of biased interpretation and unjustified conclusion. I find it implausible that a shift of risks from public to producer fails to qualify as agnotological for conceptual reasons. I take it to be more significant that the shift occurred tacitly and without recognition (a factor without relevance in the impact-centered approach). This shift involved passing off a carcinogenicity study as a toxicity study. The lack of transparency as well as the gap between design and use represent

3 Underlying this supposition is the fact that Séralini has a long-term track record of opposing the commercial release of genetically modified organisms; he has been an activist against genetically modified organisms since the 1990s. Séralini is the author of the popular anti-GMO and antipesticide book *Tous cobayes!* (Paris: Flammarion, 2013), which accuses the relevant industry of abusing the public as its guinea pigs.

crucial methodological blunders rather than the direction of the shift of risks involved.

Moreover, as explained earlier (see section 3), the notion of risk is malleable, and in view of the leeway in establishing the damage done by a particular measure, the conclusion to be drawn from the impact-centered approach for the case at hand actually loses its distinctness. On closer inspection, the notion of public and producer risks becomes blurred. Think of the argument that withholding genetically modified food means depleting the resources of many people groundlessly. This is how Kitcher (2011, 146, 238–239) evaluates opposition against the agricultural use of genetically modified organisms. Adopting such an attitude entails that a decision against the release of such organisms would actually transfer risks to the public. Risks can be conceived in widely differing ways, and this malleability makes it difficult to identify illicit methodological moves by comparing risks. Risks and their shift mostly lie in the eyes of the beholder. In the false advertising account, however, no such risk evaluation is needed. We have a conspicuous but unrecognized discrepancy between design and use, and this feature is sufficient for branding the study of Séralini and colleagues (2012) as being agnotological in kind.

Another problem of the impact-centered approach is the leeway in what is to count as a "violation of an established conventional standard." Whether standards have been violated depends on which kind the experiment is considered to be like. This difficulty emerges with respect to Séralini's experiment, but comes out blatantly in a variant of the bisphenol A case. On the face of it, this case bears all the marks of an agnotological machination. In addition to the features mentioned in section 3, we find striking variation in the results on the physiological effects of bisphenol A depending on the source of the study's funding. Whereas publicly sponsored studies do find effects of low-dose bisphenol A exposure, surveys underwritten by industry fail to come up with any effects (vom Saal and Hughes 2005, 928; Michaels 2008, 100; Foucart 2012). Frederick vom Saal, one of the most outspoken scientific critics of the use of bisphenol A, suggested that one of the reasons for the absence of significant effects in industry studies was the employment of the mentioned strain of rat that was rather insensitive to estrogen (vom Saal and Hughes 2005, 929; Foucart 2012). In fact, this test design looks like a glaring violation of methodological standards (see section 3). But look at how George Gray and colleagues

(2004), in a study sponsored by the American Plastics Council, responded to such allegations: their industry-financed study availed itself of a standard strain of rat, commercially obtainable, whereas vom Saal used his own inbred strain, which might have become oversensitive to estrogen due to inbreeding. Unfortunately, as Gray and colleagues continued, this suspicion cannot be checked because the strain (being a homemade concoction) no longer exists. Moreover, the rats were fed differently in the two kinds of studies at issue. Whereas industry studies used commercially available standard food ("rat and mouse no. 1 maintenance"), vom Saal resorted to food with allegedly elevated estrogen activity. Gray and colleagues (2004, 896–897, 899) conclude that it is probably these dissimilarities in the relevant conditions that explain the differences in the results.

The point is that both parties accuse each other of transgressing ordinary standards of research. Both opponents blame each other for deviating from conventional methodological rules such that results favoring their own position can be expected to emerge. How is this case to be judged in terms of agnotological categories? The impact-centered approach fails to get off the ground here since it seems impossible to establish who violated the relevant conventional standard. By contrast, it is unmistakable that no conclusion as to the safe use of bisphenol A (resembling estrogen chemically) could be based on a study conducted with prima facie estrogen-insensitive rats. It would have been imperative to make sure that estrogen sensitivity was normal; it is not sufficient to merely presume oversensitivity in order to spoil the opponents' studies. Vom Saal may have breached standardization requirements, but Gray and colleagues left the lack of sensitivity objection without empirically supported rejoinder. Since Gray and colleagues (2004, 909–910) conclude that no low-dose effects of bisphenol A in humans are to be expected, they utilize their study in a way unjustified by its design. As a result, we have a case of agnotology on the false advertising account.

By contrast, there is no false advertising involved in the three previously mentioned examples from epistemic research—that is, Newton, Maxwell, and Einstein (see section 3). It is true, they all lower standards, presumably for the sake of facilitating the acceptance of their favored hypotheses. But no attempt was made to use experiments for buttressing conclusions that were not supported by their design. Still, a possible agnotological maneuver in epistemic research is involved in Louis Pasteur's refutation of spontaneous generation—at least if the account given by John Farley and Gerald Geison (1974) is correct (see also López Cerezo 2015, 308). According to their reconstruction, Pasteur attempted to broaden the gap between the living and nonliving in support of his conservative political outlook, which led him to fight Darwinism. If life could not arise by natural forces alone, evolutionary mechanisms could not get started in the first place. As Farley and Geison argue, Pasteur's allegedly decisive experiments left various loopholes and often failed to engage properly with objections. For instance, he never responded to the more refined versions of the experiments of his opponent Félix Pouchet that seemed to provide evidence for spontaneous generation and escaped the criticism Pasteur had leveled against earlier versions. This could be taken to indicate a hiatus between the conclusions actually supported by Pasteur's experiments and inferences drawn by him. Pasteur lowered the standards of acceptance for his favorite hypothesis, but argued as if he had not done so. Thus, judged on the basis of this historical reconstruction and the false advertising account, Pasteur had launched an agnotological endeavor. I find this judgment plausible, although no risks were shifted from the producer to the public, and I take it as a confirmation of conceiving agnotology broadly regarding the values involved. It is not only confined to political and economic interests but may be due to worldview-related commitments as well.

In the studies on bisphenol A and Bt maize/Roundup, as reviewed in this section, the methodological flaw committed is the unrecognized incongruity between their design and the use made of them.[4] Judged in this framework, they all involve agnotological maneuvers. This result supports the claim that the two features indicating agnotological ploys are the discrepancy between items for which a study is sensitive and items highlighted in the interpretation of this study as well as the lack of recognition about such a discrepancy. The transition from the conclusions suggested by the design of the study at hand and those drawn or intimated is made tacitly, and the impression is conveyed that no such adjustment has been made. In addition, the variety of examples studied supports the conclusion that relevant values need not only be commercial or political but can also be

4 For one more study on agnotological policy advice in relation to Oslo Airport in Gardermoen, see Carrier 2018, 164–165. It goes without saying that the MMR case related in the first section implicates false advertising too. The same is true of the classic agnotological case of tobacco smoking (for the latter, see Carrier 2018, 166).

worldview-related and involve notions of the common good. For instance, the motive underlying the design of Séralini and colleagues' (2012) study seems to be to prevent public risks. As a result, the landscape of agnotological drivers is more variegated than it appears in Proctor's original approach.

6. Identifying and Coping with Agnotological Machinations

Proctor's notion of agnotological endeavors includes as a defining characteristic the intention to obscure a problem situation for nonepistemic reasons (see sections 1 and 3). Agnotology in this sense has to do with deliberate attempts to shroud parts of scientific knowledge in the mist of sociopolitical interests. It means to actively confuse people. In agnotological ploys it is not the facts that lead the way but rather the worldly aspirations of the researcher or study sponsor. The study is fashioned in a way that is bound to mislead the public. Intentions are not always easy to establish, though. They have been uncovered successfully in some cases, such as the stock examples of denying the risks associated with smoking and climate change. Climate risk deniers sometimes explicitly grant that they seek to avoid the overregulation of industry. They want to save free enterprise and cheap fossil energy, and believe that environmental protection means that government is taking control over their lives (Oreskes 2015; see also section 2 above). But in other instances, agnotological agents try to hide their motives and purposes for the apparent reason of maintaining the air of credibility. This is why we need robust indicators of agnotological ploys that do not rely on the confession of the perpetrator. Biddle and Leuschner rightly endeavor to identify proxies that serve to recognize cases of agnotology without appeal to motivation (see section 3). I agree with the goal, but suggest an alternative proxy.[5] A second objective is to pinpoint which epistemic requirement has been transgressed by an agnotological

5 A reviewer of this chapter offered the criticism that by singling out intentions as the defining characteristic of agnotology while focusing on nonintentional proxies afterward, I am guilty of false advertising myself. Yet we often distinguish between criteria and indications. Extrasolar planets are defined as bodies orbiting a distant sun, but this criterion cannot be implemented directly. No telescope is able to identify light from such a planet. This is why we need to invoke proxies that track the effect of the planet on the motion of the pertinent star. In the same vein, I seek to specify nonintentional indicators of a feature characterized by intentions.

maneuver. Such maneuvers are usually based on subtle manipulation or would be self-defeating otherwise. The challenge is to determine the precise epistemic nature of agnotological endeavors.

The claim I develop in this chapter is that one of the mechanisms invoked for creating confusion prima facie convincingly is false advertising. The methodological blunder associated with false advertising is the unacknowledged difference between the questions to which a study is sensitive and those that are allegedly answered on its basis. This incongruity and the lack of transparency that sustains it can be established without delving into the aspirations of individuals. Study design and use are in the public domain. In this account, the studies on bisphenol A and Bt maize/Roundup unambiguously involve agnotological maneuvers.

In particular, the lack of transparency can be identified by deficiencies such as missing responsiveness and ambiguities. As to the former, Kitcher pointed out that some critics tend to simply repeat endlessly the same worn-out objections that have been countered several times. They do not respond to the answers that they have received to their criticism but rather ignore these replies and start afresh each time (Kitcher 2011, 221, 229–230; see also van der Sluijs 2012, 192). As to the latter, by remaining vague or omitting elements of a study, ambiguities are created such that the study appears to attend to a certain question while at the same time permitting a reading that makes it deal with a different question. The gap between design and use is opened up by leaving key features imprecise and ambivalent. Agnotological studies need to be able to trade on expectations, and such a feat is facilitated by a lack of details, an implicit shift of emphasis, or ambivalent wording. Revealing such objective features of a study does not presuppose a psychological inquiry.

Rather than merely uncovering inappropriate bias, it is often more convincing to correct one-sidedness by conducting contrasting studies. A study of the defective kind can be supplemented with another one addressing the neglected perspective. In fact, given that there is no methodological recipe for judging a hypothesis in an impartial way, pluralism is the only means for approaching a balanced assessment. For example, in the early 2000s, scientists debated worries that the anticlotting efficacy of aspirin would decrease over the years. A couple of years later, a journalist revealed that a company competing with the chief manufacturer of the drug had launched the entire debate. This competitor produced alternative anticlotting agents and intended to boost its products via a scientific controversy. Conversely, some of the leading scientists opposing this alleged drop in effectiveness had been funded by the aspirin manufacturer (Wise 2011, 288). This mock controversy can be taken as indicating the skewing effects of commercialization, but also as buttressing the claim that the one-sided focus of certain studies can be offset by an approach biased in the opposite way. In this case, the economic interests in play could have created an agnotological predicament. But they did not because competing interests were at work. Biased conclusions were confronted with statements distorted differently, and this clash exerted a corrective influence. The pluralism thereby produced served to promote objectivity.

Speaking more generally, what helps to meet agnotological challenges is transparency and plurality. Transparency means that the partial and incomplete character of judging hypotheses is recognized and laid open. Furthermore, understanding that there is no uniquely correct way of judgment leads to acknowledging plurality. Unrecognized incompleteness and unacknowledged one-sidedness can best be revealed by pursuing research projects in which the questions addressed as well as the criteria of judgment employed differ from the studies under debate. This plurality makes it conspicuous that the agnotological judgments in question offer a partial view of the matter, but fail to acknowledge this partisan character.

We nevertheless now seem to be in quite the same quandary as initially. Pluralism appears to suggest that it is quite legitimate to keep a variety of competing approaches in play and there may be no way to single out one account in a justified manner. This could mean that we end up being overwhelmed by the multiplicity of different points of view. Yet in fact, we are in a better position to reduce the spectrum of approaches. Agnotological machinations can be identified by looking for indications of false advertising. Such approaches can be abandoned as not being serious epistemic endeavors. We are left with a qualified pluralism, and this means some progress at least. Pluralism is ambivalent. It is epistemically beneficial in enabling demanding procedures of test and confirmation, but it is also epistemically detrimental in leaving us without a definite system of knowledge. In this latter respect, the major challenge is to curb the manifold of contrasting assumptions and focus on serious competitors. Identifying agnotological endeavors is among the means to achieving such a reduction and producing a manageable range of alternatives. Some such accounts can be dropped as lacking the necessary credentials for being considered in earnest. They are disqualified as serious contenders for entering the system of knowledge. Such a reduction in the range of challenges to be taken into account justifies ignoring some dissenting approaches and thus helps to manage pluralism (see Carrier 2018, 167).

7. Conclusion

The goal of this chapter has been to clarify one of the mechanisms underlying the deliberate creation and maintenance of ignorance. The original, wider notion of agnotology focuses on the purposeful abuse of investigations for supporting nonepistemic interests. The challenge is to identify agnotological patterns that do not invoke intentions and are able to create confusion in a prima facie convincing manner. I seek to elucidate an agnotological strategy that has hitherto escaped notice—namely, false advertising. By this I mean the unrecognized discrepancy between the conclusions buttressed by the design of a study and those actually drawn or intimated. It is the distinction between design and use that characterizes agnotological ploys. Agnotological challenges are best met by transparency and plurality. The former requires acknowledging the partial character of a study, and the latter encourages conducting a different study so as to produce a more balanced picture. Pinpointing agnotological ploys is a means for weeding out approaches that look fitting at first glance, but in fact are blatantly inappropriate. Identifying such endeavors serves to reduce the range of studies under consideration and thus helps to manage pluralist diversity.

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4 Can We Sustain Democracy and the Planet Too?

Philip Kitcher

1. Introduction

For decades now, climate scientists have been building an increasingly complete and powerful case for the conclusion that the earth's mean temperature is increasing, and a principal cause is the release of greenhouse gases into the atmosphere.[1] That conclusion serves, in turn, as the starting point for analyses—less firmly grounded to be sure—that reveal many types of potentially problematic consequences, including some that are truly devastating. Indeed, if business as usual proceeds long enough, catastrophe is bound to occur. Yet despite repeated warnings, citizens of democratic regimes around the world have not been clamoring for policies that would limit industrial emissions or check the warming trend. Even before 2016 and the election of Donald Trump as president of the United States, there were signs of trouble. Pressure on politicians to consider bold initiatives aimed at tackling the problem of global warming abated, often because of other urgent issues (for example, the migration crisis that has frequently dominated European politics). Even in nations that once contained strong movements expressing public concern about climate change, the voices today are more muted than they were. For a brief hopeful moment, during the last days of Barack Obama's presidency, it seemed as though the United States, long a laggard among the affluent nations, was prepared to play a leadership role. Hopes were dashed by Trump's accusations that climate

1 I am grateful to Martin Carrier and Janet Kourany for wise advice and encouragement. An anonymous referee rightly urged me to revise an earlier draft so as to take account of recent events. The final version has been much improved by Evelyn Fox Keller's suggestions.

change is a concept “created by and for the Chinese in order to make American business noncompetitive,” and the subsequent actions to which this ludicrous verdict has led him. The Trump administration appears gleefully bent on accelerating the transition to climatic disaster.

According to a popular conception, democracy is all about voting. Democracy is frequently supposed to prevail in a specific place provided that the people who live in that place enjoy periodic opportunities to go to the polls, and when they get there, select from a list of candidates for office; it would be a travesty of democracy if the only name on the ballot were that of the “beloved leader.” Perhaps, in addition, there should be opportunities for prior free discussion in which the candidates express their views on various issues so that the marks placed on the voting slips will represent more than guesswork. If these conditions are regarded as sufficient for democracy, then there is nothing to complain about in the current apathy about climate change. The people should be trusted to govern themselves, and if they have had the opportunity to hear the contending voices—as in the debates about global warming, they surely have—their decisions should be respected. Democracy equips citizens with the power to make their own choices, even if they make decisions that their would-be guardians view as deeply destructive.

Were Plato reborn among us, it would probably be hard for him to resist saying, “I told you so.” The dim view of democracy among ancient political theorists was directed toward different structures of self-government than those present in the large societies that proudly advertise their democratic heritage—and from the ancient perspective, these would be considered peculiar types of oligarchy. Yet whatever the appropriate label, contemporary “representative democracies” absorb the chief error diagnosed over two millennia ago: they turn their back on the advice of the wise and allow for a tyranny of ignorance.[2] The perils of climate change already reveal the vast costs of doing so, even though only an infinitesimal fraction of the bill has so far been paid.

So arises my title question: Can we sustain democracy and the planet too? My answer: Possibly—although the already-tiny chances seem to

2 In the memorable words of Michael Gove, commenting on a different political issue (the Brexit question, which is, as will become apparent, related to issues of climate policy): “People in this country have had enough of experts.”

diminish by the day. Fortune may smile, delivering a technological solution, enabling people to remove greenhouse gases from the atmosphere and return the planet's climate to a tolerable state. Perhaps, if that were to occur, democracy might linger in its currently debased forms or even attain a healthier state. Barring highly uncertain advances in technology, however, a positive answer to my question would require us to be serious about what democracy is and should be; we would have to understand how ignorance subverts democracy and concern ourselves with rooting out the systematic causes of widespread ignorance. Prior to 2016, I would have estimated the chances of satisfying that necessary condition to be small but not negligible. Today it is hard to summon even that degree of optimism.

2. The Climate Consensus

Let us begin slowly with an outline of what the pertinent scientific community—the world's climate scientists—believes about global warming. There is almost complete consensus with respect to the following minimal claim: "If immediate action is taken to limit the emission of greenhouse gases, the rise in the earth's mean temperature by 2100 will be at least 2°C; if no such action is taken, the likely rise is between 3 and 6 degrees Celsius."[3] Most members of the climate science community would articulate the minimal claim in slightly different ways, but an overwhelming majority would accept the pie charts shown in the "Greenhouse Gamble" as approximately accurate (MIT Joint Program on the Science and Policy of Global Change. n.d.).

The basis for the minimal claim and more elaborated estimates combines parts of physics that have been well understood for about two centuries, and recent measurements by well-established techniques.

3 The minimal claim is sometimes disputed by scientists, including quite prominent ones who are not climate scientists. It is also challenged by a few scientists with some connections to climate science (for example, Richard Lindzen) whose research history involves support from companies with a vested interest in the continued production and use of fossil fuels. As far as I can tell, the most serious climate skeptic is the Hungarian scientist Ferenc Miskolczi; he may be the only figure with credentials in climate science and without ties to oil companies who rejects the minimal claim. I should also note that the safety of a 2°C increase above preindustrial temperatures is disputed by some prominent climate researchers (for example, James Hansen).

First, some measurements that show the recent warming trend (Watts 2009):

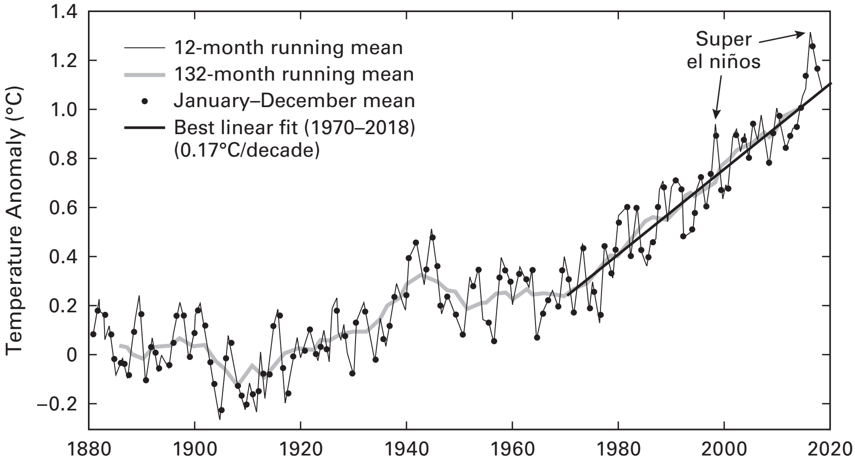

Figure 4.1
Global surface temperature relative to 1880–1920 mean. Sato and Hansen, n.d., with kind permission of the authors.

The graph provides an analysis of the recent decades and usefully correlates the data with El Niño effects. James Hansen's lucid presentations of the measurements for the past century and a quarter can be put into a longer frame by comparing them with Michael Mann's famous "hockey stick" diagram (http://www.realclimate.org/index.php/archives/2011/03/wahl-to-wahl-coverage/).

The graph displays the results of using two different techniques for estimating the temperatures in past centuries (before the practice of regular measurements), which then overlap beautifully with the actual records.

So far there is evidence for an increasing warming trend, but how can one argue further that this trend has been caused by human activities? To be sure, Hansen, Mann, and others have demonstrated an impressive uptick in the planet's mean temperature in the period since the Industrial Revolution swung into high gear, but as every student in any statistical science is taught dutifully to recite, "correlation isn't causation." The first step is to expose the full extent of the correlation between global mean temperature and the concentration of carbon dioxide in the atmosphere. Studies of

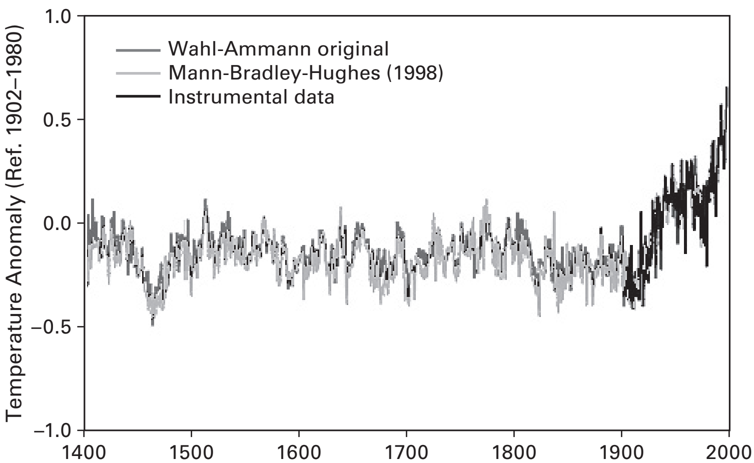

Figure 4.2
MBH 1998 compared to Wahl-Ammann 2007. Wahl and Ammann 2007, figure 5d, with kind permission of the authors.

carbon dioxide concentrations (obtained from tiny air pockets trapped in Antarctic ice) have enabled researchers to recognize the covariation of the concentration of carbon dioxide in the atmosphere with the global mean temperature over a period of 650,000 years (Siegenthaler et al. 2005).

Correlations of this magnitude are highly unlikely to come about by chance. There is, however, a known mechanism by which increased concentrations of some gases give rise to a "greenhouse effect" and resultant higher mean temperatures. From Joseph Fourier's original analyses of heat radiation and diffusion, through Svante August Arrhenius's investigations of the effects of carbon dioxide, to the refinements of contemporary theory, there is a well worked out physics of the process (for lucid accounts, see Hansen 2009; Le Treut 2010). No alternative-known mechanism could reproduce the data we have. Despite the best attempts of skeptics to conjure up rival causes, none of them survives detailed scientific scrutiny; indeed, many talented climate scientists have given their time to demonstrating the flaws in the deniers' favorite proposals. Hence there is a culprit marked with a vast mass of clues (the fine-grained correlations), for which means and opportunity can be established (the well-studied physics of the greenhouse effect), and there is no known way in which any other suspect could have done the deed. Sherlock Holmes would regard it as a ludicrously easy problem—and one hardly worthy of his attention.

Drawing consequences from anthropogenic global warming is harder, because attempts to construct models of the earth's climate are necessarily selective from the full range of known factors that could force or dampen the basic greenhouse effect. Furthermore, understanding the interactions between the *climatic* phenomenon (increasing mean temperature) and *weather* patterns is difficult, and the techniques of analysis are more controversial than those put to work in establishing the basic claim. Hence, within a large community of researchers, united on the fundamental thesis of anthropogenic global warming, there are divergences of opinion about the extent of the rise in temperature and its effects on various regions. Faced with certain kinds of questions—for example, about the estimated rise in average temperature in a particular region, such as, "Will it be possible to grow grapes for wine in Scotland?" (a question posed, of course, by the resurrected version of Adam Smith)—it is only possible to point to alternative analyses and not even satisfy the questioner by specifying responsible figures for probabilities. Yet not everything is shrouded in complete darkness; there are some consequences that are not hard to predict, and serious threats that are easy to foresee.

Rising temperatures will surely lead to the melting of ice sheets, as has already been observed with respect to both the Greenland and significantly larger Antarctic ice sheets (see E360, n.d.; *Scientific American*. n.d.). In this arena, the only matters of controversy concern the speed with which this will occur and corresponding rate at which sea levels will rise. Even by conservative estimates, however, it is evident that significant parts of the world are in trouble. The Maldive Islands are likely to vanish, and the densely populated region along the Bay of Bengal will become uninhabitable. These are only the most obvious results of the rising seas. In Europe and North America, there are significant regions with flat coastlines, ripe for inundation given, say, a rise of a few meters.

Furthermore, it is important not just to focus on the *mean* rise in sea level. The future vulnerability of thickly inhabited parts of the globe also depends on the *amplitude* of the oscillations around that higher mean. An area that has only been flooded occasionally, perhaps in the "storm of the century," may be afflicted every decade or even more frequently because given the elevated level of the usual high tide, a relatively run-of-the-mill weather event can surmount protective barriers. The problem of oscillations about a higher mean is exacerbated if global warming itself has a tendency to increase the frequency of extreme weather phenomena. As is now becoming evident, that tendency is real.

The theoretical difficulty is evident if you envisage a bell curve showing the distribution of some weather event (for example, the temperature in a particular place or height of the high tide). The effect of global warming will be to shift the mean of the distribution toward a different value—indeed, in the two examples chosen, toward a *higher* value. So events that were previously rare, such as temperatures of 40°C or high tides of 3 meters, will now occur at significantly increased frequency—they'll fall under fatter parts of the Gaussian curve instead of being at the skinny tail—and events that were hitherto unknown will become the rare catastrophes (temperatures of 45°C or tides of 4 meters). The effect is intensified, if as some data already suggest, global warming not only shifts the mean but also increases the variance.

Weather data around the world are already showing a dramatically increased frequency of extreme weather events, and despite the prudent (possibly overcautious) warnings of climatologists about the need for

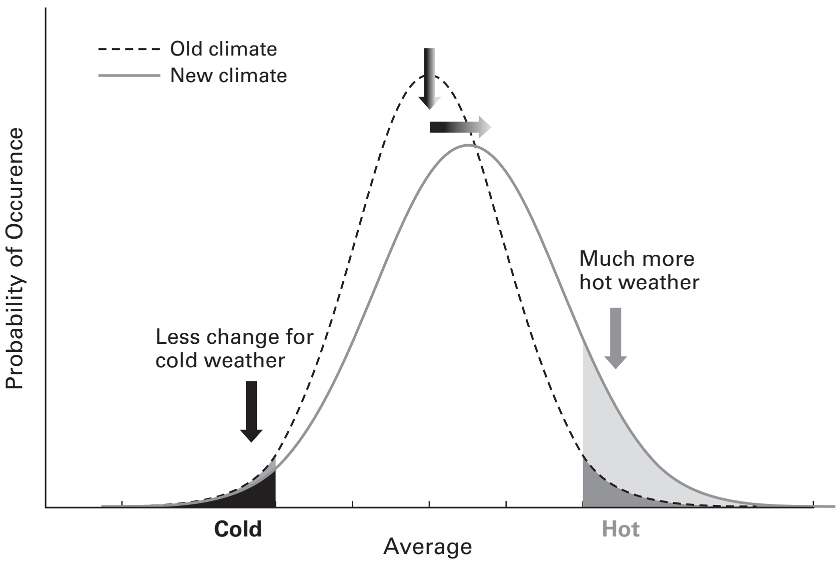

Figure 4.3
Adapted from IPCC 2001, 131, with kind permission from the Center for Climate and Energy Solutions.

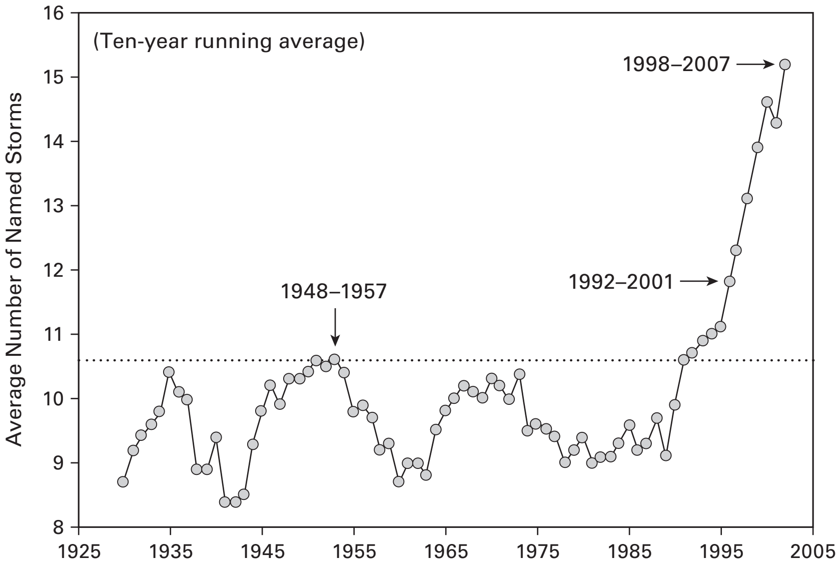

Figure 4.4
Annual frequency of North Atlantic tropical storms.

careful statistical analysis, the everyday experience of extreme weather is persuading some erstwhile skeptics that "global warming is real" (Blue and Green Tomorrow 2012).

What exactly does increased susceptibility to flooding mean? Plainly, it involves damage to homes and perhaps some loss of human lives; the first natural associations focus on the obvious effects of water pouring into coastal areas. It's easy to overlook the less direct consequences, such as the disruption of food supplies, ruining of crops, and overwhelming of sewage facilities with the possibility of massive contamination of the water supply. The focus on coastal areas also tends to divert attention from the fact that some *inland* regions are vulnerable to inundation, given sufficiently extreme storms. Thus according to some estimates, there's a serious possibility that global warming will result in flash floods in California's Central Valley, turning a major region of agricultural production into a salt marsh. More firmly established are predictions that water supplies in the Southwest of the United States will be adversely affected because under warmer temperatures, the Sierra Nevada snowpack will melt too rapidly in the spring, causing immediate flooding, quick runoff, and subsequent periods of drought.

As I have already cautioned, it would be foolish to set too much stock on detailed predictions of this sort. The models used to generate them make assumptions that can be challenged, and although in the case of the melting of snow in the Sierras the conclusions might be *more* robust, it would be pretense to think that firmly grounded high probabilities can be assigned—the kinds of probabilities that call for immediate action. Yet the supposed effects of anthropogenic global warming are not an undifferentiated mass of idle speculations; it is entirely reasonable, for example, to be concerned about low-lying areas and foresee a considerably increased frequency of weather-induced disasters. Beyond that, I suggest, proper attention to what has been shown by contemporary science reveals a wide range of sources of future trouble.

Besides the dangers already mentioned—the contamination of water supplies, alternation of floods and drought, and disruption of agriculture—future populations are likely to face modifications of existing patterns of disease transmission. Those changes increase the probability of large pandemics as well as new opportunities for the evolution of disease vectors as species come into new types of contact. In train are likely to come social tensions caused by the migration of large populations from areas that have become uninhabitable. Much of the infrastructure on which human beings depend will be vulnerable to destruction or will be in the wrong places to meet our descendants' needs. Future generations will face periodic threats of loss of shelter, lack of water, famine, and disease outbreaks that will not be readily met with the resources that remain. It is unwise to conjecture just how these consequences will occur or whom they will affect, but it would be irresponsible to think that they will not happen or that somehow the tools will be available to adapt to them.

How can anyone be so confident of these generic effects, given the admission that specific details are beyond the reach of well-grounded analyses? By posing the right question. Climate scientists cannot reliably settle the issue of whether Italy will become a desert. But contemporary research combines to show that there are serious probabilities of lots of *sudden* changes with impact on human health and survival. We should not be beguiled by a fantasy of orderly retreat from a few endangered low-lying areas, as the seas gently rise, with populations gradually replicating the fields, homes, roads, and water supplies they regretfully leave behind. Floods will likely come quickly, the water that was plentiful yesterday may be gone or contaminated today, new diseases will invade a population, and perhaps one without the resources to practice simple hygiene, and the resources of a community will have to be shared with a sudden influx of displaced people. The migration crises of the future—even those occurring three or four decades hence—will likely make the current problems—problems that have challenged governments to devise humane and responsible policies—look like child's play. Without being able to place probabilities on any of these scenarios, it's clear that many of them could play out, again and again, and perhaps in close succession, in virtually any region of the world. Instead of fixating on a particular version, except in the most evident instances (the Bay of Bengal, for example), we should ask, "What are the chances that our descendants will avoid a sufficient number of these terrible futures to live the sorts of stable human lives that the more fortunate members of the world take for granted?"

Imagine yourself in a ravaged city in the throes of a multisided civil war. If you are to survive, you must venture out, day after day, and assemble the things you need. On each of these journeys, dangers of all sorts could arise at any number of junctures: there could be bombs and mines, snipers, street battles, and gangs of marauding looters—and they could come from a variety of factions and any direction. At each particular point, the probability of some specific threat could be low—there isn't a high chance that the commando unit of the hostile purple faction is lurking around the next corner. Nevertheless, given the vast number of potential dangers, the probability that you will make your way unscathed, day after day, week after week, is low. In retrospect, you will curse yourself for not having heeded the warnings and left the city for a safe haven, while you could.

As I read the well-established cautions from contemporary climate science, that is the image the citizens of today should have in mind when they think of their descendants and the people who will come after them.

3. Failure to Act

Let's turn now to the conditions of contemporary debate. Even if there were agreement on the minimal claim, and even if the conception just suggested were firmly ensconced in the minds of many people, moving further with respect to climate change policy would still be hard. For it is often suggested that the measures that are readily envisaged for the restriction of greenhouse gas emissions would have negative consequences for economic life, if business were to go on as usual. There are risks to the wealth of nations as well as to their health, and appreciation of those risks inclines many people to wonder if the cure would turn out to be worse than the disease. If the focus is, as it should be, on the well-being of the people who will live on our planet half a century hence, nobody can responsibly judge whether their lives will be placed in greater jeopardy as a result of the natural catastrophes that global warming is likely to unleash or because of the collapse of large portions of the global economy produced by attempts to curb the industries most responsible for the emission of greenhouse gases.

The question is intractable partly because the track record of large-scale economic theorizing is so dismal. Attempts to model the global economy, given the conditions brought about by some putative climate change policy, are even more speculative than ventures in specific predictions about the climates of particular regions. Nor is it possible to predict the course of the economy if business continues as usual. Those who emphasize the supposed costs of acting to reduce emissions of greenhouse gases sometimes compare some allegedly disrupted economic future (in which climate action restricts economic growth) with smooth extrapolation from the economic past. That comparison is fallacious. Business as usual will subject the global economy to the stresses outlined in the previous section, and it would be folly to ignore the devastations of productive capacity (as well as the costs of ameliorating the tragic human consequences). Two uncertainties clash. To argue that climate action now is worse for the global economic future is a groundless conjecture.[4]

Those who resist action often pin their hopes on technological advances. They think that technology will smoothly adapt to the conditions in which future people will have to live—as if the problems were nicely announced in advance, and imaginative inventors could devise solutions. Yet as the last section argued, many of the threats are likely to come suddenly, and even if their shape can be recognized in advance, it would be silly to assume that the crucial details can be anticipated so that adequate defenses are

4 Extrapolation from the economic growth of the past century is not only problematic because it fails to take into account the shocks that climate change will bring to the global economy. Some economists believe the hitherto-unparalleled recent growth to represent a temporary departure from the far less impressive historical trajectory. See, for example, Gordon 2016.

ready.[5] Perhaps problems can be forestalled, not by concentrating on individual effects, but by addressing the root cause of the trouble. Thus arises a search for methods of modifying the atmosphere so that the warming resulting from the greenhouse effect is held in check.

Geoengineering comes in two varieties. Negative geoengineering attempts to find methods of using fossil fuels without emitting greenhouse gases (preventing further buildup of carbon dioxide in the atmosphere) or, more ambitiously, removing some of the greenhouse gases already there. The modest version—carbon capture and storage—is feasible on a small scale, but there are difficulties in achieving it at anything like the scale needed. Unless a new breakthrough occurs, carbon capture and storage will be part of a strategy for addressing the problem, but its role is likely to be quite limited. The more ambitious form of negative geoengineering, attempting to suck greenhouse gases out of the atmosphere and store them so that they are not reemitted, faces greater challenges. *If* those challenges can be overcome, our descendants would have an apparently painless solution to the problems posed by climate change. They could practice business as usual with a technological fix.[6]

Negative geoengineering tries to return our atmosphere to a state it once enjoyed—a state in which we know that human beings can live. Positive engineering, by contrast, seeks to add something to the atmosphere to counteract the greenhouse effect. The most popular version proposes to "seed" the atmosphere with sulphur particles so that a greater percentage of the incoming solar radiation will be reflected back into space. The history of attempts to jigger the environment, carried out on a humbler and apparently more controllable scale, has inclined many citizens to distrust experiments of this sort. From myxomatosis to DDT, we have learned caution, and people who are reluctant to allow genetically modified organisms into their agricultural endeavors would be strangely inconsistent if they relished the thought of filling the only atmosphere we have with particles

5 In some discussions of climate change, there's a curious faith that if a technological problem is urgent, and if the urgency is clearly recognized, the free market will automatically generate a solution. Voltaire's Dr. Pangloss seems to have converted many contemporary economists.

6 This scenario would resemble the case of the ozone hole, where the problem was resolved by banning the use of chlorofluorocarbons and finding a substitute for their industrial uses. For an illuminating discussion, see Oreskes and Conway 2010.

intended to compensate for the carbon dioxide they were continuing to emit.

Unless we emulate Dickens's Mr. Micawber, hoping that some breakthrough in negative geoengineering will turn up, the only chance of avoiding climate catastrophe is to mend our ways. Humanity needs a transition to a state in which emissions are reduced to zero—by abandoning most of our dependence on fossil fuels (the residual amount being handled with small-scale carbon capture and storage). Activities now requiring us to burn coal, oil, and gas will have to be supported in other ways, or simply given up.

Thus the form of the debate that should be going on is relatively clear. Since the problem is a global one, requiring the cooperation of all nations, the task is to find a way of lessening the risks of a regular and indefinite sequence of human catastrophes—the civil war predicament of my image—while avoiding an equally damaging devastation of the global economy, and assigning the costs fairly. The latter constraint is intended to capture the legitimate concerns of states that have not had the opportunity to become productive and economically competitive, and now reasonably see themselves as being asked to make sacrifices to redeem the sins of wealthier nations; any solution must compensate them for foregoing what the great polluters have so carelessly enjoyed.[7] If that problem has no solution, if there is no way to avoid economic disaster or secure global cooperation on fair terms, then the best that can be done is to dedicate present research efforts toward lightening the burdens inevitably faced by our descendants, insofar as we can. Yet given the uncertainties of global economic theory, any declaration of insolubility should not come too quickly, before many more avenues have been explored.[8] So I claim that in a lucid world, the problem just posed would be at the forefront of human attention, prominent in the thoughts of statespeople and citizens alike.

That is not the case, of course. It is worth inquiring why it is not.

Part of the answer is that the efforts of climate change deniers prevent the debate from ever reaching the terms in which it should be formulated. Instead of seeking a way of accommodating uncertain threats, the

7 For a more fine-grained account of the conflicting obligations we should attempt to honor, see Kitcher and Keller 2017, especially 165–173.

8 Philip Kitcher and Evelyn Fox Keller argue for a global program of broad social experimentation, accompanied by international cooperation and mutual learning. See Kitcher and Keller 2017, 104–108, 113–125.

electorate, especially in the United States, is lulled by thoughts that the worries may not be genuine, the Cassandras should not be believed, ideology has substituted for sound science—and nothing needs to be done. In their valuable study of various movements fueled by a post–Cold War, probusiness agenda, Naomi Oreskes and Erik Conway (2010) document the tricks that have been used to prevent US citizens from finding out what is good for them. None of the cases they discuss so insightfully comes close to matching the importance of their final example: the conspiracy to deny climate change.

The failure to focus a crucial collective human decision in the right way stems from widespread ignorance. Ignorance is especially poignant under circumstances where what is not known is, in a sense, "something we know," and when those who fail to know are put at risk by their ignorance. Someone's failure to know "what is known" can be their own fault—say, the ignorance derived from a culpable lack of effort. Or it might not be a matter of anybody's wrongdoing but instead simply a spin-off from structural conditions in the social system for acquiring and distributing knowledge. Finally, it might be generated through the deliberate actions of powerful individuals, who serve their own interests by placing large and even insuperable obstacles along the route from ignorance to knowledge. Oreskes and Conway (2010) demonstrate that this last scenario figures in the public ignorance about climate change.

Even the quickest tour around the internet will reveal plenty of sites on which conclusions about the earth's climate, strikingly at odds with those accepted by the consensus of climate scientists, are presented as if they were gospel. It is hard for those seriously interested in finding out about the issue to avoid claims that global warming is a hoax. If they find their way to a discussion of Mann's hockey stick, such as on the excellent and informative RealClimate site, they will also probably discover another site with a slick and witty video mocking Mann, using phrases from the famous "hacked emails" radically out of context to suggest that his scientific work is founded on dissimulation and deceit.[9] To succeed, these sources of disinformation do not have to convince those who listen to them that the perspective they supply is the truth. The task is simply to arouse enough doubt

9 See http://www.realclimate.org/. For the video condemning Mann, with links to other sites in the same vein, see http://www.youtube.com/watch?v=fAlMomLvu_4.

so that reasonable and intelligent citizens will conclude that this is an area in which things need to be sorted out, so that political decisions about what to do can and should be postponed.

Despite the many loud voices denouncing global warming as a hoax, the public acceptance of it has increased over the past few years, perhaps as a consequence of awareness about dramatic weather events. In the United States, for example, despite the pronouncements of the president and his erstwhile head of the Environmental Protection Agency, 70 percent of the population now believes that global warming is occurring, and 49 percent are “extremely sure” about this. At the same time, the percentage that attributes global warming to human activities has risen to 58 percent. Sixty-two percent of the populace is at least “somewhat worried” about the effects of global warming, but only 21 percent is “very worried” (although this is a sharp increase from the roughly 10 percent that confessed to being “very worried” in 2015). The recent increase in perceptions of a real problem seems to depend more on impressions gleaned from reports of extreme events than from attention to climate science, however. Only about 15 percent understand that almost all climate scientists have “concluded that human-caused global warming is happening” (Leiserowitz et al. 2018). Apparently, then, three decades after Hansen’s famous testimony to Congress, US ignorance is slowly giving way to a more accurate picture. The beliefs of citizens continue to lag behind expert knowledge, though, and consequently underestimate the future threats. Being “somewhat worried” but not “extremely worried” is not enough to spur a public demand for the kinds of actions needed.

Apparently, then, if democracy is to be sustained, the planet will become inhospitable to our descendants—if indeed it allows them to live on it at all.

4. Conceptions of Democracy

But what exactly does democracy require? Let’s start with two popular thoughts:

1. In democratic societies, policies should be decided in accordance with the will of the people.
2. In democratic societies, the issues to be addressed should be freely debated in public.

If these popular thoughts are honored, then it seems overwhelmingly likely that no policies to counter the emission of greenhouse gases will be instituted. Yet we might inquire, does this really provide what citizens most deeply and centrally want? Does it do what democracy is surely supposed to do—namely, enhance the freedom of the people? The ancients, deeply skeptical as they were about popular governance, recognized that the "friends of democracy" would claim that this form of government promoted human freedom (Plato 1992). Is human freedom enhanced when citizens elect candidates who are quite willing to permit a future in which the lives of those who come after us will be threatened by a sequence of unpreventable catastrophes?

Our freedom is expressed in our ability to pursue "our own good in our own way"—John Stuart Mill's (1998, 15) "only freedom worthy of the name."[10] If there are systematic causes that foil our best efforts to realize our most central aspirations, then those causes limit our freedom. When such causes operate through what appear to be our own free choices, there is bitter irony in our predicament, for the attempts to exercise freedom are destructive of what we are conscientiously trying to achieve. That, I suggest, is the predicament in which voters find themselves today, not only in the United States, when they support candidates who are at best uninterested in working toward a global solution to the problem of climate change.

My suggestion rests on an empirical hypothesis—one that should be systematically tested, although I would be surprised if it turned out to be false. According to that hypothesis, most human beings are profoundly concerned with the well-being of their descendants, not only their biological children, grandchildren, and so on, but of a wider circle of people who will live on the earth in the decades after them.[11] Human projects are not limited by our life span. Most people hope that something they do or achieve will endure, and that the places or communities they build, tend,

10 Famously, Mill adds the important proviso that pursuing our own good must not impede the kindred pursuits of others.

11 The survey by Anthony Leiserowitz and colleagues (2018, 22) cited earlier provides some evidence in favor of this hypothesis. Twenty-four percent of Americans see the most important reason for tackling global warming as "provid[ing] a better life for our children and grandchildren." This finding outranks all other reasons considered, some of which may also cover future descendants as special members of a more inclusive class.

and sustain will be inhabited by others who will develop them further as well as thrive in them. Conversely, the thought that the usage of what had been inherited from earlier generations had been sacrificed to the detriment of those who came after, or that resources had been squandered in ways that diminished the lives of the latecomers, casts a shadow over human lives. Self-reproach is especially apt if there were opportunities for understanding what was being done, and if they were carelessly ignored. So if contemporary voters were vividly aware of the gamut of challenges that an overheated planet would bring for the human population of the late twenty-first century—if the civil war image were firmly in mind—the hypothesis claims that they would regard this future as a bad one, and even one so troubling that it demanded serious attention now.

Instead of the simple ideas of democracy with which this section began, it would be preferable to ask why the "friends of democracy" are so enthusiastic about it. The answer is Plato's: because it promotes human freedom—and it might be added, does so equally for all. Ideals of freedom and equality in freedom are to be expressed in the abilities of citizens to shape the policies of their societies. If they choose wisely, those policies will enable them to realize the goals they view as central to their lives (Mill's fundamental freedom). That shaping is supposed to come about through elections under conditions of free debate. *These superficial manifestations of democracy are not ends in themselves but rather means of enhancing equal human freedom*.

All this can go wrong, *badly* wrong, if particular groups within a supposedly democratic society deliberately foster a situation in which those who choose candidates for political office are unable to recognize which policies are appropriate to the ends they seek, or even if public ignorance is induced because those in power tolerate social structures that make it hard for citizens to recognize where their interests lie. These conditions, either the deliberate manufacture of ignorance or acceptance of social institutions that foster ignorance, are antithetical to genuine democracy. Although Plato's condemnation of the choices that the masses were likely to make was wrong, based as it was on the view that people lack the *intelligence* to choose wisely for themselves, no amount of natural wit can compensate for deep deficiencies in *knowledge*.

One of John Dewey's (1982) great insights in political philosophy is that democracy is a work in progress. The democracies that now exist have grown out of processes of democratization—processes that we should see as *unfinished* attempts to promote human freedom and equality in freedom. Democracy's good press should rest not on its complete solution to all problems that threaten or limit freedom but rather on its ability to address some of them. Specifically, throughout human history, creating structures in which people have the ability to vote for elected officials, under conditions of more or less open discussion, has enabled those who suffer interference in their lives and projects to overthrow those whom they can identify as doing so. Cases that show erstwhile democratic societies slipping back into tyranny (Nazi Germany is a recent example) serve as a reminder that the solution is not perfect. Nevertheless, the machinery of democracy is frequently effective in solving the problem of identifiable oppression. If a tyrant is visible, if the limitations that afflict the lives of citizens can be traced to their actions, then the tyrant's need to seek reelection offers the opportunity to replace them and attempt to remove the oppression.

In complex societies, people's freedoms—specifically, the freedom to choose and pursue their own projects, and ones that do not interfere with those of others—can be confined without their having any ability to discern the source of the confinement, or even without there being an intentional agent who is responsible. Modern citizens face recurrent problems of *unidentifiable* oppression, and these problems are no less real nor any less deep than the readily discernible forms of tyranny to which democratization has historically responded. The development of democratic institutions has refined the machinery of elections and open discussion of positions as well as setting in place important constitutional safeguards that protect against historically salient ways in which individual freedom has been invaded. Nevertheless, the ideals of freedom (conceived in the Millian way) and equality in freedom require recognition of the pervasiveness of unidentifiable oppression along with the deep damage it does to human aspirations. Citizens whose local environments and educational opportunities make any serious choice of their preferred life directions or central goals impossible may not even see what is happening to them as a profound limitation of their freedom. Yet they are as thoroughly cramped by the institutions and structures of their societies as they would be by any of the classic historical exercises of tyrannous intervention in human lives.

Whether or not knowledge is power, ignorance can be impotence. The societies we call democracies often supply the illusion of self-determination: their citizens can troop to the polls and express their choices. When their opinions are deliberately manipulated, or the conditions that permit their preferences to diverge from their interests are taken to be acceptable, the fundamental value of democracy—the promotion of equal freedom—is decisively undermined. A commitment to democratic ideals thus calls for further democratization and measures that expose the currently invisible forms of oppression. Sustaining democracy requires channels through which information can flow to the citizens so that they can appreciate where their interests lie.

The challenge of sustaining the planet is exacerbated *in part* because those channels have been blocked and diverted. Of course, they are supposed to be there, and be in working order. Recall the second of the two simple thoughts about democracy: issues should be freely debated in public.

5. Free Debate and the Millian Arena

The idea of open discussion as crucial to democracy has had eloquent champions, including John Milton (1999) and Mill (1998) prominent among them. Both writers, like their less influential successors, take for granted a principle of evidential harmony. They tacitly suppose that those who listen to the open discussion will eventually reach a position in which they form their opinions in accordance with the evidence. Foes of democracy (Plato, for example) would worry about that presupposition on the basis of skepticism about natural human rationality: the masses are simply not intelligent enough to weigh the evidence. That worry sells the voters short. Yet damage can be done if the arena in which issues are debated obfuscates the evidential relations.

Friends of "free speech" and "open debate" typically envisage an ideal forum—call it the *Millian arena*—in which public discussion occurs. It is worth asking about the characteristics this arena should have. Who is permitted to speak in it? What issues are to be discussed? What provisions are made for ensuring that speech is accurate? What rules govern the continuation of debate? What efforts are made to ensure that the considerations brought forward are comprehensible to the people who will eventually judge the merits of the question? Defenders of open discussion tacitly take particular answers for granted. They assume that the agenda for debate is already given, and that there is a single question or a small set of them that has arisen. How has that occurred? Perhaps because particular issues are salient for the public, and the arena responds to their concerns. Yet that mechanism will not work well if the initial ignorance is deep, and if citizens do not appreciate the topics that matter to their plans and projects. Nor will it work if the agenda is controlled by a part of the society that would prefer not to promote illumination on certain issues. It is also commonly supposed that the discussion will represent all major points of view and those who speak will not be biased toward some particular position. There is, apparently, a microphone to which any member of the society may have access. As for the veracity of the speakers, that is either taken for granted or else supposed to flow from some potential check that would punish errors: those who speak falsely will be shown up, and their opinions discounted. That requires some well-appreciated mechanism for reliably determining when errors have been made. Furthermore, it is also crucial for open discussion to run an orderly course, and that it will not be protracted forever because partisans of a specific point of view find ways to reiterate their claims indefinitely, without engaging with the arguments directed against them. Finally, there must be no obstacles to public comprehension of the issues. What is said must be understandable by the citizens. Where difficult concepts arise, they must be clearly explained.

The Millian arena envisages a parade of speakers, irreproachable in their sincerity, lucid in their speech, taking their turns at the public microphone, and courteously retreating when they have said their piece. It is a charming Victorian vision. But it is not the way we live now. In contemporary societies, citizens' lives are often profoundly affected by decisions about matters that are hard to formulate in immediately comprehensible ways; climate change is one illustration among many. What is said with respect to those issues is hard to subject to checks that people can apply for themselves, so that their predicaments frequently involve recognition that two speakers are disagreeing, and each impugns the competence or sincerity of the other, without any ability to identify directly which of the two is mistaken. Major sources of information select what topics they are going to cover, and it is easy for people to fail to come to know that a particular dispute really matters to them. The voices that are heard are, all too often, either those able to pay for a time at the microphone or those selected by particular groups that control access. Finally, if the owners and managers of the arena so decide, debate can be continued through the simple stratagem of repeating what has been previously said—no matter that it was rebutted on some previous occasion, for the attempted refutation can easily be written off as erroneous or biased.

Concrete instantiations of these foibles are easy to recognize. When the business of providing information to the public is literally a business, subjected to the pressures of the marketplace, issues that rich citizens would prefer to see ignored can easily be sidelined, either through the direct means of not having them reported or by creating so much obfuscation around them that public frustration is aroused. The tactic of "fair and balanced" reporting, in which two implacably opposed partisans trade opinions for some short period of time, is well designed to realize the second approach. Eliding the difference between informing and entertaining creates a pressure against the serious work of explaining complex issues, placing further obstacles in the sober assessment of the evidence. For half a century, it has been a familiar commonplace of the philosophy of science that scientific debates typically turn on delicate questions, are frequently long and hard to resolve, and no single argument convinces all members of the expert community (Kuhn 1962). How, then, could anybody reasonably expect that responsible opinions could be formed among people who lack training in an area and have not been provided with any accessible explanations of crucial concepts when they periodically hear two excited voices raised for a few minutes in heated debate? The distance between the actual circumstances of "open discussion" and the condition of evidential illumination that open discussions are supposed to promote—satisfaction of the principle of evidential harmony—is so vast that the thought that actual public debates can create the circumstances for genuine democracy is ludicrous.

Furthermore, and perhaps most evidently, the transmission of information in contemporary democracies, even those that still have reliable public systems of dissemination, is strongly biased toward the voices aligned with those individuals and groups that hold disproportionate shares of the societal wealth. In the United States, there is a curious sensitivity to the idea that members of these groups might somehow be deprived of opportunities to speak—hence the decision to assure corporations of their status as persons, with consequent rights to air their views in public debate (a tenderness expressed in the *Citizens United* decision). This particular development illustrates a principal theme of my argument, for it shows the tendency to substitute lip service to the ideals of democracy for a deep commitment to democratic values—a commitment that would actually inspire a contrary movement. The “right to speech” is detached from its context and democratic function, and assigned to dubious entities without any serious consideration of how the attribution would bear on the freedoms of other individuals, even those who might seem more obvious targets of protection. Thus the provision that money can be directed without limit toward efforts at public persuasion is not seen as in any way endangering the ability of the vast majority of citizens to become informed about matters of the deepest concern to their lives and projects, nor consequently to limit their freedom to express their genuine interests in a vote. As such, the strained freedoms of the privileged few are given complete priority over the fundamental freedoms of the many.

Finally, even in the face of a vast scientific consensus, among whose champions there are some who have campaigned resourcefully and persistently to promote public understanding (including, among others, Hansen, Mann, Michael Oppenheimer, and the late Steve Schneider), the Millian arena we actually have permits the rich and powerful who fear the translation of that consensus into public policy to battle to a draw (recall the statistics on US acceptance of global warming). The strategy is never to stop talking and always reiterate the same points, without acknowledging that they have been answered. Over decades, this has served well in the attacks on Darwinian evolutionary theory. Hardly a debate goes by without creationist mention of the alleged incompatibility of evolution with the second law of thermodynamics. No matter that the point has been rebutted scores, if not hundreds, of times by scientists with impeccable credentials. Perhaps the latest audience has not heard those rebuttals, but even if it has, there are always rhetorical devices available for casting doubt on the cogency of the replies. Similarly, in climate change discussions, the same themes recur again and again: the alleged uncertainties of tree ring analyses, confusions about rates of ice sheet melting, alternative possibilities for explaining rising temperatures (sunspots), proposals for geoengineering (particles in the atmosphere and optimistic scenarios of carbon capture), suggestions about adapting to a warmer earth, and the like. The voices argue on, cycling among enough possibilities to defeat the hearers’ powers to keep track of all the details.

So democracy is eroded. A Millian arena (or some functional alternative) is required if the superficial manifestation of democracy—elections with a choice of candidates—is to provide the opportunity to align votes with actual interests and thus promote the citizens' fundamental freedom. The verdict on the United States should coincide with the one the ancients would have rendered: this is no democracy. US citizens live under a statistical oligopoly. Policies are shaped to suit the perceived interests of a small number of rich and powerful individuals, and this shaping uses the mass of citizens and framework of elections as a means. The machine works by creating conditions of information and misinformation so as to raise the probability that the voters will elect representatives whose decisions can be expected to conform to the choices that those who pull the levers would like to see made.

The value of the ideal formulated by Milton, Mill, and their successors is indisputable. The ability to become informed is crucial to democracy. A genuine commitment to democracy involves trying to enhance the equal freedom of all citizens. In its turn, that requires mechanisms for transmitting information on issues that are central to their lives. The ignorance so devastating to democracy is *originally* systemic. The circumstances for free discussion were shaped to accommodate issues that citizens could reasonably be expected to decide for themselves, in an age when at least some avenues for disseminating information were subject to accepted standards of responsible conduct. Under various pressures—some technological, and some economic—these institutions have evolved. Many of the problems we now face turn on the details of investigations that few people can understand, let alone appraise for themselves. At the same time, the media are ever more vulnerable to the pressures of the marketplace. So as a complex of institutions adapts to a changing environment, ignorance is systematically produced, and the Millian arena fails to fulfill its function. Yet it would be naive to suppose that the evolution of "free discussion" has gone unremarked. What began as the unplanned fostering of ignorance has been recognized by those to whom ignorance and confusion are most welcome. The "merchants of doubt" now work to maintain an evolved set of social institutions that suit their purposes.

6. It Gets Worse

The diagnosis I have offered provides only a partial explanation of the current dearth of climate action. As events of recent years have made abundantly clear, more is going on. Any adequate account of continuing apathy—attending, for instance, to the failures to set appropriate targets for reducing emissions, even when the problem is acknowledged—must pay attention to three other factors: the increasing penetration of the internet into human lives, rise of populism, and tolerance for bullshit on a grand scale.[12]

Many people think of the internet as an instrument of democratization. Their belief rests on an obvious thought: anyone can "post," so the capacity for speech is greatly enlarged; marginalized voices can now be heard. This turns out to be as empty as the boast made by William Shakespeare's Owen Glendower:

> Glendower: I can call spirits from the vasty deep.
> Hotspur: Why, so can I, or so can any man;
> But will they come, when you do call for them? (2016, act 3, scene 1)

Any of us, at any time, can launch words into the air, or write on-screen or on a piece of paper. Whether anyone else will pay attention is an entirely different question.

Responsible internet users repeatedly face the problem of assessing the reliability of the information provided by the sites they visit. The obvious strategy is to deploy the markers of expertise that they have already been accustomed to using. "Established authorities," however previously selected, are given precedence, and any expansion of trust is based on harmony with what the "authorities" tend to say. Anyone who follows that (conservative) strategy is likely to listen to previously marginalized voices only insofar as they echo already-entrenched opinions. Search engines assist the entrenchment. Their business model favors algorithms that build in "personalization": a user's past choices are reflected in the sites popping up in response to the keywords. And that tends to produce user satisfaction, continued use of the search engine, greater attractiveness to the advertisers on whose largesse the parent company depends, and ultimately larger

12 The account of the last section might explain the failure of societies, particularly in the Anglophone world, to formulate serious climate policies in the period before the financial crash of 2008—although even during this time, the seeds of later difficulties might have been germinating. Perhaps the tolerance of dangerous bullshit was already evident in the alleged connections between the attacks of September 11, 2001, and the decision to invade Iraq. Here and throughout this chapter, I use "bullshit" in the sense delineated by Harry Frankfurt (2005) in his brilliant book *On Bullshit* and my essay "Dangerous Bullshit" (Kitcher 2018).

profits.[13] The watchword is not "democratize!" but rather "find out what people want and give them more of it."

The promise to extend the range of voices heard on controversial issues is thus vacuous. Yet there is a further, insidious effect. Debate fragments. Widespread reliance on the internet amplifies a phenomenon begun while traditional media were dominant: the targeting of niches, in which people subscribing to particular types of social and political opinions are told what they want to hear.[14] In their cozy cocoons, citizens can escape from the information that might lead them to stop and think. As we will see, the economically successful strategy of conveying only what is comforting to the target audience also helps foster the tolerance of bullshit.

The political movements often characterized as "populist" are fostered by this fragmentation of the citizenry.[15] At the heart of populism lies a self-identification: a particular group sees itself as "the people." In declaring this identity, the group rejects the betrayal of its nation's values by dominant institutions, pledging to reclaim national traditions from the debasement they have suffered under influential elites. Pursuing the hoped-for

13 For a more detailed account of how this works, see Mössner and Kitcher 2017. I am deeply grateful to my coauthor, whose contributions to the pertinent pages greatly exceed my own.

14 Traditional media sources, such as television networks and newspapers, face a more difficult problem in this regard since they are supposed to be comprehensive in ways that internet sites are not. As I have discovered in several years of examining its strategies, Fox News is responding ingeniously to the problem. The first line of defense against unpleasant news is to marginalize the story: lead with lots of other reports, often of a "heartwarming" variety, and give the discomfiting information a lowly place in the broadcast. A second approach is to pair the news with something to counterbalance it: the fall from grace of a favorite is accompanied by reminders of the far more egregious "crimes" of someone the audience can be relied on to detest. If all else fails, offer a short, relatively straight account, using dull prose and carefully chosen distancing ("it's reported that" or "some say"), in contrast to the lively style used to celebrate the heroes and condemn the villains. Internet sites sometimes use the same strategies, but they also enjoy the ability to be as selective as they please. Say nothing, if you can't say something nice (as Pam Ayres puts the maxim in a short poetic appraisal of a royal wedding).

15 Whether these movements (for example, those associated with Trump in the United States, the UK Independence Party, Marine Le Pen in France, or the Alternative for Germany) are properly seen as versions of populism is debated. My account in the text follows the careful analysis of Jan-Werner Müller (2016).

restoration takes precedence over other political projects. If so-called experts warn of future harms, calling for preventive action, their counsel is dismissed. The corruption of supposed authorities must be overridden by the wisdom of the folk.

To see how movements of this kind deepen the forms of ignorance reviewed in earlier sections—or rather, go beyond ignorance to an even more corrosive cognitive deficiency—is important. But it should be preceded by a clear recognition of the legitimate grievances of those who identify themselves as "the people." Populist movements have taken hold among people who *correctly* view themselves as losers in local and global competitions. Their jobs are frequently precarious, their wages are stagnant, and the education available to their children is mediocre at best; their futures and the futures of those they care most about are dismal. The nations in which they live may have been governed by politicians of different parties, but the successive governments have been indifferent to their needs and interests. Inequality within their society continues to grow, and they are left behind.

Those attracted to contemporary populist movements would often be entirely justified in seeing their predicament in the way just described. With the breakdown of public debate and fragmentation of the citizenry, however, self-awareness becomes accompanied by other convictions, understandably adopted but not always warranted. The internet sites and media sources that they are inclined to trust reinforce a general view. The influential people and dominant institutions of the society ignore them, the true people, and treat their values with contempt. As this worldview sees it, politicians extend sympathy to others who are far less deserving, such as the lazy, those who flout the law, and newcomers who lack the long history of devotion to the country and its traditions that the people—the *true* people—have always shown.

Once attitudes of this kind are firmly entrenched, a particular issue comes to take priority for those who adopt them. Provided they can find a leader (or party) who reaches out to them, who claims to understand their concerns and predicament, the first order of business is to support their champion in what is inevitably a difficult campaign against all the forces and institutions—the elites—responsible for the nation's decline. The leader's peccadilloes, contradictions, evasions, and even apparent pursuit of policies contrary to the interest of the true people are to be explained away on various grounds. Some of the accusations leveled against the leader should be dismissed as false, spurious fabrications or malign distortions crafted by unreliable journalists—people dedicated to maintaining the power of the corrupt elites. Even when unfriendly characterizations of the leader's behavior are accurate, they fail to appreciate the difficulties of the campaign being waged. To reclaim the nation after years (or decades?) of corruption, mismanagement, and betrayal of the people is no easy matter. Fixing the mess will take time, and it frequently requires actions that are not pretty. The people should not be diverted. Understanding the great goal of restoring what has been lost, they should remain loyal to the leader as they grapple with a cunning, implacable, and shameless opposition.

In this great struggle, the function of political language shifts. Truth and evidence become irrelevant. Although it has become commonplace to castigate politicians for lying, a better diagnosis of the current proliferation of falsehoods would be to recognize the rise of bullshit (in the precise sense offered by Harry Frankfurt [2005]). Bullshitters are simply not interested in whether the sentences they utter are true or false, based on evidence or free-floating speculations. Words are tools to be used in political battles. Announcements and speeches are tailored to reassure and excite the people that the leader claims to represent—the true people, the beating heart of the nation. What that audience (and maybe others) hear in the bullshitter's words is an *attitude* toward the dire state of the country they love, a fierce determination to make it once more the place of their nostalgic dreams, a place in which they, the people, once again have a place. The literal sense of what is said is irrelevant. So, too, is the truth value.

Under these three conditions—the omnipresence of the internet, attractions of populism, and tolerance of bullshit—chances of orderly debate about a complex issue (like climate change) tend toward zero. Whether the reality of anthropogenic global warming is disputed or conceded hardly matters. A significant fraction of the citizens, not only in the United States, but in many affluent democracies, identifies calls for climate action as manipulative efforts aimed at producing outcomes that their political opponents (the elites who have already wrought so much damage to the nation) have always wanted. Even when signs of climatic shifts cannot be dismissed—when wildfires break out on unprecedented scales, or temperate regions experience extraordinary heat waves or droughts—the remedies proposed by climate activists are viewed with deep suspicion. Once the leader is firmly established, with the power to solve the nation's problems, it will be possible to cope properly with whatever difficulties arise from a modified climate. They and their wise advisers will know what to do.

Particularly irksome is the call for international cooperation as part of a plan to respond to climate change. Globalization and global entanglement have contributed to national decline. Suggestions that past polluters might be morally required to pay reparations to developing nations or that fighting climate change needs the extension of democratic institutions across national boundaries are particularly repugnant (for a defense of both proposals, see Kitcher and Keller 2017, 135–209). Populism encourages fragmentation. Nations withdraw from larger unions (witness Brexit). Groups within nations seek partition and the restoration of individual cultures. At a time when new connections and new agreements on an international scale are urgently needed, the political trends are clearly in the contrary direction.

Perhaps there will be a technological fix, such as a vacuum cleaner to suck up as much carbon as we choose to remove from the atmosphere. Perhaps the political trends of the past decade will be a passing fad, abandoned in favor of a healthier form of democracy. The new political context would still have to address the difficulties outlined earlier, overcoming the forces that blocked climate action between 1990 and 2008. More likely, the call to do something will come only after a series of unfortunate events has revealed the need to tackle the issue. Some combination of uncontrollable wildfires, extended droughts, heat wave–related deaths, famines, pandemics, floods, massive migrations, and water wars will shock the world into action. By then it will almost certainly be too late.

7. Hope?

So is there any hope?

If the schools of the affluent democracies were better suited to educate citizens for addressing complex challenges in an interconnected world, perhaps in a generation or two there would be militant majorities clamoring for climate action. Even the most optimistic educational reformers have to recognize the slender chances of achieving what is needed within a half century (at best), and that would be too slow to rescue future generations from harsh or even unbearable lives. Trying to rebuild a functioning Millian arena by addressing the ways in which public debate has been distorted looks even less promising. Once trust in media sources has been lost, it is hard to see how it can be restored. Announcing the arrival of the new, impeccably fair-minded media outlet would be greeted with derisive laughter—except, of course, by those already aligned with its source of funding.

The idea that the cure for the ills of democracy is more democracy is often attributed to Dewey.[16] A better formulation of Dewey's thought would propose *deeper* democracy as the cure for democracy's troubles. Superficial democracy treats any society as democratic if it allows for elections with choice of candidates and (maybe) some prior discussion. *Millian* democracy adds the proviso that issues must be properly debated in an ideal arena (populated by those well-mannered Victorian gentlemen). *Deweyan* democracy conceives of a democratic society as one in which the citizens are constantly engaged in mutual learning. Any Millian arena would be supplemented by countless smaller spaces, in which people with different perspectives exchanged ideas with sincere interest in truth and finding ways in which they could live together.

Contemporary democracies could make some headway toward sustaining our planet if they were to take a Deweyan turn. Imagine that citizens who disagree, but who know and respect one another, would come together in any or all the places in which they interact—around the dinner table, in the workplace cafeteria, in community meetings to discuss the schools, in their places of worship, and after the sports events in which they participate or attend together. With a commitment to studying the issues and learning from one another, they read and talk. Out of their genuine dialogue, a grassroots movement emerges—one breaking down political barriers, and enabling those who prosper and those who feel left behind to see and understand one another. Joining in a cause, they exert enough political pressure to overcome climate neglect.[17]

16 The attribution seems to be based on a misreading of chapter 5 in Dewey's (1982) *The Public and Its Problems*, in which he cites the "old claim" and immediately qualifies it.

17 I present this idea with a coauthor (Kitcher and Keller 2017) as a possible way forward. We offer dialogues that attempt to show how these discussions might profitably proceed. Our book was written before November 2016, and in retrospect appears (at least to me) not to engage sufficiently with the complaints of those citizens within affluent democracies who feel left behind by their governments and at odds with the revisions of traditional values. We intended to speak to this predicament in our book, but we underrated the depth of the problem.

Is it plausible to suppose this to be more than optimistic fantasy? Hardly. In light of the prevailing political conditions, we *might* make a *little* headway—*perhaps*. Even if efforts along these lines were to begin now, they would almost certainly fall short of what is needed. The best we could expect from them is a less terrible future than that to which our descendants would otherwise be condemned. Apparently, then, there is little hope.

Yet that judgment cannot—and *should not* or *must not*—engender quietism. The consequences for our grandchildren, and those who come after them, are too severe to justify sitting on our hands and lamenting what has been left undone. Even if there is little chance of success for any of the readily conceivable remedies, we are morally required to seek out new ones, or at least to do our best to promote one of the options envisaged, against all odds. Artists, writers of fiction, moviemakers, and academics should continue to attempt to raise climate consciousness by offering their visions of the world to come.[18] Their warnings may do something to make the lives of our descendants (slightly) more bearable.

It is entirely apt for many of these fictional scenarios to be apocalyptic reports from a world in which massive suffering and death have occurred, and in which human survival is precarious.[19] Part of the story has to be that some members of our species have survived. For they must be there, recognizable as sharing our capacity for suffering, to report back on the terrible things that have occurred and reproach us for our failure to act. Their imagined accusations make the moral point.

But stories in which human beings survive fail to make what is at stake completely clear. If we cannot come together—as local communities, individual nations, and a species—and act, decisively and cooperatively, then barring a technological breakthrough, the apocalyptic visions in which the people of the future condemn us for our myopic, selfish inaction are by no means the worst. Most likely the future will offer a world so bereft of

18 For academic efforts to play Cassandra, see, for example, Oreskes and Conway 2014; Jamieson and Nadzam 2015; Kitcher and Keller 2017.

19 In the prologue to *The Seasons Alter* (Kitcher and Keller 2017), my coauthor and I present an apocalyptic vision in the guise of a "Climate Day Address" from 2159. We supplement that in the epilogue with a much less dramatic vision of the everyday discomforts of life in 2059—on "the banality of suffering" (with thanks to Hannah Arendt).

resources, so buffeted by a different climate, that no voice within it could rise to mourn and accuse.

The rest is silence.

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For Virtuous Ends

5 Might Scientific Ignorance Be Virtuous? The Case of Cognitive Differences Research

Janet Kourany

1. The Right to Freedom of Scientific Research

Scientific freedom has been called "the elixir of civilization" (Braben 2008), and scientists' right to such freedom is recognized and protected worldwide.[1] The United Nations (1966), for example, has directed states "to respect the freedom indispensable for scientific research and creative activity." The European Union has acted similarly. According to Article 13 of its Charter of Fundamental Rights, "The arts and scientific research shall be free of constraint. Academic freedom shall be respected" (European Union 2000, 11). Other international documents also contain similar directives, such as the Declaration of the World Congress for Freedom of Scientific Research, which states that the freedom of scientific research "is a basic civil and political right"—indeed, "is a dimension of freedom of thought and freedom of speech" that is a requirement of democracy (Institute for Ethics and Emerging Technologies 2006). The constitutions of many nations (such as Germany, Italy, Greece, and Spain) contain similar directives, while other nations (such as the United States and Canada) protect the freedom of scientific research in other ways (for instance, via the First Amendment of the US Constitution and Article 2 of the Canadian Charter of Rights and Freedoms) (Santosuosso, Fabio, and Sellaroli 2007; Santosuosso 2012). As a result, encroachments on scientific freedom are met with alarm; think not

1 This chapter is a further development of Janet A. Kourany, "Should Some Knowledge Be Forbidden? The Case of Cognitive Differences Research," *Philosophy of Science* 83, no. 5 (December 2016): 779–790. As before, I wish to thank audiences at the University of Western Ontario, Saint Louis University, Universität Bielefeld, the University of Chicago, Lewis and Clark College, and the University of Edinburgh for interesting, informative, and lively exchanges about this material.

only of the recent protests responding to governmental restrictions on scientists' freedom—the Death of Evidence demonstration in Canada in 2012, and March for Science demonstrations in the United States and elsewhere in 2017—but also the extensive, sometimes passionate literatures responding to the "commercialization" and "militarization" as well as "politicization" of science. "In a world where science policy is increasingly influenced by politics, economics, and religion," said University of Alberta law professor Timothy Caulfield (2004, 125), "the concept of scientific freedom has never been more important." But how much freedom do scientists really need—or deserve?

The very question seems an anathema; interference with the conduct of scientific research violates scientists' rights and threatens the scientific enterprise. And yet all the covenants and charters and constitutions that recognize the right to freedom of research at the same time recognize, whether explicitly or implicitly, other important rights that can conflict with the right to freedom of research. For example, the Charter of Fundamental Rights of the European Union also recognizes the right to "human dignity" in Article 1 ("Human dignity is inviolable. It must be respected and protected") and the right to "the integrity of the person" in Article 3 (which includes "the prohibition of eugenic practices, in particular those aiming at the selection of persons") (European Union 2000, 9). The charter also recognizes the right to "equality between men and women" in Article 23 ("Equality between men and women must be ensured in all areas, including employment, work and pay") (13) and it prohibits sex-based and other kinds of discrimination in Article 21. Furthermore, the charter recognizes the right to "environmental protection" in Article 37: "A high level of environmental protection and the improvement of the quality of the environment must be integrated into the policies of the Union and ensured in accordance with the principle of sustainable development" (17). And so on. Asking how much freedom scientists really need or deserve thus involves asking how possible conflicts between the right to freedom of research and these other rights is to be resolved. Only by resolving such conflicts do we gain a clear understanding of the extent and depth of scientists' right to freedom of research. Of course, the answer in some areas is already largely settled. The right to human dignity along with the integrity of the person and the right to environmental protection are already recognized to be constraints on scientists' right to freedom of research—constraints as politically legitimate as the national and international declarations that recognize the right to freedom of scientific research in the first place. Scientists are no more free in the way they conduct their research to compromise the safety or dignity of research participants than they are to endanger the environment. But what of other rights and their possible conflict with the right to freedom of research?

2. A Case Study: The Right to Equality versus the Right to Freedom of Research

Consider the right to equality between men and women, and between people of different racial and ethnic groups. It has been thought that certain kinds of scientific research threaten the enforcement of this right—for example, research looking for gender- or race-linked differences in intelligence, particularly biologically based differences in intelligence. After all, in the words of well-known UK neuroscientist Steven Rose (2009, 788), “Claims that there are differences in intelligence between blacks and whites, or men and women, have always been used to justify a social hierarchy in which white males continue to occupy the premier positions (whether in the economy in general or natural science in particular).” As a result it has been debated for years whether scientists should pursue this kind of cognitive differences research, and since many have come away from the debate concluding that scientists should *not*, the funding and other supports for the research have sometimes been in short supply. As Cornell University developmental psychologists Stephen Ceci and Wendy Williams (2009, 789), distinguished contributors to this cognitive differences research but critics of biologically based cognitive differences, point out, “We think racial and gender differences in IQ are not innate but instead reflect environmental challenges. . . . [But] whereas our ‘politically correct’ work garners us praise, speaking invitations and book contracts, challengers are demeaned, ostracized and occasionally threatened with tenure revocation.” And they go on to remind us of such significant goings-on as the forced resignation of former Harvard University president Lawrence Summers in 2006 after his speech supporting biologically based gender differences in intelligence and the suspension of Nobel laureate James Watson from the chancellorship of Cold Spring Harbor Laboratory in 2007 after his remarks in a newspaper interview supporting biologically based race differences in intelligence.

Others, such as science writer Morton Hunt, recount the experiences of Arthur Jensen, H. J. Eysenck, J. Philippe Rushton, Charles Murray, Julian Stanley, Camilla Benbow, and other famous race and gender theorists—how, for instance, Jensen, a longtime proponent of genetic race-linked differences in intelligence, had great difficulty finding a publisher for his major work *The g Factor* even though he had previously published with such distinguished presses as Methuen, the Free Press, Columbia University Press, and the US Office of Education; how major professional organizations condemned his views or called for his expulsion from the American Psychological Association; how other academics argued in journal articles that research like his should not be pursued; and so on. "Far less virulent and widespread criticism than this by fellow academics has often been enough to choke off a researcher's interest in a troublemaking topic and cause him or her to turn to safer research subjects" (Hunt 1999, 75).

Within the area of cognitive differences research, then, the right to gender and racial equality has seemed to many to conflict with the right to freedom of research, and the right to gender and racial equality has seemed to them to take precedence—to legitimately *constrain* scientists' right to freedom of research.

A closer look at the situation, however, yields problems with this analysis. After all, the "virulent and widespread" academic criticism referred to above, directed at scientists such as Jensen, has tended to be legitimate *scientific* criticism, though scientific criticism frequently motivated by egalitarian political values and laden with more than the usual amount of acrimony. The criticism has dealt, for example, with the way the offending cognitive differences research has tried to make biological sense of the social category of "race" despite the genetic diversity within any socially defined (black or white or Asian, and so on) racial group (there are, for example, hundreds of different genetic subpopulations among "blacks" in Africa alone, encompassing much of the global genetic diversity of the human population at large). The criticism has dealt with the way the offending cognitive differences research has tried to capture with a single, culture-free "IQ" number and IQ ranking system the diverse forms of intelligent behavior of persons in diverse circumstances, or with the way the research has simply ignored all this diversity. And the criticism has dealt with the way the offending cognitive differences research has tried to identify innate biological links to any differences in the way women and men think and act, despite the different treatments men and women receive from the moment they are born, and the pervasive ways these different treatments end up shaping women's and men's biologies (see, for example, Rose 2009).

What's more, the continuing dialogue between the scientists producing the offending cognitive differences research and their critics has generated important new areas of research and important new insights concerning race, gender, and intelligence—such as the discovery of the ongoing upward rise in IQ scores from one generation to the next, known as the *Flynn effect*, showing that IQ scores are not simply a function of biology, and the discovery of the dramatically decreasing gender gap in test scores in math- and science-related achievement tests over the past several decades, showing that these scores also do not simply reflect biology. And most of this new research and new knowledge has *supported* rather than threatened gender and racial equality (Ceci and Williams 2009).

Within the area of cognitive differences research, then, the conclusion has seemed to many to be that the right to freedom of research has not only *not* conflicted with the right to gender and racial equality but has actually *supported* that right.

But there are problems, also, with this analysis. For notice that all this new research and all this new knowledge have been a response—positive or negative—to the claims of intellectual inferiority leveled against such groups as blacks and women, made within the context of a still sexist and racist society. And notice, also, that these claims are as old as science itself and they never seem to cease.

For centuries, for example, scientists have claimed that women are intellectually inferior to men, and for centuries the basis for such inferiority has been located in biology. In the seventeenth century, women's brains were claimed to be too "cold" and "soft" to sustain rigorous thought. In the late eighteenth century, the female cranial cavity was claimed to be too small to hold a powerful brain. In the late nineteenth century, the exercise of women's brains was claimed to be damaging to women's reproductive health—was claimed, in fact, to shrivel women's ovaries. In the twentieth century, the lesser "lateralization" (hemispheric specialization) of women's brains compared to men's was claimed to make women inferior in visuospatial skills, including mathematical skills (Schiebinger 1989; Fausto-Sterling 1992, 2000).

And now, in the beginning of the twenty-first century, the claims continue—that women's brains are smaller than men's brains, even correcting for differences of body mass; that women's brains have less white matter; that women's brains have less focused cortical activity (lower "neural efficiency"); that women's brains have lower cortical processing speed (lower conduction velocity in their white matter's axons); and so on. And once again these differences are being linked to differences in intellectual capacity—that people with smaller brains have lower IQ test scores; that less focused cortical activity is associated with lower intellectual performance; that lower cortical processing speed is associated with lower working memory performance, which is correlated with lower "fluid intelligence" scores; and so on (for a quite up-to-date account, see Hamilton 2008). At the same time, much attention now focuses on the mappings of brain activity produced by brain imaging, particularly functional magnetic resonance imaging, and the differences in "emotional intelligence" these disclose. But once again the "male brain," the "systemizer" brain, comes out on top—as the more scientific brain, the more innovative brain, the more leadership-oriented brain, the more potentially "elite" brain than the "female brain," the "empathizer" brain (Karafyllis and Ulshofer 2008).

Of course, the above is just a peek at the history of scientific claims about women's intellectual inferiority. The claims include not only these about the structure and functioning of women's brains but also claims about women's hormones, and women's psychological propensities, and women's genetic endowment, and women's evolutionary past—how all these are connected to intellectual inferiority—and the claims go back in history at least to Aristotle and his observation that women are literally misbegotten men, barely rational at all. And though the claims of intellectual inferiority continue to be contested and corrected, they also continue to be made, and the endless succession of claims and counterclaims both feeds on and helps to sustain the stereotype of intellectual inferiority associated with women.

Meanwhile, the effects are profound. For example, studies have documented the harm done to women and girls by the publication of scientific claims suggesting an innate female deficit in mathematics.[2] Reports one of

2 See, for instance, the now classic studies reported in Steele 1997; Spencer, Steele, and Quinn 1999; and Dar-Nimrod and Heine 2006; as well as the comprehensive literature review and assessment in Spencer, Logel, and Davies 2016.

the researchers involved in these studies, social/personality psychologist Steven Heine of the University of British Columbia, "As our research demonstrates, just hearing about that sort of idea"—that female underachievement in mathematics is due to genetic factors rather than social factors—"is enough to negatively affect women's performance, and reproduce the stereotype that is out there" (quoted in Ceci and Williams 2010, 221). But that harm has been recognized for years. Virginia Woolf (1929, 45; quoted in Spencer, Steele, and Quinn 1999, 5) described it almost a century ago in her essay *A Room of One's Own*:

> There was an enormous body of masculine opinion to the effect that nothing could be expected of women intellectually. Even if her father did not read out loud these opinions, any girl could read them for herself; and the reading, even in the nineteenth century, must have lowered her vitality, and told profoundly upon her work. There would always have been that assertion—you cannot do this, you are incapable of doing that—to protest against, to overcome.

Of course, much the same can be said of race-differences research and the harm that it has caused.[3]

Should the conclusion then be that in the case of cognitive differences research, the right to freedom of research *does* conflict with the right to gender and racial equality, and hence ought to be constrained by that right? Certainly many scientists have thought so. In a two-month-long debate in the journal *Nature*, University of North Carolina anthropologist Jonathan Marks (2009, 145) spoke for many when he said, "Decisions about what kinds of scholarly research questions and methods are considered worthy of attention and funding are fundamental to modern science. Stupid science and evil science . . . should not be permitted to coexist casually alongside the normative intellectual activities we admire. . . . Any science . . . that takes all work to be of equal stature, necessarily calls into question its own standing as a scholarly enterprise." In other words, restricting scientists from pursuing "evil" questions, questions "unworthy" of science—such as those that act to undermine the right to equality—is fundamental to science.

Others in the debate, however, took the opposite stand. Thus Ceci and Williams (2009, 789) proclaimed, "When scientists are silenced by colleagues, administrators, editors and funders who think that simply asking

3 See, for example, the now classic study reported in Steele and Aronson 1995; Steele 1997; and, again, Spencer, Logel, and Davies 2016.

certain questions is inappropriate, the process begins to resemble religion rather than science. Under such a regime, we risk losing a generation of desperately needed research." New Zealand University of Otago psychologist James Flynn, of Flynn effect fame, agreed: "As the philosopher John Stuart Mill points out, when you assert that a topic is not to be debated, you are foreclosing not some narrow statement of opinion on that topic, but the whole spiraling universe of discourse that it may inspire." And he added that "I invite everyone to search the social-science literature of the past 34 years and ask whether or not they really wish that everything on the subject [of biologically based cognitive group differences], pro or con, was missing" (Flynn 2009, 146). In other words, restricting scientists from pursuing "evil" questions, questions "unworthy" of science—such as those that act to undermine the right to equality—is antithetical to science and thwarts scientific progress.

Clearly the conflict between the right to freedom of research and the right to equality remains unresolved. Well, how might it be resolved? A plausible strategy, I think, would be to consider past precedents—cases in which the conflict between the right to freedom of research and other rights *has* been effectively resolved. Then we can try to model the resolution we seek on these other cases.

3. Past Precedents

Consider, then, three precedents that involved the right to freedom of research. The first two are relatively familiar, so discussion of them will be brief. Until the US National Institutes of Health Revitalization Act was passed in 1993, women and minority men tended to be neglected in US biomedical research—left out of clinical drug trials, left out of the definitions of diseases, and left out of research agendas, their health needs largely ignored (see, for example, Rosser 1994; Schiebinger 1999). What the Revitalization Act did was mandate the equal inclusion with white men of women and minority men in publicly funded US biomedical research, and make funding contingent on that inclusion. This surely formed a constraint on scientists' freedom to design their own research programs, and was justified by women's and minority men's right to equality—in this case, their right to equality of access to health care. Similarly, the earlier US National Research Act of 1974 mandated the formation of institutional review boards to oversee all research that receives funding from what is now the Department of Health and Human Services. These review boards, themselves regulated by the Office for Human Research Protections of the Department of Health and Human Services, can reject, modify, or suspend any medical or behavioral research that fails to protect the rights and welfare (the right to life, the right to autonomy, the right to human dignity, and so on) of its human subjects (National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research 1979). So here again, scientists' right to freedom of research has been constrained by other rights. Can a new constraint to protect human rights, this time in the area of cognitive differences research, be modeled on these two cases?

Certainly there are important similarities among the cases. They all rule out harmful kinds of scientific inquiry as unacceptable, and, thereby, they all shape the kinds of knowledge that scientific inquiry can deliver, but they do not shape that knowledge in any other way. So none can be said to censor particular kinds of knowledge. The National Research Act rules out as unacceptable harmful procedures for obtaining knowledge, not particular kinds of knowledge. If other ways are found to obtain the knowledge without harming research participants, that is perfectly acceptable, regardless of the content of the knowledge in question. The Revitalization Act rules out harmful—exclusionary and neglectful—kinds of research agendas as unacceptable, though it says nothing about the content of the knowledge that is gathered with those agendas. If it turns out that one or another group is, say, more prone to disease, that is also acceptable, though of course unfortunate. In a similar way, a constraint on cognitive differences research would rule out as unacceptable the kinds of harmful gender and racial group comparison questions that are included in that research, and thereby the answers to those questions that continue to be circulated, though it would do this regardless of the content of the answers—that is, regardless of whether they bespeak the equality or inequality of the groups in question.

Of course, ruling such questions out is controversial. Marks, remember, maintained that science should rule such questions out because they are "evil," whereas Ceci and Williams maintained that science should rule no questions out, "evil" or not. But if such questions *are* "evil"—that is, genuinely harmful to women and minority men—then surely they should be ruled out, in just the way in which the two other harmful kinds of scientific inquiry described above have been ruled out. And if so, a resolution of the conflict between the right to freedom of research and the right to equality in the area of cognitive differences research is at hand, one modeled on our first two precedents.

At this point you will protest. There is nothing intrinsically evil or harmful, you will say, about seeking to determine who is better at something, or about seeking to determine whether some are as good at something as others, or else everything from spelling bees and team sports to college entrance examinations and the Nobel Prize would be ruled out of bounds. What is problematic about cognitive differences research is not the questions asked or even the results sometimes obtained but instead the prejudiced—the racist and sexist—context in which the research takes place. It is this context that makes the research disempowering. For example, whites do not typically feel demeaned or disempowered when it is reported that Asians have higher IQ scores than they do. The result, in fact, seems of little consequence; IQ isn't everything. But when it is reported that whites have higher IQ scores than blacks, blacks *do* feel demeaned and disempowered. The difference, of course, is the racism that oppresses blacks and privileges whites. To be sure, finding out that blacks have lower IQ scores than whites, or that women are analytically weaker than men, could be the beginning of educational and training programs to work with the strengths and work on the weaknesses of every group to help make them the very best they can be, and even to use the special talents of each group to help the others. Finding these things out could be the start of innovative programs that support rather than undermine the right to equality. That this does not happen, or seldom happens, is a function of the sexism and racism of society, not the knowledge uncovered by cognitive differences research. In short, there is nothing wrong with cognitive group differences research taken by itself. It is our society that is wrong, not the research. So why should we limit the freedom of scientists and the potentially interesting and important insights they might offer because of the shortcomings of the society in which they do their research?

This is what I think you will say. And although the argument is hardly compelling—cognitive differences research, after all, is far less benign than spelling bees, team sports, and the Nobel Prize, with their emphases on motivating hard work and rewarding achievement—still the societal context of such research is surely significant. Turn, then, to the third of my three precedents—a fairly new case involving the conflict between the right to freedom of research and the right to personal security (that is, the right not to be killed and not to be injured or abused, which in the US Constitution is called the right to life). This time the case comes from synthetic genomics investigations within biomedical research.

4. The Third Precedent

In 2001, Australian researchers inadvertently produced a superstrain of mousepox—one that kills mice regardless of whether they have been vaccinated against mousepox or are naturally resistant to it. What the researchers had hoped to achieve when they inserted the mouse IL-4 gene into the mousepox virus was an altered virus that would provide a means of pest control by sterilizing mice. What the researchers *did* achieve was not only the superstrain of mousepox but possibly also a technique for producing a superstrain of smallpox. In the wrong hands, this information could be deadly. Since there is no known treatment for smallpox—since vaccination is our only defense—having a vaccine-resistant strain of smallpox on the loose would produce havoc.

As if this weren't enough, in a second study in 2002, researchers at the State University of New York at Stony Brook stitched together strands of DNA that they had purchased via mail order, following the map of the polio virus RNA genome available on the internet. Their technique resulted in the artificial synthesis of a "live" polio virus—one that paralyzes and kills mice. In this case, however, the researchers produced their deadly virus deliberately, "from scratch," using materials and information readily available to anyone. Indeed, in an interview in the *New York Times* after the publication of their paper, the researchers said that they "made the virus to send a warning that terrorists might be able to make biological weapons without obtaining a natural virus" (Pollack 2002).

In still further studies completed in 2005, researchers from the US Centers for Disease Control and Prevention, the Armed Forces Institute of Pathology, Mount Sinai School of Medicine, and the US Department of Agriculture pieced together viral fragments from hospital specimens and from the remains of a human victim dug up in Alaska to sequence the complete genome of the deadly Spanish flu virus, the virus that had killed an estimated forty to fifty million people worldwide in winter 1918–1919. They then went ahead and re-created the virus, which by then no longer existed anywhere on earth. The re-created virus, the researchers found, was just as lethal as historical records had led them to expect: all the mice that were infected died within days, and Canadian, US, and Japanese researchers found the same outcome with monkeys in 2007 (see Kaiser 2005; Smith 2007; Parliamentary Office of Science and Technology 2009; Selgelid 2009).

Note that all these groundbreaking studies were carried out by top researchers and published in top journals—the first in the *Journal of Virology* (Jackson et al. 2001), and the others in *Science* (Cello, Paul, and Wimmer 2002; Tumpey et al. 2005) and *Nature* (Taubenberger et al. 2005; Kobasa et al. 2007). And all these studies (as well as others that might have employed similar techniques to synthesize smallpox or Ebola) were said to promise unprecedented insights into some of the most virulent diseases ever known and the kinds of drugs and vaccines that might be developed to combat them.

Nonetheless, many have argued that these studies should not have been done at all, or at least should not have been published. Regarding the 1918 Spanish flu studies, for example, one biosecurity expert interviewed for a "Special Report" in *Nature* (von Bubnoff 2005) warned that "the risk that the recreated strain might escape is so high, it is almost a certainty" (794). Added another expert, "there is a long history of things escaping" (795). And still another said that "Tumpey *et al.* [the researchers who carried out the Spanish flu studies] have constructed, and provided procedures for others to construct, a virus that represents perhaps the most effective bioweapons agent now known" (795). For its part, the US Sunshine Project (2005), a biological weapons watchdog group, charged that the experiments "will be replicated and adapted, and the ability to perform them will proliferate, meaning that the possibility of man-made disaster, either accidental or deliberate, has risen for the entire world." And an op-ed column in the *New York Times* by two other scientists (Ray Kurzweil and Bill Joy) warned that publishing the full genome of the 1918 influenza virus was like publishing the precise design for an atomic bomb. Indeed, it was much worse, since creating and releasing the virus from the published genetic data would be easier than building and detonating an atomic bomb from only its design (given that rare materials like plutonium or enriched uranium would not be needed), and releasing the virus would kill many more people than detonating an atomic bomb (Kurzweil and Joy 2005; cf. National Research Council 2004, 23). According to these experts, in short, the present "age of terrorism" in which the studies have been conducted as well as the ever-present possibility of human error have turned what the Centers for Disease Control and Prevention (2014) called important health research into a very bad idea.

So the context of research can shape the value of research—in biomedical research, given its context of terrorism, no less than in psychological research, given its context of sexism and racism. The controversial biomedical research described above, however, unlike the controversial psychological research described previously, has produced new policy constraints on the freedom of research. In 2003, for example, a joint "statement on scientific publication and security" appeared in *Science*, *Nature*, the *Proceedings of the National Academy of Sciences*, and the journals of the American Society for Microbiology. According to this statement, editors should screen and, if necessary, request modification of or even refuse to publish manuscripts if the editors "conclude that the potential harm of publication outweighs the potential societal benefits" (Journal Editors and Authors Group 2003). The following year, the US National Research Council (2004) issued a comprehensive report, *Biotechnology Research in an Age of Terrorism*, commonly known as the Fink report. This report as well as others appearing around the same time (such as the UK Royal Society's *The Individual and Collective Roles Scientists Can Play in Strengthening International Treaties*) emphasized the need for systematic reviews of potentially harmful life sciences research *before* the initiation of the research and at regular intervals thereafter, way before publication is at issue. The Fink report went on to identify the kinds of study that require this especially careful oversight, and expanded the institutional framework already in place to carry out the oversight. It also stressed the need to develop a culture of responsibility within the life sciences to support and anticipate the oversight, to be accomplished by educational programs undertaken by all the professional societies in the life sciences, group strategies coordinated by the major societies, and the creation and promotion of codes of ethics detailing life scientists' social responsibilities. Finally, the Fink report recommended the establishment of a National Science Advisory Board for Biosecurity to provide the necessary advice, guidance, and resources for both the scientific community and the government regarding the system of review and oversight it proposed. This board was created in 2004, and included both leading scientists and biosecurity experts.

Is there any reason that new policy constraints like these for the life sciences should not be put into effect for the social sciences—new policy constraints that include research guidelines for weighing the societal harms of research against the societal benefits, educational programs and codes of ethics designed to foster a culture of responsibility among social scientists, and a new National Science Advisory Board for Social Research to provide advice, guidance, and resources for both the scientific community and the government? Such a board could count among its members both leading social scientists and representatives of the public at large, such as racial, ethnic, and gender group advocates. Is there any reason new policy constraints like these should not be put into effect for the social sciences?

Reflecting again on the two cases described above, one from the social sciences—the case of cognitive differences research—and one from the life sciences—the case of recent synthetic genomics research—two reasons might be offered to say that the cases should not be treated similarly. The first reason is that the cognitive differences research in question does not pose harms to society anywhere near the harms posed by the recent synthetic genomics research. Hence there is no need for similar constraints on scientific freedom in the two cases. The second reason is that such constraints might have very different effects in the two cases, limiting scientific progress in the one but not in the other. What can be said in response to these considerations?

To begin with, it is clear that the kinds of synthetic genomics research described previously may cause great harm to certain people—the people who work in the laboratories in which the research is carried out, the people who live near those laboratories in whose midst the pathogens may escape, the people who may end up targets of terrorists' use of the published results of the research, and so on. All these people, under the old system of standards for choosing, carrying out, and publishing research, might have gotten sick, even very sick, and even died as a result of the research, though the degree of likelihood for these various effects is hotly debated (see, for example, detailed critiques of such dangers in Parliamentary Office of Science and Technology 2009; and Centers for Disease Control and Prevention 2014). As a result, special new constraints on scientific freedom were thought necessary to minimize the risks.

So on the one hand, synthetic genomics research unconstrained may cause serious harm to various groups of people. On the other hand, cognitive differences research unconstrained, as previously stated, has already been shown to cause significant harm, though lesser harm than a serious illness, but for much longer periods than the length of an illness, to lots more people—all the people whose self-esteem and self-efficacy and ambitions and successes are lessened as a result of direct or indirect exposure to the research or aspects of the research (for example, its results or even just its questions), all the people whose self-esteem and self-efficacy and ambitions and successes are lessened as a result of the treatment they receive from others who have been directly or indirectly exposed to the research or aspects of the research, and so on: in short, all or most women and, in the United States at least, most minority men, as well as many men of color in other parts of the world. And these harms have gone on for centuries to this majority of society. Cognitive group differences research, then, arguably does pose harms to society near—perhaps even exceeding—the harms posed by the recent synthetic genomics research. If special constraints to scientific freedom are thought necessary in the one case, then why should they not be thought necessary in the other case too?

But decisions of this kind must take into account their expected effects on science as well as society. And in the case of health research, it is claimed that the new constraints on scientific freedom will not limit scientific progress. Indeed, this was one of the desiderata built into the new policy. As the Fink report made clear, "Any system of review and oversight must operate in ways that do not put the United States—and the world—at risk of losing the great potential benefits of biotechnology" (National Research Council 2004, 10). Some have gone further. They have claimed that even more stringent constraints than those now in place—constraints that would further restrict synthetic genomics research—will still not limit progress in health research, since the controversial genomics research was not really needed. For example, Jan van Aken (2006, S12), trained cell biologist, biological weapons expert, and former director of the Sunshine Project in Germany, commenting on the 1918 Spanish flu studies in particular, has pointed out that

> hundreds of influenza strains from the past five decades, including some pandemic strains, are available to researchers and are used by initiatives such as the influenza Genome Sequencing Project . . . to investigate genetic virulence factors. The added value of one extra strain, even one with an exceptionally high mortality rate, is limited, given that strains with varying degrees of contagiousness and

pathogenicity are already available and provide a wealth of research resources for comparative studies.

So we have been assured that the new policy constraints on scientific freedom imposed on the life sciences will not limit scientific progress. But we can be equally assured that new policy constraints on scientific freedom in the social sciences will not limit scientific progress either. Indeed, we can write that into the policy, as the Fink report did. What's more, we can also question, as advocates for tighter restrictions in the life sciences have done, exactly how important to the overall goals of the social sciences race- and gender-related cognitive differences research really is, and we can also question the reasonableness of those goals that may make it important. For example, the synthetic genomics research described above has aroused great concern in the United States because of its national security implications. But surely cognitive differences research has something akin to national security implications too. After all, people of color from various national origins will soon be the majority in the United States, and women already are. As a result, the competence and productivity, indeed the flourishing, of minority populations and women are becoming central to our collective well-being. A kind of research that undermines this competence and productivity therefore also undermines our security, and goals that support such research thereby become suspect.

Of course, none of this precludes losses—to society as well as science—when scientists' freedom is constrained. Flynn's remarks, quoted above, in the *Nature* debate on cognitive differences research make this depressingly clear. But Flynn never considers the gains that might have occurred along with the losses had cognitive group differences research not been pursued during the last thirty-four years—the research that might have done far more to dismantle race- and gender-related injustice than the research to which Flynn and others contributed. Nor does Flynn consider the whole spiraling universe of discourse and benefits that might now ensue if we put a halt to the subject of cognitive group differences research. When we look at some of the newer work on child development in minority populations in the United States, how the old genetic deficit models and cultural deficit models of minority child development are being replaced by models that foreground and seek to build upon minority competencies and strengths and resourcefulness in the context of racism and poverty; or when we look at some of the work in feminist science studies over the last thirty years, how the old doubts about whether women can make the same contributions to science as men have been replaced by studies showing the importantly different critical and constructive contributions to the sciences women have made—when we look at such work, we get a hint of what this new discourse and these new benefits can be.[4]

5. How Much Freedom Do Scientists Really Need or Deserve?

The three precedents considered above—the US National Research Act of 1974, the US National Institutes of Health Revitalization Act of 1993, and the Fink report and related policy directives of 2003–2004—point to a resolution of the conflict between the right to freedom of research and the right to equality—a resolution that favors the right to equality. At least this seems to be the result in the context of cognitive group differences research. But the same result should follow in other contexts in which the right to freedom of research conflicts with the right to equality—such as in the context of public health and biomedical research, where the health needs of the more affluent are privileged over the health needs of the poor (see, for example, Hotez 2008). In fact, taken together, the three precedents considered above suggest a quite general conclusion: that scientists' right to freedom of research cannot be allowed to subvert other people's rights, whether those other people are research subjects inside the research context or recipients of the effects of the research outside it. The case of cognitive group differences research, if enacted into a fourth precedent, will simply add further strength and urgency to this conclusion. It will also move science closer to the forefront of social change rather than remain holding up the rear.

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4 For some of the new models of minority child development, see, for example, the work of Cynthia García Coll—such as García Coll et al. 1996; García Coll and Magnuson 2000; Marks, Godoy, and García Coll 2014; Marks and García Coll 2018. For some of the work in feminist science studies, see Kourany 2010a, 2010b.

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6 Agnotology, Hermeneutical Injustice, and Scientific Pluralism: The Case of Asperger Syndrome

Miriam Solomon

Agnotology and hermeneutical injustice are among the most fruitful new ideas in social epistemology.[1] When the ideas were first presented, they came with examples that have become canonical: lost knowledge of abortifacients and climate change denial (for agnotology), and postpartum depression, sexual harassment, and sexual identity (for hermeneutical injustice). These examples have been useful for introducing the concepts of agnotology and hermeneutical injustice, but they oversimplify the epistemology. The purpose of this chapter is to explore a case—the diagnostic category of Asperger syndrome, embraced in the late 1980s and early 1990s, and then jettisoned in 2013—in which it is essential to acknowledge the more complex epistemic situation.[2]

1. The Canonical Examples

Londa Schiebinger (2008) examines the European reaction to seventeenth- and eighteenth-century botanical knowledge among indigenous Americans and African slaves in the Caribbean. While European invaders eagerly

1 There is some overlap between this chapter and my "On the Appearance and Disappearance of Asperger's Syndrome," in *Philosophical Issues in Psychiatry IV: Psychiatric Nosology DSM-5*, ed. Kenneth S. Kendler and Josef Parnas (Oxford: Oxford University Press, 2017), 176–186. That paper focuses on criteria for revising the DSM; this chapter looks at the epistemology of agnotology and hermeneutical injustice. I am grateful to Janet Kourany and Martin Carrier as well as anonymous referees for MIT Press for comments on an earlier version of this chapter.

2 Originally called Asperger's syndrome, the name has now been changed to Asperger syndrome to avoid misleading inferences that Hans Asperger either had or owned the syndrome.

absorbed and transmitted knowledge and cultivation practices for some New World plant medicines—such as quinine for malaria—they did not transmit knowledge of abortifacients, even when they transmitted the plants that were widely used as abortifacients. For instance, the peacock flower was discovered in the Caribbean and grown for its beauty in Europe, but knowledge of its use as an abortifacient did not accompany the horticultural practices eastward across the Atlantic. Schiebinger argues that ignorance of abortifacient properties among Europeans was not accidental. Rather, it reflected the irrelevance of practices for limiting population growth to Europeans at that time of conquest and territorial expansion as well as the fact that abortifacients are typically of less interest to men—and most Europeans traveling to the Caribbean at that time were men. While preventing births was an act of political resistance among African slaves in the Caribbean, and therefore they readily learned from indigenous American knowledge of abortifacients, Europeans had no such use for abortifacients. The result was that knowledge of abortifacients was not transmitted to Europe. This is a case in which ignorance is the result of passive processes rather than active suppression of knowledge.

Climate change denial is another well-known case illustrating the concept of agnotology. Naomi Oreskes and Eric Conway (2010) have investigated the tactics of climate change deniers. Climate change deniers aim to “manufacture doubt”: they exaggerate the degree of uncertainty in the science, using the same rhetorical techniques as the tobacco industry did to deny the health risks of tobacco. Their arguments are not taken seriously by scientists who have a detailed understanding of the evidence, but they do influence interested industrialists, politicians, and the general public. (The Intergovernmental Panel on Climate Change is an international effort among geoscientists to counter the climate change deniers with a rhetorically powerful tool of their own: the consensus of experts.) This is a case in which ignorance of climate change is actively and intentionally produced by those who stand to benefit from the ignorance.

Hermeneutical injustice, as named by Miranda Fricker (2007), is a specific kind of socially produced ignorance: it is lack of understanding of one’s experience and/or social identity because of the absence of appropriate categories of interpretation. Such categories of interpretation are absent because it is in the interests of a dominant social group to keep people from developing them.

The first case of hermeneutical injustice that Fricker discusses is that of postpartum depression. During the 1960s, women in consciousness-raising groups realized that their experiences with depression following childbirth were not individual failings but rather common phenomena with likely hormonal causes. This discovery provided a good deal of relief for the affected women, who had previously blamed themselves for depression or been at a loss to explain their experience of depression following childbirth. The phenomenon of postpartum depression was identified as early as the writings of Hippocrates, but not disseminated in the nineteenth and early to mid-twentieth century, probably for political purposes.[3]

Fricker next talks about Susan Brownmiller's case of Carmita Wood, who worked as an administrator for a professor who repeatedly gave her unwanted sexual attention. This was before the concept of sexual harassment was created to describe such behavior, and Wood found it difficult to make sense of her unpleasant experience and eventually left her job. But then Wood shared her experience in a feminist group, and as Brownmiller's narrative continues, "We realized that to a person, every one of us—the women on staff, Carmita, the students—had had an experience like this at some point. . . . And none of us had ever told anyone before. It was one of those *click, aha!* moments, a profound revelation" (Fricker 2007, 150).

It is in the interest of sexual harassers to keep those who they harass ignorant of the political dimensions of their situation. This is because sexual harassment is more difficult to protest when it is perceived personally instead of politically. It was an accomplishment of the second wave women's movement to name a set of common experiences "sexual harassment," and the concept has been useful for interpreting such experiences and taking steps to prevent them in the future.

A third case of hermeneutical injustice explored by Fricker is from Edmund White's autobiographical novel *A Boy's Own Story* (1982), and it focuses on White's lack of understanding of his own homosexuality as he was growing up due to the unfortunate stereotypes of homosexuality that were dominant in the 1950s' United States. As with the case of Wood, the

3 Spelling out the political situation—the subjugation of women in the first wave of feminist activism—would take some work. I think it can be done, but Fricker (2007) does not pursue it.

lack of understanding was both harmful and the result of social prejudices. White was harmed by the negative social stereotypes of male homosexuals because he thought that as a homosexual, he was supposed to conform to them, yet he felt that he did not. Additionally, he did not have—at that time—another more positive social identity available to him.

I have described these examples of agnotology and hermeneutical injustice in some detail because despite their differences—active versus passive ignorance, and general ignorance versus hermeneutical ignorance—they have something in common. They are all cases in which it is *easy to see* that the state of ignorance about a matter is epistemically inferior (and often also practically inferior) to the state of knowing. Indigenous American and African slave knowledge of abortifacients is implicitly admired, public ignorance of climate change is implicitly deprecated, and hermeneutical realizations are celebrated as cases of personal self-discovery. They are also all cases in which the state of knowing is *state of the art* in that it has not been superseded or even challenged by other views. Specifically, it is assumed that abortifacients work, climate change is undeniable, and the correct interpretation of personal experiences is in terms of concepts such as postpartum depression, sexual harassment, and sexual orientation. These features make the examples clear as well as the moral to draw from them: the state of knowing about something is epistemically (and frequently otherwise) better than the state of ignorance. The epistemology here is one of straightforward realism. In the next section I will describe a case that starts off sounding like one of Fricker's cases of hermeneutical injustice, but ends in a more ambiguous place because an epistemology of straightforward realism does not apply.

2. The Case of Asperger Syndrome

Hans Asperger was an Austrian pediatrician who in 1944, described a case series of four boys with what we now recognize as Asperger characteristics such as lack of empathy, little ability to form friendships, one-sided conversations, intense absorption in a special interest, and clumsy movements. He described them as having "autistic psychopathy," although to a much milder degree than some of Leo Kanner's classic cases of autism. (It is often said that he called them "little professors," but this term does not appear in his work.) Asperger advocated for these children, arguing that they deserve special education and a useful role in society.[4]

The British psychiatrist Lorna Wing revived the category in 1981, distinguishing it from Kanner's autism and calling it "Asperger's syndrome." Wing's work was well received. In 1991, Asperger's 1944 paper was translated and published by developmental psychologist Uta Frith (1991), thereby bringing it to the attention of the English-speaking world. By this time, diagnostic criteria for Asperger syndrome were beginning to appear. World Health Organization criteria were stated in 1993, and in 1994, the DSM-IV included criteria for Asperger syndrome. It was classified with other disorders in a group called pervasive developmental disorders" (PDDs) and distinguished primarily by the purported lack of language delay among those with Asperger syndrome rather than other PDDs such as classic autism.

Reception of the new Asperger diagnosis was also enthusiastic among people who identified with the diagnosis. Katherine Annear (2013) describes a typical reaction: "I grew up without a label and after having many thrown at me in my late teens, finding Asperger's was like finding a glove that fit."

The diagnostic category helped people to understand themselves or their affected family members for the first time. Here is a similar reaction from Michael John Carley: "Not only was my son being presented with an explanation, but I was finally presented with an explanation of what I'd endured my entire life. I don't have the words to describe the biblical weight being lifted off me" (Education.com 2014).

The Asperger identity (some refer to it as "Aspie," but the term is controversial) has become quite successful, even termed the "defining psychiatric malady" of the millennial generation (Wallace 2012).[5] There are questions about overdiagnosis as well as a debate about whether what is being diagnosed is a disability or merely a difference (these days conceptualized as "neurodiversity"), as Asperger himself suggested. Nevertheless, it is striking that the Asperger identity was embraced with as much enthusiasm and

4 It has recently come to light through the research of Herwig Czech that Asperger cooperated with the Nazis regarding more severely affected children and participated in sending them to their deaths. If this had been known in the early 1990s, Asperger syndrome might not have been thus named. See Donvan and Zucker 2016.

5 Thanks to Sam Fellowes for bringing the controversy about the term "Aspie" to my attention.

relief as the concepts of postpartum depression, sexual harassment, and sexual orientation described above. It seems that this was also a case of hermeneutical injustice, in which people with Asperger were previously deprived of an understanding of themselves by a neurotypical society and kept in harmful ignorance.

Despite this success, the DSM-5, published in May 2013, removes the Asperger syndrome diagnosis. In its place are autistic spectrum disorder (ASD) with degrees of severity and social (pragmatic) communication disorder (S[P]CD). ASD is a much broader category that includes nonverbal and low-functioning individuals, and S(P)CD is a newly invented category whose acceptability to the Asperger community is untested. On the face of it, it looks like the withdrawal of the Asperger diagnosis may be a hermeneutical injustice to people with Asperger syndrome—removing their claim to an identity that they have found validating and empowering. It even splits the Asperger community, putting some in the ASD category and others in the S(P)CD category, suggesting that there will be no future shared identity among those formerly diagnosed with Asperger syndrome. Yet I will argue that the situation is a good deal more complex than an unjust (probably unintentional, but still potentially harmful) hermeneutical step backward.

First, it is important to explore the reasons why the DSM-5 dropped the Asperger diagnosis. The psychiatric literature in the years prior to the publication of DSM-5 documents difficulties with the reliability of criteria for the diagnosis of Asperger syndrome. Specifically, criteria for distinguishing Asperger from other diagnoses such as high functioning autism and PDD–not otherwise specified (PDD-NOS) were either not given or resisted consistent application. Proposed special criteria, such as limiting the Asperger diagnosis to those with no language delays or those without cognitive impairment, did not in practice adequately demarcate those who were thought to have the disorder.

The DSM-5 proposes a classification for autistic disorders that is in some respects simpler than the one in the DSM-IV. Instead of assessments of three areas—language, social skills, and repetitive and restricted interests and behavior (RRIBs)—it considers language and social skills together as one area, and RRIBs as a second area. When language and social skills are combined, those formerly diagnosed with Asperger do less well on the combined measure and are less distinguishable from others with autistic disorders. The given justification for combining language and social skills is that the areas of functioning are not separate; the pragmatics of language especially requires social skills. Studies using both factor and cluster analyses show that language and social skills covary more closely than either do with RRIBs (Mandy, Charman, and Skuse 2012). Autistic spectrum disorder (ASD) is now a large and diverse category that includes those individuals formerly diagnosed with classic autism, PPDs (except for one type of case, S(P)CD, formerly classified under PDD-NOS, which I will discuss further below), and Asperger syndrome. The ASD diagnosis is given together with assessments of the degree of severity in the two areas of impaired functioning. Those formerly diagnosed with Asperger syndrome are generally less severely affected than those formerly diagnosed with autism or many cases of PDDs.

There is one type of case, formerly in the PDD-NOS category, that does not fit the new ASD criteria: cases in which individuals have communicative and social disorders but do not have RRIBs. Rather than making this type of case an apparently arbitrary exception to the general rule of requiring both social/communication delays and repetitive behaviors, it has been classified under a new category: S(P)CD. This keeps the criteria for these psychiatric disorders clear, categorical, and consistent. The cost, however, is to increase the number of psychiatric disorders and reclassify as nonautistic individuals who may have been well served by an autism diagnosis.

Since this new system of classification is only beginning to be implemented, we do not know how reliable or valid it will turn out to be. Although it is expected that it will remove some difficulties with the previous system, we do not yet know whether it introduces other difficulties—scientific, clinical, or public health. This is, of course, the case for *any* proposed changes in psychiatric classification (unless we revert to prior classifications for which we have data or there are field trials using the proposed classification). There is a study by Young Shin Kim and colleagues (2014) suggesting that the new system will work well, but this study may not generalize to typical clinical contexts.

The DSM-5 category of ASD (which most cases of Asperger syndrome fall under) is an extremely wide spectrum. One of the familiar sayings in the autism community is, “If you’ve met one person with autism, you’ve met one person with autism.” That is, there is a huge range of psychosocial experience and ability under the autism spectrum, often symbolized by a rainbow, the familiar symbol of diversity. Social identities frequently come from shared life experiences. While ASD may be a fine diagnostic category for many purposes of diagnosis, treatment, and research—although that remains to be seen—it is not promising as a social identity. In part because of the range and profundity of disability on some parts of the spectrum, there is no empowering group narrative, no shared hermeneutical understanding of areas of social experience. It is likely that ASD is too broad a category to come with a description that contributes to self-understanding and self-empowerment for those who have it.

It may be tempting to suggest here that this is a place where psychiatric diagnostic categories and social identities come apart, with psychiatry following "the science" and hermeneutics supplying "the significance." I resist this, because I think that hermeneutical insight is part of scientific thinking, and consider it possible that the category of "Asperger syndrome" or something like it will be useful in psychiatry in the future. Moreover, to the extent that psychiatry is committed to the empowerment of people with psychiatric disorders, it should perhaps welcome categories like Asperger syndrome that seem to foster such empowerment.

Certainly, some have experienced the disappearance of the Asperger diagnosis as a harm. For example, as John Elder Robison (2013) wrote,

> Just like that, Asperger's was gone. You can do things like that when you publish the rules. Like corrupt referees at a rigged college football game, the APA removed Asperger's from the field of play and banished the term to the locker room of psychiatric oblivion. Their new and improved DSM went on sale two months ago, and shrinks everywhere lined up to buy it. Meanwhile, my 2007 memoir about living with Asperger's is now deemed diagnostically obsolete.

My sympathy for such harm has been to some degree tempered by the knowledge that it is commonly part of the meaning of the Asperger identity that the person with Asperger syndrome is in a different—*and better*—diagnostic category than lower-functioning persons with autism. Parent and writer Lucy Berrington (2012) wrote that a benefit of the Asperger syndrome diagnosis was that it helped parents to accept their children's developmental delays:

> The Asperger's label has helped many who don't fit the classic autism model or stereotype to get a diagnosis and accept their autism. We don't need data to tell us that Asperger's is a less unnerving diagnosis than autism for those who haven't come to terms with their own or their children's special needs. . . . [T]he Asperger's/autism dichotomy penalizes and stigmatizes classic autism.

It was a less scary diagnosis, but at the expense of reinforcing the negative attitudes toward autism. I call this the dark side of the Asperger diagnosis. One blogger, Brigianna Spencer, puts this plainly: "Because Asperger's becomes stereotyped as the "good," "mild," "high-functioning" autism, that leaves classic autism with the stigma of "the bad kind of autism" simply by the existence of the contrast" (quoted in Berrington 2012).

Asperger syndrome diagnosis was correlated with socioeconomic status. Those lower on the socioeconomic scale were likelier to be diagnosed with PDD-NOS or high-functioning classic autism than those higher on the socioeconomic scale (Kaufmann, n.s.).

A brave blogger, Michael Scott Monje Jr. (2014), unequivocally states what he thinks is going on: "The term 'Aspie' has come to be a way for those of us who want to talk about our experiences to separate ourselves from 'those' Autistics. It allows us an identity that has been sanitized for our allistic audience—a way of performing eccentricity instead of disability, of showing we are 'like them' but 'still employable.' This is disgusting."

What is going on here is a phenomenon that Hilde Lindemann Nelson (2001) (now Hilde Lindmann) calls "hostage taking." The Asperger identity is made positive by putting down another oppressed group: those with classic autism.[6] (Nelson's example is antigay rhetoric in black rapper communities.) A positive identity, as Nelson characterizes it, is a narrative that fosters the humanity and frees the agency of the group's members. The Asperger identity celebrates the abilities of people with Asperger syndrome by explicitly or implicitly contrasting them with the disabilities of those with classic autism, and framing them as cases of neurodiversity rather than neurological impairment. This positive Asperger identity comes at the cost of damaging the identity of another oppressed group.[7]

There is some truth to the claim that people with Asperger syndrome have suffered a hermeneutical injustice because of the disappearance of the diagnostic category. The hermeneutical injustice is the loss of an identity that deeply resonated with many of them, helping them to make sense of

6 I am taking for granted here that those with an autism diagnosis are stigmatized and thereby oppressed.

7 It was a sad irony to discover, as I was writing this, that Hans Asperger was doing a similar thing in distinguishing his cases of "autistic psychopathology" from more severe cases of autism. See Donvan and Zucker 2016.

their experiences. That said, because the Asperger identity is not innocent in the hands of many who have used it, keeping the identity is also a potential injustice to those harmed by it: those with more classic autism. Hanna Rosin (2014), who has written thoughtfully about her son's receiving and losing the Asperger syndrome diagnosis within a period of a few months, describes the DSM-5 criteria as "democratic" in that they make no important distinctions between people on the autistic spectrum. I think this is a crucial insight, and one that provides an ethical argument for the DSM-5 criteria. (There is no evidence, however, that such an argument played any role in the deliberations leading to the DSM-5.)

Rosin (2014) also wrote in the same way as others about the transformative experience of receiving the Asperger diagnosis: "Almost the minute we got the diagnosis, my resistance to labeling melted, and so did my husband's. We walked willingly into another world, with its own language, rituals, and worldview." Six months after this diagnosis, the DSM-5 was published. Rosin acknowledged that the "DSM-5 no longer grants the special status that the Asperger label once supplied to a high functioning cohort."

Interestingly, though, any harm she may have experienced because of the removal of the diagnostic category did not last long. Over a year of reading and thinking, Rosin (2014) came to embrace the category of ASD, interpreting it as implying "an unbroken continuum among minds that extends from autism all the way into the realm of the normal" and hence democratic. This is not an interpretation officially sanctioned by the authors of the DSM-5. But it may become an ethically important aspect of the ASD diagnosis and deserves to be recognized in any future discussions. It also shows that identities can usefully change over time—even as underlying causes and symptoms may not change—and sometimes mark different stages in the acceptance of a disorder.

Summarizing this section, the case of Asperger syndrome differs in significant ways from the canonical cases of agnotology and hermeneutical injustice. While the Asperger category was certainly appreciated as a helpful identity by many of those with (and affected by) the syndrome, the category also implicitly disparaged those on the autistic spectrum who did not fall under it. So the overall effect is to contribute to hermeneutical injustice as well as to relieve it. In addition, the Asperger category is *not* an obvious epistemic (or practical) improvement on what came before—PDD—or what came after—ASD and S(P)CD; rather, it is in some ways better and in some ways worse than these classifications. Unlike the canonical cases of hermeneutical injustice, the Asperger category is *not* state of the art; in fact, it is currently not in use because of concerns about its validity. At present, the new categories of ASD and S(P)CD are being explored to see whether they work better. It is possible (although not especially likely) that the psychiatry community will return to the Asperger category and try to fix it. Psychiatric categories need to satisfy a variety of scientific, educational, and clinical needs (Michels 2015), and revisions to the DSM are often compromises between competing criteria.

3. Pluralism

No one thinks that the DSM-5 has carved psychiatric pathology at its joints. Some even think that there are no joints—that there is no single classification of mental disorders into "natural kinds." Psychiatric nosology is a controversial field because so little is known about the etiology of psychiatric disorders, and what is known is causally complex. Attempts to systematically classify psychiatric disorders in terms of underlying causes or mechanisms (genetic, cognitive, neurological, and so on) have had little success so far, and most psychiatric categories are defined—if only provisionally—primarily in terms of their typical behavioral manifestations.

Some (for example, Thomas Insel and Dominic Murphy) recommend beginning again and replacing the categories of the DSM with ones that explicitly model the causal structure of the brain (and carve nature at its joints). Insel's initiative at the National Institute of Mental Health is called the Research Domain Criteria project, and proposes to replace the DSM's symptom-based categories with genetic, imaging, and cognitive science criteria, which, it is hoped, will give us a better understanding of the causes of psychiatric symptoms. This project has an uncertain future now that Insel has left the institute.

Currently, the DSM categories are the best ones we have. They are provisional categories based on both symptom clusters and other validators (such as genetic information, imaging information, and clinical results), and developed for use in both research and clinical contexts. Many decisions taken in revising the DSM require compromises between research aims and clinical goals, and the compromises could have been made differently.

To put this in philosophy of science language, decisions about how to revise the DSM are underdetermined by the data, and pragmatic considerations play an important role.

If we ask the question, "Is Asperger syndrome a 'real' psychiatric disease?" the answer must be equivocal. We do not know whether the etiology of Asperger syndrome is the same as that of (some or all of) classic autism; we do not know whether the category of Asperger syndrome (whether or not the etiology is the same as classic autism) is useful for clinical, educational, or policy (or other) purposes. It is even possible that we will work with more than one psychiatric disease nosology at a time in the future—perhaps one more suited to research, and another more suited for clinical or policy use. Undoubtedly there are practical and political advantages to having a single nosology, but these advantages may be outweighed in the future by the benefits of pluralism.

Peter Zachar (2014) has argued for what he calls a "pragmatist" understanding of psychiatric classification, in which psychiatric categories are understood as "practical" versus natural kinds. I find this congenial and would add that this also points toward a pluralist attitude about proposed psychiatric categories, because we have several practical goals (research, clinical, educational, and so on). This is not a relativist or entirely constructivist approach, because some proposed categorizations will not be contenders in that they do not meet pragmatic goals.

The kind of pluralism invoked here is that of the "Stanford school" in philosophy of science, particularly the work of John Dupré (1993). It is a pluralism in which more than one theory (or classificatory system) can be the best way to represent the world, even when some theories (or classificatory systems) disagree with one another. (The "perspectival pluralism" of the "Minnesota school"—for instance, Ronald Giere, Helen Longino, and Kenneth Waters—is too modest, because it does not allow alternative theories to be inconsistent with one another.)

4. Is the Case of Asperger Syndrome Unusual?

In bringing this chapter to a conclusion, I do not want to leave the reader with the impression that the case of Asperger syndrome is unusual. In fact, I think it is typical, especially for those categories that feature in examples of hermeneutical injustice.

If we step back and take another look at the canonical examples of hermeneutical injustice—sexual harassment, postpartum depression, and sexual orientation—we may reflect that enlightening though these new categories are, they are most likely not the last word in understanding ourselves and our experiences. For instance, the category of sexual orientation is quite essentialist, representing people's sexual desires as stable, lifelong, and primarily described by dyadic sex differences. While this is useful for addressing social conservatives who view homosexual behavior as a matter of (poor) choice, it is not so useful for expressing more "queer" identities that vary according to context and over time, and may be responsive to a variety of human or other differences. Sexual harassment and postpartum depression may seem to us like stable, "true" categories, but I expect that we will find even these categories limiting in the future as social circumstances and our knowledge of these complex phenomena changes.

To complicate matters further, successful hermeneutical categories often show what Ian Hacking (1995) has called "looping effects," in which knowledge of the category shapes its further development. This is another way in which psychosocial categories are not stable "natural kinds," and simple realism is an inadequate way of understanding the ontology.

What about the situation with nonhermeneutical examples of agnotology—with cases such as abortifacients and climate change? Those who argue for a sharp divide between the natural and social sciences will probably think that these results do not apply in the natural sciences, and that natural science examples support a simpler, more linearly progressive realism. My view (Solomon 2001) is that there is no such sharp divide and a pragmatic pluralism is just as appropriate in the natural as in the social sciences. I think that all theories give us partial knowledge, and this partial knowledge frequently comes at the expense of other kinds of (also partial) knowledge. Theories conceal as well as reveal.

Agnotology is a fruitful concept, but it is not a simple one. As our knowledge grows, our ignorance does not automatically or even proportionally decrease. New knowledge makes new kinds of ignorance possible. Our best bet for good epistemic and practical outcomes is to expect and embrace pluralism.

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III IGNORANCE AS PASSIVE CONSTRUCTION

Focus-Generated Ignorance

7 How the Law Promotes Ignorance: The Case of Industrial Chemicals and Their Risks

Carl F. Cranor

1. Hypothetical Story of an Industrial Chemical: Polybrominated Diphenyl Ethers

Suppose about the mid-1970s there had been one or more stories, horrible to read, describing how a few infants had been burned badly or burned to death as a result of fires that began near where they were sleeping. As the president of a chemical company, however, suppose you had an idea that bromine compounds when added to a small flame would lower it or slow its progress before it became larger and uncontrollable. Perhaps you could create an industrial chemical that could be put into furniture, couches, car seats, foam, cribs, and electronic equipment built out of plastics (oil-based products) so that if they caught on fire, the flames would be reduced in their early stages. As a society we have developed numerous oil-based products, but they tend to burn readily, at least at higher temperatures, and thus some believe there is a need to combat their combustibility with other, flame-reducing products. Even better, suppose you owned the rights to an abundant supply of bromine so that this natural resource would be a comparatively inexpensive raw material with which to create a flame retardant. Now all you would need would be a chemical creation to add to commercial products to reduce flames should they catch on fire. You could render a public service, saving small children and some adults from horrible deaths, and make a profit at the same time.

In the 1970s this information would not have been much of a stretch, because after all there had been some flame retardants on the market based on chlorine or bromine. Yet a major problem was that several of them were known to be quite toxic. Poly*brominated* biphenyls (PBBs), a brominated relative of poly*chlorinated* biphenyls (PCBs, banned by the US Congress in the late 1970s because of their persistence, bioaccumulation, and toxicity), had devastated cattle herds, sickened farmers, and caused the death of some members of Michigan families (Egginton 1980). A similar chlorinated compound, chlorinated tris, used in children's sleepwear, had been found to be mutagenic, ultimately leading to its ban (Blum and Ames 1980).

But your product was not identical to these earlier molecules. It bore some resemblance to PCBs, but was somewhat different (it substituted bromine for chlorine and had a different connection between benzene rings) and was not a PBB. You were safe on that score. Nonetheless, because it had some uncanny similarities to PCBs, and was a halogenated substance that could be stable and persist for a substantial period of time, this could be a good thing because you desired stable, long-lived flame protections in products. (Ultimately, it also turned out to live a long time in the environment and mammalian bodies.)

This is merely a hypothetical story with some points anchored in fact. Bromine has some flame-retardant properties for small flames—though by now these appear to be greatly overblown for more robust flames (Roe and Callahan 2012). The molecular structure does resemble yet is not identical to PCBs, and some known brominated chemicals were understood to be toxic. Thus, as an actual decision maker you would not have had to begin quite tabula rasa about your chemical product, but you might not have had much understanding of brominated molecules. You had a bit of knowledge and a lot of ignorance about brominated chemicals.

Enter the law. As a law-abiding citizen, of course, you would seek to follow the law concerning chemical creations. What would the law require you to do with your polybrominated diphenyl ethers (PBDEs) if you wished to commercialize them?

2. Laws Governing Chemical Creations

Some laws try to *prevent* any *harm* from occurring via *regulatory* or *administrative* structures. Legislatures create administrative laws, structures, and public health agencies. The US Environmental Protection Agency (EPA), Food and Drug Administration (FDA), and Consumer Product Commission are three that have appropriate authority to administer different environmental health laws under each.

A second strategy, inherited from the common law of the United Kingdom, for the most part waits until someone has caused *harm*, and then a prosecutor might seek to punish the wrongdoer under the criminal law, or a private person who had been injured could utilize institutional procedures to try to remedy the wrong done to them by receiving compensation within the tort or personal injury laws. (I ignore the criminal and tort laws.)

Preventive strategies are much preferred for protecting the public health, but how is this achieved? Congress passes laws to authorize administrative agencies to prevent health risks and harms that are too particular, would need too much scientific background, and would require too much detailed attention for a legislative body to carry out.

Metaphorically, one might say that Congress creates a blueprint for how personnel within administrative agencies should issue regulations that have the force of law under authorizing legislation, such as the Clean Water Act, Safe Drinking Water Act, or Clean Air Act. This blueprint specifies certain procedures they must follow to identify risks from products as well as setting normative standards for how risks should be controlled in order to protect the public's health and other interests. In this way, Congress delegates some of its legal authority to make laws to an administrative agency to implement the law within the constraints imposed by the blueprint.

Congress also decides more specifically how products or exposures should be regulated in order to protect the public's health and the environment. For pharmaceuticals and pesticides, Congress requires that products be tested for various hazards and risks *prior* to their entering the market. The consequence is that companies may not sell their products until they have done the required toxicity testing, governmental or independent scientists have reviewed the test results to ensure that they have appropriate quality control, and the test results satisfy congressionally mandated standards to protect the public. For pharmaceuticals, the FDA reviews prescription drugs for both their efficacy in providing health benefits and their safety to ensure that adverse health side effects do not outweigh their benefits. Moreover, because pharmaceuticals are bioactive—in order to accomplish their therapeutic aims—they must be reviewed for the safety and efficacy of doses appropriate to the typical time they would be prescribed; for short-term prescriptions, there would be a different safety analysis than for drugs that might be prescribed for much longer periods.

For pesticides, the EPA reviews specified tests to ensure that the amounts of pesticides tolerated on foods “can be used with ‘a reasonable certainty of no harm’” to human health and without posing unreasonable risks to the environment (EPA 1996). In addition, the agency must take into account the special susceptibility of children and aggregate risks from exposures to a pesticide from multiple sources and “cumulative exposure to pesticides that have common mechanisms of toxicity” (EPA 1996). Like pharmaceuticals, pesticides may not be commercialized until the EPA reviews the products and licenses them for sale. Premarket laws govern a small percentage of humanly created chemicals—about 10 to 20 percent, but likely closer to 10 percent.

Virtually all other chemical creations are governed under postmarket laws. These permit products to enter commerce without any routinely required toxicity testing, or a review of the hazards and risks that the product might pose. Under the 1976 Toxic Substances Control Act (TSCA), a company was required to notify the EPA that it proposed to manufacture a new commercial chemical. Its proposal had to be accompanied by some minimal data about the substance—that is, “all available data on chemical identity, production volume, by-products, use, environmental release, disposal practices, and human exposures” (Cranor 2017, 29). No toxicity or health effects data were routinely required. If a company had conducted any toxicity tests, it had to submit them, but if it had not, there was nothing to submit. The agency “must take what it [was] given” (Applegate 2009, 104–105).

The EPA had a short period of time to review the submitted data to determine if other information, including toxicity data, was needed. If the data were needed, the EPA could request it, but if the company refused, there was no easy way for the EPA to obtain the information. It could “issue a regulatory rule” that ordered the company to provide it or seek a court order to have the data submitted. Either choice required the EPA to invest substantial time and resources simply to obtain the needed data to protect the public. In order to issue a rule or request a court order to obtain the information, however, the agency must have had enough information to support the legal action—not an easy task (more below) (Applegate 2009, 118).

What follows largely discusses the contribution of US postmarket laws to our ignorance of chemical creations for three reasons: many other countries also govern their chemicals by postmarket laws and can learn from shortcomings in US laws; it is urgent to understand the limitations of such laws, and why they had to be changed; I know them better than the laws of other countries and understand them better than the European Union's recent law, the Registration, Evaluation, Authorisation, and Restriction of Chemicals (REACH) (European Community 2006).

3. Back to PBDE Flame Retardants

In the 1970s, the particular PBDE flame retardants were comparatively new, but brominated flame retardants were not. If you wanted to introduce your PBDEs into commerce at that time, under what laws would you do so?

Since PBDEs were not pharmaceuticals, pesticides, new food additives, or any of five other kinds of products governed by different laws, they would be subject to the 1976 TSCA. In order to manufacture the PBDEs, you would need to inform the EPA about its chemical identity, production volume, by-products, uses, environmental releases, disposal practices, and likely human exposures. If you had toxicity data specifically about the PBDEs, legally you would be required to submit them as well. Of course, if you had no toxicity information, you had nothing to submit. The EPA then had 90 days (which could be extended to 180 days) to review this minimal data in order to determine if it must request additional information.

4. Agnotology

Ignorance of chemical toxicity is a product of many forces—an important one of which is the law. Before we go further into the law, though, consider more about PBDEs.

If PBDEs had been created de novo, and not against the background of some information about the possible benefits and toxicity of brominated flame retardants more generally, the picture would have looked something like this. When chemists synthesize a substance, in its "native state" they might have near-total ignorance of its properties (Proctor 2008, 4). They would know its chemical structure and some features of that, but probably not all the consequences of the structure, including initially perhaps not its boiling point, freezing point, or by-products, or how it might be used. These would all need to be explored. During experiments, chemists might accidentally discover some potential uses or even some toxic properties.

For example, bisphenol A was first synthesized in 1891, within a few years of the creation of PCBs (Biello 2008; Houlihan, Lunder, and Jacob 2008). Forty-five years later, some of the same scientists who synthesized diethylstilbestrol found that bisphenol A also had similar estrogenic properties (Biello 2008; Dodds and Lawson 1938). They considered it for possible pharmaceutical uses, but diethylstilbestrol bumped bisphenol A from consideration because it was a more potent synthetic estrogen. Although such substances enter the world with the creators' agnostic about many of their properties, knowledgeable testing along with serendipity begin to reduce ignorance and outline what scientists understand about them.

Institutions can start to influence ignorance and knowledge about substances at this point. If the product seems especially valuable, this would lead the company out of self-interest to keep the information close and not share it with the wider world. This would prevent competitors from stealing the idea and also enhance the company's competitiveness with the new product. Thus the company would know more about its product as well as its uses and value than competitors and others in the outside world. Not only does competitiveness shape *who* in the outside world knows about the substance and its properties, but if the knowledge is sufficiently precious, only a small group within a company may know about it. If the substance happens to be the central ingredient in Coca-Cola, for example, those most in the know might be only four people, and not even many of the chemists and officers in the company (Proctor 2008, 10).

If a product were sufficiently valuable for development, the law shapes knowledge and ignorance about it for commercialization. For pesticides and pharmaceuticals, companies must take numerous steps to both remove ignorance about features of the products and convey knowledge about them at least to administrative agencies and, to some extent, the wider world. There are some differences between how the two classes of chemicals are treated, but both require companies to routinely conduct specific toxicity tests and provide them to the reviewing agency as well as produce information about benefits from the products. If the company wishes to profit from its discovery, it has a legal burden to remove ignorance about its product as required under the law.

If the product were a generic industrial chemical, as the PBDE flame retardants were, the institutional ground rules would be quite different. If a company had only near-native ignorance about it, but had serendipitously discovered that PBDEs modestly reduce open flames, it might have proposed them for manufacture. At this point the company had few legal responsibilities to remove further ignorance about the PBDEs. *If* it had data about PBDEs that shed light on their toxicity, legally it would be required to submit this to the EPA (but companies do not always report such information [Cranor 2017, 58]). Without additional toxicity data, it would have nothing further to submit. Public and EPA ignorance about PBDEs' toxicity would likely continue.

If the EPA found evidence suggesting that the submitted product were toxic, it could "issue a regulatory rule" ordering the information, or go to a court and seek a "court order" to provide it. Neither is simple. To issue a rule, the agency must marshal sufficient information to support the action. The agency, however, must have information "to prove that it needs it, but it needs the information because it does not have it"—a catch-22 (Applegate 2009, 118). Moreover, even if it had enough minimal information legally to compel further studies by the company, this is not an easy task, because it takes considerable agency time (up to a few years), resources, and personnel to institute legal action.

Yet the ignorance does not stop there. If aspects of PBDEs' properties would be valuable to competitors, the company could cloak those under "confidential business information" provisions to protect its product. The EPA is required to protect such information. In addition, if within a 90-day window (which, again, can be extended to 180 days) the PBDEs exhibited no toxic properties that triggered additional review, the EPA must approve the product for manufacturing. Today it would be doubtful that PBDEs would escape the EPA's notice, since their chemical structure so closely resembles both PCBs and PBBs, both now better understood and banned toxicants (Egginton 1980). In the 1970s, there might have been a more limited basis for further inquiry.

In the future, if the production of PBDEs increased sufficiently, the company would be obliged to inform the EPA, and then the agency could demand further data (US Congress, Office of Technology Assessment, 1987, 126–127). Until that time, the flame retardants could be sold without any additional toxicity or other data becoming known to the agency or the public. If the company became aware of toxic properties, it would be legally obligated to report them, but otherwise it could remain silent.

The 1976 TSCA set the incentives for the company and the rules for the EPA to follow that kept most of us in ignorance about the toxicity of such products, and as a result, failed to protect the public's health.

The law, however, would not be done shaping ignorance. If individual scientists became concerned about a product's toxicity, they might decide that it should be further explored. They would typically submit research proposals to the National Science Foundation, National Institute of Environmental Health Science, or even EPA seeking support to conduct appropriate toxicity experiments on the PBDEs

These efforts likely would not be well coordinated, because individual researchers would aim to understand any toxicity properties of PBDEs from their own scientific expertise and backgrounds. Epidemiologists would try to compare greater-exposed with lesser-exposed (or unexposed) people, seeking to determine any difference in disease rates; animal modelers would conduct even better experiments between highly exposed, lesser-exposed, and control groups of animals, looking for possible adverse effects; and mechanistic researchers might compare PBDE mechanisms of action with other halogenated compounds, such as PCBs, looking for toxicological analogies; and so on.

Because the search for toxicity would not likely be coordinated between researchers, each would develop part of the toxicity picture (to the extent it was present), like a dot on a pointillist painting or part of a toxicological elephant. What one lab explored may or may not have been helpful to an administrative agency seeking to protect the public's health; an agency would need data from several different disciplines, and typically multiple sources of each are combined for a more complete and stable toxicological picture. Consequently, research on toxicity could be hit or miss. Yet if the EPA were sufficiently concerned about the toxicity of the product and had sufficient research funds to support appropriate scientists, it could fund coordinated studies to assist public health protections. Of course, scientists would likely review various lines research from the literature on PBDEs to better understand how their research might fit with the larger research about them.

If the EPA sought to better protect the public's health, if necessary, from a potentially toxic substance, how would it do so in a postmarket world?

How can postmarket laws produce knowledge about products and “prevent” harms, since most products were initially grandfathered in as safe, or entered commerce without any toxicity data and TSCA erected barriers to legal action until after they were in commerce and the public was exposed?

Once risks have been identified and estimated, the agency must follow congressional blueprints to issue public health “regulations” to reduce risks that have the force of law. The regulations must be implemented in accordance with both appropriate procedures and substantive health protection guidelines that Congress has passed.

These guidelines take a variety of forms, require different information, and protect the public to a greater or lesser extent. Under the older “Delaney clause” required by the premarket Food, Drug, and Cosmetic Act designed to protect the public from food additives, the FDA was authorized to assure “that no additive shall be deemed to be safe if it is found to induce cancer when ingested by man or animal, or if it is found after tests which are appropriate for the evaluation of the safety of food additives, to induce cancer in man or animal” (US Congress, Office of Technology Assessment 1987, 88). Following this mandate, if the FDA found that a substance caused cancers in humans or experimental animals, it was not permitted to be a food additive, or if its toxicity had escaped notice during premarket testing, it had to be withdrawn. At the other end of the protective spectrum under older versions of the Federal Insecticide, Fungicide, and Rodenticide Act, the EPA had to ensure that a pesticide must pose no “unreasonable risks to man or the environment, taking into account economic, social and environmental costs and benefits of the use of any pesticide” (US Congress, Office of Technology Assessment 1987, 117). The substantive safety requirements under the 1976 TSCA also required that the EPA determine whether or not the substance posed an “unreasonable risk” (US General Accounting Office 1994, 4). This term “is undefined in the [TSCA] statute, but the legislative history and subsequent judicial interpretation consistently interpret it as a greater-than-zero level that is determined by reference to health, benefits, and costs” (Applegate 2009, 109). While the Delaney clause requires less data and was quite protective of human health, the TSCA requires more data and is less protective; it permits health, economic, and environmental costs from the product’s use to be balanced against health, economic, and other benefits that might come from having it in commerce. Risks to the public’s health do not necessarily trump other consequences of the TSCA as they did for pharmaceuticals under the Food, Drug, and Cosmetic Act.

If an agency under a postmarket law must acquire data about and identify a risky substance, how does it do so? In addition and especially important, how can it *prevent* harm to the public that the law aims to protect?

5. The Failed Promise of Quick Risk Assessments

Enter risk assessment. A risk is merely the *chance* of harm or some other untoward or undesirable outcome occurring. Thus by definition, a risk has not necessarily materialized into harm to the public health or environment. The hope was that if there were reliable techniques for identifying risks before they resulted in harm, and agencies could quickly utilize such procedures to identify and quantify risks after exposures happened, public health agencies could act to reduce the *risks* in order to prevent *harm* or at least harm to some of the public.

Moreover, risk assessments, it was assumed, would utilize experimental animal studies, human and animal cell culture data, mechanistic data, and other nonhuman data to remove ignorance about a substance and reveal risks in order to reduce exposures before harm to people took place. This led to the new field of risk assessment beginning about 1970 (US Congress, Office of Technology Assessment 1987, 30). The professional Society for Risk Analysis came into existence in 1980 (Covello and Mumpower 1985; Thompson, Deisler, and Schwing 2005).

Risk assessment seemed ingenious: if the postmarket laws and procedures implemented under them functioned expeditiously, the identification and quantification of *risks* plus quick action could largely protect citizens' from harm. Some early results were promising. The Occupational Safety and Health Administration in its first few years issued protections for twenty-two carcinogens in nine final rule makings (US Congress, Office of Technology Assessment 1987, 19–21). In the last twenty-seven years only five health standards have been issued (Finkel 2019). The EPA was more sluggish in the first sixteen years of implementing the Clean Air Act, listing only seven carcinogenic air pollutants and issuing final rules on six (19–21). Under the Safe Drinking Water Act, the EPA issued eight final health regulations and proposed rules on thirty-one others in the first twelve years of the law (19–21). Under the Clean Water Act, the pace was so slow that environmental groups sued the EPA to expedite health protections under a different procedure endorsed by a federal judge in a consent degree (108–109). For a time this greatly accelerated health protections (110–111).

A 1987 Office of Technology Assessment report found a slow pace of information generation and public health protection from carcinogens. For sixty-one of the most potent carcinogens, on average individual agencies had acted on less than 50 percent of these substances within their jurisdiction (US Congress, Office of Technology Assessment 1987, 20). Taken together, the agencies had 422 "opportunities to act" to protect the public within their jurisdictions, but had issued public health protections only 28 percent of the time (20).

What is the significance of this? Despite the *theory* of quickly generating information about toxicants to conduct risk assessments based on nonhuman data, even in the most favorable circumstances—shortly after these laws were enacted and with a backlog of nonhuman data as a foundation for health protections—it failed in great measure.

What was described in the previous three paragraphs occurred in the heyday of public health protections; now it is much worse. The law and its implementation enter here as well. Twenty years out of twenty-eight were dominated by Republican administrations quite hostile to "regulation." For many of his eight years as president, Bill Clinton faced an inhospitable and antiregulatory Republican congress. The Obama administration was more committed to protecting the public, but its record was not spectacular. Finally, the United States now appears to have the most antipublic health EPA perhaps ever.

Some presidential administrations slowed regulation, some Congresses passed additional laws to frustrate or stall health protections, and sometimes individual senators used congressional processes to hinder health protections, such as holding up Senate confirmations of political appointees until the president agreed to delay implementation of a regulation (McGarity and Shapiro 1993; Cranor 2017, 53). Congressional actions often reinforced ignorance about products or erected roadblocks to knowledge. Consequently, the rate of knowledge production and public health protections was much slower than those that concerned the Office of Technology Assessment in the mid-1980s.

Under postmarket laws, substances that enter commerce will either be hazardous or not. If they are hazardous, they will pose risks to the public if exposures are great enough (not every hazard—a substance that causes or likely causes harm to humans—will pose a *risk*). To the extent that substances pose risks, and entered commerce with little or no known about their toxicity, they will continue to threaten the public until those risks are reduced or eliminated.

Of course, the affected companies would not have been idle in these circumstances. Since products are in the market, and continue to provide any benefits and profits to the companies, they have incentives to try to keep the products in the market. They would resist reductions in exposures to their products or bans of them if it would reduce their profits.

The same laws that slow acquisition of knowledge about the toxicity of products, however, have also slowed regulatory action even when adequate science was present to implement improved health protections. The legal structure, incentives, and burdens of proof of postmarket laws tend to leave scientists, public health agencies, and the public in considerable ignorance of the toxicity of products, and hence sustain a public risk for a substantial period of time. Postmarket legal requirements predispose and ultimately contribute to ignorance of the product's toxicity properties as well as the safety of the products. How does this occur?

6. Reenter Agnotology

Return to our hypothetical PBDEs in their "native" knowledge state. We assumed little or nothing was known about them when they were proposed for manufacturing under the TSCA.

Now suppose, as is the case, that researchers noticed that PBDEs had a chemical structure resembling PCBs and PBBs, both of which were banned in the late 1970s because they were persistent, bioaccumulating toxic substances that could travel long distances as well as contaminate environments, animals, and people far from their origins in manufacturing plants or disposal sites.

Scientists had found that some classes of PCBs resemble the chemical structure and toxicity effects of dioxins; others have somewhat-different toxicity properties. Dioxin-like compounds are associated "with reproductive, immunologic, teratogenic and carcinogenic effects" (Kodavanti 2005, 274). Exposures to such substances are associated with developmental delays and lower IQs, have caused cognitive delays, have probably affected sex-related behaviors (Birnbaum 1998, 103), and caused “male deficits in spatial reasoning, lack of endurance, clumsy movement, and IQs approximately six points lower at age eleven” (Grandjean and Landrigan 2006, 2172). Children are much more susceptible to PCBs prenatally and during the immediate postnatal period (Jacobson and Jacobson 1994). Background levels of dioxins and dioxin-like PCBs can also adversely affect the human immune system, reducing its efficacy to fight nascent diseases (Birnbaum 1998, 103). At exposure levels typical of some human exposures, animal studies revealed “subtle signs of neurological dysfunction, delays in psychomotor development, alterations in thyroid hormone status, and changes in immunological functions” (106). PCBs can adversely affect thyroid uptake by pregnant women, thereby adversely effecting the neurological development of developing fetuses (Woodruff et al. 2008, 1570). Finally, dioxins and some dioxin-like PCBs are now classified as known carcinogens by the International Agency for Research on Cancer (1997, 342–343; Cogliano et al. 2011).

Did the similarity in chemical structure between PCBs and PBDEs portend similarity in toxicological effects?

Researchers beginning to inquire into the toxicity of PBDEs would have known of the chemical similarity between nondioxin PCBs and PBDEs. By itself, this raises a red flag for potential biological similarity as well, but it is not decisive.

Demands for human data to show adverse effects further delay knowledge about toxicity as well as health protections. Often it is claimed that regulation should be based on human data for toxicity end points. While such information can be important for showing and confirming toxicity in Homo sapiens, it is not necessary because distinguished scientific committees such as the International Agency for Research on Cancer, the National Toxicology Program, and California’s Carcinogen Identification Committee for Carcinogens rely on various kinds of nonhuman data to identify toxic substances (Cranor 2017, 148).

Moreover, acquiescence in a demand for human data extends the period of ignorance about compounds, and leaves the public and workforce at risk for several reasons.

First, sufficient time must elapse to detect any diseases caused by exposures because there needs to be adequate time for any diseases to be induced (the induction period), and then further time must pass for a disease to develop so that it can be detected by clinical methods (the latency period) (Rothman 1986, 51). And, unless exposures cause particularly virulent and rapidly progressing diseases without sufficient allowances for induction and latency periods there would have been nothing to detect.

Second, studies in humans tend to be especially insensitive for discovering risks, making it difficult to detect adverse effects unless they cause high relative risks compared with controls or produce obvious outcomes as Thalidomide did (limb reduction and other adverse effects). In addition, although PBDEs are likely neurotoxicants, these contributions to neurotoxic end points would be difficult to tease out because people have variability in neurological properties, variability to neurological diseases, and various exposures to multiple neurotoxicants, complicating the problem of identifying whether and which neurotoxicants might also have caused problems.

Third, the natural history of the disease might be much longer than researchers had judged, resulting in studies that were too short to reveal a disease with longish latency and induction periods. This is exacerbated for comparatively recent exposures. Exposure estimates can be quite crude, which will make accurate results difficult. For rare diseases, certain kinds of epidemiological studies will have difficulty detecting them simply because sample sizes would be too small to detect a rare outcome even if it exists. For exposures that have some quite subtle adverse effects, as lead does, early research may be insufficiently refined to identify more subtle end points (Huff and Rall 1992, 433; Canfield et al. 2003). Also, an increasingly contaminated human population frustrates finding clean, uncontaminated controls for studies (Cranor 2011, 62).

If human epidemiological studies are required for public health protections, ignorance will continue, and years of preventable contamination and diseases would likely occur before risks could be reliably identified (Huff and Rall 1992, 433; Cranor 2011, 60–62).

In identifying risks from PBDEs, a different group of researchers would likely have begun studies in experimental animals to determine whether and the extent to which chemical similarity also resulted in toxicological similarity. Scientists subsequently have found that PBDEs caused adverse effects in animals similar to those caused by PCBs.

First researchers found PCBs cause similar adverse effects in other mammals and humans (after sufficient time had elapsed) (Cranor 2011, 60–62;

Rogers and Kavlock 2001, 374; Costa and Giordano 2007, 1047–1048; Kodavanti 2005, 276). PCBs decrease cognitive function in rats, primates, and mice, impair visual discrimination, alter spatial perception function, and cause deficits in learning and memory (Kodavanti 2005, 276, 278). PCBs can also "cause neurotoxicity in humans . . . and it is believed that in utero exposure is more important than lactational exposure in causing the neurotoxic effects," including behavioral changes and learning deficits (275).

A next step would have to compare the toxicity results of PCBs and PBDEs in analogous experimental animal studies. This has been done. PBDEs alter motor activity and cognitive behavior, and affect the thyroid hormones, which in turn can cause neurological effects. In general, PBDEs are of concern because "of their association with endocrine disruption, reproductive and developmental toxicity, including neurotoxicity, and cancer," along with causing animals to be "hyperactive and [show] reduced or lack of habituation, effects that worsen with age" (Johansson et al. 2008, 917; see also Birnbaum and Staskal 2003).

Moreover, these adverse effects in animals are seen "at exposure levels relevant to humans, at least in North America" (Costa and Giordano 2007, 1061). This is particularly worrisome because relatively high concentrations of PBDEs are found in breast milk in the United States, with dust and food contributing substantial amounts of PBDEs. The concentrations of PBDEs on a per body weight basis are highest in infants, and lowest (but still of concern) in adults (1051). PBDE concentrations in US citizens are ten to forty times higher than those in Europe (Babrauskas et al. 2011).

While the half-life of PBDEs in rodents is comparatively short—"in the order of several days or months"—they are much longer in humans (Costa and Giordano 2007, 1061; Sjödin et al. 2008, 1378). This gives PBDEs longer time to inflict molecular damage that can lead to tissue or organ damage later in life.

This brief review of some studies on PBDEs is quite revealing. There are credible concerns about potential adverse health effects in humans from chemical similarities between PCBs and PBDEs. The effects of PBDEs seen in animal studies support this inference. PCBs cause similar adverse effects in animals and humans, while PBDEs cause adverse effects in animals resembling those from PCBs. What has largely been missing is human data showing adverse effects. Finally, thirty-five years after PBDEs were introduced and after thirty-five years of exposures, some of those results are beginning to appear.

For instance, high concentrations of PBDEs in house dust affect men's reproductive health, tending to reduce masculine characteristics and increase certain feminine features: undescended testicles, decreased sperm count, and reduced testicle size (Meeker et al. 2009; Babrauskas et al. 2011). Similar PBDEs adversely affected thyroid hormone levels, which in turn can affect neurological function, especially in children, but also potentially in pregnant women and their children in utero (Lema et al. 2008). More recent studies have shown motor, behavioral, and cognitive decrements along with lower IQ (Babrauskas et al. 2011; Shy et al. 2011; Chao et al. 2011) and wider reproductive effects: delaying pregnancies, lowering birth weight, and decreasing infants' length and chest circumference. Some of these effects are early indicators of later health problems (Harley et al. 2011).

These findings are not as dramatic as Thalidomide babies with shortened limbs or young women with vaginal/cervical cancer caused by exposure to DES, but some are of substantial concern. Moreover, the premise of public health law is that one should not wait for dramatic adverse effects to be clearly manifested before acting to protect the public's health. Yet affected companies are likely to venture such arguments to denigrate nonhuman data: "adverse effects in mice or rats are not pertinent to humans, because humans are not overgrown rats or mice"; "just because adverse effects have been seen in rodents does not mean they will be manifested in humans because it is difficult (or impossible) to extrapolate from rodents to humans"; "rodents were exposed to high doses of toxicants, thus this is irrelevant to real-world exposures of humans to much lower doses." These common arguments are asserted even though they may not be true in general or especially pertinent to particular findings. Nonetheless, they are designed to reassure the public not to worry, remove public and legal pressure, and even mislead the public because no adverse effects of merit have been seen in humans (Cranor 2011, 79–80).

In the face of the ignorance generated by the law, science, lobbying, and other barriers, both U.S. and state administrative agencies have finally begun—but just begun—to take various actions to reduce exposures to PBDEs and protect the public (EPA 2014). This has taken about forty years. When PBDEs were first introduced there were none in human bodies; now they are ubiquitously present (Schecter et al. 2005).

The larger picture is that if the science about toxicity is not generated before products enter commerce, the PBDE story reveals that science itself can be slow to reveal adverse effects. For one thing, researchers must conceive that there is something to be explained, which must be known before an explanation can be offered.

For another thing, even if researchers sought to expedite knowledge generation in the effort to better protect the public's health, this is not easy to do. Scientific studies have their own pace, depending on the study in question and nature of the disease. For example, if the EPA seeks to generate animal data through governmental actions, it likely takes seven years for the nomination, carrying out of the experiment, and interpretation of the results. Epidemiological studies must allow sufficient time for the disease to appear, taking into account both induction and latency periods of disease (Cranor 2016, 96–97).

For a third thing, when there is an extended time period between exposure to a substance and appearance of a disease, this is far from "billiard ball" causation. One does not launch an exposure and disease appears the way one billiard ball moves after being struck by another. The latency of diseases permits skeptics to hide behind the slow revelation of causation and gives them opportunities to challenge asserted causal claims, and then hide behind the slow revelation of those too. The upshot is that postmarket laws result in a *misalignment* between aims of the laws that seek to generate knowledge about products and protect the public, and the science that can assist this effort. For many of the above reasons, the science will greatly delay discovery of any adverse effects resulting from toxic contamination (Cranor 2011, 170–172).

In addition, generating scientific evidence of risks does not occur in an academic vacuum; the entire enterprise is subject to pressures from affected companies, their lobbyists in the halls of Congress and regulatory agencies, and general public relations efforts to protect a company's products. For one, companies have incentives to refrain from producing any data that might harm their profit-producing products. Second, firms cast doubt on others' science. Initially used by the tobacco industry, this strategy was well expressed in a memo: "Doubt is our product since it is the best means of competing with the 'body of fact' that exists in the minds of the general public. It is also the means of establishing a controversy" (Brown and Williamson Tobacco Company 1969). Then there is a practice from the early years of risk assessment of companies advocating high standards of scientific proof and multiple studies before data should be accepted and regulations issued. Affected industries sought to portray themselves as being on the side of scientific angels, contending that there should be no regulation unless it was based on good or the best scientific studies. In producing knowledge about a company's products, only the best data would suffice. This was likely difficult to do, but companies did not mind; if these arguments were successful, their products stayed in commerce for a longer period of time (Cranor 2011, 161–165). And of course, the public would largely remain uninformed about risks from products. In addition, there are "less honorable" tactics and strategies designed to prevent knowledge about products (Cranor 2017, 54–60). Experts for hire may mislead regulatory agencies and scientific journals about the science involved. Sometimes scientists or their employers have modified studies showing adverse effects from exposures so that they can claim there are no or only minimal risks. The chromium industry began an epidemiological study designed to discover whether exposures to chromium across four plants increased the risks for employees. After the initial results appeared, the directors of the study decided to divide it in half, and report results for two plants in one study and two plants in another one. The consequence of this tactic was to show a lesser adverse health effect than the original study (Michaels 2008, 105–108). A few years ago, the *Chicago Tribune* revealed that a surgeon who specialized in treating burn victims, especially children, had misled or even outright lied to legislators concerning the benefits of PBDE flame retardants (Roe and Callahan 2012).

Public health agencies also sometimes face tactics in which companies create misleading information. They may "salt the [scientific] literature with questionable reports and studies, . . . which regulatory agencies have to take seriously, . . . [but their main effect] is to clog the [regulatory] machinery and slow down the process" (Michaels 2008, 46). Some industry research projects have been proposed from the outset to find no adverse effect as a means of protecting their products (Lombardi 2014). As well, there are a variety of ways that scientific studies can be designed to frustrate and mislead the impartial assessment of causal associations without actually lying about the science (Bailar 2006).

Moreover, even when the EPA or another agency has generated sufficient data to support a proposed health standard, sometimes companies succeed in having that document reviewed by the National Academy of Science for at least two years, further delaying it. If the academy finds sufficient problems with the science, however, the EPA may have to revise the proposed law before it becomes final. Should this occur, information and health protection delays would result. The National Academy of Science's (2014, 7) review is not a matter of course but instead something that can occur because political actors in the US Senate can hold legislation or appointments hostage until an agency acquiesces in its efforts on behalf of a company or industry.

Finally, even when an agency has sufficient knowledge about a compound to protect the public and then issues a health standard, one of the parties to the regulatory proceedings may appeal it to a US circuit court. Should this occur, typically the more protective standard would be delayed until the appeal was exhausted. If a court mandates corrections, finds procedural or substantive shortcomings, or invalidates the entire health standard for some reason, this further delays health protections.

7. Overcoming Ignorance about Toxicants and a Way Forward

The above consequences are largely, but not quite totally, the result of US law. By not having toxicity data about products before commercialization and then placing legal (and perforce scientific) burdens of proof on public health agencies to provide data about risks or harms before reducing risks or removing toxicants from commerce, the law creates incentives and barriers to the production of knowledge about substances while also slowing protections for citizens. To recapitulate, under the 1976 TSCA, little information had to be produced about a new product (and nothing was routinely required about its toxicity) before it entered commerce, while older products were grandfathered in as "safe." If there are no clues to its toxicity, the product could enter commerce. If there is minimal evidence about its toxicity, the EPA could order additional data, but this was not easy.

Of course, independent researchers could generate clues to toxicity. If adventitious research revealed toxic effects, the EPA could be persuaded to fund more comprehensive and coordinated toxicity testing, and even demand more data from the company. Yet even at this point, nothing could occur quickly because of the pace of scientific studies, which diseases with long latency periods can extend. If the products indeed pose risks to human health, these continue until exposures are reduced or products are withdrawn from the market.

Yet once scientists and the EPA have generated sufficient data to create improved health protections, there still may not be much progress on protecting the public. Companies have incentives to frustrate health protections by urging that more studies be done, generating competing studies that show no adverse effects, and sometimes relying on less honorable means. All this has happened. Finally, as the Office of Technology Assessment study showed, even when there is sufficient data to institute public health protections, the agency may have insufficient human, legal, and monetary resources to institute protections expeditiously.

Both substantial long-term ignorance about the toxicity of products and long delays in acting on existing scientific evidence to protect the public are attributable to the law, and how it structures the creation of knowledge and shapes legal responses once there is adequate information for health protections. Postmarket laws are reckless toward the health of citizens and permit companies to be reckless as well. Notably, however, these same problems arise under premarket laws once products are in commerce because many of the same issues of science, legal burdens of proof, company resistance, and lobbying pressures converge to burden efforts to reduce toxic exposures and protect the public.

Existing legal models, though, illustrate how substantial modifications would improve the status quo for more quickly removing ignorance about toxicity and improving health protections. Before products enter commerce, companies should be legally required to test for a variety of toxic end points, including developmental, neurological, carcinogenic, immunological, and other disease and dysfunction outcomes. Testing should seek to identify substances that are toxic to both developing children and adults with a particular focus on sensitive life stages. The testing of a product before exposures occur would improve protections for susceptible subpopulations, provide downstream users of the products with better data with which to create their products, lessen citizens' anxiety and concern about toxic ingredients, and reduce much of the current time, effort, money, and mental resources that at least some people spend to inform themselves and reduce toxic exposures in their lives (Cranor 2011, 231–238, 244–247; Cranor 2017, 201–204).

The US legal system's laws for pharmaceuticals and pesticides, requiring premarket toxicity testing, scientific review, and licensing before products can enter commerce, are notable models, described elsewhere (Cranor 2011, 24–28). The European Union's REACH legislation aims to better protect its citizens and the environment by shifting the burden to produce evidence about products onto "manufacturers, importers and downstream users to ensure that they manufacture, place on the market or use such substances that do not adversely affect human health or the environment" (European Community 2006, art. 1).

REACH also seeks to "ensure that substances of high concern are eventually replaced by less dangerous substances or technologies where suitable economically and technically viable alternatives are available." The number and generic types of tests needed for a specific substance are based on the number of tons of production volume as a surrogate for exposure, with greater exposures necessitating more extensive testing (art. 1, 28–29).

Each chemical substance manufactured, used, imported, or distributed in Europe must be *registered* with the European Chemicals Agency. Depending on the production volume of the product, it must be *evaluated* for its toxicity by undergoing certain specified tests. After test data have been submitted, the European Union makes a decision about whether to *authorize* the product to enter (or for existing substances, permit to remain in) commerce. Finally, products with toxic properties that are also socially valuable may be authorized but *restricted* because of their toxicity. A snappy EU aphorism characterizes this approach: "No data, no market." If there is no or insufficient data about a product, it may not enter commerce, or if it is already in use, it may not remain.

Licensing or permission models, such as REACH or US pesticide and pharmaceutical laws, treat access to markets as conditional on satisfying risk reduction provisions for a company's products. This is a substantially different moral and legal relationship between a country and firms seeking to do business within it than in the United States. Under US postmarket laws, companies in effect have a legal right to market products unless and until there is evidence that the products cause harm or risks of harm.

New hope appeared in 2016 when the US Congress amended the 1976 TSCA with the Frank R. Lautenberg Chemical Safety for the 21st Century Act. A number of features substantially improve the TSCA, but only if it is administered well, and in the spirit and letter of the law.

The EPA (2016) must affirmatively find "a new chemical or significant new use of an existing chemical [safe] before it is allowed into the marketplace, . . . [taking into account] risks to susceptible and highly exposed populations [these may include infants, pregnant women, children, and workers], and [ensuring] a substance does not pose an 'unreasonable risk.'" The EPA can more easily demand missing scientific evidence than under the 1976 TSCA. And there are mandatory reviews of chemicals active in commerce with judicially enforceable deadlines to increase safety assessments. Yet expediting the review of thirty thousand or more existing products will be a monumental task, taking decades and likely vastly longer (Cranor 2017, 155–156).

The success of this law will depend on how it is administered. Will new chemicals be properly reviewed for safety—supported by good toxicity testing with in-depth review—or only receive a cursory review with little to no testing? The preliminary bad news is that cursory reviews are being utilized. Since January 2017, the EPA has approved six hundred substances for commercialization—an unheard-of rate, and quite different from reviews of pharmaceuticals and pesticides. Did the agency have sufficient data on each substance so that it could make a well-informed assessment of its toxicity, and did it carefully evaluate each product for its safety? These possibilities seem unlikely (Cranor 2017, 147–148).

For existing products, even if the EPA could conduct risk assessments and improve health protections for twenty existing substances per year, an utterly astounding rate, it would take fifteen hundred years to review the likely thirty thousand "active" commercial substances meriting review. The history of EPA actions and industry intransigence, however, raises concerns about the likely success of these requirements. At the legally mandated rate of six to seven years per twenty substances, the legacy chemicals that have already entered commerce from the 1976 TSCA will exist for centuries.

That's the bad news about the Lautenberg Act. It appears to be poorly administered or actively resisted by its administrators to minimize interference with the chemical industry and its markets. Yet there are minimal elements of good news: environmental health advocates can rely on actual language in the law to possibly improve its functions or support legal suits to enforce the law, and when a new presidential administration is in charge that seeks to protect the public's health, there appears to be much in the law to facilitate its actions.

Toxicity testing before commercialization likely will increase the costs of some chemical products, but studies from the European Union suggest that these testing costs will be quite minimal for both individuals (should they have to pay an annual tax for testing) and companies (as judged by a percentage of their annual income from chemical sales). For example, a high-end cost estimate of testing thirty thousand chemicals over eleven years is 3.5 billion euros total (about US$4.12 billion). This is an enormously big number. On a per capita basis, though, this amounts to about one euro per person per year for EU citizens (Ackerman 2006, 1076–1077). By way of contrast, according to a recent study for the United States, "the [*annual*] costs of lead poisoning, prenatal methylmercury exposure, childhood cancer, asthma, intellectual disability, autism, and attention deficit hyperactivity disorder [attributable to environmental exposures] were $76.6 billion in 2008" (Trasande and Liu 2011, 2). Thus if even a relatively small fraction of diseases could be prevented by premarket testing, it would be well worth the cost.

Premarket testing will also change the rate at which substances are reviewed for their toxicity. With premarket testing analogous to pharmaceuticals or pesticides in the United States or chemicals in the European Union, companies would have quite improved incentives to produce data and facilitate agency review. Instead of resisting data production and improved health protections postmarket, they would likely urge quicker agency review with premarket toxicity testing so that they could commercialize products sooner.

The recommendations of the previous few paragraphs are not a panacea, however. Companies may still be tempted to falsify tests or undermine good scientific procedures in commercializing their products. Moreover, even with the best and most conscientious premarket testing, some toxicants will slip through. As such, there will continue to be a need for postmarket follow-ups of products. Both legislators and public health agencies will need to rethink procedures for reducing risks and protecting the public from products in commerce in order to be able to act more expeditiously on toxicants that premarket testing misses.

Finally, molecular products will continue to invade humans' highly permeable bodies; the law cannot prevent this. Implementing premarket testing and agency review laws will vastly reduce ignorance about the chemical universe as well as reduce *toxicants* to which the public is exposed.

At present, contamination by toxicants and reckless contamination by substances of unknown toxicity are the problems. The law permits this, but modifying the law to require earlier and better toxicity testing of products would go a long way toward reducing the ignorance about chemical products along with the adverse health effects that currently accompany some of them.

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Framework-Centered Ignorance

8 On Knowing What One Does Not Know: Ignorance and the Aims of Research

Torsten Wilholt

1. Introduction

Most of the chapters in the present volume discuss concrete cases in which ignorance is willfully created, strengthened, or deepened by actors seeking to further their own particular interests.[1] My aim in this chapter is at first sight different. I seek to analyze the nature of ignorance and distinguish between important kinds. My approach in doing so is to focus on the role of ignorance in the search for knowledge. While ignorance can have obvious harmful aspects, the part it plays in research exposes also its constructive potential. The analysis will show that from a philosophical perspective, our ignorance ought not to be regarded as one huge, structureless absence but rather a varied realm structured by our varying abilities to articulate and pursue questions. Such an analysis will, I hope, help us to better understand the conditions under which ignorance plays out its positive or negative influence on our intellectual well-being too.

1 Early versions of this chapter were presented at the conference Agnotology: The Production, Preservation, and Management of Ignorance at the Center for Interdisciplinary Research at Bielefeld University, May 29–June 1, 2011, and the research colloquium of the Institut für Philosophie at Leibniz Universität Hannover in summer term 2012. I have benefited from participants' feedback on both occasions as well as from individual conversations with Nikolaj Nottelmann and Angela Matthies (who contributed the sneezing example in this chapter). I am grateful for Paul Hoyningen-Huene's constructive and productive engagement with some of the ideas in this chapter. Finally, I consider myself fortunate to have made the acquaintance of Sylvain Bromberger (1924–2018) toward the end of the year 2015, if only via email. His kind and helpful messages to me displayed a mix of brilliance, wit, and intellectual humility, and have left a lasting impression. This chapter is dedicated to him.

One of the few quite general and relatively uncontroversial things that can be said about the aims of scientific research is that they are usually related to the goal of overcoming ignorance. In fact, instead of saying too much about the aims of research, the problem of this way of characterizing the overarching aim of science is that it says far too little. Our ignorance is, as has often been observed, enormous, and the really tricky problem with analyzing the aims of research is to find out how we identify those bits of ignorance that will be tackled next—or in the normative version of the question, how we *should* identify them.

In order to be able to select a particular area of ignorance and make it the target of inquiry, one first has to be aware of one's own lack of knowledge in this particular area. Our awareness of our own ignorance therefore plays a role in guiding our efforts to gain new knowledge. My aim in what follows is to explore this connection between ignorance and the aims of research.

I will take for granted not only that science typically *aims* to overcome ignorance but also that it often *succeeds*. In making this assumption, I may run the risk of being contradicted by a wide range of scholars from philosophy and sociology who have argued that whenever our knowledge expands, our ignorance grows along with it (see, for example, Mittelstraß 1998, 4; Krohn 2001, 8141; Wehling 2006, 83). This claim is sometimes explained by referring to the new questions that arise with every new item of scientific knowledge; sometimes it is also illustrated by the many uncertainties that are brought into the world with new technologies whose development often goes hand in hand with the creation of knowledge.

Yet something about the statement that extending our knowledge always or typically adds to our ignorance seems wrong or even paradoxical. (In fact, sociologists refer to it as a "well-known paradox"; cf. Gross 2007, 743.) The openly paradoxical nature of the claim makes it hard to dispute because, as Bernard Williams (2008, 5) once pointed out, in order to criticize an assertion that is articulated as a paradox, you will first of all have to point out that the paradox is paradoxical and hence cannot be true—which was obvious all along and therefore is going to make you look like someone who has missed the point of a joke. I think that Anna Leuschner (2012, 100), however, has managed to avoid creating this impression and pinpoints the problem well. She cites the example of our ignorance of the exact contribution of cloud formation to the world climate. This ignorance, Leuschner argues, was not *created* by climate research. Our Stone Age ancestors were no less ignorant about this than we are. What is decisive, she concludes, is that every new piece of knowledge creates *awareness* of a new finite subset of the infinite set of things that one does not know.

I want to bring out this point by identifying *conscious ignorance* as a subset of our *total ignorance*. Total ignorance is just the complement of knowledge. But what we know we don't know—or conscious ignorance, in short—makes up only a small part of this infinity. The rest, what we don't know we don't know, I will call *opaque ignorance*. Distinctions like these have, of course, been made before—from Jerome Ravetz's (1993) definition of "ignorance squared" or "ignorance of ignorance," to Donald Rumsfeld's much-debated rhetoric of "known unknowns" and "unknown unknowns" (cf. Daase and Kessler 2007). Paying explicit attention to the distinction can be helpful to agnotology, or so I will try to demonstrate.

For a start, it helps to avoid paradox. What the many references to our growing ignorance allude to is an increase in our conscious ignorance, not our total ignorance. An increase in our total ignorance is not impossible, but it can only be brought about by destroying knowledge. Whenever we forget something, or erroneously dismiss a thing that we had previously known, our total ignorance grows. But obviously that is not what those who speak of an increase in our ignorance have in mind. Climate research did not destroy any previous piece of knowledge on the connection between cloud formation and mean temperatures, but it brought the respective lack of knowledge to our awareness, thereby creating new conscious ignorance.

The claim that research is aimed at overcoming ignorance can now also be made more precise and differentiated into two claims that operate on different levels. On the level of the individual research project, each such endeavor is targeted at a more or less well-described area of ignorance, and aims to convert it into knowledge. This idea—that research is always *directed* at something—automatically means that only *conscious* ignorance can play a role for the goals of research. Perhaps Francis Bacon once thought that the inductive sciences always needed to start from a clean slate, or perhaps this is too simplistic a reading of even Bacon's radical brand of inductivism. In any case, postpositivistic philosophers, historians, and sociologists of science alike have stressed the fact that inquiry is always laden with theoretical preconceptions of the area into which it advances. Science does not forage into the vastness of opaque ignorance without any sense of direction or orientation. Occasional spells of serendipity, like Wilhelm Conrad

Röntgen's discovery of X-rays, or Hans Christian Ørsted's discovery of electromagnetism, do nothing to disprove this insight, as two kinds of consideration should make clear. First, the claim is only that research always needs to be *directed* at a piece of conscious ignorance, not that what is eventually found is always identical to what one has been looking for. And second, even serendipitous discovery requires a prior awareness of the possibility of a causal nexus, however tentative. As Louis Pasteur (1939, 131) famously remarked, "Chance favors only the prepared mind."

Whenever science succeeds in converting a given area of conscious ignorance into knowledge, new conscious ignorance is almost inevitably created. This might at first sight seem to undermine a claim that operates on a more global level—namely, that the central institutional aim of science is to reduce our ignorance. If we understand that the global claim refers to a reduction of our *total* ignorance and not our conscious ignorance, however, the inconsistency dissolves. An increase in our conscious ignorance can even be regarded as a first and necessary step toward the goal of reducing our total ignorance if, as I have just suggested, it is only conscious ignorance that can be intentionally targeted by inquiry. This means that opaque ignorance must usually first turn into conscious ignorance before it can be converted into knowledge.[2]

2. Conscious Ignorance

The notion of conscious ignorance thus seems to be of crucial importance. How exactly should it be understood? So far it has only been defined in a rather vague manner and might even seem to be beset with a few paradoxes of its own. What could it mean to be conscious of a particular piece of one's own ignorance? What could it mean to target one's epistemic efforts at something that one does not know? These quandaries may be understood as varieties of the famous question that Meno asks Socrates: "And how will you inquire into a thing, Socrates, when you are wholly ignorant of what it is? What sort of thing among those you don't know will you set up as the

2 Our total ignorance can only be "reduced" in a loose sense because it will always be infinite, and the chunks of it that we convert into knowledge are not large enough to diminish its cardinality. The sense in which one might nonetheless speak of a reduction is that, if J_t and $J_{t+\Delta}$ signify our total ignorance at different points in time, $J_{t+\Delta} \subsetneq J_t$.

object of your inquiry?" (Plato 1984a, 80d). A certain understanding of this question is pertinent to our present topic. If I conceive of a particular piece of ignorance as an item of nonknowledge—that is, a true proposition that I do not yet know—then it seems that I would only be able to direct my epistemic efforts at such an item if I already knew it—and knew it to be a true proposition.

What this shows is that our conscious ignorance in the present sense cannot be understood as a set of true propositions lying out there, waiting to be discovered. Instead, our conscious ignorance is best understood as a set of *questions*. While we ultimately want to *find* correct answers to questions and correct answers are true propositions, we *aim* our efforts at questions.

Nikolaj Nottelmann (2016, 45–46) has taken issue with this line of reasoning; he finds my focus on questions "excessive."[3] Nottelmann argues that while it may be impossible to self-ascribe ignorance by means of direct reference to the fact that one is ignorant of (as in "I do not know that my keys are on the dining table"), this only concerns the way in which first-person *ascriptions* of ignorance must be made, and does not in any way detract from the fact that every instance of ignorance is constituted by a fact (or set of facts) that the subject does not know. For the purposes of the present chapter, I might rest content with the claim (acknowledged by Nottelmann) that self-ascriptions of ignorance in the form of questions are of particular importance in the specific context that concerns me here (that is, conscious ignorance as it informs the aims of inquiry), which is inevitably bound to the first-person perspective. But my disagreement with Nottelmann's objection runs a little deeper. While I concede that every episode of ignorance involves a subject not knowing a fact (or set of facts), I doubt that it is the most helpful analysis to *identify* each episode of conscious ignorance with the subject not knowing a certain fact or set of facts. Suppose that Tom does not know to whom, if anyone, Sue is married, and also does not know to whom, if anyone, Sally is married, and as a matter of fact, Sue and Sally are married to each other. Do the two ignorance ascriptions really refer to one and the same piece of Tom's ignorance, as the identification of ignorance with unknown facts would suggest? Nottelmann might insist

3 Nottelmann refers to an earlier manuscript version of the present paper. I have left the passages to which he refers unchanged.

they do, and that the two ascriptions differ not with regard to the bits of Tom's ignorance that they pick out but only in the ways in which they pick out one and the same bit. I am somewhat doubtful that there is a fact of the matter grounding the correct answer to the question of whether ignorance "really is" constituted by facts or questions, and I suppose one could adopt either of these ways of speaking. But if ignorance is to be something that can motivate an effort of discovery, that one can direct inquiry at and be aware of in oneself even before one has gained the knowledge that cancels it out, then the latter way of speaking is not merely of particular importance but rather indispensible for coming to a straightforward analysis of ignorance that facilitates this perspective.[4]

In fact, as we will see, awareness of a question does not even necessarily presuppose awareness of *potential* answers to that question. So what *does* it presuppose? What are the conditions under which a person *P* can be said to be consciously ignorant with regard to question *Q*? Sylvain Bromberger (1992, 129–131, 147) has pointed out that the first and foremost presupposition that is relevant in this context is what he calls *representation*: *P* must be able to formulate *Q* in a language that *P* is competent in. From the early 1960s on, Bromberger was one of the few philosophers who approached the philosophy of science from an ordinary language perspective.[5] He is

4 One *might* also develop a perspective on conscious ignorance based on a factual conception of ignorance. What I mean by saying that such an analysis would not be straightforward can once more be illustrated with the example of Tom, Sue, and Sally. Under a factual conception, to discuss Tom's awareness of his own ignorance, one would have to say that the ignorance that Tom is aware of in case of the first proposition is the same ignorance as the one that he is aware of in the case of second proposition. Possessing awareness in each of these respects, one would have to continue, does not presuppose being aware that one is aware of one and the same item of ignorance in both instances, just as one can be aware of the evening star and morning star without being aware that they are one and the same thing. Yet this seems to me to posit that there is something that Tom is missing about his own ignorance in both cases (as in comparison, the underinformed stargazer is surely missing something about the evening star and the morning star). I would submit that Tom's awareness of his own ignorance in the proposed scenario need not in any way be considered incomplete or lacking. He is missing the correct *answers* to his questions (as he well knows), but there does not seem to be anything that is lacking *in his awareness of his ignorance*.

5 Two others were Stephen Toulmin and Michael Scriven.

probably most widely remembered for having devised a famous counter-example to the covering law model of scientific explanation—the story of the flagpole and shadow. I will shortly return to some of his contributions and hope to demonstrate that he deserves to be rediscovered as a thought-provoking theorist of ignorance.

3. Conditions of Conscious Ignorance

For now, I will follow Bromberger's suggestion to take the representation of a question seriously as an important precondition of conscious ignorance:

(1) *P* is able to articulate or at least understand *Q* and is aware of *Q*.

I take it that *P* need not necessarily be able to find their own articulation of *Q* in order to be consciously ignorant with respect to *Q*, but that understanding some articulation of *Q* is sufficient. In any case, our Stone Age ancestors' ignorance in Leuschner's example qualifies as opaque because for all we know, they fail to meet even this first precondition of conscious ignorance. As a second condition, *P* will obviously only count as ignorant if they fail to have the relevant knowledge:

(2) *P* does not know a correct answer to *Q*.

P's failure to know a correct answer will only count as an epistemic shortcoming, however, if *Q* admits of a correct answer in the first place. We understand the question, "Why does yellow bile cause warm diseases, while phlegm causes cold ones?" We do not know a correct answer to it, and yet we would not count this as an item of ignorance. This observation prompts the addition of a third condition:

(3) *Q* is sound (that is, possesses a correct answer).

Conditions (1) through (3) describe what it means to be conscious of a question—condition (1)—and ignorant with regard to it—conditions (2) and (3). Yet consciousness plus ignorance does not always amount to conscious ignorance. Consider the question of whether we would describe Christopher Columbus as consciously ignorant with regard to the question, "What is the distance between the Canary Islands and Japan?" To the best of our knowledge, Columbus satisfies conditions (1) through (3): He *did* articulate the question, the question has a correct answer, and Columbus was without a doubt blissfully ignorant of that correct answer, as the distance is almost five times as great as Columbus believed it to be. While he was aware of the question and ignorant of its correct answer, though, he was not aware *of* his own ignorance, for he thought he knew the correct answer to a good degree of approximation. A condition needs to be added:

(4) *P* believes that *P* does not know a correct answer to *Q*.

Columbus was not consciously ignorant of the correct distance, because condition (4) was not fulfilled in his case. In addition, conscious ignorance requires the awareness that the question does have a correct answer. For example, we would not describe Isaac Newton as consciously ignorant with regard to the question of how light waves are propagated. He was aware of the question, he believed that he knew no correct answer, but he mistakenly thought that this was because there *was* no correct answer. We should therefore add one last condition for conscious ignorance (cf. Bromberger 1992, 131–133):

(5) *P* regards *Q* as sound.

One insight that this analysis of conscious ignorance immediately facilitates is that conscious ignorance is quite a demanding affair, as Bromberger (1992, 131, cf. 31) already observed. Ignorance in this sense is not just an absence or negativity. It is rather a complex ability, and can often require quite an amount of knowledge and competence.

The fact that conscious ignorance can be an achievement and important first step on the way to knowledge is emphasized by Socrates in the *Meno*. Frustrated by Socrates's refutations of his repeated attempts at defining virtue, an angry Meno accuses him of causing only perplexity in others, thus "benumbing" his interlocutors (Plato 1984a, 80a). (Plato also has Meno mention Socrates's notoriety for this among his fellow Athenians. He thus calls attention to the fact that pointing out ignorance can be inconvenient and unwelcome, and thereby alludes to Socrates's later fate.)[6] But only a little while later, the famous slave boy scene gives Socrates occasion to defend himself. Having just helped the slave boy realize that his initial idea on how to double the area of a square is mistaken, Socrates remarks to

6 Later in the same dialogue, the dark foreshadowing is continued by the appearance of Anytus, who, as Plato's Athenian readers would have been aware, would become a primary prosecutor in the trial of Socrates only three years after the dialogue's fictitious date.

Meno, "We have at any rate done something, it seems, to help him discover how things are, for in his present condition of ignorance, he will gladly inquire into the matter." Meno agrees that in this case, "numbing befitted" the slave boy (84b–c). Let us call the achievement of conscious ignorance as a first step toward knowledge "Socratic progress."[7]

Not everyone who dons the mantle of Socrates, however, is interested in enlightenment and the growth of knowledge. The path between knowledge and ignorance can be traveled in both directions. You count as promoting Socratic progress only if you introduce conscious ignorance in order to replace error or other forms of opaque ignorance. If you do it with the aim of replacing knowledge or preventing its generation, you are but an ignorancemonger. While telling whether you are one or the other may therefore often be a difficult and controversial matter (as the question of whether the established belief that you are replacing by conscious ignorance constitutes error or knowledge is typically itself disputed), it is by no means impossible. Perhaps most significantly, an advocate of Socratic progress can be expected to be interested in continuing the process further beyond ignorance and in the direction of knowledge, and not treat ignorance itself as the goal. (A dead giveaway is therefore when you write up an internal memo that declares, "Doubt is our product" [cf. Michaels 2008, 11; Oreskes and Conway 2010, 34].)

While our conscious ignorance is limited as a result of its dependence on certain cognitive preconditions, it is still vast. (As most of us are aware that an infinite number of questions of the form "Is *n* a prime number?" can meaningfully be asked, one might argue that even our *conscious* ignorance is infinite in extension.) Obviously, belonging to the realm of conscious ignorance is only a *necessary* condition for becoming the target of inquiry. The question thus arises whether one can find further characteristics that help to distinguish between different kinds of conscious ignorance. Are there perhaps kinds of conscious ignorance that are more likely to be targeted by research than others?

To approach the matter of relevant distinctions within the body of our conscious ignorance, let me start by giving you a small sample of the immensity that is my personal ignorance. (As a side effect, this will give me

7 Socratic progress is, of course, a recurrent topic in Plato's dialogues; see in particular the *Apology* (1984b), 21a–e.

the opportunity to prove my outstanding qualifications for writing on the topic of ignorance.)

4. Wilholt, Philosopher of Great Ignorance

(i) I do not know the distance between Calgary and Miami.
(ii) The roof of Bielefeld University's main building is covered with pebbles. I do not know their number.
(iii) I do not know what it is that sometimes makes people sneeze when they look into the sun.
(iv) I do not know why ice remains slippery at temperatures way below its melting point, even if you (attempt to) stand still on it and thus do not produce any heat by means of friction.[8]
(v) I do not know whether it is physically possible for there to be a transuranium element with a half-life of a day or more.
(vi) I do not know whether any person at this moment knows a correct answer to any of the questions involved in (ii)–(v).

Some differences between these six items of ignorance are fairly obvious. Knowing the distance between Calgary and Miami has so far never been of any practical value to me. But this kind of knowledge is of obvious practical value to some people, which is why I consider it safe to assume that there are people who do not share my ignorance with regard to item (i). In contrast, in case (ii), I cannot easily imagine who might have a practical interest in the correct answer. I do not expect that anyone will ever know it, nor that this piece of ignorance will ever bother anyone. With regard to item (v), whether or not the correct answer to the respective question is of any practical use to anyone depends, among other things, on what the correct answer is. Some people have speculated that stable transuranium elements would have properties that make them interesting for practical uses, but this would, of course, only confer significant practical value on the correct

8 The common lore is that a thin film of water between one's soles (or skates) and the ice accounts for the slipperiness. But how can such a film remain liquid at temperatures of, for example, –20°C? Water ice can in principle be melted by means of pressure (because the density of water near the melting point is higher than the density of ice), but the pressure that your body weight exerts on the ice will lower its melting point by at most a few degrees.

answer if the answer is an affirmative one.[9] In a similar fashion, the practical value of the knowledge that would cancel out my ignorance in cases (iii) and (iv) depends on what exactly the correct answers to the questions are. The practical benefits of converting ignorance into knowledge therefore vary greatly among the questions that make up our conscious ignorance. This is evidently of great importance for the selection of targets for research. How great the practical value will be is, however, often itself an item of ignorance. While our research agenda is shaped by questions, practical value accrues only to answers. In many cases, the value of the answer can only be known after it has been found. This is one of the reasons why the selection of research topics on the grounds of practical considerations typically involves decision making under ignorance, and cannot be regarded as a simple and straightforward matter of maximizing expected value.

Variations in practical usefulness, though, are not the only differences that stand out when we examine this little sample of my conscious ignorance. I do not expect that the answers to the questions in (iii) and (iv) would be of any more practical use *to me* than the answers to (i) or (ii); nevertheless, these items of ignorance exert an attraction on me that the others don't. The deficits in my knowledge signified by items (iii) and (iv) seem to be more glaring than the others, and as such, represent a deeper kind of ignorance. It's not just that I lack a piece of information. I'm at an impasse. I'm at a loss for an answer. These descriptions of the relevant difference are, as I am aware, unsatisfying. For help, it is now high time to once more turn to Bromberger's work.

5. Bromberger, Great Philosopher of Ignorance

At the outset of his ordinary language approach to the theory of scientific explanation, Bromberger asks the question, what distinguishes those episodes that we call "explaining" from other information-giving episodes? Is it perhaps the form of the question that is being answered? Or the form of the answer given? Bromberger discards these easy answers. He thinks that there can even be two episodes where the same kind of question is asked and the same kind of answer is given in both cases, but one counts as an

9 The only practical value of possessing a correct negative answer would be that you could stop wasting time and energy on the search.

explanation and the other doesn't. Consider the following example, given by Bromberger himself in his classic essay "An Approach to Explanation" from 1962 (reprinted in Bromberger 1992, 18–51). Prisoner Tom escapes from his prison by digging a tunnel. After his escape, the tunnel is discovered, as are the tools and devices that Tom used to dig the tunnel, hide its entrance, and so on. But one question leaves the prison guards stumped: How did Tom get rid of the dirt that he had to remove from the tunnel? In a second episode, John escapes from a different prison, also by digging a tunnel. Everything is similar to Tom's case, except that in John's former prison, the guards realize that there are two obvious ways John might have used to dispose of the dirt: either by dropping it into a moat or onto a patch of garden. Both Tom and John are later recaptured and interrogated, and both reveal the details of their escapes, including the respective ways by which they actually disposed of the dirt from the tunneling. Now Bromberger observes that in Tom's case, we would without hesitation say that he *explained* to his jailers how he got rid of the dirt. Yet in John's case, he only reveals which one of two fairly obvious opportunities he actually chose, and so saying that he explained his method of disposing of the dirt does not sound quite right. In Bromberger's (1992, 25) own words, it seems "out of place, distorts things, smacks of exaggeration, is at best a near truth."

Bromberger's conclusion, in effect, is that whether or not a question calls for an *explanation* does not so much depend on the question's form as on the profundity of the ignorance that lies behind it. In the case of Tom's jailers, their ignorance is deeper because not only do they fail to know which is the *correct* answer to the question "How did Tom dispose of the dirt from the tunneling?" but they also do not even have any plausible *candidate* answers. Bromberger calls this state "p-predicament," formally defined as follows:

A is in a p-predicament with regard to *Q* if and only if, in *A*'s views, *Q* admits of a right answer, but *A* can think of no answer to which, in *A*'s views, there are no decisive objections.

I freely admit that I am in a p-predicament with regard to the question of why ice is slippery at –20°C, and similarly with regard to the question of what makes you sneeze when you look into the sun. To the extent that some tentative answers spring to mind at all, they all seem to be seriously flawed on closer reflection. I simply have no idea.

Take Bromberger's claim that it is in situations of profound ignorance such as these that questions call for explanations, and combine it with the truism that giving explanations is a central aim of the sciences, and you will raise at least an initial suspicion that ceteris paribus, p-predicaments should offer particularly attractive targets for scientific research.

I think that we can avoid the detour via the problematic notion of explanation altogether and give a more immediate motivation for the basic idea that deep ignorance in the sense of Bromberger's p-predicament makes for an especially alluring target of research.[10] It has frequently been noted that in the sciences, *fruitfulness* counts in favor of an idea. A good theory, for example, should, in Thomas Kuhn's words (1977, 322), "disclose new phenomena or previously unnoticed relationships among those already known." In this spirit, the sciences not only answer questions but also raise new questions about every answer they consider. That way, a network of questions and answers is created, bestowing significance on each other along the lines of the answerhood relation. (By describing things in this way, I am borrowing the forceful image of "significance graphs" that Philip Kitcher [2001, 63–82] has created.) Even tentative or merely potential answers can be used to raise new questions as well as establish new relations of answerhood, partial answerhood, or potential answerhood. The

10 Bromberger's identification of overcoming deep ignorance as a hallmark of *explanation* may lead to some unexpected consequences. Consider the fact that under normal circumstances, someone's p-predicament with regard to any particular question is only overcome once. Once one has learned at least one plausible candidate answer, the p-predicament is gone and will not return (unless one forgets again what one has learned, or learns of new convincing objections against it). This would mean that you can normally only receive any explanation once. It would also seem to imply that what we would usually call a better, more precise, or more complete explanation of something that has previously been explained to us in a more preliminary way is really an explanation that responds to a new and different question (if it is an explanation at all). For example, Newton's explanation of why the planets are observed in the positions that are recorded in the astronomical tables is not an improved answer to the same question that Johannes Kepler's explanation had already addressed; its explanatory import must come from answering a different question ("Why conic sections?"). I will not, however, go deeper into a discussion of the merits of p-predicament as a key to explanation. What matters to me is the recognition of p-predicament as an important special case of ignorance, which is in no way affected by the aforementioned peculiarities.

buck stops only when you arrive at a question to which there are no known plausible candidate answers—a p-predicament. Overcoming such an item of deep ignorance thus holds the promise of opening up a whole range of new possibilities for questions and answers, discovering new connections, and establishing new nodes in the network of knowledge and conscious ignorance.

Before we take a step back to revisit the relations between ignorance and the aims of research, one last technical remark is in order. Like ignorance itself, also its depth in the present sense may sometimes be judged differently by the ignorant person and from a third-person perspective. Consider the case of Urbain Le Verrier when he was searching for a theretofore-undiscovered planet whose gravitational influence would explain the precession of the perihelion of Mercury's orbit. Presumably, he would have admitted his ignorance of the cause of the precession, but not that it was a deep kind of ignorance. He did, after all, think that he had a good candidate answer at hand. Objectively speaking, however, his ignorance was as deep as anyone's necessarily was before the theory of general relativity was first articulated. The *correct* answer was not on his radar, and couldn't have been. The case fits Bromberger's definition of a b-predicament. A person is in a b-predicament with regard to a question if and only if the question is sound, but the correct answer, in Bromberger's (1992, 36) words, "is beyond what the person . . . can conceive, can think of, can imagine, that is, is something that that person cannot remember, cannot excogitate, cannot compose." Episodes of information giving that cancel out someone's b-predicament also count as explaining episodes in Bromberger's view. With regard to our present topic, however—the connection between ignorance and the aims of research—our focus should be on the cases where individuals or groups are *aware* that they are at a loss for a potential answer—that is, cases of p-predicament. It is for these cases that I will reserve the label "deep ignorance."

If my earlier considerations are not altogether misplaced, then it is this variety, conscious ignorance combined with an awareness that no plausible candidate answers are available, that should hold particular attraction for researchers. Sometimes this attraction finds expression in the writings of scientists. Thus James Clerk Maxwell (1877, 245), commenting on the lack of any plausible explanation for the experimental results for heat capacity ratios (and in particular for why they failed to conform to the predictions of kinetic theory), mused that this was "likely to startle us out of our complacency, and perhaps ultimately to drive us out of all the hypotheses in which we have hitherto found refuge into that state of thorough conscious ignorance which is the prelude to every real advance in knowledge." (In fact, the anomaly could only be explained in the twentieth century and with the aid of quantum mechanics.)[11]

But once more, the reality of the sciences stubbornly refuses to yield to philosophical generalizations. Conscious deep ignorance is not always a guarantee for the sustained attention of scientists. When Alexandre Edmond Becquerel discovered the photovoltaic effect in 1839, it did attract a lot of attention from physicists. Yet after about two years, interest waned, although the causes of the effect were a matter of deep ignorance at the time. Similarly, Brownian motion, which had been described by the Dutch physician Jan Ingenhousz as early as 1784, for a long time received only scattered bouts of attention before it became a piece of evidence in the case for atomism in the early twentieth century.[12]

Even questions that represent areas of conscious deep ignorance can thus be passed over when the sciences settle their research agenda. It is easy to think of factors that may contribute to this. Success in converting deep ignorance into knowledge and understanding may be highly regarded in the sciences, but that fact will only provide motivation for tackling an item of deep ignorance if there seems to be at least a reasonable chance of success. In cases where there are no approaches for tackling the question available from the prevalent inventory of methods, the wager of nonetheless confronting the problem is less likely to be undertaken. The research agenda is not only shaped by scientists' conceptions of what seems to be rewarding but by what seems to be within reach too.

The conclusions of these reflections should therefore be articulated with care. While belonging to the realm of conscious ignorance is a necessary condition for becoming the target of research efforts, being perceived as a problem of practical value and being perceived as a deep problem in the sense that we have identified with Bromberger's aid can be considered conditions that *favor* an item's inclusion in the current research agenda.

11 Cf. Brush 2002, 121, where Maxwell's statement is also quoted.

12 Both cases have been used as examples for phenomena that remained "meaningless" for contemporaries over an extended period of time by Ian Hacking (1983, 158).

6. Beyond Normal: Kinds of Opaque Ignorance

While conscious ignorance thus comes in various degrees of profundity, opaque ignorance may on first reflection seem to consist of one uniform block of negativity. But a closer look reveals important differences between kinds of opaque ignorance. As we have seen, conscious ignorance represents a complex ability that rests on several preconditions. Accordingly, there are different ways in which subjects may *fail* to know what they do not know. In particular, it makes sense to distinguish cases in which they are unable to articulate or comprehend a question from other cases of opaque ignorance.

Consider, for example, the question:

(P) What force makes the planets stay on their orbital paths?

For a premodern physicist in the Aristotelian tradition, this question would clearly not have belonged to the realm of conscious ignorance. In the Aristotelian framework, all supralunar bodies by their nature remain in perennial circular motion. To ask for a force that effects this would have made no more sense to an Aristotelian than the question "What force keeps a baseball flying after it has left the pitcher's hand?" would make to a modern physicist. Additionally, you could argue that Aristotelian physicists would not have had the required concept of force at their disposition to articulate or comprehend question (P). Now let us stipulate that some hypothetical Aristotelians are also unaware that they do not know the correct answer to the question

(M) How many legs does the mayfly have?

(Perhaps they have been mislead into believing that the answer is four by reading the *Historia animalium*.)[13] Assume that the physicists' unawareness of their own ignorance with regard to (M) is mainly due to a lack of interest in zoological matters. Then there seems to be a decisive difference between the two items of opaque ignorance that we have ascribed to them. While their unawareness of (M) is more or less a matter of coincidence, and could easily be overcome, discovering their ignorance with regard to question (P) is a matter of near impossibility to them. If they were to manage to do so and transform (P) into an item of conscious ignorance, it would

13 Cf. Aristotle 1910, 490a33–b3, 552b17–23, where it is claimed that the mayfly (*ephemeron*) walks on four legs.

be a tremendous intellectual achievement and possibly a first step toward an episode of revolutionary science in Kuhn's sense.[14] To introduce one last label for convenience, let us use the expression "thoroughly opaque ignorance" for the kind that remains opaque to us because the background beliefs, conceptual repertoire, and styles of reasoning available to us make us either unable to articulate the respective questions or recognize them as sound.[15]

These last observations may raise a suspicion. Perhaps our reflections on ignorance and the aims of research are bound to remain restricted to what Kuhn (1993, 35–42) calls normal science as long as we concentrate too much on *conscious* ignorance and its importance for the direction of inquiry. Perhaps revolutionary science (science beyond mere "puzzle solving") can only occur at the interface of conscious and thoroughly opaque ignorance, because it essentially involves the emergence of new conceptual frameworks and thus answers to questions that could not have been articulated before.

But putting it thus could be misleading. It could be taken to suggest that revolutionary science must of necessity involve "aimless" research—research that is not directed at any question. That, however, would be a mysterious picture of revolutionary science. How can you even engage in any practice of inquiry when there is no question that you are pursuing? On the other hand, if the novel questions characteristic of revolutionary science presuppose novel concepts that are not available within the prerevolutionary paradigm, how can the process ever get started?

The solution to this puzzle is a feature of research that we have encountered already: inquiry always consists of *pursuing* a question, but what you end up *finding* is not always an answer to that same question. Besides the possibility that you may chance on an entirely unexpected empirical discovery (as in the cases of Röntgen and Ørsted mentioned above), there is a chance that you may discover that the question is not sound. In your

14 I am grateful to my colleague Paul Hoyningen-Huene for alerting me to this difference in kind between items of opaque ignorance, and its connection to the distinction between normal and revolutionary science.

15 Hacking's (2002, 159–177) arguments show that the relevant abilities do not reduce to conceptual matters but also concern the ability to recognize styles of reasoning that have a bearing on the respective questions.

attempt to rearticulate, you may initiate conceptual change. Alternatively, you may find a potential answer to the question, but in doing so realize that every potential answer to it stands in stark contradiction to established theoretical belief. This may motivate you to seek your luck in massive theoretical modifications. In either case, it is not necessary that the question you were pursuing in the first place was not articulable in the terms of the old paradigm. Here are some examples of questions that have at some point played a role in periods of massive theoretical and conceptual change: "What would we observe if we chased alongside a light beam at the speed of light?" "Under what conditions are two events that occur at different points in space simultaneous?" "What happens when two freely falling heavy bodies are connected in mid fall?" "Why does the electron in an H atom not spiral into the core, emitting radiation of greater and greater frequency?"

These questions were, I submit, understandable even before the respective episodes of revolutionary change that they are associated with had occurred. It was in taking them seriously and pursuing them (among other questions) that Albert Einstein, Galileo Galilei, and Niels Bohr encountered deep-seated problems that led them to attempt radical theoretical-conceptual adjustments. These adjustments, in turn, enabled them to pursue other, novel questions, thus opening up whole new areas of conscious ignorance that had been thoroughly opaque before. This mechanism gives a little more flesh to the bones of the idea of working "at the interface of conscious and thoroughly opaque ignorance." It also once more underlines the importance of *deep* conscious ignorance. A question that persistently appears to point to an item of deep ignorance is an anomaly in Kuhn's sense; untiringly investigating the phenomena, models, and theories surrounding it *may* one day push open the door to a region of ignorance that was hitherto thoroughly opaque.[16]

16 Some of the questions that *appear* to signify an item of deep conscious ignorance will in the process be discovered to never actually have been associated with conscious ignorance as I have defined it. That is because I have opted for a "realistic" understanding of ignorance, under which a question only counts as identifying a piece of conscious ignorance when it does *in fact* possess a correct answer (condition 3).

7. Ignorance: A Construction Manual

In summary, examining ignorance under the aspect of the aims and directions of inquiry reveals the following landscape of ignorance. One way to conceptualize ignorance is to think of it as the totality of true propositions that we do not know. While this idea, which I have termed *total ignorance*, has certain theoretical uses, I have argued (with Bromberger) that with respect to our *conscious ignorance*—the part of our ignorance that we are aware of—it is best to think of it as a set of questions rather than a set of unknown true propositions.[17] Not only does this help to avoid Meno's paradox, it enables us to identify in greater detail the preconditions of conscious ignorance. These preconditions are, in short, the ability to articulate a question, recognize it as sound, and acknowledge one's own ignorance of the correct answer.

Conscious ignorance thus always presupposes certain capacities. If and when it replaces error or other forms of opaque ignorance, reaching the state of conscious ignorance constitutes an epistemic achievement—often a

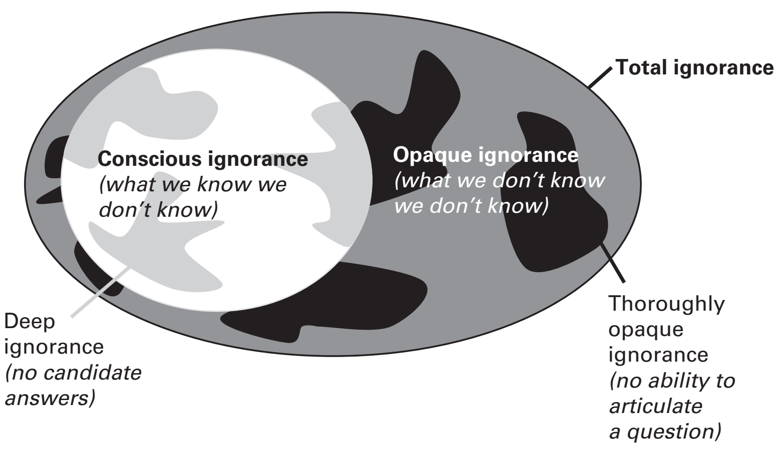

Figure 8.1
The landscape of ignorance.

17 For the purposes of making sense of figure 8.1, which represents conscious ignorance as a subset of total ignorance, we may nonetheless think of every question that belongs to our conscious ignorance as represented in that diagram by its correct answer.

quite considerable one—which I have proposed to call "Socratic progress." Socratic progress is frequently a crucial step on the way from opaque ignorance to knowledge.

Not all conscious ignorance is of the same kind. We should distinguish cases where we can identify a range of possible answers and merely do not know which of them is the correct one from cases of *deep ignorance*, where we do not even know any candidate answers yet (or none to which there do not seem to be immediate and decisive objections). These particularly challenging pieces of ignorance promise to also be particularly rewarding once the riddle has been solved, as they may potentially open up new possibilities for questions and answers, and unblock new paths of inquiry. (While deep ignorance thus marks a critical subspecies of conscious ignorance, it is important to keep in mind that other differences among items of conscious ignorance, such as differences regarding expected practical usefulness, are equally influential with respect to the aims and directions of research.)

The things we do not know without being aware of it constitute our *opaque ignorance*. Here too, different kinds can be distinguished. In some cases, we *could* easily become aware of our ignorance; it would "only" be a matter of directing our attention to the respective questions. But some items of opaque ignorance, the ones I have termed *thoroughly opaque*, are not straightforwardly accessible to us. We lack the conceptual resources, styles of reasoning, and/or background beliefs to articulate (and recognize as sound) questions that target them. Before thoroughly opaque ignorance can be replaced by knowledge, conceptual-theoretical transformations must precede. Those may come as a result of research that is targeted at conscious deep ignorance.

Is there therefore an intimate connection between deep ignorance, on the one hand, and thoroughly opaque ignorance, on the other? Again, one should answer with care. While persistent deep ignorance *can* be an indicator of the kind of anomaly that will one day lead to a crisis of the existing paradigm, it need not necessarily be so. Our inability to come up with plausible candidate answers may have other causes than the inadequacy of our conceptual-theoretical framework, such as undiscovered problems with the experimental apparatus, insufficient computational power, or pure lack of imagination, to name just a few. And on the other side, deep ignorance has no monopoly on crisis-initiating potential. Questions to which we *do* have candidate answers can turn out to resist every attempt to settle on the correct one and grow into anomalies. (The most straightforward examples of this are probably dichotomous questions like "Are electrons particles or waves?")

I opened this chapter with the apparent platitude that science aims at overcoming ignorance. Let me close with a few reflections on activities pursuing the opposite objective. Imagine that ignorance with regard to a particular point is what you *want*. What would all this mean to you then?

Obviously, if you want *X* to be ignorant about *Q*, where *X* can be an individual or collective, and *Q* is some question, you have to prevent or undermine *X*'s knowledge of the correct answer to *Q*. The most practical way of doing so is to prevent or undermine *X*'s *belief* in the correct answer.[18] But our reflections on different kinds of ignorance suggest that you can do better than that. An item of ignorance that you have created or are trying to protect may still be under threat of becoming the target of someone's inquiry. Better still than just preventing knowledge is preventing conscious ignorance as well.

Of the necessary conditions of conscious ignorance that we have discussed, three seem to present practical avenues to preventing it (while upholding the ignorance, of course). First of all, you can prevent *X*'s conscious ignorance by undermining their ability to even articulate *Q* (that is, you can target condition 1). A variety of this strategy can be seen in some cases of strict and thorough secrecy. When the military authorities classified early predictions of global warming in the early 1940s, they in effect prevented the public from even articulating questions about it (cf. Proctor 2008, 19). Alternatively, if you happen to be invested with legislative powers, you might even forbid some questions to be articulated. This is arguably what happened to John Phillips, a student of Freeman Dyson's, who in 1977 presented a term paper on the possible construction of an atomic

18 Two other ways of preventing knowledge are undermining the truth of a belief, or undermining the belief's justification or warrant. Using the first method is possible in *some* circumstances, such as if someone knows where your diary is hidden and you undermine their knowledge by simply putting it elsewhere. It is, I believe, not an option in *most* real-life cases where someone has a vital interest in preventing or undermining knowledge. The second way does not play a role in such cases either, because if someone's knowledge would pose a threat to your interests, then so would their true belief alone. In such circumstances, undermining warrant or justification only makes sense as a means of undermining belief.

bomb, using only publicly available sources of information. His term paper was confiscated by the FBI on account of an alleged infringement of the Atomic Energy Act.

If you can't prevent people from articulating a question, perhaps you can prevent them from considering the question sound (condition 5). This is a second avenue for preventing conscious ignorance. To give a fairly simple example, the question "What are the causes of global warming?" can be erased from a group's conscious ignorance if you can get that group to believe that global warming isn't taking place at all. Under that condition, questions regarding its causes will not even appear sound to this group, and the group's ignorance in this respect may thus become particularly stable—as long as you can uphold their disbelief in the question's presupposition.

Finally, let us not forget that a necessary precondition for *X*'s conscious ignorance with regard to *Q* is also that *X* believes that they do *not* know the correct answer to *Q* (condition 4). A useful way of undermining knowledge and preventing conscious ignorance at the same time is therefore to introduce error. Many cases of lying and deception in the history of knowledge testify to the effectiveness of this strategy.

It may be the case that none of these options are available to you, and the ignorance you have created is bound to remain glaringly conscious. If this is so, you should at least try to avoid the impression that the ignorance is deep—for the deep kind attracts attention. You should thus always provide some candidate answers that remain available when you have successfully undermined belief in the true answer. An example for this strategy might be the tobacco industry's efforts to establish the idea of a "sick building syndrome" in order to provide at least a candidate answer to the question of what causes headaches and other health problems among workers in smoky offices (cf. Oreskes and Conway 2010, 140). Similarly, when the manufacturers of vinyl chloride found themselves confronted with a suspiciously high number of brain cancer deaths among their workers, industry-sponsored scientists came up with the concept of "diagnostic sensitivity bias," arguing that perhaps brain tumors are more likely to be diagnosed among workers in the chemical industry than among the general population (cf. Wilholt 2009, 93). What's striking is that such alternative answers already do some work even without any efforts to establish the belief that they are the *correct* answers, or even likely to be the correct answers. The mere presence of a candidate answer that is not yet refuted lifts the pressure of deep ignorance from the question.

Now who said that the philosophy of science doesn't have some practical applications! But alas, the ignorancemongers seem to know all the good tricks already. In that case, I can only hope that the joint efforts to understand the dynamics between ignorance and the search for knowledge that the editors of the present volume have brought together will make it a little easier to expose their schemes.

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9 Strong Incommensurability and Deeply Opaque Ignorance

Paul Hoyningen-Huene

The aim of this chapter is to investigate the possible connections between deeply opaque ignorance and incommensurability (I will later explain both concepts in detail). More specifically, does incommensurability imply deeply opaque ignorance? The answer to this question will shed some light on our epistemic situation in general—namely, whether deeply opaque ignorance is the situation in which we normally find ourselves in the sciences. I should mention at the outset that this is a *theoretical* philosophy of science chapter. It therefore does not belong to critical political philosophy of science (from where much of recent agnotology research springs), nor to logic or epistemology. I will first explicate my core concepts, opaque ignorance and deeply opaque ignorance, and then incommensurability and strong incommensurability. In a second step, I will formulate a hypothesis about the connection between these concepts. In a third step, I will present three case studies from the history of science, one from physics and two from medicine, in order to illustrate the concepts and make the hypotheses plausible. In physics, I will discuss the situation of classical mechanics and precession of the perihelion of Mercury, and in medicine I will discuss the emergence of virus research, on the one hand, and prion research, on the other. In the end, I will draw some tentative conclusions about our epistemic situation in the sciences in general.

1. Kinds of Ignorance

As I will later discuss specific kinds of ignorance, it is worthwhile to first look at the general concept of ignorance. Clearly, ignorance is the absence of knowledge. How should the concept of knowledge be understood here, however? Should knowledge be understood, as mostly in epistemology, as justified true belief, and hence ignorance as the absence of justified true belief? Then my discussion of ignorance would only make sense if the existing sciences at least sometimes definitively reach the truth (otherwise science would be in a permanent state of ignorance). As this is a highly contentious position, we should not frame our exploration in these terms. We should rather use a weaker concept of knowledge such that the sciences at least sometimes reach, in an uncontroversial sense, knowledge. This is the case if we understand "knowledge" as something like "well-established (scientific) belief." In such cases, the relevant scientific community would claim that it has presumably a stable belief about a certain domain. For instance, cosmologists "know" (in this sense) that there was a big bang some 13.77 billion years ago (within 0.5 percent).[1] Thus in the following, I mean by ignorance the absence of well-established (scientific) belief. Fortunately, it will not be necessary to worry about the details of what well-established (scientific) belief exactly is.

For the more specific variants of ignorance, I will take Torsten Wilholt's terminological suggestions on the notion of ignorance as a starting point (see chapter 8). First, I distinguish between opaque ignorance and its contrast concept of conscious ignorance. In the case of *conscious* ignorance, we know what we don't know. Conscious ignorance can always be articulated by the relevant epistemic subject in the form of a question whose answer is unknown to them. Once this question is truthfully answered and the relevant epistemic subject can understand the answer, conscious ignorance is removed. In the case of *opaque* ignorance, however, we don't know what we don't know. Opaque ignorance thus cannot be articulated by the respective epistemic subject as this articulation would presuppose the subject's awareness of it.

We can now distinguish three different kinds of opaque ignorance: superficially opaque ignorance, deeply opaque ignorance, and radically opaque ignorance, respectively (for a graphic representation of the whole set of different kinds of ignorance, see figure 9.1). The difference between these three kinds of opaque ignorance results from differences among the

1 "The WMAP science team has . . . determined the universe to be 13.77 billion years old to within a half percent." See "WMAP's Top Ten," accessed December 4, 2018, http://map.gsfc.nasa.gov/.

sources of the respective form of ignorance. Let us start with the case of *superficially opaque* ignorance, in which two conditions must obtain. First, the *question* that asks for the missing knowledge can be immediately understood by the respective epistemic subject. Second, the truthful *answer* to this question can also be immediately understood by the epistemic subject. As such, the removal of superficially opaque ignorance works smoothly by first asking the relevant question (as an open question) and thereby transforming superficially opaque ignorance into conscious ignorance, and then providing the truthful answer to the question, which the epistemic subject is able to understand immediately. Why is the epistemic subject able to understand the answer immediately? Apparently there are no obstacles for the epistemic subject to integrate this answer into their existing knowledge.

Let us make the case of superficially opaque ignorance more concrete by providing two examples that may, in addition, serve to distinguish two different forms of superficially opaque ignorance. Let us assume that Mary believes that there are six planets in our solar system. As the correct number of planets in our solar system is eight, Mary is ignorant of the correct number of planets. Further, as Mary is not aware of the falsity of her belief and hence her resulting ignorance, it is a case of opaque ignorance. Yet it is a case of superficially opaque ignorance because Mary both understands the question of how many planets there are, and will immediately understand the correct answer once we tell her of Uranus and Neptune (the two planets she probably missed). The source of her ignorance is thus her unawareness of her misinformation, and both this unawareness and her misinformation can be easily removed. In the second example of superficially opaque ignorance, let us assume that Mary is totally unaware that besides the Milky Way, there are other galaxies; she never considered the question of whether there is only one or many galaxies. She is not misinformed about this lacking knowledge as in the former example, she is rather not aware of the lack. In this case, Mary would not be able to *articulate* the question about the number of galaxies herself but also would be able to *understand* the question if posed by someone else. The source of Mary's ignorance is her total unawareness of her lack of some knowledge. Again, her ignorance can easily be removed by telling her of other galaxies, and presumably, Mary will have no difficulties in absorbing this new knowledge. Hence Mary's opaque ignorance is only superficial.

Let us now turn to the second kind of opaque ignorance, dubbed here *deeply opaque* ignorance. This kind of ignorance is characterized by the circumstance that some background concepts and beliefs make it impossible, at least temporarily, to realize that one is ignorant. This means that for a transformation of deeply opaque ignorance into conscious ignorance, the pertinent background concepts and beliefs must be suspended. This may meet the difficulty that one may not even be fully aware of these background concepts and beliefs. Here is an example. Aristotle was in a state of deeply opaque ignorance about the electromagnetic nature of light in which we believe today. In his worldview, there was no conceptual space for any kind of (unquantized or quantized) electromagnetic radiation. Aristotle (and many other ancient thinkers), however, understood the *question* about the nature of light perfectly well even if they would not understand our *answer* to this question without prior dissolution of some of their convictions.

There is a special case of deeply opaque ignorance that is even more radical; I will call this kind of ignorance *radically opaque* ignorance. In this situation, the ignorant person is incapable of even understanding the question of whose correct answer they are ignorant. Take the question of the proportion of hydrogen and oxygen in water molecules. Aristotle, for instance, would have been completely unable to understand this question because he was neither familiar with the concepts of hydrogen and oxygen, nor could he have entertained the idea of a composition of water by heterogeneous components because for him, water was elementary. I summarize the different kinds of ignorance in figure 9.1.

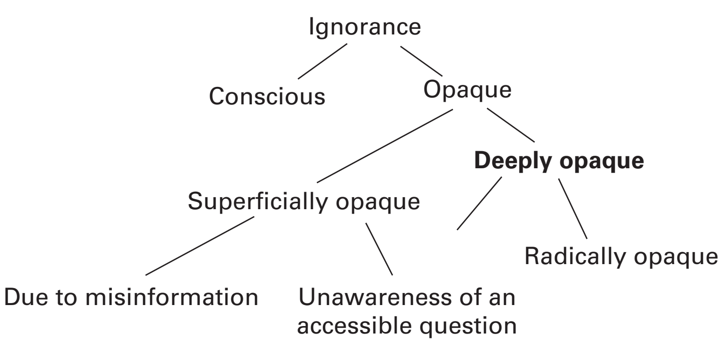

Figure 9.1
Map of ignorance.

It is worth emphasizing that states of ignorance are not necessarily permanent. As we have seen above, conscious ignorance can be removed by supplying the missing knowledge, and superficially opaque ignorance can be transformed similarly into conscious ignorance by providing the necessary background information. More important in our context, deeply opaque ignorance may be transformed into conscious ignorance, which is a necessary condition on an (scientific) engagement with the respective questions. This latter process is often described as the "discovery" of the respective question.

2. Incommensurability

For the purpose of this chapter, I will not discuss incommensurability in its most general form. Rather, I will restrict myself to what has been called semantic incommensurability.[2] *Semantic* incommensurability concerns concepts, or more precisely, two sets of concepts. The meanings of the concepts of the first set will be somewhat different from the meanings of the concepts of the second set such that a literal translation between the concepts of the two sets is not possible. To illustrate this, compare the sets of military ranks of different countries. Some ranks may correspond more or less completely, but others won't. In the latter case, what one's country's rank means cannot be literally expressed in the ranking system of the other.[3] For convenience, I will focus on Thomas Kuhn's mature articulation of incommensurability in terms of the *lexical structure* of *taxonomic kind terms*.[4]

Two lexicons of taxonomic kind terms are incommensurable in Kuhn's sense in case there is a local change in lexical structure. The structure of the lexicon is the set of relations among the respective kind terms. A change

2 The literature on incommensurability is vast. For the contrast between semantic and methodological incommensurability, see Sankey and Hoyningen-Huene 2001.

3 See, for example, the comparison of UK and US military ranks on Wikipedia, accessed December 4, 2018, http://en.wikipedia.org/wiki/Comparison_of_United_Kingdom_and_United_States_military_ranks.

4 For his first articulation of incommensurability in terms of lexical structure and taxonomic kind terms, see Kuhn 1983a, 682–683; 1983b, 713–714; 1989; 1990. For a discussion of his articulation, see Hoyningen-Huene 1993, 111, 159–160, 217–218. What I am developing in the following could also be expressed by means of Paul Feyerabend's concept of incommensurability. For the difference between the two concepts of incommensurability see Hoyningen-Huene 2005.

in lexical structure means that the taxonomy that is given by the lexicon changes. This means a regrouping of objects among extensions of kind terms (this regrouping is due to a change in the similarity/dissimilarity relations among the objects). A local change in lexical structure does not affect all kind terms in the taxonomy but rather only a small subset of them; usually this small subset consists of interrelated terms. In any particular lexicon of taxonomic kind terms, the "no overlap principle" holds. This principle states that taxonomic kind terms may not overlap, except as genus and species. This form of incommensurability leads to untranslatability (in a literal sense) between the two lexicons. It concerns many sentences that contain those kind terms that are affected by the change of lexical structure; these sentences cannot be literally translated into the vocabulary of the other lexicon.

Kuhn's standard example for a local difference between the structures of two lexicons, and hence for incommensurability, is the difference between the taxonomic kind terms that are used in the articulation of the geocentric and the heliocentric planetary system. In the geocentric system, there are two kinds of celestial bodies: fixed stars and planets. Contrary to our understanding of the term "planet," the Sun and Moon count as planets, but the Earth does not (see figure 9.2).

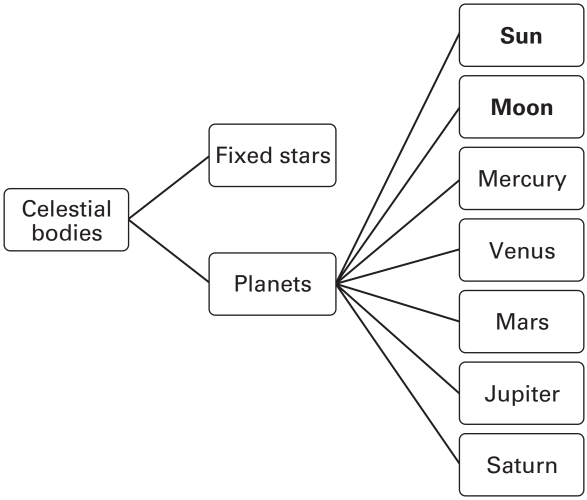

Figure 9.2
Geocentric system.

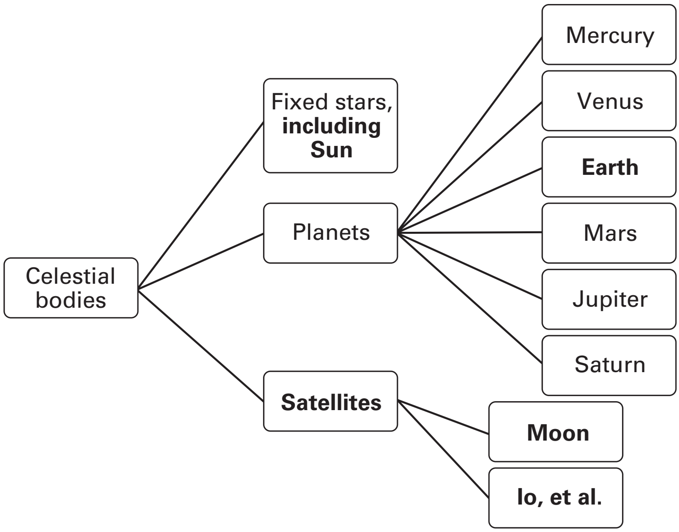

Figure 9.3
Early heliocentric system.

Three kinds of celestial bodies are now included in the early heliocentric system: the new class of satellites has been added. Furthermore, the celestial bodies that were known already in the geocentric system are now differently distributed among the different kind terms. The Sun now belongs to the fixed stars, the Earth is now a planet, and the Moon is, together with some newly discovered objects like the moons of Jupiter, a member of the new class of satellites (see figure 9.3).

So far I have only presented Kuhn's view of (semantic) incommensurability. Now I would like to draw a distinction within this kind of incommensurability that will remove a controversial issue. I want to distinguish between "milder" and "stronger" forms of (semantic) incommensurability in the following, preliminary way. In *milder* forms of incommensurability, it is comparatively easy to learn and understand the new lexicon, in spite of its structural differences with the old lexical taxonomy. In other words, it is comparatively easy to hypothetically suspend the old lexicon in order to create conceptual space for the new lexicon and then apprehend the new lexicon. In *stronger* forms of incommensurability, however, it is difficult to apprehend the alternative lexicon for two main reasons. First, in order to learn the new lexicon, one may have to suspend deeply ingrained convictions that are associated with the old lexicon. These convictions may either be so common that one is not even aware of them or may appear self-evident. In any case, it will be hard to suspend such convictions in order to create the conceptual space necessary for the new lexicon. Second, in addition, the new lexicon may be difficult to understand because it may be remote from the old lexicon, or may involve new, difficult technical material like new mathematical theories.

Clearly there is a transition area between stronger and milder forms of incommensurability; they do not form a sharp contrast.[5] Furthermore, individuals may differ in their ability to apprehend a new lexicon that is structurally different from the one they are used to (this is similar to the differences in individuals' ability to learn a second language). The strength of incommensurability in a particular situation thus may somehow vary for different speakers. As such, the distinction between these forms of incommensurability not only concerns structural differences between the lexicons themselves but also to some degree involves the abilities of the lexicon users.

The difference between milder and stronger forms of incommensurability merits further investigations, but that is beyond the scope of this chapter. I can only gesture toward two possible approaches. First, there is an approach that can roughly labeled semantic—namely, the representation of concepts by "dynamic frames." Hanne Andersen, Peter Barker, and Xiang Chen (2006) have developed this approach and applied it to incommensurability. This approach allows for different degrees of incommensurability that may well correspond to different difficulties in apprehending the new lexicon. Second, there is a psychological approach. Alexander Bird (2008, 22, 26) has described incommensurability as the result of different individuals' having different "quasi-intuitive cognitive capacities" (QICCs) that they acquired with different paradigms. These QICCs are habits that "are acquired as a result of repetitive exposure and practice. For example, what may start out as a conscious, sequential activity of reasoning, eventually becomes a one-step quasi-intuitive inference" (23). QICCs also include specific ways of analogical thinking and thinking in schemata, which are

5 This corresponds to the thesis that "incommensurability is . . . a matter of degree" (Andersen, Barker, and Chen 2006, 116; see also 128, 165).

characteristic of the respective paradigms (24–27). Given the habitual character of QICCs, it is plausible that it may be difficult to unlearn them. This, however, is the presupposition for learning a new set of QICCs that are associated with a new paradigm. Thus in terms of QICCs, one can easily understand why there are milder and stronger forms of incommensurability in the given sense, and why individuals may differ in their reactions when exposed to incommensurability. The more different the new set of QICCs is in comparison with the old set, the more difficult it will be to understand and habitually use them—that is, the stronger the resulting incommensurability will be.[6]

The crucial consequence of these two forms of incommensurability that concerns me here is the following. Mild forms of incommensurability will not usually lead to communication problems between parties across the revolutionary divide, whereas strong forms may indeed do so because of the difficulties in acquiring the new lexicon. The defenders of the new lexicon will have difficulties in making themselves understood by the proponents of the old lexicon. This resolves a long-standing puzzle about the relation of incommensurability and communication problems. Kuhn claimed that (semantic) incommensurability inevitably leads to communication problems (see, for example, Hoyningen-Huene 1993, 254–256), but for many cases, historians of science could not find indications of them (see, for example, Donovan, Laudan, and Laudan 1988, 30; Andersen, Barker, and Chen 2006, 87–91, 128, 165). Different parties divided by incommensurability disagreed, yes, but did not severely misunderstand each other. The Copernican revolutions seems to be an example of this milder form of incommensurability in which no serious communication problems emerged. Given that there are cases of revolutions with mild forms of incommensurability, Kuhn's view that revolutions inevitably involve communications problems appears to be an overgeneralization from cases with stronger forms of incommensurability. I will stop the abstract discussion at this point and provide examples of stronger incommensurability below.

6 It might also be possible to interpret this issue in terms of Daniel Kahneman's (2011) two systems of thought, or connect it with the literature on breaking away from common sense in science education (see Green 2016, 808–809).

3. Strong Incommensurability and Deeply Opaque Ignorance

In the following, I will defend the following thesis: strong incommensurability induces deeply opaque ignorance. I will first sketch three cases that illustrate strong incommensurability and at the same time make the above thesis plausible.

3.1. The Precession of the Perihelion of Mercury

The discussion of the precession of the perihelion of Mercury is part of the transition from classical physics to general relativity theory. Newtonian gravitational theory is semantically incommensurable with general relativity because of the fundamental reorganization of the conceptual web that describes gravitation. In general relativity, gravitation loses its status as a real force and becomes a part of the geometry of the given situation. Because of the strangeness of general relativity relative to classical physics, we may assume that there is strong incommensurability.

The precession of the perihelion of Mercury is an effect that was fairly accurately measured between 1843 and 1859 by the astronomer and mathematician Urbain Le Verrier. Today's value of the precession is 5599.7 arc second per century.[7] How can this effect be explained? In the hands of Le Verrier, Isaac Newton's classical gravitation theory was successful in this endeavor. There are several contributions to the total effect—first, a geometric effect due to the axial precession of the Earth, and second, the gravitational influences of the other planets on Mercury's orbit. The geometric effect represents the lion's share of the effect: 5025.6 arc second per century (this and the following numbers are today's values, not Le Verrier's, which are slightly different). The gravitational influences of the other planets depend on their distance to Mercury and their mass: the heavier a planet is, the stronger the influence, and the farther away it is, the weaker the influence. The resulting influences of the planets on Mercury's precession are as follows: Venus, 277.8"; Jupiter, 153.6"; Earth, 90.0"; Saturn, 7.3"; Mars, 2.5";

7 See, for example, Will 1993, 4. The following numbers are also taken from Will's book. Le Verrier's (1859b, 109) numbers for the contributions of the planets are in his classic *Theorie Du mouvement de Mercure*. For a standard source on the whole problem, see Roseveare 1982. For a more popular book on the subject, see Baum and Sheehan 1997.

and Uranus, 0.2". Adding up these numbers yields 5557.0 arc second per century—a figure short of 42.7" (or 0.76 percent) of the measured value.[8] One could say that this was a good result, given that the situation was complex and the calculations were quite complicated. Yet there should also be an explanation for the missing 42.7", and there were several hypotheses. There could be a yet-unknown little planet orbiting the sun within Mercury's orbit. It was given the name Vulcan, but it could never be detected; it should have been visible from the Earth. An alternative would have been a ring of planetoids within Mercury's orbit, a solar quadrupole moment, or even a quantitative deviation from Newton's inverse square law. None of these proposed explanations was totally convincing, and the nuisance of the unexplained 42.7" remained. Due to the wonderful convergence of the successive approximations to a value that was less than 0.8 percent off the correct empirical value, though, nobody was seriously worried. Nevertheless, in looking at this situation with hindsight, we can now diagnose that there was deeply opaque ignorance in the failure of classical physics at this point. As is well known, in 1916, Albert Einstein could derive the missing 43 arc seconds per century in his general relativity theory, thus making it extremely plausible that they were due to a classically inaccessible effect of general relativity.

3.2. Virus Research

In the 1870s, great progress was made in the area of infectious diseases. Mainly by Louis Pasteur in Paris and Robert Koch in Berlin, the "bacteriological paradigm" was established during these years (see Hughes 1977, 1–27; Waterson and Wilkinson 1978). It was based on the revolutionary idea that all infectious diseases are caused by living micro-organisms, called at the time "organized ferments," "germs," "microbes," "bacteria," or (unspecifically) "viruses." There was a standard technique to isolate bacteria—namely, bacterial filters. These filters were permeable to toxins but impermeable to microbes; the typical size of bacteria is 1 μm (or 1/1000 millimeter). In addition to bacteria, protozoa were identified as putative pathogens, but they are of no concern in the present context. By the end of the nineteenth century, bacteriology was a mature and well-established

8 Le Verrier's (1859a, 380) value was 38". He also discusses the possible causes of the discrepancy.

science with medical, veterinary, and plant bacteriology as specialties. The science of immunology was established at about the same time as bacteriology, initiated by Pasteur's 1880 discovery of immunization against chicken cholera.

What we today call virus research began in the late nineteenth century due to the existence of anomalies in bacteriology—namely, the existence of infectious diseases with apparently strange properties (see Hughes 1977, 29–73; Waterson and Wilkinson 1978, 23–85; Helvoort 1994). For economic reasons, two of those diseases were especially important, and their investigation was correspondingly pressing: the tobacco mosaic disease along with foot and mouth disease. It should be noted, however, that the identification of the surprising properties of such diseases was not entirely unequivocal because there were quite a few technical problems in their identification. Of course, this added to the confusion about these diseases. Here are some of their surprising properties:

- The infectious agent passed bacterial filters and was not visible in the microscope. This was distinct from the bacteria-caused diseases where the bacteria could be gained in the filter and stained, and were visible in the microscopes available at the time.
- The infectious agent could not be grown in artificial culture. By contrast, bacteria could be grown in artificial culture, and this was a methodologically extremely important procedure.
- The infectious agent could be diluted without losing infectiousness. This property is shared by all bacteria-caused diseases but not by toxin-caused diseases.
- The infectious agent was resistant to alcohol, weak formalin, and desiccation. These procedures destroyed bacteria but apparently not the new infectious agent.
- The infectivity of the infectious agent was completely destroyed by a single exposure to a temperature of 90°C. Thus the new infectious agent showed a strange instability.
- It was possible to immunize against some of the diseases. The possibility of immunization was known for some bacteria-caused diseases.

This list shows that the infectious agent responsible for these strange diseases behaved in some situations like a living bacterium, and in others

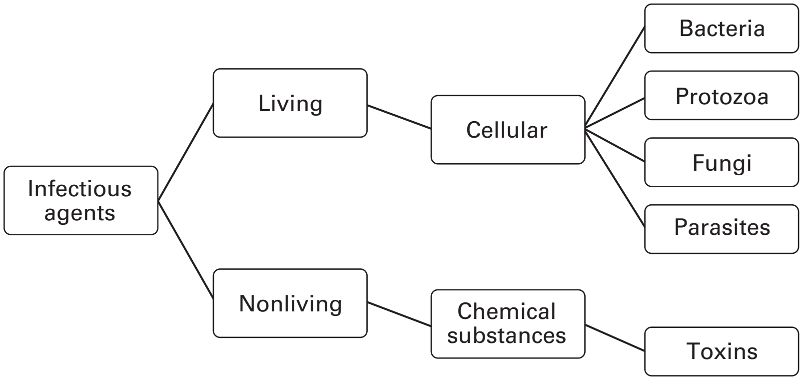

Figure 9.4
Lexicon of infectious agents, circa 1900.

like a chemical toxin. The set of these properties did not fit into any of the available categories of possible infectious agents—that is, either bacteria, protozoa, fungi, multicellular parasites, or nonreproducing chemical substances (toxins). This can be nicely seen from the lexicon of infectious agents in use around 1900 (see figure 9.4).

There were three approaches to deal with the anomalies of bacteriology: two main schools and one revolutionary yet rather desperate approach. The most conservative approach was the microbial conception: the infectious agent was a minute micro-organism, submicroscopic and filterable. Basically, it was a new class of bacteria, just much smaller than the bacteria hitherto known. Only a minor change of the lexicon was needed: just an additional class of cellular infectious agents, surprisingly submicroscopic (see figure 9.5).

The other rather conservative approach assimilated the new infectious agent not to bacteria but instead toxins. It is the nonmicrobial conception: the new infectious agent is an infectious noncellular chemical substance. This was not an extremely far-fetched idea at the time because of the flourishing enzyme research around 1900. Again, it only demanded a minor change in the lexicon—namely, the addition of a new class of toxins (see figure 9.6).

A rather revolutionary approach suggested that one should deviate from the cell theory of life. The cell theory of life was established in 1858 by Rudolf Virchow, and was seen as an integral and fundamental part of all

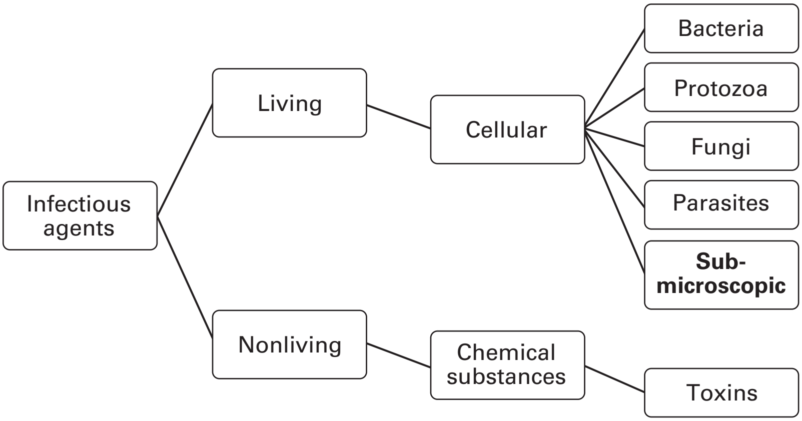

Figure 9.5
Lexicon of infectious agents, supplement A.

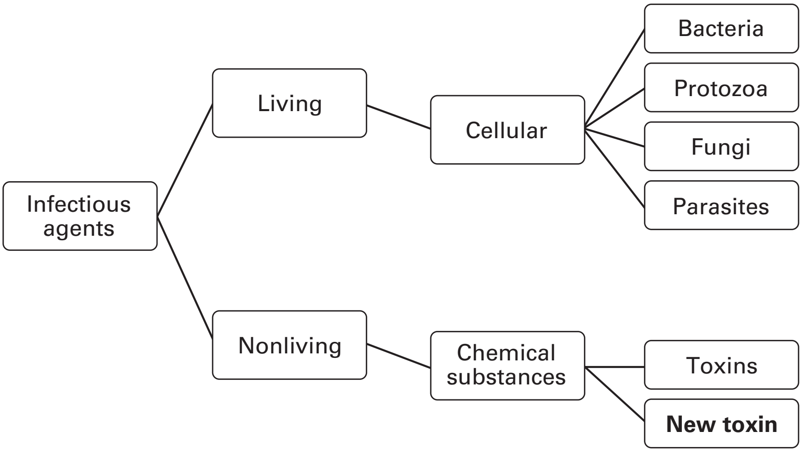

Figure 9.6
Lexicon of infectious agents, supplement B.

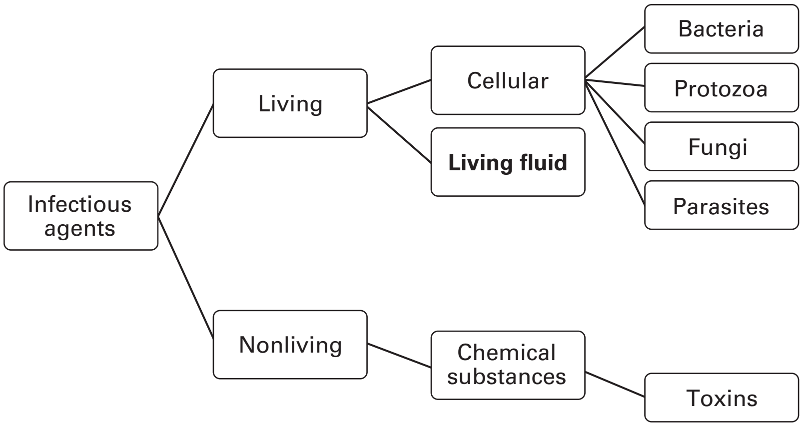

Figure 9.7
Lexicon of infectious agents, changed.

biology and medicine, including, of course, bacteriology. According to its proponents, however, the new diseases seemed to compel a deviation from the cell theory of life. They postulated that the new infectious agent was living and in fluid form—obviously an act of despair. It would have changed the existing lexicon in a revolutionary way by adding a category of infectious agents that was wholly inconsistent with all one knew about living things (see figure 9.7).

Yet as is widely known, history took a different course. It took roughly five decades of research to loosen the grip of the conceptual and therefore apparently unavoidable alternative between the living and the nonliving (Hughes 1977, 75–108; Waterson and Wilkinson 1978, 86–134; Helvoort 1994). Some important stations on this route were the following:

1915 Discovery of bacteriophages

1935 Crystallization of the tobacco mosaic virus

1939 Visualization of viruses by the electron microscope

1944 Discovery that DNA is the carrier of genetic information

1952 Discovery that the infective component of bacteriophages is DNA

1953 Discovery of the structure of DNA

The main result emerging in the 1950s was that all viruses have the same structure: there is an outer coat of (lipo)proteins, and an inner core of either

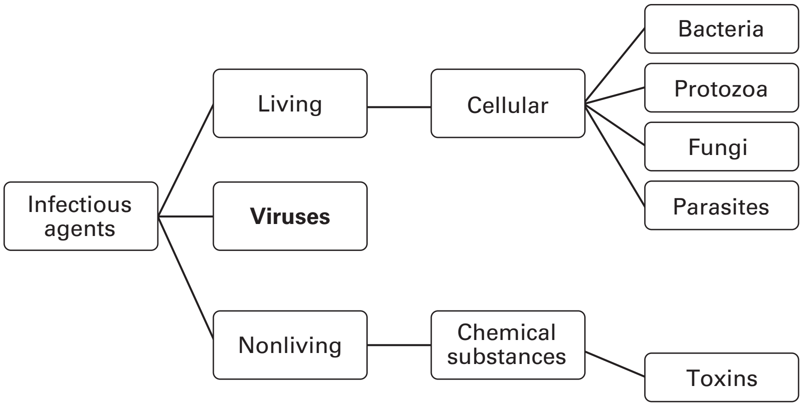

Figure 9.8
Lexicon of infectious agents, changed.

DNA or RNA. Thus today a virus is neither microbial nor nonmicrobial; it is too complex to be a macromolecule, and it is just not a "conventional" living being. The taxonomy of living and nonliving things had to be substantially changed (see figure 9.8).

In the old lexicon, there is no conceptual space for viruses: they cannot exist according to that lexicon because the alternative between living and nonliving was exclusive as well as exhaustive. This contrast between the living and nonliving precluded the conception of the virus as we know it today. Only a massive ("revolutionary") change of the framework may create the conceptual space to accommodate viruses. Given the initial history of what turned out to be virus research, there was strong incommensurability implying deeply opaque ignorance of viruses. Furthermore, the possibility of chemical substances causing infectious diseases turned out to be nonexistent: all infectious agents contain DNA or RNA, according to the general consensus about infectious diseases that held until the 1980s (see figure 9.9).

3.3. Prion Research

This brings me to my third case study, which extends the story about infectious diseases. In striking parallel to the beginning of virus research, for a number of important transmissible diseases, up to the 1980s no infectious agent could be identified (Prusiner 1982, 2004; Prusiner et al. 2004;

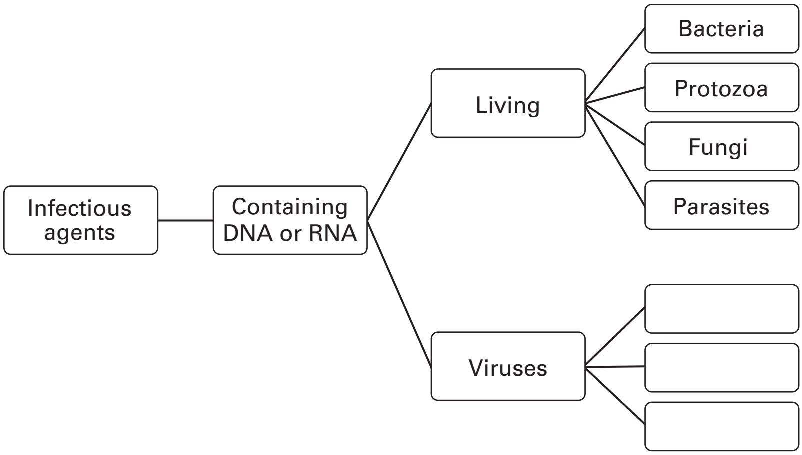

Figure 9.9
Lexicon of infectious agents, circa 1955.

Wells and Wilesmith 2004). These diseases included, among others, scrapie, which affects sheep and goats. Scrapie was first described as early as 1732. Then there was the Creutzfeldt-Jakob disease affecting humans, first described in 1920. And then there was kuru, a disease affecting humans too, first described in the 1950s in Papua New Guinea, and most important for a number of reasons, bovine spongiform encephalopathy, also called "mad cow disease," discovered in 1984.

As in the case of the virus-caused diseases, these diseases have a number of surprising properties, given the accepted paradigms of bacteria and virus-caused diseases:

- In infected organisms, there is no reaction by the immune system.
- The infectious agent is resistant to many procedures that would inactivate bacteria and viruses.
- There is a coexistence of inherited, sporadic, and infectious forms of these diseases.

Given the medical knowledge of infectious diseases in the early 1980s, it was mostly assumed that the infectious agent was a "slow virus." In fact, this appeared to be the only possible explanation: "At first, the logical conclusion was that it must be a virus. Because of the extended incubation times, these diseases were often referred to as either slow virus diseases or unconventional viral diseases" (Scott et al. 2004, 435). According to the theory of infectious diseases valid at the time, the infectious agent must contain DNA or RNA, carried by (mostly) bacteria or viruses. On the one hand, there was overwhelming empirical evidence for this claim: for practically all infectious diseases, bacteriology and virology had identified the respective bacterial or viral agents. On the other hand, there were overwhelming theoretical reasons for this claim: the infectious agent must be able to multiply in the host organism, and all conceivable mechanisms involved nucleic acids (DNA or RNA). It should be noted that in the 1980s, this claim rested on an enormously successful scientific tradition of 110 years. Stanley Prusiner (1982), however, published an article putting forward the hypothesis that the infectious agent of scrapie did not contain any nucleic acids. Instead, the infectious agent was a specific protein baptized "prion." Prion proteins were assumed to have two different forms—one normal and one disease causing. It was assumed that the protein molecules with the disease-causing form stimulate molecules with the normal form to convert into the disease-causing form, being a catalyst of sorts. This provided a novel multiplication mechanism for infectious agents.

Initially, there was massive opposition to Prusiner's hypothesis. Clearly the prion hypothesis was strongly incommensurable with the previous theory: a previously inconceivable category had to be added—namely, infectious agents devoid of nucleic acids involving a completely novel replication mechanism (see figure 9.10).

And equally clearly, the strong incommensurability of the prion hypothesis with the previous thinking about infectious diseases led to deeply opaque ignorance about the mechanism of the prion-caused diseases. Nobody could have possibly foreseen them at the time. Prusiner was awarded the Nobel Prize in 1997. Even then, some scientists believed this to be premature and Prusiner's efforts to be a case of "pathological science" because they involved "fantastic theories contrary to experience" (Rhodes 1997, 59; Green 1997).

4. Conclusion

As we have seen in this chapter, strong incommensurability induces deeply opaque ignorance at least in some interesting cases. How does this happen?

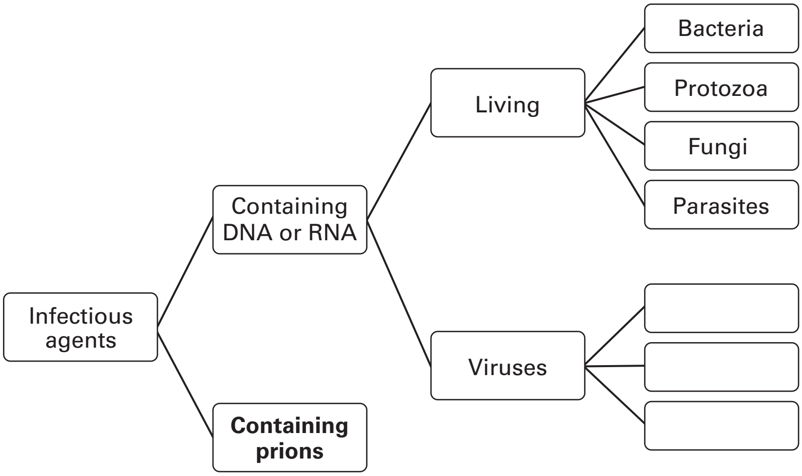

Figure 9.10
New lexicon of infectious agents.

The mechanism that generates deeply opaque ignorance is the same in all cases. Remember that a lexicon of empirical concepts has two crucial properties. First, the taxonomy of the pertinent objects is exhaustive—that is, all the pertinent objects fall under one of the concepts at any given level. Second, the no-overlap principle holds—that is, all the pertinent objects fall under exactly one of the concepts at any given level.[9] Thus the epistemic claim of any successful empirical lexicon is that its taxonomy is unambiguous and complete: every pertinent object belongs to exactly one of the categories at the lowest level, and any category is a subset of exactly one category on any higher level. Any proficient lexicon user is therefore deeply opaquely ignorant of objects that do not fit that lexical structure by, for instance, having properties that would qualify them as simultaneously belonging to two mutually exclusive categories. Such objects are not compatible with the epistemic claim of the lexicon; their

9 Kuhn (1991, 4) introduced the name "no-overlap principle." Of course, it was known much earlier that in a classification, at any given level concepts must not overlap because that would fly in the face of the very aim of a classification. Every pertinent object must belong to one and only one category at the lowest level, and a category at any given level must be covered by exactly one category at any higher level. See, for example, Immanuel Kant's *Logic*, § 111.

acceptance would require an at least partial abandonment of the given lexicon.

The development of an alternative lexicon that is incommensurable with a given one is always triggered by anomalies—namely, by the experience of objects that are incompatible with the epistemic claim of the given lexicon. Martin Carrier has concisely described this kind of clash in the three cases discussed above:

> General relativity considers the motion of point particles in a gravitational field as inertial motion. This characterization conjoins two apparently conflicting attributes: moving in a gravitational field and moving force free. Viruses cross-classify the existing categories in being infectious like bacteria are (and thus not being simply toxic), but at the same time being so small that they cannot be cells. They cross-classify correspondingly the chasm between the biological and chemical realm. Finally, prions. They are likewise infectious like bacteria and viruses, but they contain no DNA [nor RNA]. Again, although in different respect, prions cross-classify the rift between the living and nonliving kingdom.[10]

On the basis of the given lexicon, then, the anomalies present themselves as logical contradictions, at least in the given cases. The dissolution of these contradictions presupposes first an at least temporary suspension of the older lexicon and an at least provisional adoption of a new lexicon, in which a reclassification of the pertinent objects avoids the apparent contradictions. As the emergence of contradictions in any given lexicon is not expected at all, let alone the necessity of a suspension of the lexicon, lexicon users are opaquely ignorant of these possible developments. Their ignorance is even deeply opaque in cases in which the two lexicons are strongly incommensurable. It is thus plausible that strong incommensurability induces deeply opaque ignorance for conceptual reasons. Yet it is an open question whether strong incommensurability is the only possible cause of deeply opaque ignorance.

There is an interesting and somewhat-disturbing consequence of the close connection between strong incommensurability and deeply opaque ignorance. If the history of the sciences indeed exhibits considerably many cases of strong incommensurability (this is somewhat controversial), and if it is likely that this will continue into the future, then it is part of the

10 Martin Carrier, referee report, July 1, 2015. As this description is as clear and precise as can be, I simply quote it instead of unnecessarily rephrasing it.

human condition that we are permanently in a situation of deeply opaque ignorance, despite all progress in the sciences.

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Methodology-Created Ignorance

10 A View of Scientific Methodology as a Source of Ignorance in Controversies about Genetically Engineered Crops

Hugh Lacey

1. Agnotology Mechanisms

The claims—that the agricultural uses of genetically engineered organisms (GEOs) and consumption of their products are safe, and using GEOs in farming practices is indispensable for meeting the food and nutrition needs of the world's growing population—play central roles in arguments defending the legitimacy of using GEOs. They are widely endorsed by mainstream scientists.

David Magnus maintains that those who challenge these claims deploy a variety of agnotology mechanisms. In particular, construct agnotology (exaggerating the degree and kind of uncertainty that mark certain scientific results) is commonly deployed "by anti-industry NGOs to oppose the creation of genetically engineered organisms," instigate doubts and maintain ignorance among the public about the credibility of the "mainstream view . . . that most GEOs are safe and that, in principle, the technology can be safely utilized," and propose a version of the precautionary principle that "largely rejects risk management and the very idea of a science-based regulatory policy" (Magnus 2008, 251, 258).

Philip Kitcher points to another agnotology mechanism: rejecting evidentially supported claims because of the interests they serve. The opponents of using GEOs, Kitcher (2011, 238–239) maintains, "dismiss scientific reports to the effect that environmental risks are minimal as reflecting monied interests," and he continues, noting that "opposition to GEOs is largely a European phenomena, not much heard in the land of the potential producers (North America), nor in those of its potential consumers (Africa, Asia). In fact, many of the spokesmen for the world's poor are impatient with what they see as the scruples of people who do not feel any threat of starvation." There are opponents of using GEOs who are properly criticized in these ways, such as those who—alleging religious reasons, adopting trivialized versions of the precautionary principle, displaying ignorance of scientific developments, or dismissing all proponents of GEOs simply on the ground that they are purveyors of capitalist interests—would remain opponents regardless of the scientific record. That said, the opponents are not all alike. In this chapter, I will discuss opponents whose posture toward GEOs derives from engaging in agricultural practices such as agroecology, and holding that there is considerable scientific support for the claim that there are ways to engage in agriculture that are more likely than GEO-oriented (and conventional) ways to be sustainable over the long term and ensure food security (and other human rights) for the poor.

I agree with Magnus and Kitcher that agnotology mechanisms are in play in the controversies about GEOs. Contrary to them, however, I locate the mechanisms principally in the arguments defending the legitimacy of using GEOs. I will contend that the two claims stated at the outset are not well supported by the available empirical evidence and should not be endorsed, and despite this, their widespread endorsement is made possible (in significant part) because an inadequate conception of scientific methodology has wide currency among mainstream scientists. Those who uphold this conception downplay the scientific credentials of methodological approaches that are apt for gaining knowledge about certain kinds of risks of using GEOs and the possibilities of alternative forms of farming. Hence they do not attempt to obtain such knowledge, or pay attention to the input of the opponents who emphasize sustainability and food security for all. Ignorance about these risks and alternatives is thereby fostered, and claims about them that lack proper empirical support are enabled to pass for scientific knowledge. All this functions to protect the legitimacy of using GEOs from empirically based challenges and mars the deliberations of many public regulatory bodies. Proponents of using GEOs, by treating all opponents as alike, end up only showing the fallacies of points of view that are also rejected by opponents who raise serious questions about the legitimacy of using GEOs. The latter opponents (contrary to Kitcher) are to be found largely in movements in poor countries such as Brazil, where I regularly engage with them (see, for example, Carneiro et al. 2015; Ferment et al. 2015). They are mainly linked with popular rural movements with strong international connections, such as the network La Via Campesina, which proposes that practices and policies of food sovereignty, in which agroecology has a central place, provide the best way to assure food security for poor peoples (Lacey 2015b).[1] They also have strong connections with certain nongovernmental organizations, public agricultural research bodies, agricultural researchers (especially those connected with agroecology), and international bodies that deal with agricultural policies. They do not reject biotechnological innovations out of hand, introduce some (not GEOs) into their agroecological practices, and advocate for "science-based regulatory policy" (see, for example, Traavik and Ching 2007), which (they maintain) should be sufficiently encompassing so as to be informed by empirical investigation of the causes, including socioeconomic ones, of problems like food insecurity (Lacey 2005, 2017b).

These opponents challenge the two claims that using GEOs and their products is safe as well as indispensable for meeting the world's growing food needs. That does not put them in conflict with claims for which there is consensus among the relevant scientists (or with any well-confirmed scientific knowledge). Despite repeated allegations, there is not consensus concerning the two claims among the relevant scientists (Ferment et al. 2015; Hilbeck et al. 2015; Krimsky 2015). Moreover, even if there were, that might not indicate that the claims have strong evidential support, for consensus might derive from values and interests shared among the scientists that (consciously or not) draw attention away from crucial matters that need to be investigated. Of course, just citing this possibility cannot ground responsible challenges to claims that many scientists endorse; rather, such challenges depend on demonstrating that the available evidence is not adequate to support the claims and putting forward evidence (or proposing specific further research projects) that is pertinent for evaluating them as well as identifying the factors that explain why they are widely endorsed. The opponents, whose primary preoccupations are with sustainability and securing food security for everyone, assume this threefold responsibility. They do not make use of the agnotology mechanisms indicated by Magnus and Kitcher. It is true that they often suggest that scientists, who put the

1 For more on La Via Campesina, see https://viacampesina.org/en/. Food First: Institute for Food and Development Policy regularly publishes material representative of such viewpoints in English. See http://www.foodfirst.org/.

authority of science behind using GEOs, are in the grip of the values of technological progress and of capital and the market. For them, however, that is not a ground for rejecting the claims of these scientists or ignoring evidence they may put forward but instead part of the explanation of why, despite inadequate supporting evidence, these scientists endorse the two claims, and why they are unaware of the agnotology mechanisms (including the one to be discussed below with roots in an inadequate conception of scientific methodology) at play in their arguments.

2. What Is in Dispute?

The controversies about GEO crops (and their products) principally have to do with the *legitimacy* and social value of using GEOs—growing, harvesting, and distributing them, and processing and consuming their products, in the agroecosystems in which they are planted and cultivated, and in the socioeconomic contexts in which they are developed, produced, marketed, processed, and consumed—and their intensive utilization and widespread diffusion throughout the world in the agricultural practices that produce major crops as well as the place that should be accorded to research, development, and implementation of GEOs in national and international agricultural policies (for details and documentation, see Lacey 2005, 125–147). Judgments concerning legitimacy draw on claims made about benefits, risks, and alternative farming practices.[2] Legitimacy presupposes *efficacy* (Lacey 2005, 2016), and although there are questions about the long-term efficacy of using particular varieties of GEOs and the risks that may arise when their efficacy declines, and some exaggerated claims have been made about what can be expected in the future (Lacey 2017a), efficacy will not be at issue in the present argument.

2.1. Risks and Alternatives

Arguments for the legitimacy of using GEOs draw on claims like the following:

> *No risk*: Current and anticipated uses of GEOs for agricultural and related commercial purposes occasion no significant risks to human health or the environment

2 Regarding the real and promised benefits of GEOs (not discussed in this chapter) and for whom, see Lacey 2005, 165–181, 2017a.

that cannot be (and normally are) managed adequately under scientifically informed regulations, and the GEOs currently being used have occasioned no significant harm.

No alternative: There are no alternative kinds of farming that could be practiced—in place of the currently deployed GEO-oriented ways and those being developed (including those being developed with new and more complex methods of genetic engineering)—that could be expected to produce comparable benefits connected with productivity, sustainability, and meeting human needs, and would not occasion unacceptable risks (for example, not producing enough food to feed the world's growing population). GEOs are necessary to feed the world, and will gain an increasingly important role in doing so.

The opponents, who emphasize sustainability and food security for all, make competing claims like the following:

There are risks: Available scientific evidence does not support endorsing "no risk," and in part this reflects serious shortcomings in the risk assessments that have informed regulatory deliberations. Furthermore, using GEOs has caused harm to human health as well as the environment and social arrangements (some of which may be irreversible), and further harm is risked by continuing to use them and expand their use—where the mechanisms involved include those linked with the necessity of using some GEOs in conjunction with agrotoxics, and others linked with the socioeconomic context of the research, development, and use of GEOs.

Better alternatives: Agroecological methods (among others) are being developed that enable high productivity of essential crops with less serious risk. They promote sustainable agroecosystems, utilize and protect biodiversity, contribute to the health and social emancipation of poor communities, and are particularly well suited to enable rural populations in developing countries to be well fed and nourished. Without their further development, the current patterns of hunger are likely to continue.

Values influence judgments about what is considered a risk, what risks are serious enough to require investigation, what are desirable properties of agroecosystems, and hence judgments about what the specific objects of scientific inquiries about risks and alternatives should be (Lacey 2005, 2017a, 2017b). The values that are incorporated into the opponents' stance (and also into the precautionary principle) include social justice, popular participation, empowerment of the excluded, ecological and social sustainability, respect for the full range of human rights, and equity within and between generations (Lacey 2005, 138; 2015b). There is tension between these values and those accorded highest priority by the proponents of GEOs: values connected with technological progress—including granting high ethical/social value to expanding the capacity of human beings to control natural objects especially as embodied in technological innovations, innovations that increase the penetration of technology ever more intrusively into ever more domains of human life, and the definition of problems in ways that may permit scientifically informed technological solutions (Lacey 2005, 18–24)—and with capital and the market, such as economic growth (Lacey 2005, 137).[3]

Contrasting "no risk"/"there are risks" and "no alternative"/"better alternatives" enables us to clarify the role of scientific research in dealing with questions of legitimacy and its interplay with values (Lacey 2016, 2017b). As indicated above, proponents of using GEOs often assert that "no risk" and "no alternative" are backed by scientific consensus. The opponents counter that while much remains open to further investigation, the preponderance of available evidence points toward "there are risks" and "better alternatives" (Lacey 2015b). Their argument, without foreclosing that there could be roles for GEOs under certain conditions alongside other agricultural methods, leads to the conclusions that they are not needed now and their current uses are (on balance) harmful. Furthermore, they point to the kinds of research that are needed to provide additional scientific input relevant for making sound judgments about risks and alternatives (Lacey 2005, 2015b).

2.2. Conceptions of Scientific Methodology

Behind the disagreements about risks, alternatives, and what is supported by scientific evidence lies a (usually unarticulated) disagreement concerning scientific methodology. To make this apparent, I will make use of the notion of *methodological strategy* (Lacey 1999, 2005). The principal roles of a strategy are to constrain the kinds of hypotheses, models, and theories that may be entertained in a research project, and so specify the kinds of phenomena and possibilities that may be explored as well as the conceptual resources that may be deployed, and select the kinds of empirical data that are relevant for appraising the hypotheses that are entertained.

3 "There are risks" refers to risks to social arrangements; "no risk" does not. Proponents of using GEOs tend to maintain that bringing social effects into the discussion reflects "ideology" or "politics"—perhaps relevant in regulatory deliberations, but not in "scientific" risk assessments.

To a first approximation, I consider scientific inquiry to be systematic empirically based inquiry, conducted under strategies that are apt for gaining and confirming knowledge and understanding of the phenomena being investigated (Lacey 2005, 64–65). This characterization leaves two matters open. First, the results of scientific inquiry may not all have the same cognitive status. When investigating the ecological and social complexities of agroecosystems—for instance, those connected with the disputes about the risks of using GEOs and alternatives to doing so—even extensive scientific inquiry, conducted within available time frames, will often not be able to produce results that meet the empirical and cognitive standards required to establish items of confirmed scientific knowledge. It may, however, provide sufficient evidence to *endorse* a hypothesis, that is, to judge—after taking into account the consequences of acting informed by it, if it were false, and their ethical salience—that the evidence supporting it is sufficiently strong to legitimate acting or forming policy in ways informed by it (Lacey 2015c). Second, different kinds of strategies may be needed to investigate different kinds of phenomena—for example, one kind to investigate the structures of plant genomes and ways to alter them, and others to investigate the environmental and social effects of using them as well as the possibilities of sustainable agroecosystems.

2.2.1. Decontextualizing Strategies The great success of scientific inquiry is often held to derive from the adoption in research of *decontextualizing strategies* (DSs) (Lacey 2016), and sometimes it is held that the products of research conducted under DSs satisfy superior cognitive standards, or that the nature of scientific inquiry is to privilege adopting DSs.[4] Under the most widely used DSs, theories are constrained to represent (or model) phenomena in relation to their (hypothesized) underlying structures, the processes and interactions of the structures and their components, and the laws that govern them.[5] Representing phenomena in this way decontextualizes them. It dissociates them from any link they may have with human agency, value, sensory qualities, and social arrangements, and whatever possibilities they may afford by virtue of their places in particular social,

4 For an elaboration on DSs, previously called "materialist strategies," see Lacey 1999, 2005.

5 These DSs are also *reductionist strategies*; causal interaction from higher to lower levels of organization of phenomena and systems is not entertained under them. DSs that are not reductionist are not relevant to the present discussion.

human, and (frequently) ecological contexts, including (in the case of GEOs) those they may afford by virtue of specific features of the agroecosystems in which they are planted and cultivated together with the socioeconomic contexts in which they have been developed, produced, marketed, and processed. Theories entertained under DSs dispense with the categories, including intentional and value ones, routinely deployed for describing and understanding what is experienced as well as deliberating when making decisions. Thus, for example, under DSs, GEOs are investigated for their genomic and molecular biological properties, and the effects that are triggered by these properties and changes of them, but not for the effects of using them that follow from their being objects to which intellectual property rights obtain. Complementing these constraints on admissible theories, empirical data are selected, sought out (often using mechanized surrogates for human observers), and reported (or mechanically stored, manipulated, and transmitted) using descriptive categories that generally are applicable by virtue of measurement, instrumental, and experimental operations. Data are not selected concerning, say, who owns and uses GEO seeds, and under what conditions, or the impact their use has on biodiversity, small-scale farmers, and worldwide food security.

Adopting DSs has been extraordinarily fruitful, and we may expect that it will continue to be so. Under DSs, knowledge and understanding of an enormous and varied array of phenomena have been obtained, and since DSs admit of considerable variety deriving from the different kinds of laws and explanatory models that may be incorporated into a strategy's constraints, they are also highly versatile. Their fruitfulness and versatility contribute to explain why many hold it to be of the nature of scientific inquiry to adopt DSs predominantly (if not exclusively), and why this view is so deeply entrenched that the possibility that there might also be other fruitful strategies is rarely entertained explicitly in the scientific mainstream.

When proponents of using GEOs insist that they have the backing of scientific authority, they are effectively taking for granted a methodological view like the following:

> *Primacy of DSs*: The adoption of DSs has primacy, perhaps virtual exclusivity, among the methodologies of scientific research.

For them, the research that has led to the development of GEOs and confirmed the efficacy of using them is exemplary of scientific research, since only DSs (those adopted in biotechnology and molecular biology) are adopted in it; and sound scientific risk assessment and research aimed at improving agricultural practices would be marked by the virtually exclusive adoption of DSs. The tradition of modern science has tended to foster "primacy of DSs," and upholding it is reinforced where economic growth and related values are considered socially preeminent, and technoscientific innovation is considered a driving force of economic growth (Lacey 2016), so that now the reach of DSs keeps expanding with no end in sight. Moreover, when it is maintained (as it widely is today) that engaging in research conducted under DSs is a principal and indispensable source for meeting human needs in general, and specifically, for making improvements in agricultural and other practices, there may seem to be no reason to look beyond "primacy of DSs."

2.2.2. Context-Sensitive Strategies The opponents of using GEOs who are also proponents of sustainability and food security for all recognize that many technoscientific innovations lead to generally available benefits, and hence that research conducted under DSs is often socially significant. They question, however, that there is factual support for the role claimed for that research in contributing to meeting human needs and improving agricultural practices (Lacey 2015b, 2016, 2017a). They point out that currently unmet needs (for example, for food security for many poor people) have social, economic, and historical dimensions and causes, as do other problems endemic to the current hegemonic food/agricultural system, such as unsustainable and excessively polluting practices, and the destruction of fragile ecosystems that accompanies efforts to obtain access to more farmland. If something is to be done about these problems, the fundamental causes of their origin and persistence need to be identified. But DSs do not suffice for investigating the causal networks in which the problems are enmeshed.[6] To this end, strategies that do not involve dissociation from the social, economic, and historical contexts of phenomena are also needed; I call them *context-sensitive strategies* (CSs).

6 That there is abundant food being produced today, more than enough to feed everyone alive now, may largely be attributed to the innovations of the green revolution that have been informed by research conducted largely under DSs. But as is now manifest, producing such abundant supplies of food is compatible with hunger and malnutrition persisting on a large scale, and the farming practices that enable it causing environmental and social devastation. Without CSs the causes of these phenomena cannot be investigated.

Furthermore, some kinds of evidence, needed for addressing the oppositions about risks and alternatives, can be obtained only from research in which CSs are adopted. Adopting CSs is needed to investigate the possibilities—which if realized, might help to redress the problems referred to in the previous paragraph—that may be open to alternative forms of agriculture not based on the intensive utilization of GEOs or other technoscientific innovations, such as agroecology, the alternative highlighted in the statement of "better alternatives." The successes of agroecology are well documented and lend support to endorsing "better alternatives" (Rosset and Altieri 2017; Lacey 2005, 212–223; Lacey 2015a; section 4 below). Agroecology integrally incorporates an approach to farming, a body of scientific research and its results, and a social movement (Rosset and Altieri 2017; Lacey 2015a). In the scientific research of agroecology, CSs are adopted that enable agroecosystems to be investigated with respect to how they fare in light of such desiderata as productivity, ecological sustainability and the preservation of biodiversity, social health, and the strengthening of local people's culture and agency, frequently with a view toward discovering the conditions under which an appropriate balance of the desiderata may be brought about in particular agroecosystems (Lacey 2015a).

Certain kinds of risks of using GEOs also cannot be adequately investigated without adopting CSs as well as DSs, such as risks that may be occasioned by mechanisms that are grounded in GEOs being commercial objects whose uses are constrained by claims of intellectual property rights (Lacey 2016, 2017b). They include risks that may be a consequence of the inadequate enforcement of regulations designed to ensure the safety of using GEOs, and the risks (intensifying those they share with "conventional" capital-, input-, and machine-intensive forms of farming) of undermining alternative forms of farming, displacing and impoverishing rural workers as well as weakening the conditions for them to exercise their agency, and bringing the world's food supply increasingly under the control of a few market-oriented corporations, potentially intensifying food insecurity throughout the world (Lacey 2015b, 2017b). Noteworthy among the risks that cannot be investigated where only DSs are adopted are those that may arise when GEOs are introduced—with the stated objective of dealing with problems of small-scale farmers (for instance, production in precarious agroecosystems) and their communities (say, hunger and malnutrition)—under the same socioeconomic conditions that occasioned the problems in the first place and account for their persistence. Vitamin-enhanced genetically engineered crops, for example, are being developed with the aim of combating diseases caused by vitamin deficiencies, but the research involved disregards that currently developed GEOs fit into and generally require the same socioeconomic arrangements in which this problem came about and persists (Lacey 2005, 171–180), and that implementing such projects requires inserting farming practices into international market structures, and in doing so, that there might be harmful human, social, and ecological consequences and (above all) “better” alternatives.

The opponents, who are preoccupied with sustainability and food security for all, give high salience to the investigation of the possibilities of sustainable agroecosystems and agroecological practices, and the possible effects of using GEOs on the environment, people, and social arrangements. The strategies adopted in the research that led to the development of GEOs, however (and in general DSs), are insufficient to conduct this investigation adequately, for it requires adopting CSs as well as DSs. This research thus incorporates the methodological view:

> *Strategic pluralism*: The methodologies of science must allow for the adoption of CSs as well as DSs—in order to enable investigation of the full and diverse range of phenomena of which understanding may be sought.

CSs are complementary to DSs; they do not displace DSs from their roles in investigations for which they are apt, and all research conducted under CSs may be able to make use of some knowledge gained under DSs

To maintain that there are phenomena that cannot be adequately investigated exclusively under DSs is not to be “antiscience,” “ideological,” or “ignorant,” nor a rejection of “science-based” risk management and regulatory policy. Whether or not there can be systematic, empirically based investigation conducted under CSs—producing results that are positively appraised in light of the same cognitive criteria that are used for appraising results obtained under DSs—remains open to the test of the practice of a robust methodological pluralism. As indicated above, I think that research conducted in agroecology shows that this test can be passed. What is important is that unless CSs are adopted, it is not possible to confirm the kinds of knowledge needed to make sound endorsements about risks, alternatives, and the causal networks of unmet food needs. Denying the epistemic credentials of the results obtained under CSs on the ground of the "primacy of DSs" fosters ignorance about these matters, and when they are denied, holding "primacy of DSs" can function as an agnotology mechanism—and it currently does so when "no risk" and "no alternative" are endorsed.[7] Whether one holds "primacy of DSs" or "strategic pluralism," currently available empirical evidence does not favor endorsing "no risk" and "no alternative" (as will be elaborated in the next sections). If "scientific" research is restricted to adopting DSs virtually exclusively, "scientific" evidence that supports endorsing "there are risks" (except those investigated within standard risk assessments; see the next section) and "better alternatives" could not be obtained, but there could be such evidence obtained from systematic empirical inquiry conducted under CSs as well as under DSs. This has far-reaching implications for how to understand "science-based risk management and regulatory policy."

3. Risks

Although all parties recognize that there may be risks occasioned by using GEOs, there are disputes about their significance—their character, ethical seriousness, magnitude, extent, contexts in which they might arise, mechanisms, likelihood of the harm risked actually being brought about, manageability under well-designed regulations, the range of methodologies needed to investigate them, and whether using GEOs has already brought about significant harm. Empirical evidence supporting "no risk" would derive from the failure, after making appropriate efforts, to find empirical evidence supporting that there are significant risks—and its endorsement should depend on sufficient research of the appropriate kind having been conducted.

3.1. Standard Risk Assessments

Generally those who affirm "no risk" consider that the appropriate kind of research is that which informs standard risk assessments. This is research, conducted under DSs in laboratories or small-scale field studies, concerning

7 If one wants to limit the meaning of "science" to systematic, empirically based investigation in which only DSs are adopted, and so by definition not consider adoption of CSs to be "scientific," so be it. But then it would be disingenuous to maintain that risk assessment relevant to public policy deliberations should be exclusively "science based."

the anticipated potential health and environmental effects of using a variety of GEOs.[8] In it, the potential effects and the mechanisms that might occasion them are characterized using theoretical categories acceptable within DSs, and so the mechanisms considered are physical, chemical, or biological. "Risk," "harm," and "safe" are value-laden terms, however, and so they have no place among the categories acceptable under DSs. Hence in order that the results of the research may be pertinent for risk assessments, prior to conducting them, some of the anticipated potential effects are labeled "risks" ("ethically significant risks")—in accordance with value judgments that effects of that kind would be harmful. Then the potential effects so labeled are investigated empirically in order to find out about their magnitude, the conditions in which they may actually be brought about, the probability of their occurrence, and the conditions for effectively regulating and thereby containing them (Lacey 2005, 2017b). Those who endorse "no risk" generally maintain that the main evidence for it has come from empirical studies concerning the potential significant risks of GEO varieties that have been released for agricultural and commercial use, and that "no risk" has been properly endorsed because sufficient studies have been conducted, and in light of them it has been judged that none of these varieties occasions risks (potential effects that have been labeled "risks") of significant magnitude and likelihood of actually occurring that cannot be adequately managed under approved regulations. This point of view underlies the proposal that standard risk assessments, based on the kind of investigations just described, are constitutive of and sufficient for "science-based regulatory policy." It also provides the rationale that supports legislation, obtaining in most countries, that no variety of GEOs may be released for agricultural and related commercial purposes, unless regulatory bodies certify that it has passed an appropriate and sufficient array of standard risk assessments that also provide the basis for approved regulations governing its use.

It is a value judgment, though, not a scientific result that sufficient properly conducted standard risk assessments are the appropriate basis for appraising risks and endorsing "no risk" (Lacey 2005, 2017b). In accordance

8 Risk assessments should be conducted case by case, and variety/environment by variety/environment. Using some varieties of GEOs may occasion serious risks, and using others may not, and in some environments but not in others.

with it, risks that cannot be investigated in these assessments (under DSs) need not be considered in science-based deliberations about the commercial release of GEOs. For those who maintain "primacy of DSs," this value judgment may be seen simply as a consequence of affirming that regulatory policy should be based on "sound science." But that affirmation is a value judgment too. Why are risks that require CSs for their investigation not relevant to science-based regulatory policy? The matter cannot be reasonably settled by fiat, and scientists qua scientists have no special competence to deal with value judgments. "No risk" cannot become an item of established scientific knowledge so long as judgments made about it depend on this value judgment. Furthermore, it could not become one, unless adequate rebuttals were made of the opponents' counterclaims that evidence (obtained in research conducted under CSs) challenges it and the risk assessments that have actually informed regulatory deliberations have serious shortcomings.

In light of the complexities, uncertainties, and time limitations surrounding risk assessments, and the impossibility of anticipating all the risks that might arise in the future, it is unlikely that a large stock of scientific knowledge can be established about risks. Even so, evidence may be obtained that convincingly supports endorsements about risks (see the "Conceptions of Scientific Methodology" section above). When making endorsements, value judgments are always implicated in some ways, including when endorsing "no risk" (and "there are risks") and making the judgment (in public policy deliberations) that the available evidence is sufficiently strong to endorse "no risk" (Lacey 2015c; cf. Douglas 2009).[9] "No risk" might become convincingly endorsed, if there were good reasons to hold the value judgment about the sufficiency of standard risk assessments, and (for some) commitment to the values of technological progress and of capital and the market may be considered to provide such reasons. Elsewhere I have argued that there are mutually reinforcing relations between commitment to "primacy of DSs" and holding values of technological progress (Lacey 1999, 2005). This often underlies taking efficacy to be (ceteris paribus) sufficient for legitimacy (Lacey 2016) and misidentifying endorsements as items of established scientific knowledge. Whatever agreement

9 The standard organs for scientific communication and evaluation are not well designed to take into account the role played by values in making endorsements.

may exist among mainstream scientists concerning the endorsement of "no risk" may be accounted for largely by the fact that "primacy of DSs" is widely held among them. "Primacy of DSs" also tends to be held among members of regulatory bodies that deal with GEOs. In both cases, this may be reinforced by their holding values of technological progress that, in turn, is reinforced by holding values of capital and the market (Lacey 2016). When regulatory deliberations about risks are informed only by evidence obtained in the course of standard risk assessments, they will be marred by ignorance that has been effectively generated and maintained by holding "primacy of DSs," and the consequent ignoring or downplaying of relevant research conducted under CSs (see "The Appropriateness of Relying Only on Standard Risk Assessments" section below).

Those who hold the opponents' values contest the value judgment that sufficient properly conducted standard risk assessments are the appropriate basis for appraisals of risks and are likely to endorse "there are risks." Of course, their values cannot provide a ground for rejecting "no risk." Holding them makes them aware, however, that the role played by "primacy of DSs" in regulatory deliberations is part of the functioning of an agnotology mechanism, and motivates their insistence on the importance of conducting the empirical investigations under CSs that can contribute to eliminating the ignorance. In addition to contesting the *value judgment*, the opponents challenge the apparent *factual claim* that properly conducted standard risk assessments have been carried out for the GEO varieties that have been commercially released.[10]

3.2. Shortcomings of Standard Risk Assessments Actually Conducted on GEOs

Consider first criticisms made of the *factual claim*. Critics have alleged that standard risk assessments—which have been made of the varieties of GEOs released for agricultural and commercial use, and have actually informed the deliberations of regulatory bodies—have been marred by a variety of

10 Rarely do advocates of using GEOs show awareness that the opponents question the value judgment as well as factual claim. Their efforts to rebut criticisms of the factual claim often appeal to the "technical" character of the studies conducted in standard risk assessments and the authority they accord to "technical" scientific experts concerning them; these depend on holding "primacy of DSs."

shortcomings.[11] These shortcomings include that (1) in many cases, they are not based on "sound science"; (2) typically they presuppose the principle of substantial equivalence; (3) they have not adequately taken into account all the sources and kinds of health risks that need to be investigated, and whether some of the risks that have actually been identified can be adequately managed; and (4) rarely are they subject to further testing in light of ongoing monitoring in the contexts of their actual use.

Regarding (1), the proponents of GEOs ironically often wave the banner of "sound science," which they take to involve the virtually exclusive adoption of DSs. Yet most of the standard risk assessments considered by regulatory bodies are based on research conducted by scientists employed or funded by agribusiness corporations; their results are restricted as "confidential," and in the name of protecting intellectual property rights, barriers are put in the way of independent review and attempted replicability. The point here is not just that confidential studies might be hiding something. Throughout the tradition of modern science, it has constantly been emphasized that "sound science" requires that transparency, public scrutiny, and independent replicability be the norm (Royal Society 2012)—which may be overridden in exceptional circumstances, such as wartime security—in order to counter possible conflicts of interest and agnotology mechanisms. Especially since agribusiness corporations have a strong interest in "no risk" being endorsed, public scrutiny is crucial for having confidence in the results of the risk assessments. Proponents of GEOs point out that independent studies have not provided evidence against "no risk," and they frequently insinuate that opponents, by insisting that such studies be conducted, are really just deploying the mechanisms of construct agnotology. This is disingenuous. It is true that independent studies have not provided *compelling* or *definitive* evidence against "no risk." There have been relatively few of them—in large part because agribusiness normally denies

11 I state the allegations selectively, summarily, and without appraisal, and only as they pertain to assessments of risks to human health. For a useful single source for documentation of the risks and the evidence backing them, and who makes them, see Traavik and Ching 2007. The alleged shortcomings are frequently said to involve the play of additional agnotology mechanisms (which I will not discuss), including lack of transparency, attributing scientific authority to claims that have questionable empirical backing, not attempting to acquire (or suppressing) relevant evidence and find out about relevant evidence that is not published in English-language scientific journals, and premature closure of investigation.

access to the seeds of GEOs needed to conduct them, it makes use of the legal mechanisms of intellectual property rights to restrict their use, and the contractually approved uses of these seeds do not include using them for scientific studies (Dalton and Diego 2002; Pollack 2009; Waltz 2009). This reinforces skepticism about the results of the confidential studies, especially since there is a growing number of independent standard risk assessments that do provide prima facie (albeit not definitive) evidence of serious risks (Ferment et al. 2015; Krimsky 2015). This skepticism is deepened in light of the response of scientists linked with agribusiness to these studies, for it tends to include campaigns aimed at discrediting the scientific quality of the studies and besmirching the scientific reputation of the independent researchers—without carrying out replications of the studies in a way designed to eliminate the unacceptable features that the independent studies allegedly have, and so bypassing the time-tested approach of the modern scientific tradition to resolving disputes (Krimsky 2015; Lacey 2017b).

In regard to (2), the principle of substantial equivalence, in the US Food and Drug Administration version, states that "in most cases the substances expected to become components of food as a result of genetic modification will be the same as or substantially similar to substances commonly found in food such as proteins, fats and oils, and carbohydrates." It is appealed to in order to build into regulatory deliberations that the default presumption (as with varieties grown in "conventional" farming) is "no risk," GEOs need no more stringent scrutiny than conventional varieties, and hence a high burden of proof needs to be met in standard risk assessments conducted on GEOs in order to override this presumption. This helps to explain why regulatory bodies tend to show little interest in rebutting the alleged shortcomings referred to connected with (1), (3), and (4). The status of this principle, however, is a matter of dispute. Many scientists question that it is empirically well based, and some maintain that it has been disconfirmed. Its role in regulatory deliberations, which frequently is required by legislation that is linked with international trade accords, does not have the support of empirically backed scientific consensus (cf. Traavik, Nielsen, and Quist 2007).

In regard to (3), growing GEOs requires the extensive use of inputs (that vary from variety to variety) that are often toxic and derived from petrochemicals, so that the risks occasioned by their use cannot be separated from the risks occasioned by using "packages" that contain the GEOs together with the inputs required for effectively using them (Lacey 2017a). Thus the risks to human health (and harmful effects that may have already occurred) include those that may arise from ingesting pesticide and herbicide residue as well the engineered genetic materials, and exposure to pesticides and herbicides used with the growing of GEO crops. These risks have physical, chemical, and biological mechanisms, and can be investigated under DSs (as they are in some of the studies cited below), and so they can be addressed in standard risk assessments. Nevertheless, the risk assessments concerning human health that have informed regulatory decisions about GEOs rarely go beyond investigating the risks of ingesting the modified genetic materials. They do not take into account (among others) risks that may be occasioned by ingesting the residue of the associated inputs (see, for example, Séralini et al. 2014), links between exposure to glyphosate (the herbicide to which many varieties of GEOs are resistant) and fetal and birth abnormalities that have been identified, for example, in Argentina (Antoniou et al. 2011; Paganelli et al. 2010), and possible links between increased exposure to pesticides and deaths from colon cancer in Brazil (Martin et al. 2018).

Finally, concerning (4), the alleged shortcomings already discussed motivate questioning the sufficiency of risk assessments that are not regularly revisited in light of the ongoing monitoring of the impact of consuming products containing GEOs (and chemical residues) along with the environmental and social impacts of growing them. Uncertainties are always likely to be present in risk assessments, and ongoing monitoring may provide further data, especially concerning potential long-term harm, that might lead to reversals of judgments, but the possibility of long-term epidemiological studies of the health risks of using GEOs, for instance, is severely inhibited by the opposition of agribusiness and many governments to the labeling of GEO-products.

These alleged shortcomings concern the risk assessments that have informed the decisions actually made about the commercial release and regulation of GEOs and their products. They could be eliminated by opening the assessments to scrutiny and replicability in independent studies, being responsive to problems raised in such studies, and complementing them with the ongoing monitoring of risks (and harm that may be actually caused) in contexts of use and then an openness to make revisions in light of the results of the monitoring.[12]

3.3. The Appropriateness of Relying Only on Standard Risk Assessments

Nevertheless, according to the opponents, even if the alleged shortcomings were taken care of and then the standard risk assessments actually carried out on a variety of GEOs provided no support for "there are risks," that would not amount to adequate support for endorsing that the variety can be safely used in the environmental/social contexts in which it is actually used. They maintain that adequate appraisal of "no risk" depends on outcomes of research conducted under CSs as well as those of standard risk assessments.[13] Hence their contesting the value judgment that sufficient properly conducted standard risk assessments are the appropriate basis for assessments of risks would be unaffected by taking care of the alleged shortcomings. For the opponents, the risks that need to be assessed are those that might be occasioned (or the harm that already has been occasioned)—taking into account all the causal mechanisms involved, socioeconomic as well as physical, chemical, and biological mechanisms—in the agroecosystems in which the GEOs are planted and cultivated, and the socioeconomic contexts in which they have been developed, produced, marketed, processed, and consumed.

The investigations that are part of standard risk assessments are conducted in experimental spaces that can deal only with the short-term impacts of using GEOs on health and the environment that are occasioned by physical, chemical, and biological mechanisms grounded in GEOs being biological and technoscientific entities (Lacey 2017a, 2017b). The opponents question not the necessity and value of conducting them well but rather their sufficiency. These investigations (since they only use DSs) do not take into consideration, for example, that some risks are likely to be magnified as GEOs are more widely used, they may derive from inadequate regulatory oversight of the actual uses of GEOs, it could take years before some harmful effects become apparent (Lacey 2017b), risks of occasioning irreversible harm may arise by virtue of the dominant place that GEO-oriented agriculture has assumed in the global food/agricultural and market system (Lacey 2015b), and some risks are occasioned by mechanisms that

12 Kitcher's (2011, 105–137) notion of "well-ordered science" might profitably be used to elaborate this suggestion.

13 I am using "risk" in its colloquial sense of "potential harmful effect," not in the technical sense (typically used in standard risk assessments) according to which the probability of a risk can always (in principle) be calculated.

are grounded in GEOs being commercial objects whose uses are constrained by claims of intellectual property rights. Short-term experimental studies, which are insensitive to potentially relevant variables that may be operative in the many and variable social and environmental contexts in which GEOs are used, can provide no evidence that harmful effects of the kinds mentioned are not being risked. Even if standard risk assessments were exhaustively conducted, the necessary and appropriate kinds of research still remain to be conducted under CSs.

The opposition ("no risk"/"there are risks"), therefore involves not just disagreement about which claim is best supported by the available evidence; it is implicated in the methodological opposition expressed in "primacy of DSs"/"strategic pluralism"). Holding "primacy of DSs" leads to discarding research conducted under CSs, and as such, fosters ignorance concerning evidence that is needed for reasonably resolving the opposition. Thus when "no risk" is endorsed on the basis of standard risk assessments, holding "primacy of DSs" functions as an agnotology mechanism. Of course, conducting the research under CSs, deemed crucial by the opponents, would be difficult, time intensive, controversial, and expensive, and curtailing the use of some GEOs, pending the outcome of the research (as required by the precautionary principle), would be costly to the corporations that have developed them for commercial use.

4. Alternatives

Even if, following research in which both CSs and DSs were adopted, "no risk" were routinely to become endorsed for the varieties of GEOs tested, it would not follow that GEOs should be given high salience in public agricultural policies—for there might be compelling reasons to endorse "better alternatives." In many regulatory deliberations, "no risk" functions in concert with "no alternative." The proponents of using GEOs maintain that currently proposed alternatives are not capable of "feeding the world" (in the long run as well as obviously in the short run), and thus endorsing "better alternatives" runs the risk that not everyone will be fed and nourished—a risk so momentous that compared to it, the risks that the opponents cite fall into insignificance. While this does not amount to establishing "no risk," the fact that members of regulatory bodies tend to endorse "no alternative" helps to make sense of the casual attitude that they tend to display toward the criticisms referred to above, and their considering the research proposed to be conducted under CSs to be irrelevant and distracting as well as time intensive, costly, and "politically motivated."

Be that as it may, if there were good empirically derived grounds available to endorse "no alternative," that would indeed recast the argument toward endorsing a suitably qualified statement of "no risk" and justifying giving priority to using GEOs in public agricultural policies. Those grounds could not be available, however, so long as compelling rebuttals are not produced of the opponents' claims that the available evidence (obtained from research in which CSs as well as DSs are adopted) points toward endorsing "better alternatives," and without the further development of agroecology, the current patterns of hunger are likely to continue. Such rebuttals cannot ignore the growing body of research that suggests that if the current patterns of food insecurity are to be redressed, and the food and nutrition needs of everyone met, a variety of farming approaches needs to be consolidated, and it is a matter of priority and urgency to make funding available for research that contributes toward developments of agroecology (and other approaches that are simpler, cheaper, more sustainable, and locally appropriate), in which introducing technoscientific innovations (like GEOs) is not the driving force (see, for example, De Schutter 2010, 2014; Food and Agriculture Organization 2014; McIntyre et al. 2009; Pretty 2008; Pretty et al. 2006; Rosset and Altieri 2017). In most of these publications, agroecology is highlighted and its current successes are noted; it is indicated that further research needs to be conducted concerning the proper place of agroecology among the variety of needed agricultural approaches (and what its limitations might be), and how it may vary with the characteristics of local agroecosystems, the needs of different locales, and the cultures and values of their inhabitants; it is not foreclosed that there may be a role for using GEOs in the varied mix of needed agricultural approaches; and it is stressed that GEOs should not be prioritized at the expense of agroecology in current agricultural research.[14] Research that addresses these matters cannot be conducted without adopting CSs, so that sound empirically based

14 It is not preordained that research on alternatives that adopts CSs will lead to the conclusion that there is no role, or only a minor one, for GEOs. In a few of the publications cited, a significant role for using GEOs is anticipated; in others (see, for example, Rosset and Altieri 2017) it is argued that the research that has been conducted strongly supports this conclusion.

judgments concerning them cannot be made on the basis of empirical data obtained only in investigation that incorporates "primacy of DSs."

I am not aware of any writings by those who endorse "no alternative" and prioritize the research and development of GEOs that show awareness of these matters, or attempt to rebut the claim that current patterns of hunger are likely to continue unless agroecological and related methods are developed and implemented.[15] The research reported in those writings is conducted under DSs. It includes results that inform standard risk assessments and generalizations drawn from them. But mostly it addresses questions like, What traits, potentially useful to the objectives of agribusiness along with the related interests of governments, farmers, and consumers, can be engineered into crop plants using the techniques of genetic recombination? The answers contribute to strengthening and expanding the uses of GEOs in agriculture, and may inform decisions made by public policy bodies, encourage funding bodies to invest more in research on the possibilities of GEOs, and motivate individual scientists to engage in it. Yet they cannot provide support for "no alternative," or generally, apart from where the interests just mentioned are dominant, giving priority to developing and implementing GEO-intensive agriculture. In practice, the proponents of GEOs tend to take "no alternative" for granted, and adopt the attitude that if using GEOs is efficacious and viable, little more legitimation for developing and using them is needed other than that it serves their interests. Holding this view is reinforced by the widely held conviction that in modern democratic societies, the trajectory toward the future is largely determined by technoscientific innovations (developed in the course of research conducted under DSs) that may contribute to economic growth (Lacey 2016).[16] Moreover, the influence of agribusiness in regulatory bodies, governments, and the press ensures that there is little public awareness of the science-based questioning of the safety of using GEOs and possibilities of alternatives, and its dominance in agricultural (including seed) markets

15 Agroecology is occasionally mentioned in these writings, but just to dismiss its significance by suggesting that it is a kind of romantic throwback to an idyllic past, or perhaps an approach with a role in some limited niches.

16 Research conducted with DSs might contribute to support claims such as that "there are no alternatives to using GEOs within the current trajectory of capital and the market or within the hegemonic food/agricultural system" (Lacey 2005, 230–235), but "no alternatives" does not follow from this.

becomes a causal factor that is preventing the development of alternatives. Nevertheless, it remains the case that in the discourse of legitimacy of using GEOs, "no alternative" plays an important role in countering arguments that "there are risks" (Lacey 2017a), but having good grounds to endorse "no alternative" depends not on the power exercised by large agribusiness corporations and their allies but instead on successfully rebutting that the available evidence points toward endorsing "better alternatives."

The opponents, by virtue of upholding "strategic pluralism," advocate conducting research that deals with the possibilities for strengthening and expanding agroecological practices—and also with the limitations that they may have (Lacey 2015b) and so that research could lead to obtaining evidence that would put "better alternatives" in doubt. Moreover, although they may hold that prioritizing research connected with GEOs is misguided, their upholding "strategic pluralism" per se poses no impediments to engaging in research that explores the possibilities of producing and using GEOs. On the other hand, holding "primacy of DSs" impedes conducting research on the possibilities of the alternatives like agroecology. It thereby hinders engaging in the kind of research (conducted under CSs) that could produce empirical data relevant for testing "no alternative" (and permits ignoring results actually obtained in that research), and it is an obstacle to overcoming ignorance on issues that are at the heart of important policy decisions. Just as regarding matters concerning risks, holding "primacy of DSs" functions as an agnotology mechanism when dealing with questions about alternatives.

5. Concluding Remarks

I have maintained that in the discourse of legitimation that has accompanied the introduction and spread of GEOs, holding a particular view of the nature of scientific methodologies—"primacy of DSs"—functions as an agnotology mechanism. It leads to the fostering of ignorance about the risks that are occasioned by mechanisms grounded in GEOs being socio-economic objects as well as the possibilities of agroecology: research that could provide knowledge about these risks and possibilities is not pursued; the scientific credentials of research conducted under CSs that might generate such knowledge are rejected; and claims (such as "no risk" or "no alternative") that lack adequate empirical support are effectively enabled to pass for scientific knowledge. A necessary condition for remedying this state of affairs along with the distortion of regulatory deliberations that it engenders is to adopt the conception of scientific research as systematic empirical investigation conducted under whatever strategies (DSs or CSs) are apt for gaining knowledge and understanding of the phenomena being investigated.

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Community-Based Ignorance

11 Expanding the Agnotological Toolbox: Methods of Sex and Gender Analysis

Londa Schiebinger

Agnotology is not merely an intriguing theoretical approach to the history and philosophy of science—something we flash about as a multifaceted gem at academic convocations; ignorance produced through systemic bias can also be expensive in terms of lives and costs. Agnotology traces the cultural politics of ignorance. It takes the measure of our ignorance, and analyzes how our knowledge has been influenced by struggles determining who is included and who is excluded, which projects are pursued and which are ignored, whose experiences are validated and whose are not, and who stands to gain in terms of wealth or well-being, and who does not. Sex and gender analysis form a particularly urgent terrain for agnotology, and are the topic of this chapter.

Sex and gender bias can create systematic ignorances that limit scientific creativity, excellence, and benefits to society. Between 1997 and 2000, for example, ten drugs were withdrawn from the US market because of life-threatening health effects. Eight of these posed “greater health risks for women than for men” (General Accounting Office 2001). Developing a drug in the current market may cost as much as $1.3 billion. Not only are these drugs expensive, when they fail, they cause human suffering and death. What produces such colossal failure?

To better understand agnotology in relation to sex and gender, it is important to distinguish three distinct but interlocking approaches to gender equality taken by historians and philosophers of science, governments, universities, and individual scientists and engineers over the past several decades. The first, “fix the numbers of women,” focuses on increasing women’s participation (National Science Foundation 1982; European Commission 2003). The second approach, “fix the institutions,” promotes gender equality in careers through structural change in research organizations (National Science Foundation 2001; European Commission 2011b). The third approach, "fix the knowledge" or "gendered innovations," stimulates excellence in science and technology by integrating sex and gender analysis into all phases of basic as well as applied research—from setting priorities, to funding decisions, to establishing project objectives and methodologies, to data gathering and analyzing the results (Schiebinger et al. 2011–2019c; Nielsen et al. 2017; Nielson, Bloch, and Schiebinger 2018). It is this third approach that interests agnotologists. Sex and gender analysis provide one powerful set of agnotological tools.

The ultimate goal of gendered innovations is to enhance excellence in science and technology. Excellence includes controlling for sex and gender bias, employing methods of sex and gender analysis, and and opening innovative areas to research. Innovation is what makes the world tick. Including a gender perspective in science, medicine, and engineering can stimulate creativity and gender equality as well as make research more responsive to society.

1. Unconscious Gender Bias in Science and Technology

Historians and philosophers of science and technology have documented gender bias in science over the past forty years (for a review of gender in science and technology research, see Schiebinger 2014). Examples in technology abound. Women, for example, are often left out of basic engineering design. Automobile crash test protocols consider short people (mainly women, but also many men) to be "out-of-position" drivers because they sit too close to the steering wheel. Out-of-position drivers are more likely to be injured in accidents (Hallman, Yoganandan, and Pintar 2008). The notion that persons of small stature are out-of-position drivers implies that the problem is the smaller-than-the-norm driver. In fact, the problem resides in the technologies (that is, car seats and settings) that have not been proportioned to take the safety of all drivers into consideration.

Gender bias also extends to men. One fertile illustration is the neglect of modern methods of male birth control. The contraceptive pill for women is often hailed as one of the major technological innovations of the twentieth century. Yet similar breakthroughs have not occurred for men (Oudshoorn 2003). Many men wish to share the burden of contraception with their women partners, but current technology does not offer them that option.

Instances of bias bristle in archaeology, primatology, biology, biomedicine, health, and artificial intelligence. Archaeologists, for example, have called into question origin stories featuring "man the hunter" and "woman the gatherer" that both build on and reinforce Western gendered-based divisions of labor (Gero 1991). Cell biologists have questioned the failure to record the sex of cells, which has major implications for, say, the future of stem cell therapies (Taylor et al. 2011; Shah, McCormack, and Bradbury 2014). Paleoanthropologists have criticized the convention of sexing relatively small skeletal remains, such as the famed Lucy, female based on size alone, and as a consequence, identifying the places they are found as homesites (Hager 2008). For decades now, the gender studies of science and technology have documented how science is not objective or value neutral but instead biased with regard to gender (as well as ethnicity and much else besides) (Schiebinger 1999).

The problem is that gender bias is largely unconscious, and unwittingly practiced by highly educated, intelligent, and well-meaning scientists and engineers. Systemic ignorance of this sort is difficult to eradicate. Scientists and engineers are alternatively galled or giddied once they learn to "see" gender.

2. Gendered Innovations

The good news is that ignorance produced by systematic bias can be understood and overcome. Historically, feminists have critiqued science and technology after the fact. Based on forty years of scholarship in the field, gender experts are now turning critique toward a positive research program that—from the beginning—integrates gender analysis into basic and applied research (Faulkner 2001; Klinge 2008; Schiebinger and Klinge 2013). Sex and gender analysis is crucial to all stages of research from strategic considerations for establishing priorities and theory, to more routine tasks of formulating questions, designing methodologies, and interpreting data (see "Methods of Sex and Gender Analysis" box). Gendered innovations is an approach to science and engineering that began to emerge in 2005 (I coined the term in 2005; see Schiebinger 2008). It develops practical methods of sex and gender analysis for science and engineering. These

Methods of Sex and Gender Analysis

- ✓ Rethinking Research Priorities and Outcomes
- ✓ Rethinking Concepts and Theories
- ✓ Formulating Research Questions
- ✓ Analyzing Sex
- ✓ Analyzing Gender
- ✓ Analyzing How Sex and Gender Interact
- ✓ Analyzing Factors Intersecting with Sex and Gender
- ✓ Rethinking Standards and Reference Models
- ✓ Participatory Research and Design
- ✓ Rethinking Language and Visual Representations

state-of-the-art methods of sex and gender analysis are key methods to add to the agnotology toolbox.

Methods of sex and gender analysis work alongside other methodologies in a field to provide further "controls" (or filters for bias), enhancing excellence in science, medicine, and engineering research, policy, and practice. As with any set of methods, new ones will be fashioned and others will be discarded as circumstances change. The value of their implementation depends on the creativity of the research team.

This chapter highlights nine of these methods, each paired with specific applications. These methods and case studies were developed through a series of international workshops held between 2010 and 2018 that involved gender experts and appropriate technical experts for each case study—whether focused on stem cell research, osteoporosis research in men, algorithms, or assistive technologies for the elderly. Together these methods and case studies demonstrate how gender analysis can vanquish ignorance and produce new knowledge (for case studies, methods, and citations, see Schiebinger et al. 2011–2019c).

3. Rethinking Research Priorities and Outcomes: HIV Microbicides

The first method examines research priorities and outcomes. Governments, industries, funding agencies, and researchers themselves set priorities for future research. Research priorities respond to numerous social imperatives and background assumptions, such as intended markets, funding levels, lobbies, and notions about gender. Questions related to gender include: How do gendered norms, behaviors, and attitudes influence research priorities? Do the established practices and priorities of funding agencies enforce gender bias, or encourage gender equality?

Every research project begins by setting priorities—that is, choosing how to invest limited social and intellectual resources, and what questions to pursue. Discussing research priorities and outcomes is complex, and one example here will not suffice. Nonetheless, the case study, HIV microbicides, provides a simple and remarkable illustration. Over the past few years, Andrew Szeri, a professor of mechanical engineering and former dean of the graduate division at the University of California at Berkeley, shifted research priorities in his lab from applied physics to biomedical engineering. As Szeri (2009) explained, “The mathematical methods (on which I rely heavily) haven’t changed much at all. It is, rather, the goals of the projects that have. The goals of the research changed from understanding the physics of a problem to developing models that could be used to evaluate devices or treatments for medical conditions.”

This shift in research priorities led to two gendered innovations: the first has to do with participation (Who does science?) and the second has to do with outcomes (What science is done?) (Schiebinger et al. 2011–2019h). For starters, Szeri’s shift dramatically increased the number of women in his lab. Engineering is a field where—despite national and international efforts—women remain underrepresented. While many schemes exist to increase women’s participation, few have considered how research foci, funding decisions, and project objectives impact women’s and men’s proportional participation in research (Rosser 2008; Marchetti and Raudma 2010). This example suggests that increasing the number of women requires more than programs focused on removing subtle gender bias from hiring and promotion practices, stopping tenure clocks, offering leadership training, and the like; such interventions are necessary but not sufficient. Increasing the numbers of women may also require “fixing the knowledge,” or reconceptualizing research to include methods of sex and gender analysis in creative, forward-looking ways (Schiebinger and Schraudner 2011).

Second, changing research priorities also expanded research in the field of fluid mechanics. Although the fluid mechanics research conducted in Szeri's lab has many applications, Szeri was particularly interested in developing woman-controlled HIV microbicides. About thirty-three million people are infected with HIV worldwide; some 72 percent of HIV-related deaths occur in sub-Saharan Africa, where the prevalence of HIV infection is six times higher than the world average. HIV flourishes in regions where women's subordinate status makes it difficult to negotiate safe sex (Gilbert and Selikow 2010; Joint United Nations Programme on HIV/AIDS 2010). Currently, female condoms are the only woman-controlled HIV prevention option, but they are detectable and may require partner consent. They are also less available and more expensive than male condoms (Mack et al. 2010).

Szeri (2009) and his coworkers sought to assist women, especially in cultures where they cannot say "no" to sex or rely on their partners to use condoms. Szeri's lab developed a vaginal gel to deliver microbicides. The physics of the problem is complex: the gel needs to coat the vagina completely and not fall out with the pull of gravity (Szeri et al. 2008; Lai et al. 2008). These gels can deliver birth control too, if desired.

Each case study requires a number of gendered innovations methods. An important method in this case is analyzing the factors intersecting with sex and gender. Gels can deliver microbicides and contraceptives, but they also act as lubricants. In some parts of sub-Saharan Africa, dry sex—in which the vagina is dried through the use of herbs and traditional medicines—is practiced (Verguet, Holt, and Szeri 2010). Engineers will need to work with anthropologists and, potentially, user groups to explore how cultural practices will intersect with biophysics for HIV prevention.

4. Rethinking Concepts and Theories: Genetics of Sex Determination

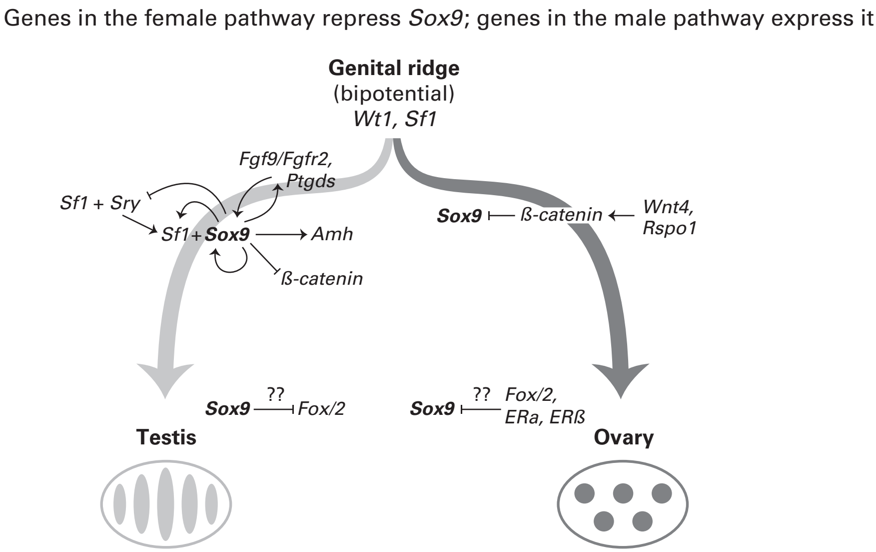
Agnotology extends to uncovering bias in basic concepts and theories driving a field. Theories provide a framework for explaining and predicting phenomena. Concepts relate to how data are described and interpreted, including how particular phenomena are categorized. Theories and concepts frame how research is conducted within a particular field or topic area, influencing what constitutes an interesting research topic, what needs explanation, what counts as evidence, how evidence is interpreted, and what methods are considered appropriate. The case study, the genetics of sex determination, provides an example of how questioning a basic concept—in this case, the notion of the female developmental pathway as a "default"—opened new areas to research.

Until about 2010, research on sex determination (the differentiation of the embryonic bipotential gonad into a testis or ovary) focused primarily on testis development (Uhlenhaut et al. 2009; for background, see Richardson 2013). Andrew Sinclair's 1990 *Nature* paper famously identified a Y chromosome gene as the sex-determining region Y, which along with its downstream targets, such as Sox9, became the focus of research. Female sexual development, by contrast, was thought to proceed by "default" in the absence of the sex-determining region Y.

According to the *Oxford English Dictionary*, "default" means "failure to act; neglect" or "a preselected option adopted . . . when no alternative is specified." In the case of sex determination, "default" became the prevailing concept for female pathways—that is, an ovary results in the absence of other action. In the case of the genetics of sex determination, biologists failed to question the "default" model for ovarian development inherited from the 1950s and 1960s. The notion of a "passive" female fit with current scientific theories and gender assumptions in the broader society. The active processes controlling ovarian development were ignored—they remained a field of ignorance.

Rethinking foundational concepts, questioning the notion of "default," led to new questions about ovarian development and the discovery of a cohort of genes required for ovarian function (Schiebinger et al. 2011–2019f). Gender analysis led to three innovations in this field:

1. The recognition of ovarian determination as an active process (Veitia 2010). These investigations have also enhanced knowledge about testis development, and how the ovarian and testicular pathways interact.
2. The discovery of ongoing ovarian and testis maintenance. Research into the ovarian pathway revealed that the transcriptional regulator FOXL2 must be expressed in adult ovarian follicles to prevent "transdifferentiation of an adult ovary to a testis" (Uhlenhaut et al. 2009). Subsequently, researchers found that the transcription factor DMRT1 is needed to



"The bipotential genital ridge is established by genes including *Wt1* and *Sf1*, the early expression of which might also initiate that of *Sox9* in both sexes. *ß-catenin* can begin to accumulate as a response to Rspo1-Wrt4 signaling at this stage. In XX supporting cell precursors, *ß-catenin* levels could accumulate sufficiently to repress SOX9 activity, either through direct protein interactions leading to mutual destruction, as seen during cartilage developement, or by a direct effect on *Sox9* transcription. However, in XY supporting cell precursors, increasing levels of SF1 activate *Sry* expression and then SRY, together with SF1, boosts *Sox9* expression. Once SOX9 levels reach a critical threshold, several positive regulatory loops are initiated, including auro-regulation of its own expression and formation of feed-forward loops via FGF9 or PGD2 signaling. If SRY activity is weak, low or late, it fails to boost *Sox9* expression before *ß-catenin* levels accumulate sufficiently to shut it down. At later stages, FOXL2 increases, which might help, perhaps in concert with ERs, to maintain granulosa (follicle) cell defferentiation by repressing *Sox9* expression. In the testis, SOX9 promotes the testis pathway, including *Amh* activation, and it also probably represses ovarian genes, including *Wnt4* and *Foxl2*. However, any mechanim that increases *Sox9* expression sufficiently will trigger Sertoli cell development, even in the absence of SRY."
(Sekido et al. 2009)

Figure 11.1

Molecular and genetic events in mammalian sex determination (Sekido et al. 2009).

prevent the reprogramming of testicular Sertoli cells into granulosa cells (Matson et al. 2011).

3. New language to describe gonadal differentiation. Researchers have dismissed the concept of “default,” and emphasize that while female and male developmental pathways are divergent, the construction of an ovary (like the construction of a testis or any other organ) is an active process. Each pathway requires complex cascades of gene products in proper dosages and at precise times.

5. Formulating a Research Question: Heart Disease in Women

Research questions typically flow from priorities as well as the theories and concepts that frame research (see above). Research priorities—along with concepts and theories—function to delimit the questions asked—and by implication, the questions not asked, and frame research design and choice of methods.

The choice of questions asked is often underpinned by assumptions—both implicit and explicit—about sex and gender. Heart disease research in women offers one of the most developed examples of gendered innovations. Although heart disease is a major killer of women in developed countries, it has been defined primarily as a male disease, and “evidence-based” clinical standards have been created based on male pathophysiology and outcomes. As a result, women are frequently mis- and underdiagnosed (Regitz-Zagrosek 2012a; Taylor et al. 2011).

Improving women’s health care has required new social, medical, and political judgments about women’s social worth, and a new willingness to support women’s health and well-being. Analyzing sex and gender in heart disease has also necessitated formulating new research questions about disease definitions, symptoms, diagnosis, prevention strategies, and treatments. Once sex and gender were factored into the equation, knowledge about heart disease increased dramatically. As is often the case, including women subjects—from diverse social and ethnic backgrounds—in research has led to a better understanding of disease in both women and men (Schiebinger et al. 2011–2019g).

To take just one example in this area, consider how underlying pathophysiology may differ between women and men (Bairey Merz et al. 2010).

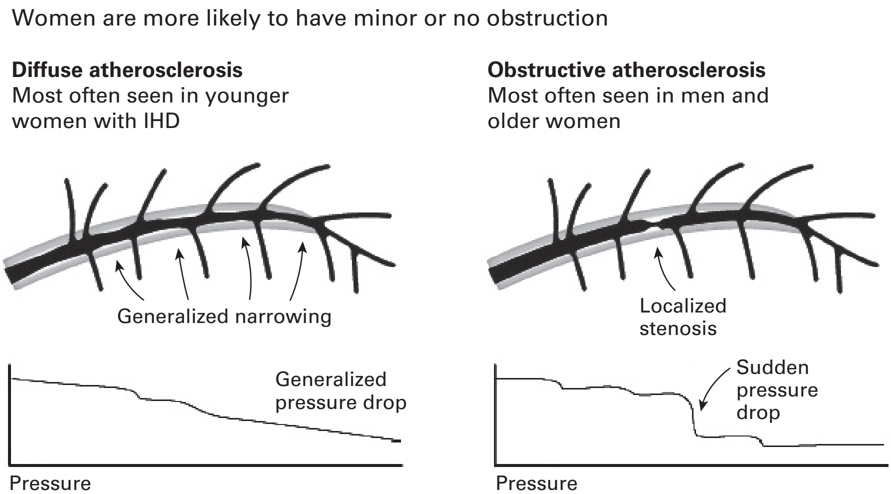


Figure 11.2
Coronary angiograms for patients with chest pain. Adapted with permission from Gould 1999.

Coronary angiography, the "gold standard" for diagnosing patients with chest pain, typically results in a diagnosis of obstructive coronary artery disease in men, but frequently fails to identify the cause in a large proportion of women (Bugiardini and Bairey Merz 2005; Shaw, Bugiardini, and Bairey Merz 2009). As a result, many women with chest pain, but "normal" angiograms, may be told that they have no significant disease and sent home.

New studies show, however, that the prognosis for these women is not benign: women with a primary diagnosis of "nonspecific chest pain" may suffer a heart attack or stroke shortly after being discharged from hospitals (Robinson et al. 2008). This may be true for men too. Large-scale randomized trials are needed to better understand the pathophysiology and optimal therapies for women and men with angina and "normal" angiograms.

Now, after twenty years of research, sex and gender analysis has prompted policy changes, increased the representation of women subjects in heart disease research, and enhanced knowledge about diagnosis and treatment in women, men, and gender-diverse individuals. In addition, robust prevention campaigns have utilized understandings of gender to promote heart-healthy behaviors, such as exercise and tobacco-smoking cessation.

6. Analyzing Sex: Stem Cells

Overcoming ignorance requires a robust understanding of sex analysis. Sex (the biological qualities of being female, male, and/or intersex) is an important variable to consider when setting research priorities, developing hypotheses, and formulating study designs. In biomedical research, sex may need to be analyzed in humans, animals, organs, tissues, cells, and their components (Institute of Medicine 2012; Pardue and Wizemann 2001). In engineering, sex may need to be analyzed at the levels of user physiology and biomechanics in both product and systems design.

Analyzing sex involves at least five steps: reporting the sex of research subjects or users; recognizing differences that exist *within* groups of females and males, and that significant *overlap* can occur between groups; collecting and reporting data on factors intersecting with sex in study subjects or users/consumers, such as age, socioeconomic status, and ethnicity; analyzing and reporting results by sex; and reporting null findings. This final step is important: researchers should report when sex differences (main or interaction effects) are not detected in their analyses to reduce publication bias and improve meta-analyses.

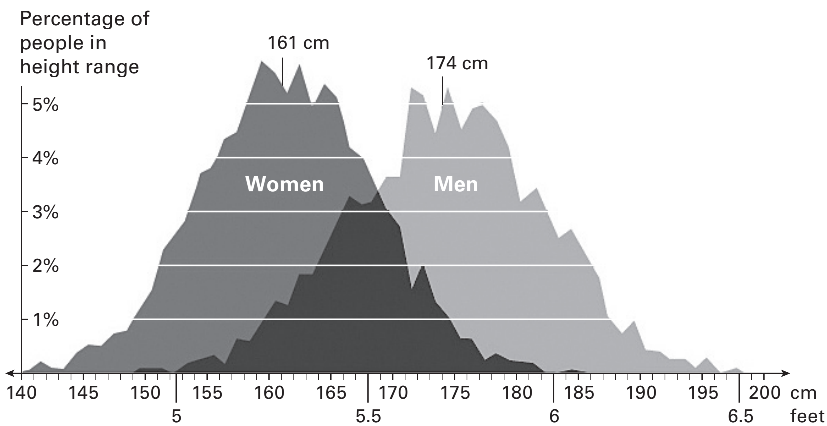


Figure 11.3
Height of adult women and men. Within-group variation and between-group overlap are significant. Image created with data taken from Centers for Disease Control and Prevention 2007, adults ages 18–86.

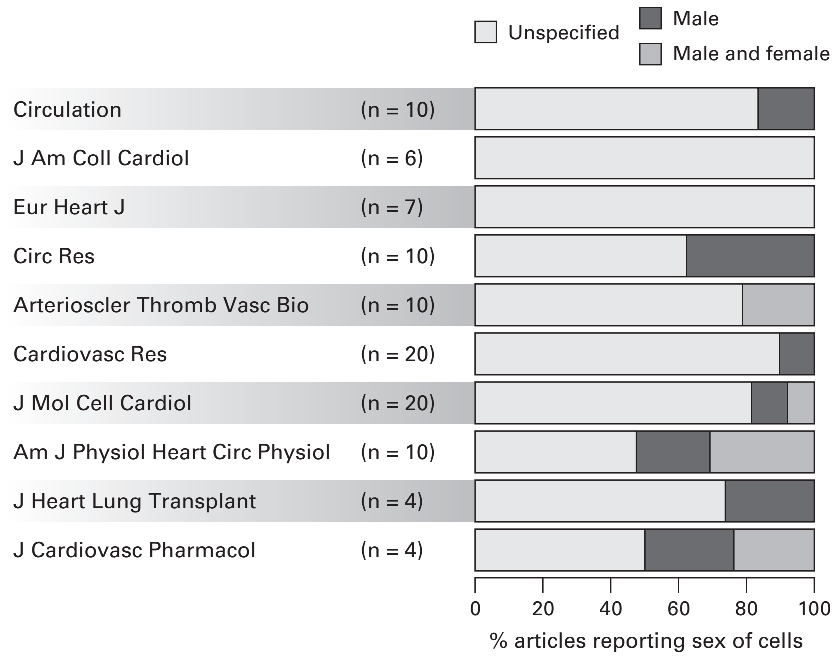


Figure 11.4
Percent of articles reporting sex of cells used in experiments (Taylor et al. 2011).

The method, analyzing sex, is basic and commonly used in gendered innovations case studies, including animal research, environmental chemicals, nutrigenomics, and inclusive crash test dummies. Here I use an example from stem cell research. Stem cell therapies hold great promise for treatments for debilitating diseases, such as Parkinson's disease and muscular dystrophy, although few are currently in use.

Let's go back to why ten drugs were withdrawn from the US market. What creates systematic ignorance in this instance? There are many reasons why drugs fail—and fail more often for women. One reason is that, oddly enough, most research today is still done in males (Beery and Zucker 2001). A Mayo Clinic study showed that for the most part, the sex of cells is not reported (Taylor et al. 2011). This is money wasted on research that is lost to future meta-analysis.

Taking sex into account will be important to advancing basic knowledge. Research has documented potential sex differences in the therapeutic capacity of stem cells. Muscle-derived stem cells, for example, show variability in proliferation and differentiation. Studies show that XX cells showed a higher regenerative capacity than XY cells. Yet few researchers consider the sex of cells—which can lead to failed research. An international research team from Norway and Australia worked with stem cells in mice. They appropriately used male and female mice, but they used all female stem cells—this was an unconscious and arbitrary decision.

It means that in the discovery phase, they did not see anything unique to male stem cells. Nor did they detect important differences in function between male and female cells. The result of not considering the sex of the stem cells was that their male mice died, and they didn't know why. Eventually, through a gendered innovations workshop in Norway, the team realized they should also consider the sex of the stem cells. They found that sex matching the donor and recipient yielded the best results. But all combinations of donor and recipient interactions should be tested before being ruled out (see figure 11.5).

The effects of sex, though, may also vary by the type of stem cell used, the type of disease treated, and hormonal and environmental factors, plus their intersections. It's complicated, but research that takes these factors into account leads to gendered innovations and better outcomes.

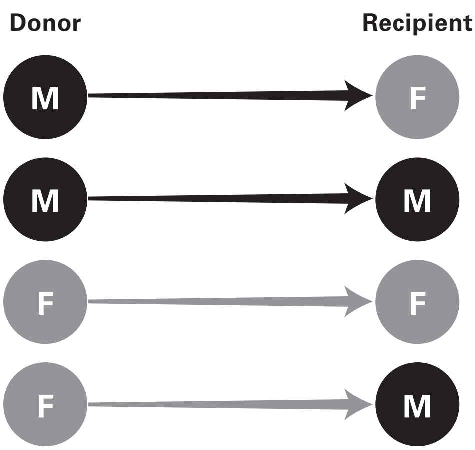


Figure 11.5
Considering sex in stem cell therapy. All combinations of donor/recipient sex interaction should be tested before being ruled out. Donor and recipient sex also interact with other factors, such as cell type, disease being treated, and hormonal, immunological, and environmental variables (Schiebinger et al. 2011–2019p).

7. Analyzing Gender: Machine Translation

Analyzing gender is a major tool for identifying unconscious bias. Gender refers to social and cultural factors that influence attitudes, beliefs, and behaviors. Gender includes *gender identity* (how individuals and groups perceive as well as present themselves), *gender norms* (spoken as well as unspoken rules in the family, workplace, institutional, or global culture that influence individuals), and *gender relations* (the power relations between individuals with different gender identities) (Schiebinger et al. 2011–2019b).

Gender is a primary linguistic, cognitive, and analytic category in science, health and medicine, and engineering. Yet gender assumptions often go unquestioned and hence remain invisible to scientific communities. These background assumptions unconsciously influence scientific priorities, research questions, and choices of methods. Gender comes into play when cultural attitudes shape and are shaped by researchers' gender assumptions and behaviors as these relate to the proposed research; research subjects' and users' gender needs, assumptions, and behaviors as these relate to the proposed research; how these assumptions interact (Schiebinger et al. 2011–2019c).

When gender assumptions remain unexamined, they may introduce bias into science and engineering. Take, for example, Google Translate. Machine translation becomes increasingly important in a global world. In March 2011, when I was in Madrid, I was interviewed by some Spanish newspapers. When I returned home, I zoomed the articles through Google Translate and was shocked that I was referred to repeatedly as "he." Londa Schiebinger, "he said," "he wrote," or occasionally, "it said" (Schiebinger et al. 2011–2019k). State-of-the-art translation systems, Google Translate and its European equivalent, SYSTRAN, have a male default.

How can such a cool company as Google make such a fundamental error? Google Translate defaults to the masculine pronoun because "he said" is more commonly found on the internet than "she said." We know from Ngram (another Google product) that the ratio of masculine to feminine pronouns has fallen dramatically from a peak of four to one in the 1960s to two to one since 2000 (Twenge, Campbell, and Gentile 2012). This radical change in language parallels exactly the women's movement and robust governmental funding to increase the numbers of women in

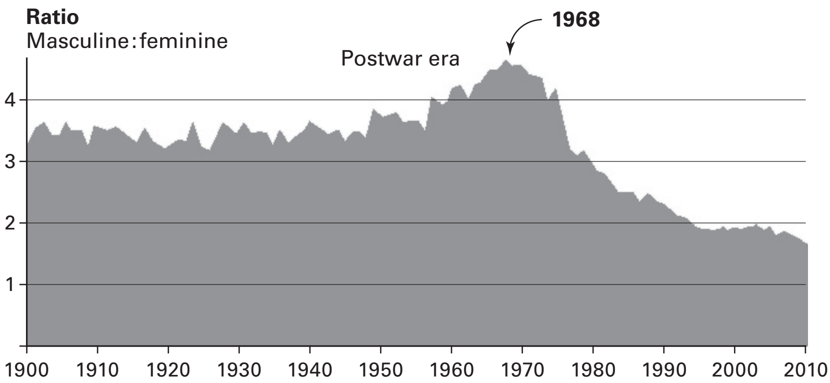


Figure 11.6
Ratio of masculine to feminine pronouns in US books, 1900–2008. Changes parallel increases in women's labor force participation, education, age at first marriage, and so on. Data from American English corpus of the Google Books database (about 1.2 million books), reproduced in Twenge, Campbell, and Gentile 2012.

science. With one algorithm, Google wiped out forty years of revolution in language—and it didn't mean to. This is unconscious gender bias.

The fix? In 2012, the Gendered Innovations project (Schiebinger et al. 2011–2019c) held a workshop and invited top researchers from Google and Stanford. They listened for about twenty minutes, they got it, and they said, "We can fix that!" Fixing it is great, but constantly retrofitting for women is not the best road forward. I had to ask myself how is it that Google engineers, many of whom are educated at Stanford, made such a simple mistake? What are we at Stanford doing wrong? For one thing, we don't teach gender analysis in core engineering courses—something we are now trying to fix.

Some products can be fixed, but what if Apple, Google, and other companies started product development research by incorporating gender analysis? What innovative new technologies, software, and systems could be conceived? The point I want to make is that this unconscious gender bias from the past amplifies gender inequality in the future. When trained on historical data (as Google Translate is), the system inherits bias (including gender bias). When a translation program defaults to "he said," it again increases the relative frequency of the masculine pronoun on the web that may reverse hard-won advances toward gender-equal language.

It turns out that even though Google wanted to fix the problem, it had been unable to. Much engineering is path dependent: once the basic platform is set, it becomes difficult to change. Importantly, Google Translate is creating the future: technology, such as our devices, programs, and processes, shape human attitudes, behaviors, and culture. In other words, past bias is frequently perpetuated and, as this case demonstrates, *amplified* into the future, even when governments, universities, and companies themselves have implemented policies to foster equality.

There are now many such examples of social bias, often unwittingly, amplified by artificial intelligence. In Google Search, for instance, men are five times more likely than women to be offered ads for high-paying executive jobs. Nikon's camera software is designed not to take a photo if someone is blinking, but misreads Asians as perpetually blinking. Predictive models in genetic medicine collect data primarily from individuals of European ancestry, meaning that disease-risk predictors may fail dramatically for non-European populations (Datta, Tschantz, and Datta 2015; Popejoy and Fullerton 2016; Zou and Schiebinger 2018). Interdisciplinary teams of computer scientists, humanists, and social scientists are proposing new techniques to counter bias in artificial intelligence (Schiebinger et al. 2011–2019j). Humans can and must intervene in automated processes to create technologies that promote social equalities.

8. Analyzing How Sex and Gender Interact: Assistive Technologies for the Elderly

"Sex" and "gender" are distinguished for analytic purposes. As we have seen, "sex" refers to biological qualities, and "gender" refers to sociocultural processes. In reality, sex and gender interact (that is, mutually shape one another) to form, for instance, individual bodies, cognitive abilities, and disease patterns. Sex and gender also interact to shape the ways that engineers design objects, buildings, cities, and infrastructures. Moreover, sex and gender intersect in important ways with a variety of other social factors, including age, educational background, socioeconomic status, ethnicity, geographic location, sexual orientation, and so on.

Sex and gender along with other factors intersecting with sex and gender all interact to create individual behaviors, health outcomes, attitudes, and so forth, across the life span. Although women and men are

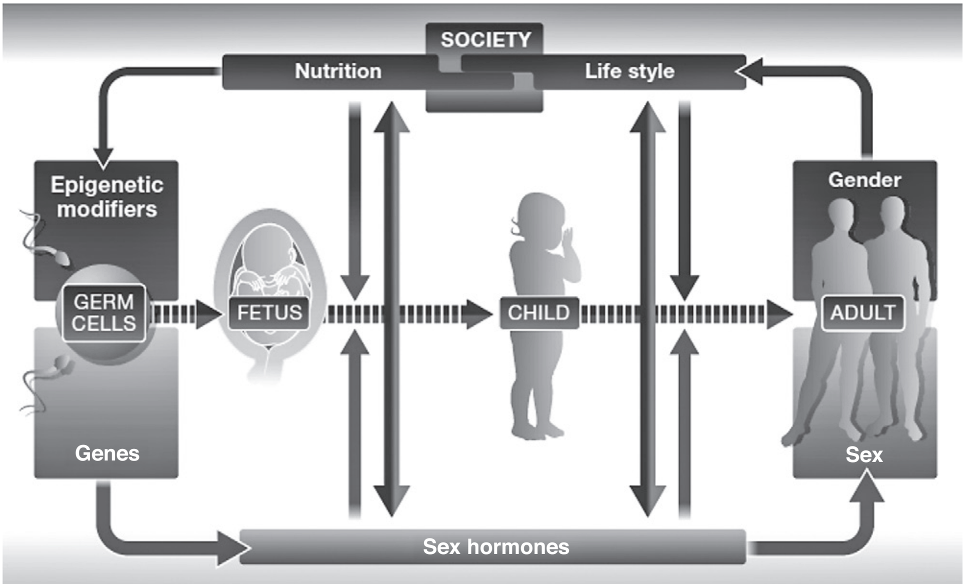


Figure 11.7
Complex interdependency of sex and gender throughout the human life cycle (Regitz-Zagrosek 2012b).

fundamentally alike, sex and gender can work together to produce differing outcomes.

Take the example of assistive technologies for the elderly. The world population will age dramatically by 2050. Large elderly populations will place a growing strain on human caregivers as well as health and social systems. This case study looks at the "value added" by considering both sex and gender when designing these technologies (Schiebinger et al. 2011–2019a).

Assistive technologies support independent living for the elderly. When developing these technologies, it's crucial to look at *sex* differences. Women tend to live longer, for example, but may have more debilitating disease; men, for instance, lose their hearing earlier. In addition, it is important to look at *gender* differences: as they age, women and men have different partnering patterns (elderly women more often live alone), experiences in household management, and receptivity to technology.

Gender issues become particularly significant as assistive technologies become more personalized. Engineers in the United States, Europe, and Japan are developing robots for elderly people. Georgia Tech, for example,

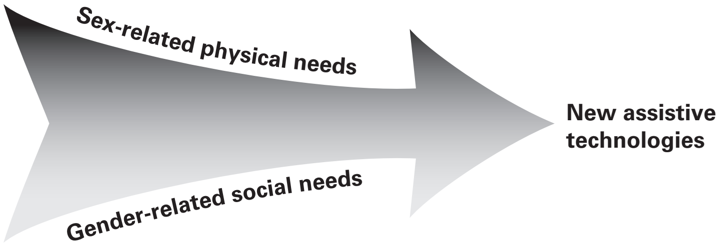


Figure 11.8
Designers need to consider both sex and gender when creating new assistive technologies.

has created a robotic nurse named "Cody" that can bathe elderly people. Bathing is an intimate relationship that requires careful thought—for women and men. Carnegie Mellon is developing what it calls HERB (Home Exploring Robot Butler), which can fetch household items for you, remind you to take your medicine, or even clean up the kitchen. If there is a robot to clean up the kitchen, I'm ordering it immediately!

As these robots enter our lives, we humans will gender them (Schiebinger et al. 2011–2019e). Studies of machine voices—synthetic or machine-generated voices—show that human listeners assign gender to machine voices; that is to say, we interpret these machine-generated voices as the voice of a woman or man, even when designers have tried to create a gender-neutral voice (Nass and Brave 2005; Lee, Liao, and Ryu 2007). Siri, Apple's first iPhone voice, is interesting in this regard. Ask Siri why she is a woman. One of her responses is, "I was not assigned a gender," implying that it's not Apple's fault that you, the listener, ascribe gender to her.

As soon as humans interpret a voice as male or female, we tend to overlay our cultural stereotypes onto the machine. Considering sex and gender when designing new assistive technologies will be one important factor to ensure that the products are safe and effective for all users.

9. Rethinking Standards and Reference Models: Human Thorax Model

Standards and reference models are integral to science, health and medicine, and engineering; they are used in educating students, generating and testing hypotheses, designing products, and drafting legislation. Science, medicine, and engineering often take the young, white, able-bodied, 155-pound male as the norm. When studied at all, other segments of the population—women, the elderly, and minorities—are frequently considered as deviations from that norm. Let me make three general points before turning to the specific example of the human thorax model. Agnotologists will wish to consider how standards and reference models shape and are shaped by gender norms:

1. Standards often default to the male. The majority of automobile crash test dummies, for instance, model only the fiftieth percentile US man. Dummies designed to represent females were developed in 1966, but these are only scaled-down versions of the standard, mid-sized male and do not model female-typical biomechanics, spinal alignment, and so on (Schiebinger et al. 2011–2019m). Consequently, women and multiethnic populations who do not fit the "norm" sustain more injuries than men in comparable crashes.
2. Gender norms may influence the choice of reference species. Primatologist Linda Fedigan, for example, has discussed the 1950s' vision of the "killer ape," primates engaged in bullying aggression toward females and violent infighting among males. This image of aggressive primates was drawn almost exclusively from studies of savanna baboons—taken as a "reference species"—in a process that Fedigan (1986) has called the "baboonization" of primate life.
3. Reference subjects influence gender norms. For instance, in rodent research, "reference females" are usually nonpregnant and nonlactating. Behaviorally, these females are less aggressive than males—a finding congruent with cultural assumptions about females. Changing the female mouse model to a pregnant or lactating animal would alter the outcome of a behavioral study. Female mice are aggressive in controlling food sources when pregnant or caring for pups (Brown, Herbison, and Grattan 2010).

The human thorax model, an international automobile-safety project, illustrates the importance of inclusive modeling (Schiebinger et al. 2011–2019i). This project simulates forces exerted on the thorax of human cadavers from both front- and side-impact automotive crashes. Historically, models for automobile safety testing have been based on fiftieth percentile male anthropometry.

Using the male body as a default—as the norm to represent humans in general—is a practice dating from at least the Renaissance (Leonardo da Vinci's *Vitruvian Man* or Andreas Vesalius's human skeleton drawn from a male body). These practices were confirmed in the twentieth and twentieth-first centuries with, for instance, Le Corbusier's (1954) *Modulor*. These reference models create systematic ignorance surrounding safety for smaller, lighter people, mostly women yet smaller men too (say, from Asian countries). They also exclude larger, heavier people, mostly men.

A number of methods have converged to enhance research in this area. First, rethinking reference models has led to data collection in automobile-safety projects, such as the human thorax model, to include humans from the fifth, fiftieth, and ninety-fifth percentiles. Second, rethinking research priorities helped granting agencies and industry prioritize safety for broad populations. Third, the method of analyzing factors intersecting with sex and gender has opened new areas to future research. These include:

1. Studying the effects of age and menopausal status on thoracic bone architecture (bones become weaker as people age, especially in women after menopause).
2. Including geographically diverse populations (populations differ by size; Asian men, for example, might be more similar in size and weight to European/US women than to European/US men.)
3. Modeling breast tissue. Breast tissue can be damaged in accidents—and can be significant, especially for women who are breastfeeding. Breast tissue also often determines how the seat belt sits across the body and can be important in how the ribs absorb shock.

Designing research to consider radically diverse human body sizes of different ages, sex, as well as from differing geographic regions can dramatically enhance automobile safety.

10. Analyzing Factors Intersecting with Sex and Gender: Osteoporosis Research in Men

While it is important to analyze sex and gender, and how they interact, other factors intersect with sex and gender, and agnotology needs to also include this analytic tool. These factors can be biological, sociocultural, or psychological, and may include genetics, age, sex hormones, reproductive status, body composition, comorbidities, body size, disabilities, ethnicity, nationality, geographic location, socioeconomic status, educational background, sexual orientation, religion, lifestyle, language, family configuration, environment, and so on.

This method applies to nearly every research project—and it is frequently a game changer. Intersecting factors, such as ethnicity or socioeconomic background, may reveal subgroup differences among women and men that would be obscured by analyzing only gender or sex. Researchers can investigate how sex and/or gender intersect with other significant factors by identifying all relevant factors, defining those factors, and identifying intersections between those variables.

Several examples above looked at problems that arise when the male is taken as the norm (as in the case of the genetics of sex determination, heart disease, or machine translation). The case of osteoporosis research in men, however, reveals that taking the female as the norm—that is, assuming a female default—can be detrimental to men (Schiebinger et al. 2011–2019l).

Osteoporosis has long been defined as a disease primarily of postmenopausal women—an assumption that has shaped its screening, diagnosis, and treatment. Why is this a problem? It is true that women suffer more than men do from osteoporosis, and at an earlier age. Men over age seventy-five, however, account for a third of hip fractures—and when men break their hips, they die more often than women (Burge et al. 2007). We don't know why.

Despite the relatively high numbers of men who suffer from osteoporosis, the basic diagnostics for the disease were developed using young, white women, aged twenty to twenty-nine years (Centers for Disease Control and Prevention 2002). The gendered innovation in this particular case study came in 1997 when a reference population of young men was established to diagnose osteoporosis in men. Although reference populations for men have now been developed, disease in men is still identified using the female diagnostic cutoff. It remains unclear whether this cutoff applies to men (Szulc, Kaufman, and Orwoll 2012).

The discerning reader will have zeroed in on the fact that the reference populations discussed above are white. The method of analyzing factors intersecting with sex and gender pushes researchers to consider differences among men with different lifestyles. Bones respond to biological preconditions as well as lifestyle, such as diet, smoking, and exercise (Fausto-Sterling 2005, 2008). Lifestyles can differ dramatically across cultures, ethnicities, and socioeconomic class such that significant differences can exist between individuals of the same sex and, ostensibly, same race. For example, widely used bone mineral density reference values for white US men have proven inappropriate for white Danish men (Høiberg et al. 2007). Current studies are analyzing cohorts of men from China and Sweden, for example, to understand these types of differences (Cawthon et al. 2016). The goal is to maintain healthy bones in diverse populations.

11. Participatory Research and Design: Water Infrastructure

Research is typically carried out by university-trained researchers. The agnotological method of note here is participatory research," which incorporates the lived experience of users into research—especially in the areas of product design and community projects (Rommes 2006; Schraudner 2010).

Gender becomes relevant to participatory research. Much knowledge is developed by either women or men because labor (formal employment as well as uncompensated domestic and caring work) divides along gendered lines. The case study, water infrastructure, highlights how community participation improves water services in sub-Saharan Africa.

Nearly one billion people worldwide lack reliable access to water. In sub-Saharan Africa, women and girls spend some forty billion hours annually carrying water (Schiebinger et al. 2011–2019q). Here the gendered innovation is tapping into this local knowledge. Because carrying water is women's work, many women have detailed knowledge of soils and the water they yield—knowledge that is vital to civil engineers when placing wells and water taps. Projects that draw these women into the process through participatory research improve the efficiency of water projects.

And it is a win-win situation: when girls are not carrying water, they tend to go to school—and potentially break the cycle of poverty.

12. Conclusion: Policy Interventions

At the core of modern science lies a self-reinforcing system whereby the findings of science (crafted in institutions from which women and minorities were excluded) were used to justify women's continued exclusion

(Schiebinger 1989). Women's long legal prohibition from scientific institutions was buttressed by elaborate gender ideologies. These exclusions and ideologies have created distortions in science. The much touted self-correcting (or value-neutral) methods of science do not uncover these systematic ignorances (based in bias).

The methods of sex and gender analysis have emerged over the past decades to correct this situation. To implement them requires a self-reinforcing system of another kind—one consciously implemented to overcome systematic gender bias. Policy is one driver of science and technology, and can help integrate gender analysis into science and engineering. Interlocking policies need to address gatekeepers, such as granting agencies, editors of peer-reviewed journals, universities, and industry leaders.

Granting agencies and foundations have the power to shape research through their calls for proposals and funding. Designing sex and gender analysis into research and innovation is one crucial component contributing to world-class science and technology. The European Union has prioritized gender in its multiyear funding framework, Horizon 2020 (European Commission 2011a). Article 15 promotes "gender equality and the gender dimension in research and innovation." The European Commission (2011a) states that "integrating gender/sex analysis in research and innovation (R&I) content . . . helps improve the scientific quality and societal relevance of the produced knowledge, technology and/or innovation." All applicants for public funding are asked, "Where relevant, describe how sex and/or gender analysis is taken into account in the project's content" (for a list of granting agencies and their policies for integrating gender analysis into research, see Schiebinger et al. 2011–2019n; see also European Commission 2013).

Following the European Commission's lead, a number of European national research councils include sex and gender analysis in research—notably Ireland, Norway, Spain, and Germany (Irish Research Council 2013–2020; Research Council of Norway 2013–2017; Gobierno de España Ministerio de la Presidencia 2011). In the United States, the National Institutes of Health (NIH) has required researchers to reconceptualize medical research to include women and minorities in federally funded research since 1993, though enforcement has been difficult. In 2016, the National Institutes of Health implemented its requirement that all publicly funded research consider sex as a biological variable (Clayton 2015). NIH intends to extend the policy to gender analysis as quantifiable measures of gender improve. The World Health Organization (2002) mainstreams gender analysis into all “research, policies, programmes, projects, and initiatives.” The Canadian Institutes of Health (2003) has long committed to “integrating sex and gender into health research.”

Policies implemented to enhance research at its inception—in the funding phase—can be reinforced by policies to encourage excellence in publication. Importantly, editors of peer-reviewed journals can require sophisticated sex- or gender-specific reporting when selecting papers for publication. A growing number of peer-reviewed journals in health and medicine have implemented such policies (Schiebinger et al. 2011–2019o). The International Committee of Medical Journal Editors integrated sex and gender analysis into its guidelines in December 2016. The *Lancet* and European Association of Science Editors have published guidelines for authors and journal editors for evaluating manuscript for excellence in sex and gender analysis (Schiebinger, Leopold, and Miller 2016; Heidari et al. 2016). Medicine is well on its way in this regard. To my knowledge, no computer science or engineering journals have such policies.

To support research, it is important for universities to integrate knowledge of sex and gender analysis into the curriculum. Integrating knowledge of sex and gender into medical school curricula in fact may be a matter of life and death. Doing so throughout the curriculum ensures adequate knowledge and skills for future physicians in the etiology, pathogenesis, clinical presentation, diagnosis, treatment, and research of diseases. Charité in Berlin, under the leadership of Vera Regitz-Zagrosek, has been perhaps most successful in this regard. Gender medicine-related content was integrated throughout all six years of training from early basic science to later clinical modules. Factors important to the process were the support of the dean and a “change agent,” such as a faculty member who oversaw the process, facilitated interactions with all key players, and established a supporting organizational framework. An evaluation of this program at Charité showed that sex and gender elements were integrated into 21 percent of the lectures, 12 percent of the seminars, and 8 percent of the practical courses (Ludwig et al. 2015).

Steps are currently being taken to incorporate sex and gender into core engineering courses as well as computer science and artificial intelligence. Computer science, for example, should incorporate the basics of sex and gender analysis, ethics, and other social analytics into core courses. These should not be treated only in separate ethics courses but instead integrated into core materials alongside technical training. It is crucial to train the next generation.

Finally, products and systems that incorporate the smartest aspects of gender can open new markets (Schiebinger et al. 2011–2019d). Products that meet the needs of complex and diverse user groups enhance global competitiveness and sustainability.

Agnotology has much to offer the humanities, social science, natural sciences, and engineering. Sex and gender analysis provide one powerful set of agnotological tools. As this chapter demonstrates, integrating gender analysis into research sparks creativity by offering new perspectives, posing new questions, and opening new areas to research. Gendered innovations stimulate excellence in science and gender equality, and by doing so, make science more sustainable.

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