The Pessimistic Induction and The Epistemic Status of Scientific Theories

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THESIS

Submitted as partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Philosophy
in the Graduate College of the
University of Illinois at Chicago, 2014

Chicago, Illinois

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to my father Rafet, who kindled the fire of philosophy in my heart, and to my mother Saliha, whose love has held us together

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kalbimde felsefe ateşini yakan babam Rafet'e ve sevigisi bizi bir arada tutan annem Saliha'ya

ACKNOWLEDGMENTS

If I had to name someone, I would blame Nick Huggett for the existence of this dissertation for some of its key components made their rudimentary appearance in a term paper I wrote for his Philosophy of Science seminar in Fall 2008. His penetrating intelligence and deep understanding of philosophy of science gave me a clear idea about how to develop and refine them. I am also thankful to him for taking over the responsibilities as chair in my final year.

Since 2008, the project has evolved quite a bit, which wouldn't have been possible without Colin Klein, who has been a great doctoral supervisor, which is a feat all the more impressive because this was his first time. I am most fortunate that my time at UIC mostly overlapped with his, for his guidance has been as intelligent, responsive, encouraging, patient and caring as it gets. Besides his outstanding performance as a supervisor, Colin has also been an inspiring teacher. His Metaphysics seminar in Spring 2008 substantially advanced my understanding of the core issues in the philosophy of science and shaped the approach I pursue in my dissertation. His presence at UIC was a great opportunity for not only the faculty of philosophy and neuroscience departments, but also for the philosophy graduate students to whom he was like a brother. I am honored and grateful to have worked with him.

I should also mention Dave Hilbert, who generously lent me his valuable insights on where to turn—or more often, on where not to turn—at important crossroads during the writing process. He also did an exemplary job as our DGS during my 6 years at UIC. My fellow graduate students' track record in winning competitive research fellowships and academic awards testifies to this.

ACKNOWLEDGMENTS (Continued)

Finally, I would like to thank Wendy Parker, who read my dissertation and volunteered to be on my committee at an extremely busy time of her career. I am grateful for her generousity.

Three others have also left deep marks in my thinking. First among them is Lucas Thorpe, who patiently persevered through my philosophical infancy and encouraged me to exorcise my logical positivist demons. Despite his young age, Lucas has been a fountain knowledge, a truly gifted teacher and an example of how one not only does philosophy but also lives it. Though I still consider myself a—reformed—positivist, without Lucas I would've never understood what I am disagreeing with.

The second person on the list is Daniel Sutherland. I have learned so much from Daniel that it is pointless to attempt an enumeration. He has one of the most intense cases of philosophical itch I have ever witnessed. He is also the best teacher I have ever had. Yet, he is startlingly humble. His pedagogical secret—I have come to believe over the years—is his willingness to think like a student. His patience and attention to detail not only enables each and every student to get something valuable from his classes but also yields deep insights in issues many other philosophers would inanely consider to be inane.

The last of the three is my dear friend Bob Fischer, who is an inspiration with all his perseverance, intellectual productivity and dedication. Bob and I first met as classmates in Fall 2007 in Daniel's Metalogic seminar and since then I have learned from Bob more than I have learned from most of my professors. I am truly fortunate to have his sustained critical input into my work, even after he left for Texas in 2011. To this day, with his stubbornly reasonable criticism, Bob has been blasting away my reasonably stubborn philosophical prejudices. But first and foremost, I am grateful for his friendship.

Here is a shamefully inadequate summary of my outstanding intellectual and personal debt: I am thankful to my brother and friend Çağrı T. Öztürk, who proof-read my work and lent his clearly superior intellect to improve the accessibility of my ideas, my friends Joshua D. Norton, who maturely endured my unending philosophical immaturity on every conceivable subject and generously lent his firm shoulder and his comfortable couch at times when the future looked more uncertain than usual, John van Dyke, who shared his—back then—unpublished superconductor research and held my hand through some difficult physics, Ahmet Raşit Öztürk, who kindly shared his valuable insights on data-modeling and the use of Cytoscape in bioinformatics, Radu Ioan Motoarca, who is a brilliant and often underestimated mind, Joseph Gottlieb, whose sophistication often discovers fine truths in places where my simple mind can't see the trees from the forest, Adam Betz, who is not only incredibly witty and but also brave enough to say so whenever the emperor makes a nude appearance, Michael Hurwitz, who is a pleasure to think together with, and Rima Kapitan who brings joy to wherever she goes. I should also mention my personal gratitude to Çağatay K. Öztürk who is my brother and my friend, and Keziban Der and Tiziana Vistarini who were good friends and roommates for several years. Saniye Vatansever also deserves much praise for being an insightful philosophical confidant for the better half of a decade, among other things for which I shall remain grateful evermore.

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LIST OF ABBREVIATIONS

NHST	Null Hypothesis Significance Testing
Q-PIM	Quasi-Particle Interference Modeling
TOKAMAK	Toroidal Plasma Confinement Device
VLBA	Very-long-baseline Array
VLBI	Very-long-baseline Interferometry

SUMMARY

All prominent critiques of the Pessimistic Induction implement either one of the following two strategies: generalism or partialism. Generalists argue that the Pessimistic Induction is a bad inference; it can't warrant pessimism about any of the current best scientific theories. Partialists on the other hand concede that the Pessimistic Induction is good inference. However, they argue that the scope of pessimism it warrants is not unlimited; there are parts of current best theories for which pessimism is not warranted.

Generalism and partialism both have problems. Generalism is overly ambitious and can't save any current best theory from pessimism. Partialism is overly concessive; in an effort to not overreach it surrenders too much to the pessimist.

In my dissertation I develop and explore a new strategy that I call "exemptionism", which aims to save some of current best theories in their entirety by demonstrating that they are completely exempt from the threat posed by the Pessimistic Induction. A current scientific theory is completely exempted from the Pessimistic Induction by this strategy if the evidence today's scientific community has for that theory is quantitatively and qualitatively superior to the evidence past scientists had for their best theories. Unequivocal and simultaneous satisfaction of four conditions is sufficient for the quantitative and qualitative superiority in question. These are domination, stability, computerization and efficiency conditions.

It turns out that some current best theories in physical sciences (*i.e.* the Fermi-Landau Liquid theory, the NICE model and the Standard Model) satisfy each one of the four conditions.

The evidence today's scientific community has for these theories is unequivocally superior to the evidence any past best theory ever enjoyed. Therefore, these theories are completely exempt from the Pessimistic Induction. In the absence of other reasons for pessimism, we are warranted to be optimists about the entirety of these three theories, not just their bits and pieces.

However, the optimism the exemptionist strategy can warrant has its limits. While there is hope that some other theories also satisfy the four conditions, most theories in behavioral and life sciences do not unequivocally satisfy the efficiency condition. Therefore, the Pessimistic Induction succeeds as far as the theories in those fields are concerned. The revolutionary history of science is sufficient grounds for expecting future revolutions that will replace or substantially revise them.

1. WHAT IS THE PESSIMISTIC INDUCTION?

Scientists tell us amazing things about the world. They tell us that the universe started with a bang about 13.7 billion years ago and its expansion is accelerating, smoking causes cancer, red wine in moderation is good for you, most of the human genome consists of junk base pairs that don't code any useful information, and the industrial civilization is warming up the climate.

But should we really believe these claims? Looking at the track record of past scientists, shouldn't we be pessimistic about the endurance of the claims of today's scientists? After all, scientific communities of the past disagreed with all of the claims I mentioned above: They thought that the universe always existed and was static, smoking was safe and red wine was not, almost all genetic material had to code for something that directly confers an evolutionary advantage, and the climate was stable.

The idea I have just described is called the "Pessimistic Induction": The history of science is a history of revolutions, where after each revolution the scientific community of the day confidently assured us that *this time around they got things right.* However, since revolutions continue happening and scientists can't seem to keep their stories straight, we should perhaps treat them like the boy who cried wolf and be pessimistic about the eventual fate of their most recent stories. It may be even time that we stopped listening to scientific experts in policy making and knock on the doors of the publicly-funded research labs and ask for a full refund.

Conceived in this way, the Pessimistic Induction is a piece of inductive reasoning with a wide scope; its projection is supposed to cover all currently accepted scientific theories.

There are several ways of formulating this wide-scope inference as an explicit argument. The first way is metaphysical:

(a) The history of science is a history of revolutions.

(b) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past on the grounds that they were false all along even if they were predictively successful and were accepted as true by scientists until the revolution.

(c) Current best theories have a common property with those abandoned or substantially revised best theories of the past: They are also predictively successful.

∴ Current best theories are also going to be abandoned or substantially revised on the grounds that they were false all along even if they are accepted by contemporary scientists as true at the moment.

Until very recently, most philosophers of science took themselves to engage only the metaphysical formulation. Thus, they took the question of scientific realism as the main contention of the Pessimistic Induction: *Are our best scientific theories true, or false but predictively successful?*

However, the metaphysical formulation of the Pessimistic Induction is not a compelling argument. Premise (b) is implausible. As Mizrahi (2012, p. 5) observes, the arguments one might have for believing (b) are not compelling arguments themselves:

(1) When scientists abandon theory T_1 in favor of T_2, it is because they think that T_1 is false and T_2 is true.

(2) In each scientific revolution scientists abandon at least one theory in favor of another theory (possibly a substantially revised version of the original theory).

∴ In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past on the grounds that they were false all along even if they were predictively successful and were accepted as true by scientists until the revolution.

(3) In each scientific revolution scientists abandon at least one theory in favor of another theory (possibly a substantially revised version of the original theory).

(4) The best explanation for (3) is that abandoned theories are false.

∴ In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past on the grounds that they were false all along even if they were predictively successful and were accepted as true by scientists until the revolution.

These arguments are not compelling because they presuppose a view of science against which there are numerous counterexamples, such as the Law of Octaves and the Bohr Atom Model, which were abandoned or substantially revised not because they were considered false but because they were limited in scope. In other words, premise (b) of the metaphysical formulation is based on a questionable account of what theory change is.

This problem can be resolved by dropping the clauses regarding the actual or perceived truth values of the theories from the argument, which yields the following 'neutral formulation' of the Pessimistic Induction:

(a) The history of science is a history of revolutions.

(b*) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past even if they were predictively successful.

(c*) Current best theories have a common property with those abandoned or substantially revised best theories of the past: They are also predictively successful.

∴ Current best theories are also going to be abandoned or substantially revised.

The neutral formulation is silent regarding the truth values of the theories of the past and merely focuses on the fact that they were abandoned or substantially revised regardless of their truth or falsehood. Therefore, it by-passes the objection Mizrahi raises against the metaphysical formulation.

Neutrality regarding truth values also enables us to break free from the *true vs. false but empirically successful* dichotomy, by offering us a chance to recast the Pessimistic Induction within the wider framework of the *optimism vs. pessimism* question: Are today's best theories the best and final theories evidence will ever warrant, or will they share the fate of their historical counterparts by being abandoned in favor of newer theories?

However, the neutral formulation shares a weakness with the metaphysical formulation. Both arguments focus on the predictive success of theories as the property shared between the historical sample and projected class of the induction. Although this is technically correct, it is also highly misleading. If the Pessimistic Induction is a compelling argument, what constitutes grounds for pessimism is not the empirical success of our best theories. The grounds for pessimism, if they are to be found anywhere, are to be found in the fact that all our successful theories have been accepted on comparable epistemic grounds. It is clearly not what the theory accomplishes that makes it suspect. True theories are empirically adequate by definition.[1] The justification process leading to the acceptance or abandonment of theories is what warrants the inductive generalization involved in the Pessimistic Induction.

[1]This statement is to be taken with a grain of salt, just like any other 'truth by definition.' In particular, its truth depends on how we understand empirical adequacy. If empirical adequacy is understood to be a theory's *ability* to make specific and exclusively true predictions when coupled with an adequate and sufficiently true methodology (auxiliary assumptions about instrumentation plus background theory), then a true theory is empirically adequate 'by definition.' However, a true theory may not be empirically adequate in another sense: it may fail to make specific and exclusively true predictions *in actuality.* When a true theory is coupled with an inadequate or largely false methodology, it may make false predictions or make no predictions whatsoever. For instance, if you couple the Big Bang cosmology (which might be a true theory) with an incorrect theory of thermal radiation, you will get false predictions about the last scattering surface even if the Big Bang cosmology were true.

Therefore, we should replace the neutral formulation with the following epistemic formulation:

(i) The history of science is a history of revolutions.

(ii) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past.

(iii) The epistemic status of the current best theories is not different from—and therefore is not superior to—the epistemic status of the best theories of the past that were abandoned or substantially revised.

∴ The current best theories are also going to be abandoned or substantially revised in the future.

The epistemic formulation does two things at once. Like the neutral formulation, the epistemic formulation too is metaphysically neutral; it is silent regarding the actual or perceived truth values of the best theories of the past. Therefore, it avoids Mizrahi's objection that the argument is based on an unrealistic view of theory change. Second, the epistemic formulation highlights one important fact about the dialectic character of the argument that is hidden by the metaphysical and neutral formulations of the Pessimistic Induction. What warrants pessimism about current best theories is not that they are successful but that they are presumably produced and justified by comparable epistemic processes as their predecessors.

For these reasons, a charitable critic of the Pessimistic Induction ought to engage the epistemic formulation rather than the metaphysical and neutral formulations. This is what I am going to do in this manuscript for the most part, and "the Pessimistic Induction" henceforth will refer to the epistemic formulation of the argument, unless noted otherwise. The only exceptions will occur in Chapter 3 where I will review the prominent objections against the Pessimistic

Induction, which mostly engage the metaphysical formulation. In Chapter 4 and onwards, I will consider solely the epistemic formulation. Since the neutral formulation is neither a charitable reading of the argument nor is engaged by any prominent critics, I will not return to it in the following chapters.

1.1 To Meta or Not To Meta?

The Pessimistic Induction is sometimes called the "Pessimistic Meta-Induction." However, the literature is divided about whether the argument is to be characterized meta-inductively or not. Psillos (1999), Worrall (1994), Doppelt (2007), Lange (2002), Stanford (2006) and Lewis (2001) either consciously mean both meta and non-meta characterizations, or unconsciously slide back and forth between them. Leplin (1984; 1997), Magnus & Callender (2004) and Bishop (2003) characterize it exclusively as meta-inductive. Kitcher (1993), Enfield (2008), Elsamahi (2005) and Fahrbach (2011) characterize it exclusively as non-meta.[2] Chang (2003) and Chakravartty (2004) on the other hand offer formulations that are consistent with both the meta and non-meta characterizations of the argument.

It was Leplin, Worrall, Ladyman and—more recently—Psillos who popularized the idea of characterizing the argument meta-inductively. Among these, Leplin was the first:

> [T]heories that excelled under criteria now employed in evaluating theories ultimately proved unacceptable; therefore, it is likely that the best current theories,

[2]Peter Lipton, occasionally seems to agree with Leplin and others in that the Pessimistic Induction (he calls it the "disaster argument") is a meta-inductive argument (2000, p. 198). However, the appearance could be deceptive in Lipton's case because he takes "induction" to be an umbrella term, which covers all ampliative (non-deductive) inference forms (2000, p. 193).

> and even better ones that we might imagine overcoming what defects we now recognize in current theories, will prove unacceptable. Thus, we have a strong inductive argument against the ultimate acceptability of theories that we have strong inductive grounds to accept. (Leplin, 1984, p. 193)

Leplin and his followers pitch the argument as a meta-induction, which is not explicit in the metaphysical, neutral and epistemic formulations I offered above. However, this apparent divergence is not a disagreement about the form of the argument. It is rather a disagreement about what science is. For Leplin, and many of his contemporaries, scientific reasoning is distinctly inductive. That's why he and his followers see the Pessimistic Induction as a meta-induction; according to their view, it is a piece of inductive reasoning about inductive reasoning itself.

The inductivist view of science is highly problematic. It would be an oversimplification to characterize scientific reasoning as decidedly inductive, if not an outright falsehood. Science uses not only induction, but also refutation, inference to the only/best explanation, eliminative, combinatorial and computational methods of discovery and justification that are not classically inductive in any recognizable form. More importantly for the purposes of the debate however, the meta-inductive characterization violates the principle of charity. It renders the argument vulnerable to the objection that it is self-defeating. After all, if the Pessimistic Induction is an inductive argument demonstrating that inductive inferences are unreliable, then the Pessimistic Induction itself is likely to have a false conclusion.

The trouble is avoided completely by dropping the meta from the characterization. That is why, with the exception of Chapter 3 where I am going to review prominent critiques of the argument, among which some construe the Pessimistic Induction as meta, I will stick to the non-meta characterization.

1.2 What Lies Ahead

My overall objective is to convince you that there is room for a substantial form of optimism in the face of the Pessimistic Induction. The fact that the history of science is indeed a history of revolutions is not grounds for pessimism about all theories. *In the absence of other grounds for pessimism*,[3] some current best theories are likely final. It is likely that they are the best theories that evidence will ever warrant. They are unlikely to share the sad fate of their predecessors; they are here to stay.

I will eventually (in Chapter 4) develop a new strategy to demonstrate this claim. But before that I will first do two things. In order to preempt a possible misunderstanding, I will give an overview of the purported origins of the Pessimistic Induction in Chapter 2 and show that contrary to the impression one is likely to get from a cursory glance at the Pessimistic Induction literature, the argument (even the metaphysical formulation that most people have in mind) didn't come from Larry Laudan's (1981) "A Confutation of Convergent Realism." Not only can earlier formulations of the Pessimistic Induction be found in Mary Hesse's (1976) and Henri Poincaré's (1902) works, but also there are good grounds for thinking that Laudan's argument

[3] It is important to not overlook this qualifier. The Pessimistic Induction is not the only argument that can warrant pessimism. There are other potentially compelling arguments that suggest that none of our best theories is likely to be final. For instance, the problem of underconsideration is one such argument. I am not going to engage the problem of underconsideration or any other pessimistic arguments in my thesis. Therefore, the reader should take my seemingly awefully strong claim with a pinch of salt.

was intended as an undercutting defeater against the No-miracles argument for scientific realism, not as a stand-alone argument for antirealism or pessimism of any sort.

From the origins of the Pessimistic Induction, I will then move on to the prominent objections against the metaphysical formulation of the Pessimistic Induction in Chapter 3. There I will argue that virtually all prominent critics of the argument without any detectable exception follow either one of the two following strategies:

- <u>The Generalist Strategy:</u> The Pessimistic Induction is a bad argument. Therefore, it can warrant pessimism about no current best theory of any mature science.
- <u>The Partialist Strategy:</u> The Pessimistic Induction is a good argument and it is partly successful. However, it doesn't warrant pessimism about certain parts and aspects of the current best theories of mature sciences.

As we shall see again in Chapter 3, there are several attempted general defeaters against the inference involved in the Pessimistic Induction, including the statistical and epistemic objections I will review in Sections 3.2.1 and 3.2.2. However, both have serious problems.

The partialist alternative on the other hand, is too concessive. In an effort to be not so ambitious as the generalists, partialists surrender to pessimism without a fight some potentially defensible parts and aspects of the best theories. Some among these partialist objections manage to thwart pessimism concerning some bits and pieces of our best theories. However, it is also disappointing that these bits and pieces are often highly uncontroversial among the scientific community and have little relevance to policy making, which is a situation that reinforces the frequently voiced conviction that the philosophers of science engaged in the Pessimistic Induction debate don't have many significant theoretical or practical insights to contribute.

In other words, the generalist strategy is overly ambitious and it fails to achieve its goal. It attempts to save all of our best theories in their entirety, yet fails to save any. The partialist strategy is more successful, but it is on the other extreme of the spectrum; in an effort to not overreach, it surrenders too much to the pessimist. The partialist success is limited to warranting optimism about disconnected bits and pieces of our best theories. Therefore, it fails to warrant a substantial form of optimism.

In Chapter 4, I will turn my attention solely to the epistemic formulation of the Pessimistic Induction. After briefly looking at Ludwig Fahrbach's recent generalist—and unmoving—objection, I will use his insights to articulate a new strategy that works against the epistemic formulation by striking a balance between the extremes of generalism and partialism. Here is a brief description of this new strategy:

- The Exemptionist Strategy: The Pessimistic Induction is generally successful, but there are identifiable exceptions to it. The evidence today's scientists have for some current best theories is in some cases significantly better than the evidence their predecessors had for their best theories. Such current theories whose epistemic status is better than their predecessors are *completely exempt* from the Pessimistic Induction.

The improvement in epistemic status of theories the exemptionist strategy relies on is possible in part due to an almost global exponential increase in the quantity of evidence we have for all theories, which correlate strongly with the exponential growth of the demographics of science. More importantly, due to recent computerization of research, the quality of evidence for some of our best theories has also vastly improved.

The qualitative improvement in question manifests itself in three ways: Computer use increases the quality of the already available evidence by allowing scientists to produce more effective data models, make more error-free calculations and conduct more reliable measurements. Computerization of science, either through instrumental control or through the use of classical simulations, also allows scientists to devise experiments and observations that would have been logistically impossible without them. Therefore, they increase the attainability of evidence by allowing scientists to test theories that are not testable by conventional observations and experiments. Finally, computerized research occasionally provides us with a novel source of evidence that I will call "evidence *ex silico*," which has epistemic autonomy from theory and methodological parity with the traditional sources of evidence.

These three developments separately as well as jointly allow today's scientists to not only overcome the logistical barriers that hindered theory testing in the past but also diversify the available sources of evidence that they can tap. Therefore, they render the sum total of the evidence they have for our best theories qualitatively superior to the evidence their predecessors had for their best theories.

In other words, the following is the logic of the exemptionist strategy that I will deploy: In a sequence of successor theories in a field of science, if there is a clear quantitative and qualitative improvement in the evidence the most recent theory enjoys relative to its predecessors, premise (iii) of the Pessimistic Induction

(iii) The epistemic status of the current best theories is not different from—and therefore is not superior to—the epistemic status of the best theories of the past that were abandoned or substantially revised.

won't hold. Therefore, such theories are *completely exempt* from pessimism. We can be optimistic about such theories in their entirety, not just their bits and pieces. Here are four conditions that I will argue to be jointly sufficient for an exemption:

- Domination Condition: The theory currently dominates its field; there are no other serious contenders.
- Stability Condition: The field has been stable for long enough that the exponential increase in research capacity has led to an unprecedented accumulation of potential evidence in favor of the theory.
- Computerization Condition: Computerization of the field has vastly increased the quantity and quality of evidence produced in the field and diversified the sources of evidential support the theories in that field enjoy, making the current dominant theory unique among all the theories ever dominated that field.
- Efficiency Condition: The field has been efficient and not wasteful in utilizing increased research capacity resulting from the exponential growth and computerization of science.

When these four conditions are simultaneously and unequivocally satisfied, we have what I call an "exempting defeater"[4] against the epistemic formulation the Pessimistic Induction. The exempting defeater in question doesn't function like a general defeater which overturns the argument to save all of our best theories. It rather protects them piecemeal.

In Chapter 5, I will take a close look at how some of our best theories unequivocally satisfy the most crucial one of these four conditions: the computerization condition. As a result of our investigation, we shall see that three of our best theories—Fermi-Landau Liquid theory in condensed matter physics, the NICE Model in Solar System cosmology and the Standard Model in particle physics—are completely exempt from the Pessimistic Induction. I hope that a convincing case can be made that these four conditions will also hold for some other theories in physical sciences and a smaller set of theories in biological sciences, such as the Theory of Natural Selection and perhaps even General Relativity though I will leave that as an unexplored conjecture.

On the flip side of the coin, exemptionism is necessarily limited in scope. Its limits, which I will explore in Chapter 6, leave a substantial number of contemporary theories outside the

[4]An exempting defeater, unlike a general defeater, does not prove the inference it defeats to be a generally unreliable inference. Instead it demonstrates merely that the inference is specifically unreliable when certain conditions are satisfied. Descartes' *dream argument* is an example of an intended general defeater against all inferences we make about the external world from sensory experience.

The following scenario illustrates how an exempting defeater against such inferences would work: Suppose you are in my office and on my office desk there is a balloon which appears red. From your sensory experience, you infer that there is a red balloon on my desk. However, I walk in and inform you that the balloon is actually white, but it appears to be red because there is a hidden red spotlight focused at it. Assuming that my testimony is reliable, you should conclude that the inferences you make from your color perception are unreliable in this specific setting though they may be generally reliable otherwise.

protection of the exempting defeater. Especially those fields that have been unstable, that couldn't employ computerized research to its full potential and that are bogged down by serious methodological problems are left entirely outside the scope of exemptionism as I defend it. In other words, no theory in such fields can be protected against the epistemic formulation of the Pessimistic Induction by the exempting defeater. Economics, behavioral sciences, health sciences and imaging-based neuroscience are probably among these fields, to name a few.

However, this doesn't entail that no theory in those fields is true or even warranted by the current evidence. First of all, the exempting defeater that I will articulate in Chapter 4 is not necessarily the only possible exempting defeater. In particular, I believe that the advances in information technology could make it possible to substitute the computerization condition with a possibly stronger condition, which would also entail a quantitative and qualitative change in the epistemic status of current theories. Fifty years ago, hardly anyone could have imagined how many steep barriers in research will be overcome by the computerization of research. Similarly, I can't rule out an even more powerful methodological future advance that will make look pale the improvement in the quantity and quality of evidence made possible by computerization. However, until such an advance happens, I believe that the computerization condition, or something like it, will remain an integral part of the exemptionist strategy.

That is also why the Pessimistic Induction provides strong empirical grounds to have pessimistic expectations about the eventual fate of the theories which don't presently satisfy all four of the conditions, including the computerization condition.

This may appear to be a serious drawback of exemptionism, but it is really not. Here is why: Science has achieved near-complete domination over every aspect of human social organization. Everyone wants and believes science to be on their side. From the leaders of religious movements to the outspoken self-identified 'skeptical warriors', from the physicians who diligently read all leading peer-reviewed journals in their areas to the patrons who frequent their local homeopathic 'wellness' centers, from the pharmaceutical company that sells a new anti-cholesterol drug that 'may help reduce the risk of a fatal heart attack up to 25%!' to the marketer who sells running shoes with separate toes, everyone wants science as their ally, or at least as their foe's foe.

This is alarming because when an institution acquires so much power over people's beliefs and choices, that power will certainly be abused. Claims that do not live up to the even loosest standards of critical thinking will be made and taken as final answers because they are produced and warranted in a scientific setting, as the well-documented troubles of pharmaceutical research indicate.

However, some scientific theories are much better justified than others. Exemptionism is not only sensitive to this fact, but also turns it into an advantage and lets us choose among various mature theories by comparing their epistemic statuses.

In this regard, what looks like a limitation of exemptionism is actually a virtue, for it helps us maintain a healthy view of scientific rationality. Not all scientific theories are created equal; Not every piece of scientific consensus has the same epistemic status. The authority of scientific rationality is a species of the authority of reason, which is legitimate only so far as it satisfies stringent and evolving standards of critical thinking. The four conditions I single out as jointly-sufficient conditions for an exempting defeater are nothing but some—but not necessarily all—concrete corollaries of those standards.

It looks as though we have an important quest ahead of us. Before embarking on this quest however, let's first take a brief look at the history of the Pessimistic Induction.

2. ON THE GENEALOGY OF THE PESSIMISTIC INDUCTION

In Chapter 1, I offered two separate formulations of the Pessimistic Induction. The first one was the metaphysical formulation:

(a) The history of science is a history of revolutions.

(b) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past on the grounds that they were false all along even if they were predictively successful and were accepted as true by scientists until the revolution.

(c) Current best theories have a common property with those abandoned or substantially revised best theories of the past: They are also predictively successful.

∴ Current best theories are also going to be abandoned or substantially revised on the grounds that they were false all along even if they are accepted by contemporary scientists as true at the moment.

The second one was the epistemic formulation:

(i) The history of science is a history of revolutions.

(ii) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past.

(iii) The epistemic status of the current best theories is not different from—and therefore is not superior to—the epistemic status of the best theories of the past that were abandoned or substantially revised.

∴ The current best theories will be abandoned or substantially revised in the future.

There are two crucial differences between these formulations. The first one is what is at stake. The metaphysical formulation pitches the contested issue as the question of scientific realism (*i.e.* "Are our theories true, or predictively successful but false?") whereas the epistemic formulation is neutral regarding truth or falsehood. Instead, the epistemic formulation focuses on the choice between optimism and pessimism (*i.e.* "Are our theories here to stay, or will they too be abandoned or substantially revised.") The second difference is the property that each formulation takes as the basis of the inductive generalization. Inductive arguments work only when the members of the class over which the generalization is made resemble each other in relevant respects. The way the metaphysical formulation contends the similarity in question is to focus on the common property *predictive success* of the best theories of the past and present. The epistemic formulation on the other hand, highlights the epistemic status of past and present theories.

In the present chapter, I shall take a look at the origins of the Pessimistic Induction as an argument. My goal here is understanding if and how the origins and history map onto the two formulations on our radar.

Studying the origins of the Pessimistic Induction can be an object lesson in divergence between popular opinion and historical fact. In order to understand how the debate evolved into what it is today as well as to preempt a possible misunderstanding of the exemptionist strategy I will defend in Chapter 4 against the epistemic formulation of the Pessimistic Induction, we need to understand where the Pessimistic Induction came from and how it differs from where people often say it came from.

This is so because of two reasons: popular opinion among the participants of the Pessimistic Induction debate often misjudges the history in crucial respects and it has been mostly the popular opinion that made the debate what it is today. The present chapter aims to put forward a succinct account of both the correct historical origins of the argument and the popular opinions that have shaped the debate. In doing so, we will not only put the critical responses to the argument and the current state of the debate into historical perspective, but also avoid the misunderstanding about the objective of exemptionism.

While looking at the history of the debate, my focus will be on the various characterizations of the argument. I shall observe that the most common characterization is closer to the metaphysical formulation, with a twist. While the metaphysical formulation I have offered isn't a meta-inductive argument, it is relatively common in the Pessimistic Induction debate to see the argument as an induction about induction itself. For reasons I explained in Chapter 1, this is a mistake.

2.1 The No-miracles Argument and The Historical Gambit

While reading the literature on the Pessimistic Induction, the casual reader might get the impression that the argument received its seminal expression in 1981 with Larry Laudan's "A Confutation of Convergent Realism." As I will demonstrate later on though, this is one of the points on which popular opinion and history diverge.

The following are three examples where the Pessimistic Induction is identified as the brainchild of Laudan:

> The 'Pessimistic Induction' [...] suggests that the 'no miracle' argument flies in the face of the history of science. Laudan's 'historical gambit' consists of a list of theories of the past— which "could be extended *ad nauseam*"—that are characteristically false yet once were viewed as empirically successful and fruitful. (Psillos, 1996b, p. s306)

> Laudan's argument against scientific realism is simple but powerful. It can be summarized as follows: The history of science is full of theories which at different times and for long periods had been empirically successful, and yet were shown to be false in the deep-structure claims they made about the world. It is similarly full of theoretical terms featuring in successful theories which do not refer. Therefore, by simple (meta-)induction on scientific theories, our current successful theories are likely to be false. (Psillos, 1999, p. 96)

> It is worth noting that we do not need to construct a meta-induction, as Laudan does, to use theory change against traditional realism. (Ladyman, 1998, p. 413 in footnote)

These three examples understate the actual situation because in the overwhelming majority of the cases Laudan is credited casually in the bibliography section or implicitly in the text, and neither lend themselves easily to quotation. However, among those who explicitly endorse—or at some point endorsed—Laudan's paper as the origin of the Pessimistic Induction, there are John Worrall, James Ladyman, Stathis Psillos, Peter Lewis and Kevin Kelly.[5]

Laudan's paper was ostensibly a critical response to the No-miracles argument for scientific realism. Since the metaphysical formulation of the Pessimistic Induction I identified in Chapter 1 is closely tied to the scientific realism debate, we need to understand the No-miracles argument and how Laudan responded to it to make sense of how his argument is situated in relation to the Pessimistic Induction.

The No-miracles argument is probably the most straightforward case for scientific realism, the thesis that the best theories in mature sciences are true. The argument was first put forward

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[5]Patrick Enfield (2008, p. 881) is a recent exception to this norm.

by Hilary Putnam and Richard Boyd. Its logical structure is most clear in the following passage from Putnam's "What is Realism?":

> [T]he modern positivist [*i.e.* the antirealist of the time], has to leave it without explanation [...] that 'electron calculi' and 'space-time calculi' and 'DNA calculi' correctly predict observable phenomena if, in reality, there are no electrons, no curved space-time, and no DNA molecules. If there are such things, then a natural explanation of the success of these theories is that they are partially true accounts of how they behave. And a natural account of the way in which scientific theories succeed each other—say, the way in which Einstein's Relativity succeeded Newton's Universal Gravitation—is that a partially correct, partially incorrect account of a theoretical object—say, the gravitational field, or the metric structure of space-time, or both—is replaced by a better account of the same object or objects. But if these objects do not really exist at all, then it is a miracle that a theory which speaks of gravitational action at a distance successfully predicts phenomena; it is a miracle that a theory which speaks of curved space-time successfully predicts phenomena; and the fact that the laws of the former theory are derivable "in the limit" from the laws of the latter theory has no methodological significance. (1976, pp. 177-8)

Looking at this passage, one might be puzzled by the fact that today's scientific realists usually pitch the No-miracles argument as an inference to the best explanation rather than an inference to the only explanation.[6] Perhaps this was an interpretive mistake on the part of the scientific realists who were trying to follow Putnam.

However, the change in the sales pitch is no mistake. Suppose Putnam is right: We can think of only one explanation for the success of science and it is scientific realism. How can this justify an inference to the truth of scientific realism? If the fact that we can think of only one explanation is an indication of anything, it is likely an indication of our ignorance. Moreover,

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[6]This distinction does not make sense within the hypothetico-deductive model of explanation, which requires that only truths explain. For an early recognition of this point, see Hospers (1946, p. 345).

Putnam and his fellow naturalists are very careful in gift-wrapping scientific realism as a thesis that is open to empirical test (Laudan, 1981, p. 19). If there are no available alternatives to a thesis then we should suspect that either the thesis is not really empirically testable or there are alternatives that we are missing out.

Both possibilities spell serious trouble for scientific realism backed up by the inference to the only explanation. Moreover, non-realists such as Bas van Fraassen and Daniel Dennett have since proposed naturalistic explanations for the predictive success of science. On their view, scientific theories are model-theoretic constructs which are under selective pressure for predictive success (van Fraassen, 1980, pp. 39-40). Hence, even if the unobservables in those constructs do not correspond to anything in the world, it would be reasonable to expect eventual predictive success.

The quasi-Darwinian account Dennett and van Fraassen defend could also explain why law-like expressions picking out some parts of a successful model would be derivable in the limit from the law-like expressions picking out another successful model. The explanation is basically convergent evolution. So, "derivability in the limit" is not something only a realist can explain.

The No-miracles argument is not very compelling when pitched as an inference to the only explanation as Putnam suggested. In this regard, pitching the No-miracles argument as an inference to the best explanation was a step forward, not backward. So, the most promising version of the No-miracles argument is an inference to the best explanation: The best—or the best naturalistic—explanation of the predictive and explanatory success of scientific theories is that they are true.

As I have already mentioned, one way the antirealist could respond to this challenge is to offer a non-realist explanation for predictive success. Another way of responding to the challenge is by denying the very legitimacy of the challenge. For instance, one might argue that there is no need for explaining scientific success because there is no distinctive and objective scientific success. According to this view, which was defended by Paul Feyerabend and by some radical Kuhnians, success is a radically theory-dependent notion. A scientific theory appears successful if and only if the judge of success is already sold on the theory.

A third antirealist strategy against the No-miracles is to attack inference to the best explanation. There are two ways of implementing this strategy. First, one can argue that inference to the best explanation as an inference type is problematic. This venue was explored by van Fraassen (1989) in *Laws and Symmetry*.[7] The second way of attacking inference to the best explanation is by arguing that the inference to the best explanation token invoked in the No-miracles argument is problematic. This is exactly what Laudan aimed to do in his 1981 paper, "A Confutation of Convergent Realism."

The popular opinion among the participants of the Pessimistic Induction debate however is that it was that paper where the Pessimistic Induction first appeared in print. As we shall see, the popular opinion is incorrect. Laudan's argument does not have an inductive form. Moreover, the Pessimistic Induction is more than a century old, and it seems to have been

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[7]See especially pp. 143–146.

independently discovered at least twice well-before Laudan launched his attack against the No-miracles argument.

Laudan recognizes that most realists (he calls them "empirical realists") construe their view as an empirically testable position. Laudan concedes empirical testability of scientific realism and attempts to show that "realism, at least in certain of its extant forms, is neither supported by, nor has it made sense of the available historical evidence." (1981, p. 20)

The "extant forms" which Laudan is referring to is a family of philosophical positions, which Laudan calls "convergent realism." They share the following theses:

(R1) Scientific theories in the mature sciences are typically approximately true and more recent theories are closer to truth than older theories in the same domain.

(R2) The observational and theoretical terms within theories of a mature science genuinely refer.

(R3) Successive theories in any mature science will preserve theoretical relations and the apparent referents of earlier theories.

(R4) Acceptable new theories should explain why their predecessors were successful insofar as they were. (1981, p. 20-21)

Laudan argues that the only argument for (R2) is the No-miracles argument, which presents reference (or simply truth as correspondence) as the best explanation for success. However, according to Laudan the No-miracles argument requires the following premises:

(S1) The theories in the mature sciences are successful.

(S2) A theory whose central terms genuinely refer will be a successful theory.

(S3) If a theory is successful, we can reasonably infer that its central terms genuinely refer.

(S4) All the central terms in the mature sciences do refer. (1981, p. 23)

Laudan claims that none of these premises except (S1) is acceptable. It is not clear why Laudan thinks that (S2) is needed for the No-miracles argument. A good—or even the best—explanation does not need to make the explanandum likely.[8] (S4) also needs not be defended by the realist because it is implied by the conjunction of (S1) and (S3). Since Laudan grants that (S1) is not to be contested, his real target should be just (S3), which is indeed very close to the target of the metaphysical formulation of the Pessimistic Induction.

Laudan neither calls his argument an "induction" of any sort nor does he put it in an inductive form. Instead, Laudan offers us a substantial but not exhaustive list of successful theories of the past which turned out to be non-referring: the electrical fluid theory, the caloric theory, the optical, gravitational and physiological ether theories. (1981, p. 25-26)

Laudan thinks that this list continues and "for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring." (1981, p. 35) Laudan concludes,

> "(S3) is a dubious piece of advice in that there can be (and have been) highly successful theories some central terms of which are non-referring [... and] the realist's claim that he can explain why science is successful is false." (1981, p. 27)

In his later work, Laudan calls his argument the "historical gambit," but does not change the form or aim of the argument:

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[8]This suggests that Laudan might have been operating under the deductive-nomological account of explanation.

> [T]he intuitions which motivate the realist enterprise by arguing (among other things) that many (now discredited) scientific theories of earlier eras exhibited an impressive sort of empirical support, arguably no different in kind from that enjoyed by many contemporary physical theories. Yet we now believe that many of those earlier theories profoundly mis-characterised the way the world really is. More specifically, we now believe that there is nothing in the world which even approximately answers to the centrally explanatory entities postulated by a great many successful theories of the past. My approach in [the 1981] essay, which we might call the historical gambit, was to show that these historical cases call into question the realist's warrant for assuming that today's theories, including even those which have passed an impressive array of tests, can thereby warrantedly taken to be true. (1984, p. 157)

In this regard, Laudan's original formulation of the argument was not inductive, let alone being meta-inductive. When taken literally, his argument is deductive:

(1) The history of mature sciences contains examples of successful yet non-referring (*i.e.* false) theories.

(2) If success were best explained by truth in all cases, there wouldn't have been successful yet non-referring theories.

∴ Therefore, success is not best explained by truth in all cases.

(Corollary) The No-miracles argument is undercut, we have no reason to believe that all central terms in mature sciences refer.

In other words, Laudan's argument was an attempt at providing an undercutting refutation of the No-miracles argument by demonstrating that (S3) is not true, or at least that the connection between success and truth is not exceptionless enough to justify the universal generalization in (S4). In this regard, Laudan is not offering an inductive argument for antirealism. He is offering counterexamples against an argument whose conclusion is formulated as an exceptionless universal generalization.

It is one thing to try to undercut a realist argument with counterexamples and quite another thing to offer an argument for antirealism regarding present or future science. In this regard, Laudan's argument is not a stand-alone argument for antirealism. In fact, his argument is consistent with the truth of the current scientific theories. However, in order to convince us the realist now needs to come up with an argument that does not rely on an exceptionless hypothetical explanatory link between empirical success and truth.

In this regard, it's hard to agree with those who maintain the notion that the "Pessimistic Induction" refers to an enumerative inductive argument for antirealism, which first appeared in print in Laudan's 1981 paper. The notion appears wrong-headed in three respects: First, Laudan's paper does not state the argument inductively. His statement of the argument appears to be deductive. Second, a careful reading reveals that his argument is an intended undercutting refutation of the No-miracles argument, not as a stand-alone argument for antirealism. It is clear that the same historical evidence can be marshaled for an inductive argument for antirealism, but there is no evidence that this is what Laudan intended to. Third, something much closer to the Pessimistic Induction as we know it appeared in print at least twice before Laudan, as we shall see in the next section.

2.2 The Origins of the Pessimistic Meta-Induction

The earliest place one encounters a statement of what we today call the Pessimistic Induction is Henri Poincaré's 1902 book *Science and Hypothesis*:

> The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he sees them abandoned one after another;

> he sees ruins piled upon ruins; he predicts that theories in fashion to-day will in a short time succumb in their turn, and concludes that they are absolutely in vain. This is what he calls the *bankruptcy of science*. (Poincaré, 2001, p. 122)

This statement is much closer to the way most realists understand the Pessimistic Induction than Laudan's argument. Unlike Laudan's historical gambit, Poincaré's *man of the world* is making an inductive inference from the abandonment of the past to the present and even future theories.

It is important to observe here that the inference Poincaré's *man of the world* is making is consistent with both metaphysical and epistemic formulations of the Pessimistic Induction I have offered in Chapter 1. Although Poincaré doesn't make any explicit reference to truth or falsehood of the abandoned theories in the passage, one can interpret "absolutely in vain" as implying falsehood. In this regard, Poincaré can be credited for the discovery of both the metaphysical and epistemic formulations of the Pessimistic Induction.

Another place where the Pessimistic Induction appeared in print before Laudan is Mary Hesse's 1976 paper "Truth and the Growth of Scientific Knowledge." In the paper, Hesse presents a thesis that she calls the "principle of no privilege," according to which there is "no privilege for theoretical ontologies of our science relative to other scientific theories." (1976, p. 261)

Although Hesse implies that the principle is justified by historical evidence, she does not explain exactly how. However, she believes that the principle of no privilege "arises from induction drawn from the history of science," which must be blocked if realism is to be defended. (1976, p. 271) This suggests that the most plausible argument for the principle is an inductive one: Many theories of the past were successful, but they and their ontologies were abandoned in favor of newer ones. Therefore, we have inductive evidence to predict that today's theories and their ontologies will share the same fate.

Hesse points out that the principle leads to full-fledged antirealism (which she occasionally calls "relativism"):

> Every scientific system implies a conceptual classification of the world into an ontology of fundamental entities and properties; it is an attempt to answer the question "What is the world really made of?" But it is exactly these ontologies that are most subject to radical change throughout the history of science. Therefore, in the spirit of the principle of no privilege, it seems that we must say either all these ontologies are true [...] or we must say that they are all false. But they cannot all be true in the same world, because they contain conflicting answers to the question [...] Therefore, they must be all false. (1976, p. 266)

In two respects Hesse's argument from the principle of no privilege is significantly closer to the contemporary inductive formulation of the Pessimistic Induction than Laudan's historical gambit. First, as I have already indicated, Hesse must have had some inductive justification in mind for her use of the principle, whereas Laudan's historical gambit appears to be deductive. Second, Laudan's argument does not entail that antirealism is true; in its original form it merely refutes an argument for scientific realism. Hesse, however, uses the principle of no privilege to argue that realism is false and antirealism is true.

This last point also indicates that Hesse's inductive reasoning more explicitly coincides with the metaphysical formulation of the Pessimistic Induction than Poincaré's *man of the world.* Given the fact that most critiques of the Pessimistic Induction seem to engage the metaphysical formulation, Hesse is probably a more sensible target than Laudan.

I do not think that correcting the inaccurate attributions I have documented in the present chapter is extremely crucial for our purposes. After all, Poincaré, Hesse and Laudan all relied on overlapping sets of historical evidence, and there is an inductive argument begging to be made from the union of those sets. Therefore, I will just finish this section with a cautionary remark: The literature often attributes ideas to Laudan's 1981 paper that are not in it, and it is quite possible that Laudan would not approve of this attribution. Moreover, it is important to distinguish between the Pessimistic Induction as a critique of the No-miracles Argument and the Pessimistic Induction as a stand-alone argument for antirealism.

My interest in this manuscript is confined to the epistemic and—occasionally—metaphysical formulations of the Pessimistic Induction. The metaphysical formulation is a stand-alone argument for antirealism whereas the epistemic formulation is a stand-alone argument for broader pessimism. In this regard, one must not read exemptionism, which I will articulate in Chapter 4, as an objection against Laudan's historical gambit. The historical gambit succeeds. The No-miracles argument is unsalvageable for the reasons Laudan and other critics of the No-miracles point out. The objective of the exemptionist strategy is to defeat the epistemic formulation of the Pessimistic Induction, it is not to save the No-miracles argument.

Now that we have put the messiest bit of the history behind us, we can look at these prominent objections with an eye on opportunities for learning from their shortcomings.

Conclusion

The brief review I offered in this chapter suggests that at least after Laudan's 1981 paper there has been some consensus about the nature of the Pessimistic Induction: It is an inductive argument, and it provides historical evidence which allegedly discredits current scientific theories. The review also revealed strong grounds for thinking that the goal of Laudan's paper is to offer a non-inductive undercutting defeater against the No-miracles argument rather than a stand-alone inductive argument for antirealism. Finally, Hesse and Poincaré formulated stand-alone inductive arguments for antirealism well before 1981.

In Chapter 3, where I will look at prominent objections against the Pessimistic Induction, I will consider objections against the metaphysical formulation, despite the fact that those objections often fail against the epistemic formulation. I will do so because the objections themselves are targeted at the metaphysical formulation.

3. GENERALIST AND PARTIALIST OBJECTIONS AGAINST THE METAPHYSICAL FORMULATION

In Chapter 1, I offered two alternative formulations of the Pessimistic Induction, one metaphysical and one epistemic. In Chapter 2, I looked at the origins of the Pessimistic Induction and argued that despite the frequent references to Laudan's undercutting refutation of the No-miracles, what most participants of the debate have in mind is closer to the metaphysical formulation, whose earlier formulations can be found in Poincaré (1902) and Hesse (1976):

(a) The history of science is a history of revolutions.

(b) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past on the grounds that they were false all along even if they were predictively successful and were accepted as true by scientists until the revolution.

(c) Current best theories have a common property with those abandoned or substantially revised best theories of the past: They are also predictively successful.

∴ Current best theories are also going to be abandoned or substantially revised on the grounds that they were false all along even if they are accepted by contemporary scientists as true at the moment.

In the present chapter, I will shift my focus to the prominent objections against this metaphysical formulation of the Pessimistic Induction. I will argue that these responses follow two strategic approaches: generalism and partialism.

3.1 Two Strategies

In Chapter 1, I observed that thanks to the epistemic formulation, one need not construe the Pessimistic Induction as an argument for antirealism. It is possible to deploy the Pessimistic

Induction in a way that is neutral about the question of truth or falsehood of present or past theories. However, most participants of the Pessimistic Induction debate construe the argument metaphysically, *i.e.* as a stand-alone argument for antirealism.

This is why the participants of the scientific realism debate are often also participants of the Pessimistic Induction debate, and until recently the near consensus among them was that the Pessimistic Induction is one of the best arguments for antirealism. For example, James Ladyman in 'What is Structural Realism?' claims that as far as the realist/antirealist divide is concerned "arguably the two most compelling arguments around are the 'no miracle' argument, and the 'pessimistic meta-induction'. Unfortunately these pull in opposite directions." (1998, p. 409) Psillos agrees: "Over the last two decades, the debate over scientific realism has been dominated by two arguments that pull in contrary directions the 'no miracle' argument and the 'Pessimistic Induction'." (1996b, p. S306)

Most of the critics of the Pessimistic Induction are unsurprisingly scientific realists and they muster a significant effort to defeat it. In fact, the very landscape of contemporary scientific realism is almost completely carved out by the ways in which realists respond to the Pessimistic Induction.

However, one doesn't have to be a realist to engage the Pessimistic Induction critically. There are also those who find the argument uncompelling, such as Peter Lipton (2000), without endorsing scientific realism.

Despite their different goals and commitments however, all the prominent critiques of the Pessimistic Induction follow either one of the following two strategies:

- The Generalist Strategy: The Pessimistic Induction employs a bad inference. Therefore, it doesn't warrant pessimism about any current best theory.
- The Partialist Strategy: The Pessimistic Induction is partly successful. However, it doesn't warrant pessimism about the parts of current best theories that manifest some theoretical continuity with the best theories of the past.

As I will argue shortly, the generalist strategy is overly ambitious and the objections that follow the generalist strategy do not achieve the purported goal of saving from pessimism all current best theories in their entirety. The partialist strategy is more successful, but it is on the other extreme of the spectrum; in an effort to not overreach, the objections that follow the partialist strategy surrender too much. That's why the partialist success is limited to warranting optimism typically about only disconnected bits and pieces of our best theories. In other words, generalism attempts to save all of our best theories in their entirety and fails to save any while partialism doesn't attempt to save any of them in their entirety therefore it doesn't warrant a substantial form of optimism.

In Chapter 4, I will offer an alternative strategy that I shall call "exemptionism," which strikes a balance between the extremes of generalism and partialism, hence promising to save some of the current best theories in their entirety from the pessimistic threat presented by the epistemic formulation of the Pessimistic Induction.

However, let's first look at the generalist and partialist objections against the metaphysical formulation of the Pessimistic Induction and discuss their limitations.

3.2 Generalist Objections to the Pessimistic Induction

There are two generalist objections to the Pessimistic Induction. The first is statistical and its variations have been defended by Lewis (2001), Magnus & Callender (2004), Park (2011) and Mizrahi (2013). The other one is an epistemic objection by Lipton (2000). Being generalist objections, both try to establish the conclusion that the Pessimistic Induction is a bad argument.

3.2.1 The Statistical Objection

The proponents of the statistical objection argue that the Pessimistic Induction is a statistical fallacy known as "non-random sampling." According to this objection, the purported inductive basis of the Pessimistic Induction is not a random sample of all successful past and current theories. Instead, it is a cherry-picked collection assembled with a revolutionary bias.

In this regard, the statistical objection denies the truth of the first premise of both the metaphysical and epistemic formulations of the Pessimistic Induction:

(a)/(i) The history of science is a history of revolutions.

Relatively underdeveloped early variations of the statistical objection can be found in Lewis (2001) and Magnus & Callender (2004). However, its most mature and detailed defense to date is found in Mizrahi (2013).

Following Godfrey-Smith (2011), Mizrahi distinguishes between two kinds of inductive generalization: seen-one-seen-all generalizations *vs.* generalizations from a random sample. Correspondingly there are two possible ways of reading the Pessimistic Induction. According to Mizrahi, the seen-one-seen-all reading is extremely implausible whereas the Pessimistic Induction commits the non-random sampling fallacy if it is to be read as an intended generalization from a random sample.

Seen-one-seen-all generalizations rely on natural or artificial kind membership to make inferences about the properties of unobserved members of the population. For example, if one eats a banana and doesn't like the taste, he can safely infer that he doesn't like bananas. After all, the taste of a banana is determined by the chemicals that naturally occur in all other bananas. However, seen-one-seen-all inferences are not generally reliable since most properties of complex entities are not fixed by kind membership. For example, imagine a person who has read only one book in his life and found it boring. When asked "Why don't you read another one and see if you like it?" if this person replies "Read one, read all. They are all boring." we would think that the person is making a hasty generalization. This is the gist of Mizrahi's objection against the seen-one-seen-all formulation of the Pessimistic Induction. Claiming that one knows all or even most theories shall be abandoned after seeing one abandoned theory is like claiming that one knows all or most books to be boring after reading one boring book. I completely agree with Mizrahi on this issue: thinking that the abandonment of just one theory is sufficient grounds for pessimism about all or even most theories is absurd.[9]

When making a generalization from a random sample, one is supposed to look at a randomly selected sample within a population, where "every member of the population you are drawing

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[9]It is less clear however whether Godfrey-Smith had intended the *seen-one seen all* category as Mizrahi characterizes it. It is possible that he would recognize the *inferences predicting a change in a growing population* as a subtype of *seen-one-seen-all*.

conclusions about has the same chance of making its way into the sample." (Godfrey-Smith, 2011, p. 40) If one observes that all or most members of that random sample have a certain property, then she can infer that all or most members of the population probably have the same property.

Just randomness however, is not a sufficient condition to make a generalization from a random sample a good inductive inference. Unlike seen-one-seen-all generalizations, here the size of the inspected sample also matters. As a rule of thumb, the larger the inspected sample is, the more secure the generalization becomes. The converse also holds; the smaller the inspected sample, the less secure the generalization.

Finally, randomness of a sample and its size—relative to the size of the population—are not necessarily independent. If a human-assembled sample is conspicuously small in comparison to the size of the population it is drawn from, one plausible explanation for the difference in size is non-random sampling. If the overwhelming majority of the population have no chance of getting selected into the sample, only a small sample can be assembled.[10]

These sensible considerations combined with the assumption that the Pessimistic Induction is a generalization from a random sample constitute the basis of Mizrahi's objection: the samples offered by the pessimists as the basis of the inductive generalization are all too small

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[10]Sample size—therefore, it being small—is a relative property. There is no finite sample size that is small or large in and of itself. A sample of 5 patients suffering from a disease that infected only 5 people in total is not a small sample. On the other hand, a hundred thousand asteroids is a small sample when taken

in comparison the presumably vast population of all successful theories[11] and when we look at larger samples, we see that they don't support the inductive generalization.

In other words, Mizrahi accuses the pessimist of committing the non-random sampling fallacy; the revolutionary history of science that is alluded to in the first premise of the metaphysical and epistemic formulations of the Pessimistic Induction is a myth. Lists like Laudan's do not constitute representative samples, because they are not random.

> The theories in [Laudan's] list were not randomly selected. Rather, they were selected precisely because they are considered to be successful but strictly false [therefore, abandoned]. (p. 3220)

As I shall argue shortly, this accusation is unwarranted. Superficially however, Mizrahi's objection seems to enjoy very strong support. He provides us with two large samples, one

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[11]We should not confuse Mizrahi's claim that Laudan's list is too small with a similar sounding claim McMullin makes about the same list.

McMullin argues that Laudan's list is inflated. According to McMullin, some theories and theoretical terms Laudan includes in the list were never successful. For instance, crystalline spheres and theories of spontaneous generation would not qualify, because they were merely "intuitive postulations of an underlying reality" and they failed to "prove continuously fertile and capable of increasingly further extension." (1984, p. 17) Therefore, they must be taken off the list, which weakens the inductive inferences that can be based on it. However, McMullin is not a committed generalist. He recognizes that other items in Laudan's list fare much better, and he—along with Devitt (1984)—follows a distinctively partialist line I call "shallow revolutions thesis", which I will discuss in Section 3.3.1 (*also see* footnote 9).

Mizrahi on the other hand, concedes—for the sake of the argument, apparently—that all the theories and theoretical terms in Laudan's list are both successful and abandoned. Yet, he argues that the list is still inadequate as a sample not just because it is too small, but also because it is a non-random sample.

In other words, Mizrahi's statistical objection is distinct from but is not incompatible with McMullin's observations. McMullin neither is a generalist like Mizrahi nor does he imply any revolutionary sampling bias on Laudan's part, which is the core charge of the statistical objection.

containing 40 historical and contemporary theories, and another containing 40 historical and contemporary supposed laws of nature. Then he tells us how he analyzed the samples:

> I divided the sample of 40 theories into three categories: accepted theories (*i.e.*, theories that are accepted by the scientific community), abandoned theories (*i.e.*, theories that were abandoned by the scientific community), and debated theories (*i.e.*, theories whose status as accepted or rejected is in question) (p. 3221)

Then he informs us that only "15% of all theories [in the sample] are abandoned theories (*i.e.* considered false)" (p. 3322) (The figure rises to 27% under the generous assumption that all of the debated theories are actually false and are to be abandoned.) The situation is even worse for the pessimist as far as laws are concerned. Only 12.5% of all laws in the sample are abandoned laws. These low percentages clearly give credence to Mizrahi's claim that the pessimists committed the non-random sampling fallacy. If all successful theories in the history of science were equally likely to end up in Laudan's list, then the list would not have consisted exclusively of abandoned theories, which are in the minority.

Mizrahi's approach seems iron-clad, but there are two problems with it. The first one has to do with Mizrahi's larger samples. Some of the sources Mizrahi used when compiling these two samples (*i.e.* Oxford University Press' *A Dictionary of Biology*, *A Dictionary of Chemistry*, *A Dictionary of Physics*) are not meant to be used for historical reference. Here is an excerpt from the preface of OUP's *A Dictionary of Biology*:

> [This volume] consists of all the entries relating to biology and biochemistry in this dictionary [*i.e. The Concise Science Dictionary*], together with those entries

TABLE I

DISCOVERY DATES OF MIZRAHI'S ACCEPTED, DEBATED AND ABANDONED LAWS

Accepted Laws	**Debated Laws**	**Abandoned Laws**
Conservation of Mass (1780)	Virial Equation (1911)	Maxwell-Boltzmann Law (1859)
Carnot Principle (1824)	Charles' Law (1802)	Newton's Law of Gravitation (1687)
Periodic Law (1869)	Raoult's Law (1882)	Newton's Laws of Motion (1687)
Partition Law (1891)	Joule's Law (1845)	Law of Octaves (1865)
Hubble's Law (1927)	Avogadro's Law (1811)	Titius-Bode Law (1766)
Hess Law (1840)	Boyle's Law (1662)	
Conservation of Energy (1676, 1807, 1819)	Pressure Law (1802)	
Mass-energy Equation (1905)		
Stefan-Boltzman Law (1879)		
Planck's Radiation Law (1900)		
Wien's Displacement Law (1893)		
Law of Chemical Equilibrium (1865)		
Law of Constant Proportions (1806)		
Law of Multiple Proportions (1808)		
Law of Reciprocal Proportions (1792)		
Coulomb's Law (1785)		
Fermat's Principle (1662)		
2nd Law of Thermodynamics (1854)		
0th Law of Thermodynamics (1853, 1872, 1874)		
Kepler's Law (1609)		
Graham's Law (1831)		
Hooke's Law (1660)		
Grottius-Draper Law (1817)		
Bragg's Law (1913)		
Balmer's Law (1885)		
Moseley's Law (1914)		
Law of Isomorphism (1819)		

	Mean & Median Year of Discovery	
Accepted Laws	**Debated Laws**	**Abandoned Laws**
1824 (Mean) & 1840 (Median)	1816 (Mean) & 1811 (Median)	1773 (Mean) & 1766 (Median)

> relating to geology that are required for an understanding of palaeontology and soil science and a few entries relating to physics and chemistry that are required for an understanding of the physical and chemical aspects of biology (including laboratory techniques for analysing biological material).

In other words, these sources were never intended to be employed as historical reference. They were instead meant to be sources for students studying for their A-level exams and scientists who want to learn more about neighboring disciplines. Any contemporary science reference directed at students is, by its very nature, pedagogical. Its authors will have a clear and well-justified systematic bias against abandoned theories and supposed laws of nature, especially when those theories and supposed laws do not appear as the limiting cases or rudimentary formulations of their successors. After all, if there is no note-worthy conceptual, technical or methodological similarities between abandoned theories and laws on the one hand, and accepted contemporary theories and laws on the other, there is no point in mentioning them in a book that was written for teaching contemporary and applied sciences.

In this regard, Mizrahi's samples may not constitute a significant improvement over what the pessimists have provided us with since Laudan's promise that the list of successful false theories can be extended *ad nauseam.* In their present condition, Mizrahi's use of his samples seem to be vulnerable to the same criticism he raised against Laudan and his pessimistic followers. If Mizrahi's intention was to make an optimistic generalization from a random sample, then he is also committing the non-random sampling fallacy, but towards the opposite direction that he accuses the Pessimistic Induction of.[12]

There is also another and more serious problem with Mizrahi's objection against the Pessimistic Induction. To see this problem, we need to look at the individual items in Mizrahi's samples.[13] The first thing to notice is that the abandoned and debated theories and laws tend to be older than the accepted theories and laws. For instance, the median year of the first formulation of laws under the "Accepted" column is 1835, whereas the median of the "Abandoned" column is 1766. (see Table 3.1[14])

So, even if Mizrahi's samples are unbiased as he claims them to be, they still don't support his objection against the Pessimistic Induction. After all, his samples show that many to-be-abandoned theories and laws were once upon a time accepted earlier in the history of science. So, what do we care if today there are more accepted theories than abandoned theories? Taking this as evidence for the claim that the Pessimistic Induction is a fallacy ignores the fact that

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[12]I suspect that the point may also apply to non-pedagogical histories of science, although neither Mizrahi nor his fellow proponents of his objection attempt to employ them. Historical accounts necessarily are biased towards the cultural background in which they are written. It is natural to expect, therefore, that our understanding of the history of science to exaggerate the prominence of the currently accepted theories over the abandoned theories of the past.

[13]Mizrahi appears to accidentally list the Periodic Law twice under two different names the "Periodic Law" and the "Law of Periodicity." I correct the duplication.

[14]I used the following method in calculating the medians and means: Most of the laws in the list have uncontroversial years of discovery. However, for a few there are more than one alleged year of discovery. To err on the side of caution, I used the latest reported year of discovery for abandoned and debated laws and the earliest for accepted laws. The fact that the mean year of discovery of abandoned laws still turned out to be half a century earlier than the mean for the accepted laws is telling.

many of the currentlly accepted theories are likely to share the fate of the previously accepted theories.

This is why we should look at what happens to theories during their ”life time” and trace their individual histories. When we do that a clearer picture emerges. For instance, take the items in Mizrahi's "Abandoned Laws" column: Newton's law of gravitation and Newton's laws of motion. If we had a chance to conduct an opinion poll on the 18th and early 19th century scientists and create a sample based on the results of that poll, we would almost certainly list these two items in the "Accepted Laws" column. Similar cases can be made for some of the other laws and theories that are listed in Mizrahi's "Abandoned Laws" and "Debated Laws" columns. The moral of the story is straightforward: Even if his samples are free from systematic anti-revolutionary bias, which is debatable, the columns in Mizrahi's tables represent merely the current and malleable opinions of the current scientific community.

When we supplement Mizrahi's samples with the histories of discovery, acceptance and abandonment of each theory, a distinctly pessimistic pattern emerges: The theories and laws that were once accepted by the scientific communities of the past tend to be abandoned later on by more recent communities.

Project that pattern into the future, and we have an inductive argument that predicts the eventual abandonment of today's accepted theories, just like Newton's laws were eventually abandoned though they were accepted once upon a time.

This is the case however, only if the process that produces and justifies newer theories is the same as the older theories. If, per premise (iii) of the epistemic formulation of the Pessimistic

Induction, the epistemic status of the current best theories is not different from—and therefore is not superior to—the epistemic status of the best theories of the past which were abandoned, the statistical objection fails even if the overwhelming majority of all theories is still accepted. In other words, the epistemic formulation of the argument is unscathed even if the sample grounding it is biased as Mizrahi and others claim it is.

The situation is analogous to a production line with a faulty quality control process: Suppose a factory produces automobile brakes. Initially the factory has a low output, say 100 units per month. Then they increase production exponentially and they start producing new and supposedly better designed brakes. However, suppose for the sake of example, they retain the quality control process as it is. After a year or so, the first units they produced start failing, leading to terrible accidents. But suppose, by the time of the first failures the newer units are still working just fine. Should we accuse someone of committing the non-random sampling fallacy when he predicts the eventual failure of the newer units, or should we expect a failure all across the board in all models due to the fact that they all went through the same quality control and consider a mass factory recall? The latter is clearly more reasonable.

Similarly, if the processes that produced and justified the older—and eventually abandoned—theories also produced and justified the newer—and currently accepted—theories, it doesn't matter if the abandoned theories are in the minority. The picture we should get is precisely the picture in Table 3.1 where abandoned and debated theories are older than the accepted theories. Just like the brake factory should consider a mass recall including even the newest models, we too must expect the abandonment of our current best theories, even the newest ones. This is warranted pessimism.

The reason why the statistical objection fails to defeat the Pessimistic Induction can be found in Mizrahi's misidentification of the Pessimistic Induction as a generalization from a random sample. The Pessimistic Induction is not a generalization from a random sample. Nor is it a seen-one-seen-all generalization. The Pessimistic Induction belongs to a third kind of inductive inference which goes completely undetected by Mizrahi—and Godfrey-Smith whose classification he is following—: It is *an inference predicting a change in a growing population.*

In order to predict whether the majority of the current members of a growing population will undergo a change in the future, one must not look at random, unbiased samples where *every member of the population you are drawing conclusions about has the same chance of making its way into the sample.* Since change takes time and the population keeps growing, a biased sample which consists exclusively of older members of the population will be more representative of the future state of the current members than a random sample which also includes younger members. Just as in the brake factory example, the samples one must focus on are random, unbiased samples, but non-random samples assembled with a severe bias towards older members of the population.

The population of scientific theories, just like the the population of brakes produced by the factory in the example keeps growing, exponentially.[15] As Mizrahi's own unbiased samples

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[15]We will review ample evidence for the exponential growth in science in Chapter 4. However, even the years of discovery of the laws in Mizrahi's sample support the claim that there has been an exponential

illustrate, abandoned theories are older than accepted theories. Therefore, if one were to predict the future of currently accepted theories, one must look at samples that consist mostly or even exclusively of the older, abandoned theories, which is precisely what Laudan's list does.

That's why the claim that the Pessimistic Induction is an instance of the non-random sampling fallacy is groundless. The Pessimistic Induction isn't an instance of the fallacy because it isn't an inference that fits Godfrey-Smith's *seen-one-seen-all vs. generalization from a random sample* dichotomy. The Pessimistic Induction is instead *an inference predicting a change in a growing population.* Such inferences don't require random samples to warrant predictions. In fact, as far as such predictive inferences are concerned, insisting on random sampling that draws from both *distant-past* and *recent* theories would be a fallacy itself, because biased samples that over-represent older members of the population and under-represent younger ones are more representative of the future of the population than random, unbiased samples.

In other words, the generalist strategy through the statistical objection attempts to save all of our current best theories yet it fails to save any.

3.2.2 The Epistemic Objection

A second objection that follows the generalist strategy is Peter Lipton's epistemic objection against the Pessimistic Induction. According to Lipton, the Pessimistic Induction (he calls it "the disaster argument") is a bad argument because it violates a fundamental epistemic norm: It fails to satisfy the *tracking requirement.*

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growth in the supposed laws of nature during the last three centuries with a marked boom in the 20th century.

Lipton's manuscript 'Tracking The Track Records' is relatively short but he has many irons in the fire. He first considers the problem of inductive justification of inductive arguments, *i.e.* "Hume's circle":

> The Humean argument concludes that no inductive arguments are legitimate, but an essential step is the claim that the inductive justification of induction would be even worse than the rest, since it, unlike mundane first-order inductions, is viciously and worthlessly circular, not merely unjustifiable. (2000, p. 181)

However, Lipton's diagnosis is not completely convincing. Particularly it is not clear why meta-induction is any worse than ordinary induction. They both rely on the same assumption (*i.e.* nature is uniform) and logically speaking relying on an assumption twice is no worse than relying on it once. If the assumption is wrong, both inductions are bad. If the assumption is true, both are good. There isn't a third alternative.

Still, Lipton takes the problem seriously and suggests that we can circumvent Hume's circle by applying a test that all good inferences must pass; they must all satisfy an epistemic norm called the "tracking requirement": A good inference is counter-factually reliable; had the conclusion been false, your evidence would have been different. So, you would not have made the inference in the first place (Lipton, 2000, p. 185). According to Lipton all good inferences (including ordinary and meta-inductions) satisfy the tracking requirement and there are also some meta-inductions that satisfy the tracking requirement.

Lipton first uses the tracking requirement to attack the No-miracles argument. He claims that the argument is "clearly inductive, since the success of a scientific theory does not entail its truth." (Lipton, 2000, p. 193) This shows that what Lipton means by "inductive" is *ampliative* *in the broad sense* which includes all non-deductive inference forms including abduction and induction proper. Particularly, Lipton seems to think that Laudan's (S3)[16] needs inductive justification. However, van Fraassen's alternative explanation from selective pressure on theories demonstrate that (S3) has no basis. Even if our theories were false, they would still have been successful (Lipton, 2000, p. 196). So, even if realism were false, the evidence for the No-miracles—*i.e.* the predictive success of current best theories—would have remained the same. Therefore, Lipton concludes, the No-miracles argument fails the tracking requirement.

Lipton then proceeds to attack the Pessimistic Induction using the same objection. On his view, the Pessimistic Induction is a bad inductive argument because when construed as an enumerative induction[17] the Pessimistic Induction fails the tracking requirement. He argues, "If present theories were true, the past theories would still have been false." (Lipton, 2000, p. 198) As a result, he is likewise disappointed by how overly concessive the partialist objections to the Pessimistic Induction are, which I shall look at later in this chapter:

> Most of the standard responses to the [Pessimistic Induction] are strikingly concessive. A common realist reaction is to retreat from truth to verisimilitude by claiming that our best theories are approximately true [...] Although realism may require a notion of verisimilitude or approximate truth [...] this response seems to me at once both to underestimate how badly wrong, by present lights, many past theories have been, and to overestimate the force of [the Pessimistic Induction.] Another common response to [the Pessimistic Induction] is to retreat to a form of semi-realism,

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[16] *i.e.* "If a theory is successful, we can reasonably infer that its central terms genuinely refer."

[17] Lipton predicts an abductive version of the Pessimistic Induction and offers objections. However, this portion of the paper is a bit difficult to penetrate. Due to Lipton's tragic and untimely death, it is now impossible to obtain a clarification.

> according to which we should commit ourselves to the truth of those aspects of theories that have shown marked stability over the history of science. This response is also problematic, partly because it is quite unclear what these stable elements are. Entities and abstract structures are the most common candidates, but neither seems in general to have been suitably stable. More importantly, [...] this response again concedes too much to the [the Pessimistic Induction]. (Lipton, 2000, p. 199)

I think the spirit of the objection is right. Particularly, Lipton is correct that the partialist strategy is unnecessarily concessive. However, his generalist alternative *via* the epistemic objection that the Pessimistic Induction violates a fundamental epistemic norm is unmoving. There are three reasons for this failure.

First, it is not clear why we should take the tracking requirement as an epistemic norm. It seems that there are excellent inductive arguments that clearly fail to satisfy the tracking requirement. For example, suppose S meets with his advisor every week and in ten consecutive weeks S promised his advisor that he will have a draft of his dissertation by the next meeting. But, when the next meeting comes, S does not have the draft in question, and renews his promise. Suppose you are S's advisor. It has been ten weeks and in the previous nine meetings S gave you promises that he failed to keep. Now, would you infer—inductively—that in the eleventh week S won't have a draft either? Yes, you would, and as far as I can see no one in their sane moments would deny that such an inference is as good as inductive inferences get.

However, the inference fails the tracking requirement. Suppose you make the inference and you are right. S does not have a draft in the eleventh meeting. Now, let's evaluate the counter-factual statement: Had S had a draft in the eleventh meeting, my evidence would have been different (he would have had a draft in one of the previous meetings). I won't pretend to have a fully worked-out epistemology of counterfactuals, but this counterfactual seems to me indisputably false. I take this to be a *reductio* of the claim that the tracking requirement is an epistemic norm all inductive inferences—let alone, all inferences—are supposed to satisfy.

Second,—and this objection comes from John Worrall—the tracking requirement is unusable for judging ampliative arguments. "Had the conclusion been false, our evidence would have been different" is almost always false in genuinely ampliative inferences, because what is inferred in an ampliative inference never logically entails the evidence. The only cases where the tracking requirement is satisfied are implicitly deductive inferences, disguised as ampliative.

An example of such an implicitly deductive inference would be a doctor diagnosing a condition from a symptom. The doctor infers—ampliatively, it seems—that the patient has the condition because if he hadn't, he would not have had the symptom. But, this presupposes the conditional "one has the condition if he has the symptom." So, the inference satisfies the tracking requirement because it is actually a deductive inference. (Worrall, 2000, p. 213)

I have a final complaint about Lipton's treatment of the Pessimistic Induction which applies to some others (*e.g.* Leplin, Psillos and Worrall) in varying degrees. The supposed meta nature of the Pessimistic Induction is not really relevant for evaluating its success. There is no reason to think that inductions about inductions are any better or worse than ordinary inductions. Moreover, barring the outdated inductivist account of scientific reasoning, there is no reason to construe the Pessimistic Induction as a meta induction. As I have argued in Chapter 1, actually, there are good reasons to focus exclusively on the non-meta formulations instead.

For these reasons, Lipton's epistemic objection too fails to satisfy the stated goal of the generalist strategy. It attempts to save all current best theories from pessimism and fails to save any.

This concludes the discussion of two most prominent generalist objections against the Pessimistic Induction. They both attempt to demonstrate that the Pessimistic Induction fails to warrant pessimism about any of our current best theories. Had they been successful, these generalist objections would have been sufficient to completely thwart the pessimistic threat posed by the metaphysical formulation. However, as they stand, they both fall short of this ambitious goal.

3.3 Partialist Objections to the Pessimistic Induction

The second strategy that is commonly employed against the Pessimistic Induction is partialism, which is substantially less ambitious than the generalist strategy. The objections that follow the partialist strategy do not attempt to thwart pessimism concerning the entire truth content of all current best theories. Instead, they defend only certain parts of the current best theories.

Partialist objections can all be traced back to Leplin's following observation:

> If [...] science achieves truth, there must be some historical counterargument [to the Pessimistic Induction] suggesting that science does or will produce theories whose rejection ought not to be anticipated. We will need some account of *continuity in science at the level of theory.* (1984, p. 199)

I think the first part of Leplin's advice is sound: If scientific realism in particular, or optimism in general are warranted, there has to be some historical counterargument to the

Pessimistic Induction. However, as I will argue in Chapter 4, such a historical argument does not require a commitment to the "continuity in science at the level of theory." A current theory could present a radical theoretical break from its predecessors, yet a historical counterargument to save it from pessimism could still be found.

The partialists however, follow Leplin's advice and concentrate their efforts on demonstrating a continuity in science at the level of theory. As a result, the partialist objections are all historical arguments for the thesis that the history of science is not discontinuous.

We can classify the objections following the partialist strategy under the defenses of three theses:

Shallow Revolutions Thesis: The revolutions that constitute the purported inductive basis of the Pessimistic Induction are too shallow to warrant pessimism about all truth content of all current best theories.

Verisimilitude Thesis: The best theories of the past might have been abandoned or substantially revised because they were largely false, but they were similar to the present best theories, which allows for the conclusion that the best theories of the past were approximately true and the history of science is a gradual progress towards the truth.

Retention Thesis: The best theories of the past might have been abandoned or substantially revised because they were largely false, but some of their parts (*e.g.* entities or structures they posit) have been consistently retained in newer best theories. We may not have justification to be optimistic about the entirety of the current best theories, but we have justification to be optimistic about their parts which stood the test of time.[18]

3.3.1 The Shallow Revolutions Thesis

The proponents of the shallow revolutions thesis argue that the revolutions that constitute the purported inductive basis of the Pessimistic Induction are too shallow to support unqualified pessimism. The history of science might have been a history of revolutions, but revolutions have very rarely been deep enough to warrant pessimism regarding all truth content of all current best theories.

On this view, the second premise of the metaphysical formulation of the Pessimistic Induction

(b) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past on the grounds that they were false all along even if they were predictively successful and were accepted as true by scientists until the revolution.)

is an exaggeration. Revolutions rarely if ever involve abandonment or substantial revision of scientific theories. Certain scientific claims and beliefs are indeed abandoned or revised during revolutions, however such claims and beliefs are typically not important from a theoretical point of view. Therefore, there is a significant degree of *continuity in science at the level of theory.* That's why optimism is warranted about the theoretically important bits and pieces of today's best theories, whatever those pieces may be.

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[18]The positions defended *via* the retention thesis are sometimes called "semirealism," a purported middle-ground between realism and antirealism.

Two prominent defenders of the shallow revolutions thesis are Michael Devitt and Ernan McMullin. Devitt argues that it is questionable whether all terms of a successful scientific theory are *seriously* believed to refer by the proponents of that theory. For instance, "phlogiston" may not have been *seriously* believed to exist by the proponents of the theory of combustion that explains combustion as the loss of the phlogiston stored in combustible materials. So, he concludes, we can defend a limited version of realism, which is committed only to the existence of things and truth of claims which scientists *seriously* believe (1984, pp. 162–163).

The limited nature of realism which Devitt defends indicates that he is a partialist; the Pessimistic Induction might succeed at justifying pessimism regarding some theoretical terms and claims, but not all; not the important ones at any rate. In particular, those terms that are *seriously* believed to refer by scientists are safe.

Devitt's defense of partialism faces difficulties. First, it is hard to assess the seriousness of the beliefs of scientists. Suppose a scientist utters the following sentence: "I believe that the particle μ exists." How *serious* are we supposed to take her professed belief to be? Undeniably, the proponents of the phlogiston, ether and caloric fluid theories would have uttered—and indeed did utter—similar statements in their time.

Even if we were to somehow measure the seriousness of individual scientists' beliefs, we would most likely discover that there are significant disparities concerning the level of commitment of different scientists to a given hypothesis. Devitt's objection doesn't tell us how to evaluate non-unanimous seriousness with varying degrees of commitment. Suppose half of the scientific community *seriously* believes that μ exists. Is that sufficient to make μ safe during the next revolution? What about one-third?

The problem however is deeper than the lack of a clear procedure to decide the sufficient conditions for the kind of seriousness Devitt has in mind. The real difficulty with Devitt's position is that it is psychologistic; there are many psychological and social factors that could influence the strength of a person's convictions, even when those convictions concern their area of expertise. It is not clear why an expert's strength of conviction—or lack thereof—would have anything to do with likely truth of such convictions, unless we assume that scientists are exceptional people who seriously believe things only or mostly when they are true. That assumption however, is in part what is being contested by the Pessimistic Induction.[19]

McMullin also defends the shallow revolutions thesis. According to him, entities like ether and caloric fluid "were often, but not always, interpretive additions, that is, attempts to specify what "underlay" the equations of the scientist in a way which the equations really did not sanction." (1984, p. 17) Revolutions sometimes lead scientists to abandon such interpretive additions, but that's not a reason to be pessimistic about those bits and pieces of successful theories that are *not* interpretive additions.

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[19]It might be possible perhaps to reform Devitt's position and replace the seemingly psychologism with an invitation to conceptualize an ideal scientist who is perfectly rational and impartial and who would believe things seriously only when they are true.

However, it is not obvious why a perfectly rational and impartial scientist would *seriously* believe the truth of any empirical claim. As van Frassen points out, the probability that a successful empirical theory is true is always less than or equal to the probability that it is merely adequate.

McMullin in this regard agrees with Devitt. Both think that the revolutions that constitute the purported inductive basis of the Pessimistic Induction are too shallow to support full-fledged pessimism. Therefore, they both think that a partialist defense is tenable.[20]

There are two problems with McMullin's view. First, his complaint that "ether was an interpretive addition which was not sanctioned by the equations" is unmoving. After all, almost never the equations of a theory will sanction a unique set of unobservable entities. It is a demonstrable truth of model theory that any empirical theory has infinitely many distinct models satisfying it. So, nothing except for some observable entities and their properties will be sanctioned by the equations. If we should refrain from judgment when it comes to believing things that are not sanctioned by the equations, then we should refrain from judgment about the existence of almost all unobservables.

The second problem for McMullin was raised by Worrall in response to a similar argument by Phillip Kitcher (1993), who argued that Fresnel's "luminiferous ether" was an idle, "presuppositional" term, which played no part in the empirical success the theory. In reality,

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[20]McMullin—but perhaps not Devitt—was also a generalist, albeit probably a reluctant one. As I pointed out in footnote 1, McMullin's objections against some particular items in Laudan's list could be read as an attempt weaken the Pessimistic Induction by reducing the size of Laudan's sample. This could help justify the generalist claim that the Pessimistic Induction cannot warrant pessimism to any extent.

In this regard, the generalist aspect of McMullin's position bears similarities to the statistical objection I discussed in Section 3.2.1, as small sample size is certainly among the weaknesses Mizrahi attributes to Laudan's argument. However, though a small sample size would indeed weaken the inductive inferences one makes based on that sample, smallness of sample by itself doesn't entail sampling bias, which is the core charge of the statistical objection.

Fresnel's—successful—effort towards generalizing his light mechanics to refraction in crystals were:

> in turn undoubtedly guided by Fresnel's "realist" belief that there could only be one light-carrying medium and the "natural" assumption that, in the general case, the coefficients of elasticity of that medium in the three orthogonal directions in space will be different. Fresnel did get some important heuristic mileage out of certain general mechanical-dynamical ideas concerning some sort of mechanical medium with some sort of vibrating parts. (Worrall, 1994, p. 337)

So, there is no denying that the term "luminiferous ether" played a significant role in making the theory successful and fruitful. Dismissing it as an idle, theoretically unimportant presupposition would be a mistake.[21]

These considerations lead us to the conclusion that the shallow revolutions thesis is not an adequate response to the Pessimistic Induction. It either collapses into a version of antirealism, or goes against historical facts. Probably due to these reasons the shallow revolutions thesis seems to have fallen out of favor among realists these days.

But even if we managed to succeed in crossing out some items from the inductive basis of the Pessimistic Induction, because of the partialist nature of the objection at hand the achievement is necessarily limited. After all, the best that can be achieved by the shallow revolutions thesis is to establish some degree of continuity in the history of science. What one is warranted to be realists or optimists is carved out by the limits of that continuity. That means giving up a

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[21] Worrall too argues that Laudan's list of successful non-referring theories is not as large as Laudan implies. However, Worrall concedes that we have to admit that it also contains some successful non-referring theories such as Fresnel's. (1994, p. 335)

substantial chunk of all current best theories, since they are all to some extent discontinuous from the history of science.

3.3.2 The Verisimilitude Thesis

Some such as Karl Popper try to defend the verisimilitude thesis: Although best theories of the past might have been strictly speaking false, they were similar to the present theories with respect to their truth content, which allows for the conclusion that the best theories of the past were approximately true and the history of science could be seen as a gradual progress towards the truth. Popper believed that we can measure how similar the truth content of two theories are by a scale he called "verisimilitude" (or "truth-likeness"). He attempted to formulate the conditions of verisimilitude set-theoretically and probabilistically. Both attempts were proven to be futile by David Miller (1974) and Pavel Tichý (1974). The demonstration is rather straightforward in the set-theoretic case.

According to the set-theoretic formulation, a theory A is more truth-like than a previous theory B if and only if at least one of the two conditions is satisfied:

> Condition 1: True consequences of theory B is a subset of the true consequences of A, and the false consequences of A is a proper subset of the false consequences of B.
>
> Condition 2: True consequences of theory B is a proper subset of the true consequences of A, and the false consequences of A is a subset of the false consequences of B.

The following is a proof of the claim that neither Condition 1 nor Condition 2 are satisfiable by any actual scientific theory.

Let A and B be two theories. Among the consequences of both theories there is at least one false proposition and not all true propositions are consequences of either theory. Suppose for *reductio*, A is more truth-like than B. This can happen only if Condition 1 obtains or Condition 2 obtains.

Suppose Condition 1 obtains, which entails the existence of at least one false proposition p such that p is a consequence of B and not a consequence of A. By logical closure, every proposition that is entailed by the consequences of B are also consequences of B. So, for a true proposition q that is among the consequences of neither A nor B, $p \vee q$ is a true consequence of B but not a consequence of A. That contradicts the assumption that the true consequences of B is a subset of the true consequences of A. Hence, Condition 1 can't obtain.

Now suppose Condition 2 obtains, which entails the existence of at least one true proposition p such that p is a consequence of A and not a consequence of B. By logical closure, every proposition that is entailed by the consequences of A are also consequences of A. So, for a false proposition q that is among the consequences of A and B, the false proposition $p\&q$ is a consequence of A but not a consequence of B. This contradicts the assumption that the false consequences of A is a subset of the false consequences of B. Hence, Condition 2 can't obtain.

So, Popper's set-theoretic definition of verisimilitude is satisfied by only those theories whose consequences do not contain any falsehoods or do contain all truths. The probabilistic formulation is likewise unworkable. (Miller, 1974)

During the past four decades, there have been further attempts to reform the idea of verisimilitude, some by Popper himself and some by his followers, which employ possible world semantics. As of 2014, they too face serious difficulties. (Chakravartty, 2007, p. 217–218)

In this regard, the partialist approach of the verisimilitude thesis doesn't succeed even as far as the shallow revolutions thesis did. It aims to save bits and pieces (*i.e.* the parts that are responsible for the truth-likeness) of the current best theories from pessimism, but fails to save any because it fails to give a clear and workable account of what truth-likeness is.

3.3.3 The Retention Thesis

The difficulties with the shallow revolutions thesis and the verisimilitude thesis explain why the retention thesis is so popular among scientific realists. According to the retention thesis, the best theories of the past might have been largely false but some aspects of those theories have been consistently retained in the newer theories that replaced them. We may not have grounds for an unqualified realism about the current best theories, but we have grounds for realism concerning the retained parts (theoretical terms, abstract structures, conceptual frameworks) that stood the test of time.

The verisimilitude thesis and retention thesis are closely related. Popper and other proponents of the verisimilitude thesis tried to cash out the similarity of theories in terms of a comparison of their true and false consequences. The proponents of the retention thesis—perhaps wisely—try to locate the similarity in an ontological/referential overlap. Moreover, if one can defend the retention thesis then one can get to the same realist conclusion about past and present theories just as easily as one could defending the verisimilitude thesis. After all, both objections are distinctly partialist in their goals: They aim to warrant realism mostly about the parts of current best theories that are historically continuous with the theories of the past. In other words, the two objections are not completely orthogonal. The difference is merely the fact that the retention thesis focuses on ontology rather than the truth content, and manages to bypass the problems various formulations of verisimilitude suffer from.

Among the proponents of the retentionist thesis, there is disagreement about what is being retained. Here I am going to look at four leading theories: Hacking's experimental realism where experimentally manipulated entities are retained, Worrall's structural realism where the structure of theories is retained, Psillos' *divide et impera* and Hesse's principle of growth.

3.3.4 Hacking's Experimental Realism

One prominent case for the retention thesis is Ian Hacking's experimental realism which he describes as the view that "at least some of the entities [*i.e.* processes, states, waves, currents, interactions, fields] that are the stock in trade of physics" exist (1984, p. 155). According to Hacking, the entities that exist are those which experimental scientists manage to manipulate and use to intervene into phenomena.

Hacking contrasts experimental realism with a supposedly stronger version of realism which says that our best theories are—at least approximately—true. Hacking calls this stronger version "realism about theories."[22]

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[22]This is a misnomer. If the entities we experiment with exist, at least some of our theories should be approximately true. So, two views are not mutually exclusive.

According to Hacking, the attempts to justify realism about theories mostly focus on the predictive and explanatory success of our theories. However, the justification for experimental realism comes from the fact that some unobservable entities, *i.e.* the ones we can use to create and manipulate other phenomena, become more than theoretical stipulations. These entities, Hacking argues, become experimental tools and are retained even when our theories face substantial revision, whereas "long-lived theoretical entities which don't end up being manipulated commonly turn out to be wonderful mistakes." (Hacking, 1983, p. 275) So, even if we accept the conclusion of the Pessimistic Induction about the general truth content of the current best theories, we need not give up our belief in electrons, magnetic fields and such.

The details of Hacking's position are interesting. Particularly, the way experiment is supposed to justify the belief in unobservable entities is worthy of attention. Hacking argues,

> Experimenting on an entity does not commit you to believing that it exists. Only manipulating an entity, in order to experiment on something else, need do that. [However] it is not even that you use electrons to experiment on something else that makes it impossible to doubt electrons. Understanding some causal properties of electrons, you guess how to build a very ingenious, complex device that enables you to line up the electrons the way you want, in order to see what will happen to something else. Once you have the right experimental idea, you know in advance roughly how to try to build the device, because you know that this is the way to get the electrons to behave in such and such way. Electrons are no longer ways of organizing our thoughts or saving the phenomena that have been observed. They are now ways of creating phenomena in some other domain of nature. Electrons are tools. (1984, p. 156)

One might wrongly believe that Hacking's view is methodologically no different from the stronger version of realism that he wants to disassociate his view from. One might think that—I believe Hacking would deny this—both versions of realism require an inference to the best explanation. Take a paradigmatic electron experiment. In this experiment we ‘spray’ electrons onto a surface to create a reverse photo-electric effect. Hacking would tell you that the experiment provides grounds for the belief that electrons exist because we used some causal powers we associate with them to create phenomena in some other domain of nature. However, aren’t we inferring what actually happened in the experiment from what we see, which was merely a piece of metal emitting light? Now, of course the best explanation for the success of the experiment is that whatever we sprayed on the metal were electrons. But, without the inference to the best explanation you can’t move from success to the existence of electrons.

This accusation fails to appreciate the role of the causal theory of reference that Hacking has in the background. He doesn’t need an inference to the best explanation to infer that electrons exist because the repeated success of the experiment shows that whatever it is that you spray, it has such and such causal powers. Since the manifestation of those causal powers fixes the reference of “electron,” you get the existence of electrons without inference to the best explanation.

Of course, one might explain away the apparent intervention that the metal spontaneously emitted light, or perhaps an evil demon made it emit light. This radical skeptical challenge misses the point. Hacking does not claim that the experiment *entails* that electrons exist. It is of course possible that electrons might not exist despite what we see in the experiment, but the experiment gives us sufficiently strong grounds to warrant belief.[23]

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[23]The literature is relatively silent on how exactly Hacking’s argument is supposed to be formulated, which is not helped by the fact that Hacking himself never offers a very clear statement. His few critics

In other words, Hacking's view has an assumption that is hardly self-evident: The reference of "electron" is fixed by an experimental manifestation and manipulation of the causal powers attributed to electrons by the theory.[24]

I do not want to give the impression that I am pooh-poohing Hacking. I think his position is the most compelling among those who employ the partialist strategy. However, if Hacking's only reason for adopting experimental realism is the Pessimistic Induction—which, I think is not the case—then he is being overly concessive. We do not need to weaken optimism by defending a version of the retention thesis, which can save only bits and pieces of current best theories. Instead, we can defend some of the current best theories in their entirety by using the exemptionist strategy I promise to deliver in Chapter 4.

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such as Reiner & Pearson (1994) appear to offer very uncharitable readings which overlook the sections of the text where Hacking (1984) talks at length about the Kripke/Putnam causal theory of reference and our "debt to Putnam." Hacking praises Putnam for realizing that the reference for a term like "electron" is not fixed by the law-like statements of within a theory where "electron" occurs. It is instead fixed by a stereotype which is—as far as natural kind terms are concerned—a bundle of causal powers associated with an entity "electron" is intended to refer to. (Hacking, 1984, p. 157-9; Putnam, 1975) If we rule out radical skepticism, when we witness those causal powers, we may conclude that the term refers.

In this regard, I think what I have offered is a compelling and charitable reconstruction of Hacking's argument. Nevertheless, it's hard to be sure that my reconstruction is the correct philosophical exegesis of Hacking's actual view (personal communication with Nancy Cartwright).

[24]The referential success of "electron" doesn't imply the truth of all or even most scientific beliefs about electrons. Just like I might successfully refer to the Holocaust (as in the utterance "Millions in Europe were killed during the Holocaust.") without having many true beliefs—and though I may have many false beliefs—about the Holocaust. Scientists likewise can successfully refer to electrons without many of their beliefs about electrons being true. What they need to have is a theory that attributes certain causal powers to all and only electrons, and that they wield and witness those powers in experimental manipulation of phenomena. Indeed, if it were not for the overwhelming testimonial evidence, it would have been more difficult to ensure that my use of "the Holocaust" is referentially successful than ensuring the success of experimenter's use of "electron." Although I am not in a position to have witnessed the Holocaust, the experimentalist routinely witnesses and weilds the causal powers of electrons.

3.3.5 Worrall's Structural Realism

Another prominent realist who defends a version of the retention thesis is John Worrall. He argues that the structure of successful theories are mostly retained through theory change while the postulated entities such as ether, caloric fluid, molecules and electrons come and go. So, we should be structural realists, who believe that scientific theories get at least the structure of the world right.

Worrall uses the transition from Fresnel's mechanical wave theory to Maxwell's electromagnetic wave theory as an illustration of his position. He argues,

> [A]lthough from the point of view of Maxwell's theory, Fresnel entirely misidentified the *nature* of light, [Fresnel's] theory accurately described not just light's observable effects but its *structure*. There is no elastic solid ether. There is, however, from the later point of view, a (disembodied) electromagnetic field. The field in no clear sense approximates the ether, but disturbances in it do obey *formally* similar laws to those obeyed by elastic disturbances in a mechanical medium. (Worrall, 1989a, p. 158)

According to structural realism, the claims Fresnel's theory made about the *nature* of light were abandoned but those concerning the *structure* of light were retained in Maxwell's theory. In this regard, it is clear that Worrall's structural realism is a version of the retention thesis.

According to Worrall, the way we tell that the theories are structurally continuous is by observing that their mathematical formulations are similar. He argues that such a similarity could manifest itself in two ways. First, as in the transition from Fresnel to Maxwell, the equations could be incorporated into the new theory almost without modification. A "more common pattern however," Worrall informs us "is that the old equations reappear as limiting cases of the new." (1989a, p. 160)

As Ladyman argues however, Worrall's position is ambiguous. There are at least two possible versions of structural realism: one epistemic and one metaphysical.

On the epistemic version all we can know about the world through our scientific theories is its structure expressed formally by the equations of our theories. This is so presumably because the knowable content of a scientific theory is primarily a structure, *i.e.* a set of relations that obtain between the individual objects in the world. This is basically a Ramsey-sentence approach, where the theoretical terms in any scientific theory could be eliminated by replacing the theory with its Ramsey-sentence. Ladyman reminds us of the drawbacks of ramsification: A structure described by a Ramsey-sentence is trivially true of any set of objects so long as the set has the right cardinality. (Ladyman, 1998, p. 412) Therefore, the epistemic version is untenable. (1998, p. 413)

On the metaphysical version, all there is in the world is structure. Scientific theories may seem to talk about objects as well as the relations that obtain between those objects. However, the talk of objects is a heuristic device for structures; an object is nothing more than a set of relational properties. Ladyman argues that this version is more promising, however it faces significant challenges as well. One such challenge is that it requires that we replace our object-based ontology with a structure-based ontology, which may not be possible. (1998, p. 422)

Psillos raises similar concerns about structural realism. He argues that there are two epistemic paths one can take to come up with structural realism and both paths are problematic.

The first one is what Psillos calls the "upward path," where you start "from empiricist premises and reach a sustainable realist position." (Psillos, 2001, S13) According to Psillos, this path was first taken by Bertrand Russell, who defended the view that

> only the structure—*i.e.* the totality of formal, logico-mathematical properties—of the external world can be known, while all of its first-order properties are inherently unknown. This logico-mathematical structure, [Russell] argued, can be legitimately *inferred* from the structure of the observed phenomena. (Psillos, 2001, S14, emphasis original)

There are two possible readings of the upward path. On the strong reading, *all* of the logico-mathematical properties of the external world can be inferred—therefore can be known—from the structure of the observed phenomena. On the weak reading, only *some* of the logico-mathematical properties can be inferred from the structure of the observed phenomena.

The strong reading is implausible. For the strong reading to be true, the set of inferences that take us from the structure of the observed phenomena to the logico-mathematical properties of the external world must constitute a surjective relation from the former to the latter. Under empiricist epistemology, no amount of observed phenomena can constitute evidence for the existence of such a surjective relation. Therefore, the upward path is closed under the strong reading.

The weak reading is also problematic, because it is trivially true. Even if the logico-mathematical properties of the external world were radically underdetermined by phenomena we encounter, *some* of those logico-mathematical properties can still be inferred from the structure of the observed phenomena, such as the fact that the set of things in the external world is non-empty. Certainly, that's not a strong enough result to justify the name "structural realism."[25]

Since there are problems with both the strong and weak[26] versions of the upward path, the upward path to structural realism doesn't seem viable.

The downward path attempts "to start from realist premises and construct a weaker realist position." (Psillos, 2001, p. S18) There could be two versions of structural realism one may try to reach using the downward path: restrictive structural realism (RSR) and eliminative structural realism (ESR).[27] According to RSR, there is presumably more than mere structure in the world but only the structure can be known. According to ESR, all there is is structure (Psillos, 2001, pp. S18-19).

There are three possible versions of RSR. Each consists of exactly one of the following theses:

(A) We can know everything except the individuals that instantiate a definite structure.

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[25]One might try to buff up the weak reading by arguing that a substantial subset of all logico-mathematical properties can be inferred. However, in order to know whether the inferable subset is substantial or not, one must have a good estimate on the size of the set of all logico-mathematical properties. I can't see how this can be done under empiricist epistemology.

[26]Perhaps there is a pragmatic version of the buffed-up weak formulation, which Psillos and Ladyman might be overlooking. Instead of construing substantialness of a subset relative to the size of the superset, the structural realist could argue that even a small subset of logico-mathematical properties could be substantial if it consists of pragmatically favored logical-mathematical properties members. This is an open possibility, which I will not explore further. However, I wouldn't be surprised if at the end of such a pragmatic investigation the structural realist and the constructive empicist find themselves in perfect agreement. After all, we might very well decide that the pragmatically privileged class of properties are nothing but isomorphisms between the phenomena and the external world.

[27]These two versions correspond to Ladyman's distinction between epistemic and metaphysical versions of structural realism.

(B) We can know everything except the individuals and their first-order properties.

(C) We can know everything except the individuals, their first-order properties and their relations.

If (A) is the right version of RSR, then RSR agrees with ordinary realism on just about everything. We know the nature of all interactions between individuals and all—intrinsic or extrinsic—physical properties of individuals. Then "the only possibly substantive" disagreement between ordinary realists and structural realists is about the names of objects. (Psillos, 2001, p. S20) So, (A) fails to distance itself from the stronger ordinary realism. This is a big problem because if Psillos is right, structural realism (A) will not fare better against the Pessimistic Induction.[28]

If a structural realist endorses (B), then he has to offer us a principled reason why knowability of all relations between individuals is not sufficient for the knowability of their first-order properties as well. In the absence of such a reason, the version (B) collapses into version (A). (Psillos, 2001, p. S21) Here the proponent of (B) could try to argue for the possibility of

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[28](A) (and possibly B, depending on what first-order properties individuals have) is compatible with some versions of Kantian Humility, especially Rea Langton's (1998, pp. 12–13) where one can know only the relational properties of things in themselves, but never their intrinsic properties. Kant's own metaphysics was arguably incompatible with (A). After all, under (A) (and (B)) it is possible to know the minimum possible cardinality of the set of individuals.

However, for Kant, the concept of number is necessarily rooted in sensible intuition, and therefore cannot be applied to numena.

> [T]hrough sensibility we do not cognize the constitution of things in themselves merely indistinctly, but rather not at all, and, as soon as we take away our subjective constitution, the represented object with the properties that sensible intuition attributes to it is nowhere to be encountered (Kant, 2007, B62)

In other words, Kant was an idealist and not a realist. (A) on the other hand isn't idealism; it's a limited form of realism.

irreducibly non-relational intrinsic properties, such as 'spatial handedness' or 'temporal directionality.' However, this would be an uphill battle because the only arguments for the thesis that these properties are irreducible to relational properties come from physical sciences. If those properties fall outside the extent of RSR, then no such intrinsic properties can be known. If they cannot be known, then no argument could be made for the claim that they are not reducible to relations.

If the right version of RSR is (C), then nothing but the formal structure of the world can be known. However, (C) faces the same problems that plagued Russell's account. The formal structure of the world, which is devoid of genuine individuals, first-order properties and relations, could be nothing more than the structure of phenomena. (Psillos, 2001, p. S21)

Here is why: Suppose S is a structure that is knowable as in (C). *Ex hypothesi* S specifies no individuals. It specifies no relations between individuals. It specifies no first-order properties of individuals. What S can specify is higher-order properties of individuals and relations that hold between those properties. What does S sound like? S sounds like a sensory medium where things, their relations and first-order properties are known only from how they relate to each other. In this regard, (C) is not realism. It's a form of antirealism.

Therefore, Psillos concludes, "the 'downward path' to RSR either fails to create a sustainable restriction on [ordinary realism] or, insofar as it focuses on the knowability of purely formal structure it fails to be realist enough." (2001, p. S22)

Finally, regarding ESR, Psillos repeats Ladyman's worries about metaphysical realism in a stronger tone. Psillos argues,

> [T]o hypothesize structure is one thing (and, in certain instances, it may be legitimate). But to say that they don't supervene on their elements is quite another. It implies the wrong ontological thesis that they require no individuals in order to exist and the wrong epistemic thesis that they can be known independently of (some, but not any in particular, set of) individuals which instantiate them. (2001, p. S23)

In this regard, structural realism faces serious difficulties. However, even if these difficulties were overcome in the future, still it has the same disadvantage as experimental realism: It is unnecessarily concessive. We need not weaken optimism by defending a version of the retention thesis. Contrary to Leplin's advice, we do not have to demonstrate a continuity in science at the level of theory in order to defend optimism against the Pessimistic Induction.

3.3.6 Psillos' *Divide et Impera*

Some philosophers choose to defend a hybrid of the verisimilitude thesis and the retention thesis. For instance Statis Psillos argues that theories have 'constituents' and some constituents of a theory may be more truth-like than the others. He argues that the truth-likeness of a specific theoretical constituent of an otherwise false theory can explain why that theory had empirical success and led to novel predictions regarding a specific phenomenon. This is what Psillos calls the "*divide et impera* move" which is a two step defense against the Pessimistic Induction: (I) identify the theoretical constituents of past genuinely successful theories that essentially contributed to their success and (II) show that these constituents are retained in subsequent theories. (Psillos, 1996b, p. S310) This, Psillos argues, will "reconcile the historical record with the realist claim that successful theories are typically approximately true." (1999, p. 99)

Psillos also offers a particular case where we can apply the *divide et impera* move: the transition from the Caloric Theory to Thermodynamics. The application is supposed to show that the Caloric Theory was approximately true, despite the fact that the central term of the theory, *i.e.* "caloric fluid," did not refer. According to Psillos, approximate truth is possible without referential success of central terms when the truth in question is independent from the reference of the central terms. Specifically, what was true of the Caloric Theory was the hypothesis that "the quantity of free heat remains always the same in simple mixtures of bodies." (1994, p. 167) Historical proponents of the theory such as Lavoisier, Laplace and Carnot firmly believed the truth of this hypothesis, but their commitment to the identification of heat with a material fluid was rather weak. (1994, pp. 167–168)

There seems to be one major problem with Psillos' *divide et impera* move. If the realist has to drop the referential success requirement for the central terms of scientific theories, then the antirealist has already won the game. If that is the case, the Pessimistic Induction would not justify full-fledged skepticism about the entirety of our best theories, but would be good enough to undermine our belief that there are atoms, electrons and black holes, for the scientific terms picking them out probably do not refer, just like "caloric fluid" didn't refer. If this is the extent of realism we will have to live with, we might as well pick up our marbles and go home now.

To be fair, Psillos never claims that approximate truth is possible with *no* referential success. For instance, the Caloric Theory is approximately true because the theory's use of "heat" was referentially successful, presumably. However, Psillos does indeed give up on the referential success of some central terms of theories, such as "caloric fluid."[29]

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[29]Clearly, the truth of "the quantity of free heat remains always the same in simple mixtures of bodies" requires more than the referential success of "heat." For instance, it requires that free heat is a conserved

Again, this is typical partialist concessionism. Psillos's *divide et impera*, just like other forms of retentionism, surrenders too much to pessimism. But unlike Hacking's experimental realism, *divide et partial* is not even trying to save the central terms of the current best theories. In this regard, it is not clear whether what would be left could legitimately be called "realism."

3.3.7 Hesse's Principle of Growth

Hesse in her "Truth and the Growth of Scientific Knowledge" also offers a partialist objection against the Pessimistic Induction that falls somewhere between the retention thesis and the verisimilitude thesis. Her defense is based on what she calls the "principle of growth." According to the principle of growth, which is allegedly a blunt historical fact, science "exhibits cumulative predictive success." (Hesse, 1976, p. 271) Hesse argues that the this blunt fact implies that scientific revolutions don't need to lead to an unequivocal abandonment of old ontologies.

Hesse argues that during a scientific revolution, seemingly abandoned theoretical sentences of an old theory are translated and retained as observational sentences of a new theory. (1976, p. 273) Hesse argues,"some theoretical sentences [...] are carried over fairly directly from a past theoretical framework to our own, that is, which do not depend for their truth on the existence and classification of particular hypothetical entities, but are nearer to pragmatic predictive test." (1976, p. 274)

For example, the term "phlogiston" did not actually refer to anything. However, theoretical statements of the phlogiston theory such as "Phlogiston is inflammable" and "When metal

quantity. However, such requirements don't make Psillos' position more appealing than some versions of structural realism which hinges on the same requirements.

calcinates, phlogiston is given off" were translated to the language of the newer chemical theories and retained as pragmatic principles guiding observation. Particularly, current best theories tell us that during a metal/acid reaction, Hydrogen is given off, which is flammable. (Hesse, 1976, p. 272)

One serious worry we might have about Hesse's view concerns her idea of theory-theory translation. She recognizes that translations between two different theories will be plagued by the Quinean indeterminacy of radical translation (Hesse, 1976, p. 270) and her solution to this problem is that theory-theory translation conforms with the principle of charity (1976, p. 273). However, it is not clear if the principle of charity can do any heavy lifting for realism. After all, the principle of charity applies equally to the languages of all theories, including rather pseudoscientific ones. This raises the question that if we construe the principle of charity strongly enough to save statements like "Phlogiston is inflammable," then what could stop us translating statements like "The evil ghost cast a curse on your soul [and that is why you have an itchy nose]" to the language of modern medicine? "You are allergic to cats" appears to be a perfectly charitable translation.

3.3.8 A Few Concluding Remarks on Partialism

The sheer size of Section 3.3 may give the false impression of genuine diversity and strong disagreement among the follower of the partialist strategy. However, as my analysis shows, what we get from partialists are variations of two approaches. They either defend the shallow revolutions thesis or the retention thesis. However, both paths deny the second premise of the Pessimistic Induction and follow Leplin's advice: They both deny that science at the level of theory is substantially discontinuous. As an outcome, they aim to locate the grounds for realism within the bits and pieces of the current best theories which are continuous with the best theories of the past.

As we get closer to the present day, the diversity further decreases because the problems with the shallow revolutions and verisimilitude theses have become overwhelmingly obvious. The partialists of today therefore mostly agree that the retention thesis is realism's only hope to survive the Pessimistic Induction. Their disagreement is about the question "what is being retained during theory change?" Is it the existential claims concerning entities such as electrons and molecules? Is it the structure which is expressed by the axiomatic or quasi-axiomatic formulations of theory?

The partialist convergence of 'convergent realism' is remarkable in the sense that it encourages careful work on the history of scientific progress. However, it is less than ideal as a critical response to the Pessimistic Induction because it is unnecessarily concessive. The partialist objections at best succeed in thwarting pessimism regarding only the historically retained bits and pieces of current best theories. In Chapter 4, I will propose an alternative strategy, *i.e.* exemptionism, that can warrant optimism regarding the entirety of some current best theories.

3.4 Conclusion

In this chapter I looked at several prominent objections against the metaphysical formulation of the Pessimistic Induction. These objections follow either the generalist or the partialist strategy. The generalist strategy is ambitious; it aims to warrant optimism about the entire truth content of all current best theories. The partialist strategy is less ambitious; it tries to warrant optimism about only certain parts of current best theories.

Under the generalist strategy, I have looked at two objections: one statistical objection and one epistemic objection. The statistical objection contended that the proponents of the Pessimistic Induction commit a statistical fallacy called the "non-random sampling." According to this generalist objection, the history of science is not actually a history of revolutions; the revolutionary picture that we saw is an illusion created by the biased samples pessimists push onto us. If one were to assemble unbiased, genuinely random samples of the past and current best theories, one would realize that the ratio of abandoned theories to accepted theories is rather low.

However, the statistical objection faces serious difficulties. First, the alternative samples that are supposedly unbiased (such as Mizrahi's tables of laws and theories) exhibit a possible bias for currently accepted laws and theories. More importantly, the mean and median age of abandoned laws and theories are significantly greater than the mean and median age of currently accepted laws and theories. Assuming that scientific revolutions don't happen overnight and they take substantial time to unfold, and given the exponential growth of science, the population of scientific theories should contain more accepted theories than abandoned theories at any given time *even if none of those accepted theories will remain accepted in the future.* This is possible because the Pessimistic Induction is not *a generalization from a random sample* as Mizrahi claims it is. Instead, the Pessimistic Induction is *an inference predicting a change in a growing population.* Such inferences don't require random sampling to warrant their conclusions.

In other words, the Pessimistic Induction doesn't need samples that contain more abandoned theories than currently accepted theories to succeed. The claim that the history of science is a history of revolutions is perfectly consistent with genuinely random samples that consist mostly of currently accepted theories.

Lipton's epistemic objection following the generalist strategy doesn't do much better. His quarrel with the Pessimistic Induction is that it fails the tracking requirement; even if our theories were final, the historical record wouldn't have been any different. Therefore, the conclusion of the Pessimistic Induction doesn't track the evidence offered in its support.

However, insisting that the tracking requirement is an epistemic norm that every good inference must satisfy would be demanding too much. There are perfectly good inductive inferences that fail to satisfy the tracking requirement. Therefore, we cannot reject the Pessimistic Induction on the grounds that it fails the tracking requirement.

In other words, both of the generalist objections that I looked at in this chapter fail to achieve the ambitious goal they set for themselves. They attempt to save all current best theories from pessimism, yet it is hard to see how they can save any.

The partialist objections defend one (or combinations) of the following three theses: shallow revolutions, verisimilitude and retention. Among these the shallow revolutions and verisimilitude theses face serious problems. The former either goes against historical evidence or reduces to a form of antirealism, whereas the latter has conceptual problems making it either hopelessly vague or unsatisfiable. The only successful partialist objections are the ones that defend the retention thesis. Hacking's defense of experimental realism in particular could definitely warrant a version of realism about current best theories, where we have grounds to be optimistic about the experimentally manipulated entities. However, experimental realism is not a full-fledged form of optimism. It can save only bits and pieces of current best theories. No current best theory is protected in its entirety by Hacking's experimental realism, and this is simply too concessive given the fact that the exemptionist strategy can do significantly better.

In Chapter 4, we will shift our focus to the epistemic formulation of the Pessimistic Induction and articulate the exemptionist strategy in question. This new strategy can warrant not just semirealism but a more wholesome form of optimism by successfully defending the entirity of some current best theories against the Pessimistic Induction. In this regard, exemptionism strikes a balance between the unrealistic ambition of generalism and unnecessary concessiveness of partialism.

4. A NEW HOPE: EXEMPTIONISM

In Chapter 3, I looked at prominent objections against the metaphysical formulation of the Pessimistic Induction and observed that though some of these objections succeed to some extent, their success has been rather modest. The best they achieve is a form of semirealism concerning only unconnected bits and pieces of current best theories. There has been no compelling defense against the metaphysical formulation of the Pessimistic Induction that manages to save from pessimism any contemporary theory in its entirety.

Part of the reason why the said success has been modest is the overly ambitious nature of some of the objections in question. Some objections against the Pessimistic Induction follow a generalist strategy. They are attempted general defeaters against the inference involved in the Pessimistic Induction. However, compelling general defeaters are difficult to come by, explaining why such objections are unconvincing. The optimists who follow the generalist strategy are like military tacticians whose ambitious effort to defend a large front allows the enemy's forces to infiltrate their lines of defense.

The popular alternative to this common generalist leaning, which I call "partialist objections," are too concessive. In an effort to be not so ambitious, partialists surrender potentially defensible parts of our best theories to pessimism without a fight. Some of these partialist objections manage to thwart pessimism concerning some bits and pieces of our best theories. However, it is also disappointing that these bits and pieces are often highly uncontroversial among the scientific community and have little relevance to policy making.

In this discouraging setting, it would be ideal if we can come up with a strategy that saves at least some of our best theories from the Pessimistic Induction in their entirety, including their controversial components. This would strike a balance between the overly ambitious—and largely unsuccessful—generalist strategy, and the overly concessive—and only moderately successful—partialist strategy.

Moreover, as we have seen in Chapter 1, the metaphysical formulation of the Pessimistic Induction is not the best formulation of the argument. Actually, a charitable critique must engage the epistemic formulation instead. Therefore, the strategy we need should not only strike a balance between generalism and partialism but also be effective against the epistemic formulation of the Pessimistic Induction.

Today this lofty goal is within our reach. If we can demonstrate that the epistemic status of some of our best theories is superior to the epistemic status of the best theories of the past, we can be optimistic about those theories in their entirety. This demonstration is possible, I contend, through what I call the "exemptionist strategy," an alternative to the generalist and partialist strategies.

The objective of this chapter is lay out the outline of the exemptionist strategy by articulating the conditions for the possibility of a successful demonstration of the epistemic superiority in question. In Section 4.1, I will look at the epistemic formulation of the Pessimistic Induction once again to refresh our memory on why it hinges on the current best theories and their predecessors being epistemically on par. There, I will observe that the quality and quantity of evidence in favor of a theory is the relevant factor that plays into comparisons of epistemic status. In Section 4.2, I will turn my attention to a proposed generalist objection by Fahrbach against the epistemic formulation, which draws from some global trends in the demographics and operationalization of science, and assess if the trends indicate a general improvement in the quantity and quality of evidence in favor of current best theories, and warrant a successful objection against the Pessimistic Induction as Ludwig Fahrbach (2011) argues.

A careful analysis of the trends, which I will offer in Section 4.3, reveals that Fahrbach's generalist objection fails. The global trends which he relies on—as well as those he misses out, such as the quantitative contribution of computerization of research—do not add up to a successful objection against the epistemic formulation of the Pessimistic Induction. Therefore, contrary to Fahrbach's opinion, optimism concerning all current best theories is not warranted.

Nevertheless, understanding the trends in the demographics and operationalization of science is still crucial; They hint at four distinct conditions, which could support optimism about individual theories satisfying them. In Section 4.4, I will put these four conditions together and construct the exempting defeater I promised earlier. There, I will also explain in reasonable detail why the four conditions are jointly sufficient to exempt a theory in its entirety from the epistemic formulation of the Pessimistic Induction.

4.1 What is Epistemic Superiority?

The Pessimistic Induction is an inductive argument. Inductive arguments have an inherent weakness: the inferences invoked in such arguments are warranted only when the inductive basis is *sufficiently similar to the inferred cases in relevant respects.*

Recall for instance the brake factory example from the previous chapter: There is a factory which produces automobile brakes. Initially the factory has a low output, say 100 units per month. Then the operators of the factory increase production exponentially and start producing new and supposedly better designed brakes. After a year or so, the first units they produced start failing, leading to terrible accidents. But suppose, by the time of the first failures the newer units are still working just fine.

In this scenario, suppose an engineer who works at the factory notices the failure of the older units and predicts the eventual failure of the newer units. What is needed to warrant such a pessimistic prediction?: the inductive basis (*i.e.* the failed brakes) being *sufficiently similar to the inferred cases in relevant respects.*

In this regard, it is possible to exploit this inherent weakness and argue that the inductive basis of the Pessimistic Induction is not sufficiently similar to all inferred cases in *relevant respects.* If we succeed, we could defeat the Pessimistic Induction without following Leplin's advice and having to demonstrate a *continuity in science at the level of theory.*

In the brake factory scenario, what constituted *sufficient similarity in relevant respects* was the fact that quality control process remained the same throughout the history of the factory. If the quality control didn't catch the faults in the earlier models, it probably didn't catch the faults in the later models.

What counts as similarity in *relevant respects* as far as scientific theories are concerned is made explicit by premise (iii) of the epistemic formulation of the Pessimistic Induction which I offered in Chapter 1:

(i) The history of science is a history of revolutions.
(ii) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past.
(iii) The epistemic status of the current best theories is not different from—and therefore is not superior to—the epistemic status of the best theories of the past that were abandoned or substantially revised.
$\therefore$ The current best theories will be abandoned or substantially revised in the future.

In other words, if the Pessimistic Induction is to succeed, the epistemic status of our best theories has to be comparable to—if not the same as—the epistemic status of the best theories of the past. Therefore, surprisingly perhaps, what we need is not necessarily *continuity in science at the level of theory* as Leplin advised us to demonstrate. If we can find just the right kind of epistemic discontinuities in the history of science, we can save at least some of our theories in their entirety from the threat posed by the Pessimistic Induction.

The similarity of epistemic status is *relevant* to the reliability of the inference invoked in the Pessimistic induction because when an inductive inference is about the right epistemic attitude (*i.e. pessimism or optimism?*) regarding certain theories and is made by epistemic agents who are interested in the question whether they should be optimistic or pessimistic about those theories, it is clear that the evidence they have for those theories is *relevant*.

If this claim sounds implausible, imagine a murder trial where the prosecution makes four arguments. Each argument places the accused at the crime scene at the time of the crime. Suppose, three of these arguments are based on the testimony of a single witness, which the defense attorney manages to discredit through a compelling cross-examination. The fourth argument however, is based on something entirely different. Let's say it is based on the fact that the finger prints of the accused were found on the murder weapon. Would it be sensible to inductively infer that the fourth argument is likewise discredited from the fact that the previous three have been manifestly so? No, that would not be sensible at all because the evidence for the fourth argument is different in kind from—and epistemically superior to—the evidence for the other three.

Analogously, the inference the Pessimistic Induction invites us to make works only if the evidence the present scientific community possesses for some of the current best theories is not different from—and epistemically superior to—the evidence the past scientific communities possessed for their best theories.

In other words, for the inductive inference invoked in the Pessimistic Induction to warrant pessimism regarding all current best theories, premise (iii) has to hold, again for all current best theories. The main contention of this manuscript is that in the age of simulationist science, (iii) holds only for some current theories whereas it doesn't hold for some others. So as to determine the extent and limits of optimism, it is imperative that we identify the current best theories whose epistemic status is superior to the best theories of the past.

As I hinted at earlier, the epistemic status of a theory has two components: a quantitative component consisting of the amount of supporting evidence the theory enjoys, and a qualitative component consisting of the reliability and diversity of the sources of supporting evidence. As a rule of thumb, having quantitatively more evidence and qualitatively diverse—and ideally more reliable—sources of evidence in favor of a theory improves the epistemic status of that theory.

So, if we compare the quantity and quality of evidence today's scientific community has for a theory to the quantity and quality of evidence the past scientific communities had for their theories, and observe a vast increase in the amount and quality as well as an unprecedented diversification of the sources of evidence, we could be optimistic about that contemporary theory.

The idea I have just described is the gist of the exemptionist strategy that I will articulate in detail in Section 4.4. When and if this strategy succeeds, it can warrant optimism not just about bits and pieces of contemporary theories as in the case of partialist objections, but also about some individual theories in their entirety. Unlike generalism, exemptionism is not overly ambitious; it fights only winnable battles. Unlike partialism, exemptionism is not overly concessive; it doesn't surrender defensible territory.

In the next section we shall look at some global trends that will help us identify the conditions under which a theory can be exempted from the Pessimistic Induction *via* exemptionism.

4.2 Fahrbach's Demographic Objection

It is a historical fact that during the last three centuries there has been an exponential growth in the demographics of virtually every field of science. If more scientists mean more evidence, one might take this to be evidence that virtually all mature fields of science today have theories that enjoy more evidence than the theories of the past.

This conspicuously generalist line is not without precedent. As I anticipated in Chapter 1, Fahrbach (2011) defends such a view, which I will call the "demographic objection." As I will argue in Section 4.3, the demographic objection fails. But before explaining why it fails, let's try to understand what motivates the demographic objection.

The demographic objection appeals to the exponential growth in the demographics of scientific communities to warrant optimism regarding all or almost all current best theories. I had developed—and had since abandoned—a version of this objection in a draft in 2008.[30] Since then it has been independently discovered and defended by Ludwig Fahrbach (2011).

Fahrbach's demographic objection is based on the claim there is more evidence in favor of the current best theories than there ever was in favor of the best theories of the past that have been abandoned. In this regard, Fahrbach's approach tries to appeal to a difference between the epistemic statuses of the past and current best theories. Therefore, unlike the majority of the participants of the debate, the way Fahrbach engages the Pessimistic Induction suggests that what he has in mind is much closer to the epistemic formulation than the metaphysical formulation.

The way Fahrbach attempts to justify his claim about the epistemic difference is to examine two demographic variables that jointly constitute a measure of "scientific work" done:

> "[S]cientific work" means such things as making observations, performing experiments, constructing and testing theories, etc. Let us examine two ways of how the amount of scientific work done by scientists in some period of time can be measured: the number of journal articles published in that period and the number of scientists working in that period. (2011, p. 8)

Fahrbach proceeds to show that both variables (*i.e.* the number of articles and active scientists) have been increasing exponentially during the last 300 years.

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[30]Note to Committee: The paper was written for Nick's Phil of Science Seminar in Fall 2008.

Fahrbach's observations are correct. We can read the increase off the demographics of scientific communities around the world. A local example that reflects the magnitude of growth in the number of active scientists over the last century is the membership figures of the American Physical Society. The number of members APS had in 1899 was 36, whereas the number is over 50,000 today. If we presuppose a steady growth ratio, this would mean that the society expanded its membership body roughly by an average growth of 6% each year since 1899. However, when scientific societies are first founded they always have a handful of members, and they sometimes grow quickly, which does not necessarily correspond to an actual growth in the size of the scientific community in the country. APS membership figures in recent years (2008-2012) give us a more realistic sense of the actual growth, which is around 3% per year.[31]

Another source that could help us estimate the actual increase in the number of active scientists is the number of PhDs conferred in physics in the United States over the last century. As Figure 1 indicates, until the mid-1970s there has been a significant increase in the number of science and engineering PhDs conferred in the US[32] which supports the claim that there is a direct correlation between the membership figures of APS and the number of active scientists in the US.

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[31]These figures are taken from the website of the American Physical Society, http://www.aps.org/membership/units/statistics.cfm

[32]Moreover, as Figure 1 illustrates, the number of doctorate granting institutions in the US quadrupled over the last 80 years.

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Figure 1. Exponential growth in the demographics of science. (Thurgood *et al*, 2006, original figure numbers are 2-4, 3-1a, 3-1b and 2-1)

The stagnation between the mid 70s and 1990 might seem worrying, but it is really not a serious challenge against the point I am making here because it does not indicate an actual plateau in the world-wide active researcher population. The global distribution of research effort has been clearly shifting as Asian countries—spearheaded by PRC—started to catch up with Western science in the 1990s. (Rousseau & Jin, 2005, p. 3. Also see Figure 4, which indicates exponential growth in research expenditures in PRC during the last decade.) Therefore, it is not an outrageous speculation to suggest that the stagnation in the US must have been compensated by the growth of the scientific communities in South and Southeast Asia, where more than one-third of the global population resides. Moreover, even if there were a worldwide plateau, the exponential growth upto that point would still mean that the global scientific community has grown immensely in comparison to what it was mere decades ago.

Still, let's be cautious and settle for a conservative estimate: The total number of active scientists in the world has grown by 3% per year since 1899. This entails that the number of active scientists today is roughly 20 times what it was roughly a century ago. If we assume that this conservative estimate was true also of the 18th and 19th centuries, and the number of active scientists at a given moment is directly proportional to the amount of scientific research output, we can infer that the total research output throughout the 19th century was less than 1/20th of the 20th century research output and this ratio drops below 1/400th when we compare the 18th and 20th centuries.

Another reason to believe there was a vast increase in research output is relatively more recent. Something unique and remarkable happened in the second half of the 20th century: Since the 1950s, computers have gradually become an integral part of research, replacing and enhancing human labor as much faster and more reliable calculators and data analyzers. This increased the amount of *potential* research output per scientist at an unprecedented exponential rate.[33]

After all, the capacity of the integrated circuits used in computers increases according to the Moore's Law, which is a 60% to 100% annual steady growth, dwarfing the 3% estimate we had before for the size of scientific communities. Fahrbach completely misses this crucial point

[33] more on this potential shortly, under "research capacity."

and focuses only on the demographic trends. Computerization of science however, immensely increases overall productivity of researchers and possibly renders the demographic growth trivial in comparison. We will see numerous examples of this in Chapter 5. But one individual case is especially telling. In the 50s, before the advent of digital technology and computerization of research, experimentalist particle physicists had to sift through thousands of analog photographs and inspect them individually to confirm a rare muon event which was captured by only a few photographs out of roughly twenty thousand. The inspection of photographs kept busy for months not only the entire staff of several labs, but also in some cases their spouses. (Galison, 1987, p. 196) Today, thanks to digital technology the same task can be performed in a few hours on a modest home computer.

All these observations suggest that an overwhelming portion of total scientific research throughout the history was done only recently, which is an observation that has been made independently by Fahrbach (2011, p. 149).[34] [35]

Let's use a crude illustration to visualize how the Pessimistic Induction works and how Fahrbach's demographic objection is supposed to defeat it: Before Einstein's General Relativity the best theory of physical space was the Fresnel-Maxwell picture where physical space was

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[34]Farhbach estimates that "80% of all scientific work ever done has been done since 1950." However, this estimate is overly conservative due to the fact that Farhbach doesn't take computerization into account.

[35]Fahrbach's claim as a very conservative estimate is supported by the growth of academic publications, which appears to match almost exactly the 20-fold per century increase in the number of active scientists. See Figure 2. There are differences between fields in terms of the number of publications and patents, but the trend is almost universally exponential growth. See Figure 3.

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Figure 2. Exponential growth of active academic journals (Mabe & Amin, 2001, p. 154, original figure number is 3)

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Figure 3. Abstract, peer-reviewed paper and patent records by field, semi-logarithmic scale (Larsen & von Ins 2010, p. 584, original figure number is 4)

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Figure 4. R&D expenditure in PRC (in 100 million yuan) (Rousseau & Jin 2005, p. 3, original figure number is 2)

theorized to be euclidean and filled with an elastic medium. Before the Fresnel-Maxwell theory, there was the Newton-Galileo theory where space was believed to be absolute and euclidean yet empty. During the shift from the Newton-Galileo to the Fresnel-Maxwell theory the claim that space was empty was abandoned. During the shift from the Fresnel-Maxwell to Einstein, the supposed elastic medium and the absolute euclidean characteristic of space both were abandoned. If we follow the reasoning recommended by the Pessimistic Induction and admit that the epistemic status of General Relativity is no better than its predecessors, we should expect a new theory change that will result in the abandonment or substantial revision of General Relativity. So, optimism about the truth General Relativity is unwarranted no matter how successful it is.

However, Fahrbach invites us to believe, due to the exponential growth of science, the quantity of evidence in favor of General Relativity is much much greater (by a factor of hundreds if not thousands) than any evidence there ever was in favor of its Newtonian-Galilean and Fresnelian-Maxwellian predecessors. Therefore, optimism might be warranted this time around. Since the same argument can be made in defense of not only General Relativity but also all of our current best theories, as the generalist Fahrbach reasons, we have a successful generalist objection against the Pessimistic Induction.

However, as we shall see in the next section, Fahrbach's demographic objection is not more successful than the other generalist objections against the Pessimistic Induction. It is, like the other two generalist objections, overly ambitious.

4.3 Why The Demographic Objection Fails and What We Can Do About It

The argument from the exponential growth of science for optimism about not just individual theories like General Relativity but also all of current best theories hinges on a crucial assumption: on the assumption that demographics of science and research output (and the quantity of evidence actually produced) are directly correlated across all fields of science so that the quantity of evidence accumulated in favor of current best theories would be vastly greater than that in favor of the best theories of the past all across the board, not just for this or that theory.

Here is Fahrbach's objection, formulated as an explicit argument:

(F1) The trends in the demographics of science indicate a global exponential growth. (Fact)
(F2) The research output correlates with the demographics of science. (Assumption)
∴ There is overwhelmingly more evidence in favor of current theories than there ever was for the theories of the past.

Corollary The third premise (iii) of the epistemic formulation of the Pessimistic induction is false. The epistemic status of all current best theories is different from—indeed, is superior to—the epistemic status of the best theories of the past that were abandoned or substantially revised.

The facts relating to the demographics and operationalization of science I reviewed in Section 4.2 and expressed in (F1) are definitely good news as far as the viability of optimism is concerned. However, the global demographic trend referred to in (F1) isn't by itself sufficient for the general defeater Fahrbach hopes to construct against the Pessimistic Induction and disarm the pessimistic threat.

The first problem, is the fact that (F2) isn't a plausible assumption. Having more scientists doesn't necessarily mean having more evidence. What we can infer from the exponential growth in demographics and the Moore's Law is not that there has been an exponential increase in research *output*. What we can infer is that there has been an exponential increase in research *capacity*. There are various ways of measuring research *output*, which could be conceived as the quantity of evidence that is produced. But however it is measured, it needs to be distinguished from research *capacity*, which is the research *output* a given community can produce under favorable conditions. The demographic trends Fahrbach detects do indeed track a global increase in research *capacity*, but not necessarily a global increase in research *output*.

As we shall see in Chapter 6 in more detail, increased research capacity in some cases remains largely unutilized due to various factors ranging from the sociological dynamics of particular fields, the nature of the phenomena they study, and the methodological problems they face.

However, even postulating a direct and universal correlation between demographic growth and increased quantity of evidence wouldn't be enough to warrant the generalist line Fahrbach is pursuing. After all, what is needed isn't just *more* evidence; it is *better and more reliable* evidence, preferably coming from a diverse set of epistemically autonomous sources. A corollary of the Pessimistic Induction that I discussed in Chapter 1 is what it implies about the evidence we had for the theories of the past. If all we have in favor of the current best theories is *more of the same* evidence that failed us in the past, then pessimism would still be warranted.

John Worrall raises a similar point, and more vividly:

> "[I]n order to justify rejecting the conclusion of the 'Pessimistic Induction' we would surely need some reason to think there was *a difference in kind* between earlier theories and the present ones. As it stands, rejecting the idea that our current theories are likely to be replaced on the grounds that our current theories are better supported than the earlier ones [...] would be rather like rejecting the idea that the current 100m sprint record will eventually be broken by pointing to the fact that the current record is better than the earlier ones." (Worrall 2007, pp. 129-130)

In other words, it is not enough that most of all scientific research in the history of science has been done since the 1950s, as Fahrbach correctly observes. A successful generalist objection also requires that we find a qualitative difference between the epistemic status of all current best theories and all the best theories of the past. This requirement is so strong that it is probably impossible to satisfy.

Therefore, the global trends Fahrbach and I identify are sufficient grounds neither for the the belief that there is a global quantitative increase of evidence nor for the belief that there is a global qualitative improvement of our theories' epistemic status. If there is any improvement in the epistemic status of our theories, it is *localized to certain fields and theories.*

There are four main considerations that we should keep in mind while looking for the localized improvement in question.

First, not all fields of science have one 'best' theory. In some fields there are more than one serious contending theory. This would inevitably dilute down the evidential support a given theory would enjoy even if we were to assume that the quantity of evidence produced correlates directly with research capacity.

Take the contending theories that purport to be *the* fundamental theory of nature: String theory, M-Theory and their many variations, and Theory of Loop Quantum-Gravity. Even if most of the research in fundamental physics has been done in the last 50 years, still the research effort is diluted down by the large number of serious contenders.[36]

That's why, for the increased research capacity to effect an unprecedented accumulation of evidence in favor of a contemporary theory, that theory must dominate its field; it must be the only current serious contender to be the final theory. Let's call this the "domination condition."

[36] This concern is sometimes voiced within a complaint against overspecialization. However, it is important to keep them separate. As a field of science advances, theories and methods become more detailed and complex, which necessitates specialization and sometimes leads to situations in which a scientist specializing on one part of a theory or related method can't interact effectively with the work of those who specialize on other parts of the theory or related method. This is not to say, however, that the evidence will be diluted down as in the case of over-diversification of contending theories. As long as a field is methodologically efficient (see "efficiency condition" below and Chapter 6 for details), specialization doesn't have to hinder the accumulation of evidence in favor of the contender theories.

Second, even if the quantity of evidence produced correlates directly with research output, not all evidence produced end up supporting the best current theories. Some areas of science are not stable in the theoretical sense, which means that they are undergoing or have recently undergone a revolution or a significant theoretical shift, even if there is a best theory that is the favorite of most scientists at present time. That means, a significant portion of the research capacity would have been used to produce evidence to falsify the predecessor of the contemporary theory without confirming any of the predictions of the contemporary theory.[37] That's why, in order for the global increase in research capacity to result in an unprecedented accumulation of evidence in favor of a theory, the field hosting the field must be stable. Let's call this the "stability condition."[38]

Third, the evidential role of computerization is not as straightforward as I implied earlier. Actually, not every field of science has been able to tap the power of computerized research equally. Moreover, even when computers and digital simulations are used extensively as research tools in a field, still they might not make a significant evidential contribution to the field. I will say more about how and when exactly computer use in science improves the evidential support a theory enjoys in Chapter 5. But for the time being let's note that in order for computer use in research to increase the quantity and improve the quality of evidence in favor of a contemporary

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[37]It can in principle do both. It's hard to assess how often this happens though. Due to the dialectic situation, the burden of proof is on the critic of the Pessimistic Induction. Therefore, I am going to assume that stability of the host field is a necessary condition for the accumulation of evidence in favor of a current theory.

[38]To be fair to Fahrbach, he builds the stability condition into his argument by considering "only the successful theories that were accepted in the last 50–80 years." (p. 151)

theory, the field hosting the theory must use computers in ways that help scientists overcome previously insurmountable logistical barriers in theory testing. Alternatively, when a theory enjoys the support of a specific kind of simulation-born evidence (or "evidence *ex silico*" as I shall call it) that is epistemically autonomous from other more traditional sources of evidence, then computerization may be seen as grounds for thinking that the evidence that today's theories enjoy come from a much more diverse set of sources than their predecessors. Let's call this evidential requirement the "computerization condition."

The computerization condition is satisfied by a theory when computer use in the field hosting the theory not only improves the attainability and reliability the evidence that was already available in that field but also diversifies the sources of evidence the theory enjoys. I will say more about in the next section what constitutes a diversification of sources and why it matters.

The satisfaction of the first three conditions is not enough. After all, research capacity can still remain underutilized or outright wasted, which is a point Fahrbach doesn't seem to appreciate enough. There is no reason for thinking that even all good research produces evidence, let alone thinking that methodologically problematic research can produce evidence. Despite the exponential increase in the research capacity, long term stability of a field and even effective employment of computers as research tools, if the methods employed in a field are problematic, then the dominant theory in that field won't enjoy an unprecedented accumulation of evidence. When methodological problems in a field are wide-spread and far-reaching enough, they will inevitably taint evidential quality of the majority of findings in that field. In other words, if a substantial amount of research in a field is based on ineffective or counterproductive research practice, it does not matter if there are islands of reliable evidence in that field. The efficiency condition is unlikely to hold unequivocally for any theory in that field. When this is the case, even dominant theories in the field will not be protected from pessimism. I will say more about this evidential requirement in Chapter 6. Let's call it the "efficiency condition."

Simultaneous satisfaction of these four conditions is arguably sufficient for an increase in research capacity in a given field to translate into an unprecedented increase in the quantity and improvement in the quality of evidence for a theory. Therefore, when these four conditions are simultaneously satisfied by a scientific theory, premise (iii) of the Pessimistic Induction won't hold for that theory.

However, it would be too extreme to claim that all four conditions are necessary to overturn (iii). In particular, the satisfaction of the computerization condition by a theory might be merely one of the possible ways in which a significant improvement in the quality of evidence and unprecedented diversification of the sources of evidence can be effected. Future advances in information technology could make it possible to substitute the computerization condition with a possibly stronger condition, which would also entail a quantitative and qualitative change in the epistemic status of current theories. Fifty years ago, hardly anyone could have imagined how many steep barriers in research will be overcome by the computerization of research. Similarly, I can't rule out an even more powerful methodological future advance that will make look pale the improvement in the quantity and quality of evidence made possible by computerization.

However, until such an advance happens, I believe that the computerization condition, or something like it, will remain an integral part of the exemptionist strategy.

Since not all scientific theories are able to satisfy all four conditions, not all scientific theories can be defended against the Pessimistic Induction by this strategy. Hence, we should abandon Fahrbach's overly ambitious generalist line and focus on saving only some current best theories. However, since the strategy makes no reference to any privileged parts or pieces of the theories that satisfy all four conditions, those theories that satisfy all four will be protected from pessimism in their entirety.

4.4 The Exempting Defeater

The following is a statement of the four conditions I have alluded to and the exempting defeater they jointly constitute.

A theory is exempt from the Pessimistic Induction if[39] the following four[40] conditions are unequivocally satisfied by the theory:

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[39]This is to be read as a material conditional, though certainly there are conceptual connections between the conjunctive components of the antecedent, and the consequent.

[40]If desired, the following fifth condition can be added to the list:

> The Consistency Condition: The theory and its predictions do not contradict any other scientific theories that satisfy the other four conditions.

I am reluctant to include the consistency condition in my list because I think it is an open metaphysical question whether reality is consistent or para-consistent. However, this is not a red-herring. The inclusion has important consequences for the scope of the exempting defeater I am putting forward. If we do include the fifth condition, either theory of special relativity or quantum mechanics will not be protected by the defeater, since they seem to make contradictory predictions (for a dissenting view on this issue, see Jarrett (1989)).

- Domination Condition: The theory currently dominates its field; there are no other serious contenders.
- Stability Condition: The field has been stable for long enough that the exponential increase in research capacity has led to an unprecedented accumulation of potential evidence in favor of the theory.
- Computerization Condition: Computerization of the field has vastly increased the quantity and quality of evidence produced in the field and diversified the sources of evidential support the theories in that field enjoy, making the current dominant theory unique among all the theories that have ever dominated that field.
- Efficiency Condition: The field has been efficient and not wasteful in utilizing increased research capacity resulting from the exponential growth and computerization of science.

The purpose the domination and stability conditions serve has to do with ensuring a quantitative improvement. Unless a theory survived as the uncontested consensus in a field for a reasonable amount of time, the quantity of evidence accumulated in favor of it isn't likely to be significantly more abundant than the evidence that was accumulated in favor of its predecessors. The computerization and efficiency conditions serve a dual purpose. The efficiency condition is necessary for both a qualitative improvement and a quantitative improvement. The Computerization condition ensures a qualitative improvement whereas it is sometimes a complementing factor in a quantitative improvement.

However, as I hinted at before, especially the computerization condition might be substituted with something else in the future. In this regard, *the* exempting defeater these four conditions constitute may not be *the only possible* exempting defeater. There could be alternative exempting defeaters, though I am skeptical that at the moment there is anything that can replace computerization. After all, we don't want to lower the bar too much. For an advance in instrumentation or methodology to play the role the computerization condition plays, it needs to be epistemically autonomous from the previously available sources of evidence. We can't simply point at the electron microscope and declare the theories it produced evidence for exempt from the Pessimistic Induction. The electron microscope, as revolutionary and illustrious as it is, was in a sense *more of the same.* The contributions computers have made to scientific research cannot possibly be reduced to merely *more of the same.* But more on this in Chapter 5.

4.5 What is a Novel Source of Evidence and Why Source Matters

Previously, I claimed that computer use not only increases the attainability and reliability of traditional sources of evidence, but also yields a novel and substantial source of evidence: *ex silico*. This happens, I conjectured, when scientists employ a sub-species of simulations called genetic simulations.

In order to make sense of this claim and appreciate why diversity of sources matters, we must first understand what traditional sources of evidence are. There are three traditional sources of evidence in empirical science: analysis, observation and experiment.

I won't try to give precise definitions of or necessary and sufficient conditions for identifying the source of an evidence token. However, discussing some paradigmatic examples of evidence from each source and using them to arrive at loose but workable descriptions will help us see how the individuation is to be made in practice.

Two paradigmatic tokens of analytic evidence are Newton's Bucket thought experiment and the EPR argument against the completeness of Quantum Mechanics. So, arguments and thought experiments invoking mathematical or geometrical principles or empirical postulates to support or undermine a theory are tokens of analytic evidence.

Two famous examples of observational evidence are Hubble's discovery of the redshift and Eddington's measurement of the positions of the stars during the 1919 solar eclipse. These evidence tokens come from the source I have identified as observation because they involve only or mostly passive empirical observations that support or undermine a theory.

Experimental evidence is also empirical, however it is more actively interventionist as opposed to being passive. Beaumont's experiments on gastric physiology and Rutherford's alpha-bombardment experiments belong to this source because they both involve active intervention into or redirection of natural phenomena. A more recent instantiation of the experimental source is the Large Hadron Collider experiments at CERN.

Since *ex silico* evidence is relatively recent and entirely parasitic on a computationally demanding approach, it is difficult to point at a canonized example. Moreover, *ex silico* evidence shares some features with the three traditional sources of evidence but the nature of the computer use involved (i.e. genetic programming) creates decisive differences which make *ex silico* a distinct source of evidence.

In Chapter 5, I will look at these differences in more detail by examining an example. However, what makes genetic simulations constitute a new source of evidence is their epistemic autonomy from theory, and methodological parity with analysis, observation and experiment.

One might wonder why the source of evidence matters. To see why this is the case again recall the murder trial case where the prosecution had made four arguments. Each argument had placed the accused at the crime scene at the time of the crime. Three of these arguments are based on the same source, the testimony of a single witness, which the defense attorney managed to discredit through a compelling cross-examination. The fourth argument however, was based on something else: the finger prints of the accused were found on the murder weapon. We concluded that it would not be sensible to make a pessimistic induction from the failure of the first three arguments to the failure of the fourth argument because the forth argument comes from a different evidential source.

That is why, if we show that computer use allows scientists tap a novel and substantial source of evidence, our hand against the Pessimistic Induction would become much stronger. In order to do this, we must show that the alleged source (*i.e.* genetic simulations) has a substantial degree of epistemic autonomy from theory, and is methodologically distinct from other sources of evidence. To see why, let's first look at a parallel question: Why does it matter whether experiment and observation have epistemic autonomy from theory?

One of the most serious challenges that have ever been raised against the realist and objectivist interpretations of science is the Kuhnian thesis that there is no such thing as experimental or observational autonomy. What you see in the lab is not objective evidence because it is determined by your culture, your beliefs, your theory. Andrew Pickering, an ardent follower of Kuhn, puts the point most succinctly:

> It is *unproblematic* that scientists produce accounts of the world that they find comprehensible: given their cultural resources, only singular incompetence could have prevented them from producing an understandable version of reality at any point in their history. (Pickering 1981, p. 236)

> [T]here is no obligation upon anyone framing a view of the world to take account of what twentieth-century science has to say. The particle physicists of the late 1970s were themselves quite happy to abandon most of the phenomenal world and much of the explanatory framework which they had constructed in the previous decade. There is no reason for outsiders to show the present [high energy physics] world-view any more respect. (Pickering 1984, p. 413)

At the heart of the Kuhnian thesis there is the idea that no observation or experiment is epistemically autonomous from the theories they are used to test, because the instruments that are used in making measurements and observations are 'theory-laden.'

As a personal anecdote by Alan Chalmers illustrates nicely, theory-ladenness of instrument use can be evidentially devastating in some cases, *i.e.* when the connection between theory and experiment or observation is vicious:

> I remember being troubled by an experiment I was obliged to have a senior class conduct. It involved measuring the deflection of a coil suspended between the poles of a magnet as a function of the current passing through it. What troubled me was that I knew what was inside the ammeter being used to measure the current, namely, a coil suspended between the poles of a magnet. In this experiment the deflection would be proportional to the current readings whatever the relationship between current and deflection provided only that both coils were governed by the same relation. (2003, p. 495)

However, as I will discuss in more detail in Chapter 6, it turns out that the thesis that there is a strong connection between instrumental theory-ladenness and lack of epistemic autonomy is a false generalization. Thanks to the extensive and compelling case studies by post-Kuhnian historians and philosophers of science, we now know that experiment and observation in science often have epistemic autonomy from theory (Galison 1987, Franklin 1989, Chalmers 2003). Theory-ladenness doesn't have to lead to epistemic parasitism because the relationship between theoretical assumptions on the one hand and observational and experimental results on the other is not typically vicious. Instrument use in observation and experiment might import theory, but it doesn't have to import the theory that is being tested.

Looking at the disagreement between the Kuhnians and their critics is useful because it alerts us to a potential and parallel Kuhn-inspired worry against my contention that computer use in science can and does yield a novel and substantial source of evidence. After all, if computer use is theory-laden and the relationship between theoretical assumptions and computer use is vicious, then computer use in science lacks epistemic autonomy. As a result, we will have no choice but to admit that computer use cannot constitute a distinct source of evidence.

In Chapter 5, we will take a close look at particular uses of computers in science. We will see that in most cases, including classical simulations, the worry is unwarranted. Such simulations *don't have to be* viciously theory-laden.

The situation looks even more promising as far as evidence *ex silico* is concerned because it is a novel and substantial souce of evidence. By nature, evidence *ex silico* is epistemically autonomous from theory yet methodologically on par with traditional sources of evidence. Therefore, it *cannot be* viciously theory-laden.

4.6 Conclusion

It is important to stress that the exemptionist strategy I sketched in the present chapter and the exempting defeater it utilizes cannot support a general form of optimism enveloping all fields and dominant theories of all contemporary sciences. The scope of optimism that can be supported by the exempting defeater is limited those theories which satisfy all four conditions simultaneously and unequivocally.

I would also like to highlight another crucial point. The four conditions above (or any other exempting defeater we might discover in the future by replacing computerization with something else) are jointly sufficient to exempt theories only if we make the following seemingly reasonable assumption: Having quantitatively more abundant evidence and qualitatively different kinds of evidence in favor of an unfalsified theory makes it more credible, and elevates the epistemic status of those who believe the theory. This assumption is an *a priori* principle that is supposed to apply to all unfalsified theories. Regardless of the content of the theory having more evidence, better evidence from more diverse sources should in principle increase the subjective probabilities we assign to the theory.

Although the assumption seems reasonable, it is hard to say much in its defense without begging the question. The opposite is also true. Short of resorting to *a priori* skepticism concerning the relationship between empirical evidence and theories, it's hard to say anything against the assumption. One thing we might say about it however, is that it is a bedrock epistemic commitment we need to take on board if we are going to use the exemptionist strategy.

Granted that the assumption is true, I conjecture that under close scrutiny the four conditions I specified will hold for some theories in physical sciences and a smaller set of theories in biological sciences. A full demonstration of how far this conjecture holds would be such a large scale enterprise that it is surely beyond the scope of this manuscript. However, in Chapter 5 I shall argue that three theories in physics (i.e. Fermi-Landau Liquid theory, the NICE Model and the Standard Model) unequivocally satisfy the computerization condition. Based on the plausible assumption that they also satisfy the other three conditions, these theories enjoy an unprecedented accumulation of evidence coming from an unprecedentedly diverse sources of evidence. Therefore, not withstanding other reasons for pessimism, we are warranted to think that these three theories are here to stay in their entirety. As far as the phenomena they are set out to account for are concerned, they are probably the best theories evidence will ever warrant. We can be optimistic about them, even though the history of science that led to them is riddled with revolutions.

In Chapter 6, I will look at the flip side of the coin and discuss how most theories in behavioral and life sciences may fail to unequivocally satisfy the efficiency condition. If my observations are correct, the theories in these fields could completely be left out the scope of the exempting defeater I deploy.

The limited scope of the exemptionist strategy may appear to be an undesirable outcome, however it really is not. After all, one ought to be suspicious of any wholesale refutation of the Pessimistic Induction because the only reason why we should be optimistic about the current best theories is the evidence we have for those theories. Science is a mosaic of numerous independent or semi-independent disciplines or fields each hosting multitudes of theories; it is not a monolith. Conceivably, while the epistemic status of the theories hosted by some fields might have improved over time in relation to the older theories accepted by the researchers in those fields, the epistemic status of the theories in other fields might not have improved. So, instead of seeking some overly ambitious generalist, *one-size-fits-all* answer (as Fahrbach did most recently) to the question whether the epistemic status of our best theories has improved over time, we should look for a separate, *custom-made* answer for each theory and its respective field, and be content when the answer we find even if it warrants only individual exemptions from the Pessimistic Induction.

5. COMPUTERIZATION OF SCIENCE AND EVIDENCE

In Chapter 1, I observed that the Pessimistic Induction can be formulated as follows:

(i) The history of science is a history of revolutions.

(ii) In each revolution so far, scientists abandoned or substantially revised some of the best theories of the past.

(iii) The epistemic status of the current best theories is not different from—and therefore is not superior to—the epistemic status of the best theories of the past that were abandoned or substantially revised.

∴ The current best theories are also going to be abandoned or substantially revised in the future.

Again in Chapter 1, I conjectured that premise **(iii)** is not universally true: There are some fields in contemporary science which host theories that enjoy a superior epistemic status in comparison to their predecessors. Then in Chapter 4, I put forward four conditions whose satisfaction would indicate a superior epistemic status and can exempt theories from the Pessimistic Induction. When these four conditions are simultaneously and unequivocally satisfied by a current theory, we have sufficient grounds to believe that its epistemic status is superior to the best theories of the past. So, a current theory is completely exempt from the Pessimistic Induction if the theory simultaneously and unequivocally satisfies the following four conditions:

- Domination Condition: The theory currently dominates its field; there are no other serious contenders.

- Stability Condition: The field has been stable for long enough that the exponential increase in research capacity has led to an unprecedented accumulation of potential evidence in favor of the theory.
- Computerization Condition: Computerization of the field has vastly increased the quantity and quality of evidence produced in the field and diversified the sources of evidential support the theories in that field enjoy, making the current dominant theory unique among all the theories that have ever dominated that field.
- Efficiency Condition: The field has been efficient and not wasteful in utilizing increased research capacity resulting from the exponential growth and computerization of science.

The domination and stability conditions hold for some theories. Examples include theories in the fields of condensed matter physics, cosmology, particle physics, biology and geology. Some fields not only have dominant theories but also the research capacity that has been available in those fields is typically many orders of magnitude of what was available to the previous generations of scientists in the same or parent fields. This suggests that the quantity of evidence accumulated in favor of some current best theories might be many orders of magnitude of the quantity of evidence past generations had in favor of their best theories, assuming that these theories also satisfy the efficiency condition.

However, showing **(iii)** to be false for a theory requires more than a mere quantitative increase in the amount of evidence that is available to the members of the current scientific community. It also requires a change in the quality and the diversification of the sources of the evidence in question, as captured by the computerization condition.

Recent computerization of scientific research offers us an opportunity to make a compelling case for the thesis that there is such a qualitative improvement at least as far as some current theories are concerned, which renders the sum total of evidence in favor of them superior to their predecessors.

In the present chapter, my main objective is to convince you that the thesis in question is true. Computerization condition holds for at least some theories, specifically the Fermi-Landau Liquid theory in condensed matter physics, the NICE Model in Solar System cosmology and the Standard Model in particle physics. I will argue for this claim by offering a close analysis of how computer use has not only improved over the last few decades the attainability and reliability of the evidence that used to be available to scientists, but also made available a novel and substantive source of evidence. This source of evidence, which I will call "*ex silico*", is genetic computer simulations.

The claim that computer simulations could play a substantive epistemic role in theory testing is a controversial claim, which has been criticized recently by Eric Winsberg. He argues that due to several factors—including the discretization problem and what he calls the "semi-autonomy" of simulations from theory—computer simulations cannot provide evidence for or against theories.

The central claim of the present chapter disagrees with Winsberg on the epistemic potential of simulations. Especially in Sections 5.2.3, 5.3, 5.4 and 5.5 where I offer case studies of simulationist science, I will critically engage Winsberg's pessimistic view that simulations can't play a significant evidential role in science. In a nutshell however, Winsberg's pessimism about simulations results from his overestimation of the discretization problem, an underestimation of the instrumental role numerical simulations play in theory testing and his mistaken belief that epistemic autonomy from theory necessarily renders a practice evidentially insignificant.

Since Winsberg's view is shared by many, I will start with a brief summary of it, before responding to it in detail.

5.1 Pessimism concerning The Evidential Potential of Computer Simulations

The claim that computer simulations can play a substantial evidential role in science is disputed by Eric Winsberg. In his influential book *Science in the Age of Computer Simulation*, Winsberg defends a somewhat pessimistic view of the evidential use of computer simulations in research.

In particular, Winsberg thinks that most representational and numerical simulations don't provide evidence for our theories. His reason for pessimism is what he calls the "semi-autonomy of simulated science from theory." The semi-autonomy results from two factors. First, in some cases like climate simulations, the theoretical framework underdetermines the simulation because the theory contains large gaps that need to be filled out by the simulationist. This imports in a great deal of background theory into the simulationist practice. (2010, pp. 70–71) Second, and more generally, because simulations must represent a typically continuous structure in a discrete medium, they necessarily run against a logistical difficulty known as the "discretization problem," which forces the scientist to make seemingly arbitrary modifications on the simulated model, such as assigning physically unrealistic properties to sub-systems in the simulation.

The semi-autonomy of simulations and the misrepresentations and distortions it involves, makes Winsberg endorse the thesis that simulation models are *fictions*.

> Fictions [...] are representations that are not concerned with truth or any of its philosophical cousins (approximate truth, empirical adequacy, etc.). (Winsberg 2010, p. 73)

However, according to Winsberg fictions are not entirely useless. They help ""smooth over the inconsistencies between the different model-building frameworks and extend their scope to domains where they would individually otherwise fail." (2010, p. 91)

So, on Winsberg's account, simulations are scaffolding that helps theories extend the domains where they apply. However, because of the fictional elements of the scaffolding, one must give up on the hope that the extended success indicates truth, approximate truth, or even empirical adequacy. In fact,

> The practice of using fictions in building credible simulations is worthy of closer scrutiny by philosophers of science interested in the various arguments for and against scientific realism. [T]hese techniques are successfully and reliably used across a wide domain of fluid dynamical applications, but both make use of "physical principles" that do not purport to offer even approximately realistic or true accounts of the nature of fluids. I argue that these kinds of model-building techniques, therefore, are counterexamples to the doctrine that success implies truth—a doctrine at the foundation of scientific realism. (2010, p. 92)

Let me not overstate my disagreement with Winsberg's position. I agree with him about the fictional character of representational simulations. I agree with him that this fictional character is by and large an inevitable consequence of the semi-autonomy of simulation from theory. I also agree with him that it would indeed be a mistake to think that the predictive success of a simulation suggests that the simulation represents the world accurately.

My disagreement with Winsberg is about the exact role these admittedly fictional, semi-autonomous simulations can and do play in science. Yes, simulations themselves aren't true. But it is very misleading to claim that they "are not concerned with truth or any of its philosophical cousins (approximate truth, empirical adequacy, etc.)." Actually, the use of simulations in science is often evidentially oriented. Simulations are not merely heuristic scaffoldings. They help scientists obtain evidence for—and often against—theories.

In particular, as I will illustrate in Section 5.3.1, some simulations (*i.e.* numerical simulations) are not representational. They are glorified calculators; their epistemic role is purely instrumental. They provide evidence for theories, such as the Fermi-Landau liquid theory, by helping scientists confirm the predictions of the theoretical model without themselves positing the existence of anything in the world. Some others, as we shall see in Section 5.3.2, are representational. But the fact that they are riddled with gaps and possible fictions doesn't undermine their evidential significance. After all, not all evidence is confirmation. Sometimes, as in the case of the NICE model, representational simulations provide evidence for a theoretical model by demonstrating that the model is capable of retrodicting a historical event correctly. Yet in another sub-type (*i.e.* genetic simulations) the evidence comes from the simulation's apparent epistemic parity with other traditional sources of evidence.

I suspect that beneath the disagreement Winsberg and I have, there lies differences in how we conceptualize the semi-autonomy of simulation from theory. I, unlike Winsberg, don't find semi-autonomy to be a threat.

I concede that he is right about cases of extensive gaps, but this doesn't threaten the argument I am making. After all, the theoretical models that are ridden with serious gaps typically fail the dominance and efficiency conditions, which disqualifies them from an exemption. Moreover, as we will see in our examination of a representational simulation providing evidence for the NICE model, simulationists can fill out the gaps in our theoretical models non-arbitrarily. Therefore, the mere fact that there is a gap in the theory doesn't mean that it cannot be confirmed or disconfirmed by simulations. Second, the discretization problem is not so challenging in fields where non-linearity is eliminable. There are theoretical detours such as Quasi-particle Interference modeling that minimize the impact of the transition from continuous to discrete. Third, Winsberg seems to be completely blindsided about the instrumental role of some numerical simulations. In this regard it doesn't matter if a simulation maps onto the theoretical model well. A computer model can be just a computational tool that is used to overcome a logistical barrier in theory testing without needing to refer to anything that exists in nature or correspond to anything the theory postulates. Finally and most importantly, Winsberg's assumption that autonomy from theory is epistemically ruinous is mistaken. As we will see in Section 5.4, what makes evidence *ex silico* possible is the epistemic autonomy of genetic simulations from theory and methodological parity with other sources of evidence. In other words, epistemologists of simulationist science shouldn't shun epistemic autonomy from theory, they should embrace it.

This should come as no surprise to the student of history of the methodology of science. Whenever a new instrument, measurement technique or approach is proposed, the first question that comes to mind is whether it is epistemically autonomous from theory and has methodological parity with other sources of evidence. After all, if epistemic autonomy doesn't hold for the theory, the new method may be subject to vicious theory-dependence. If it is not methodologically on par with previously available methods, the new method is unreliable or derivative from some other method that has already been in use. If the former is the case, the method is evidentially vacuous. If the latter is the case, it is evidentially redundant.

This is why, for example, Carnap wanted to establish the autonomy of observation from theory. If the language of observation can be purged of all theoretical terms, then we can use observation as a neutral, therefore objective arbiter of theoretical disputes. (Galison 1984, p. 7) If such a reduction cannot be effected, Carnap reasoned, then observation itself would be theory-dependent.

Today it is not fashionable to think of the demonstration of autonomy as a meta-linguistic undertaking. Instead, we see it as an epistemic and meta-methodological task. However, establishing at least partial autonomy from theory and parity with other methods is still the holy grail for the proponents of all emerging methods, approaches and instruments.

This epistemic and methodological task was successfully undertaken by Peter Galison who showed that experiment (particularly, high energy physics) is autonomous from theory and has methodological parity with analysis and observation. Galison argued,

> Experimentalists do not march in lockstep with theorists, and quite frequently their experiments span several particular theories, and even groups of theories.
>
> [T]he content of experimentation has practically nothing to do with "observation" as a problem in the perception of instrument readings (Galison 1987, p. 8)

However, unlike Carnap, Galison only looked for partial autonomy from theory.

> For both epistemological and historiographical reasons, we must recognize that experimental and theoretical training, skills, and judgments are not necessarily coextensive. In the future, as other branches of natural science undergo the schism between theory and experiment, we will need a better qualitative picture of the relation of theorists to experimenters. It will have to capture the partial autonomy of each, without implying that they never interact. (Galison 1987, p. 255)

In other words, it is unrealistic to expect an experimental practice to have full autonomy from theory or to be fully dependent on it. The real "issue should not be whether theory enters, but where it exerts its influence in the experimental process and how experimentalists use theory as part of their craft." (Galison 1987, p. 245) As Galison's investigations concerning high energy physics experiments demonstrate, experimental practices often enjoy a sufficient degree of partial autonomy from theory, which allows experiments to provide evidence for or against theories.

In sections 5.3.1–3, I will make the same argument, but regarding simulationist practices and show that Winsberg's pessimism relating the semi-autonomy of simulationist science from theory is unfounded. In fact, if simulations are to yield evidence about, or against theories, they need to be partly autonomous from theories.

But first, let's take a brief look at how computer use improves the availability and reliability of evidence by allowing scientists overcome logistical barriers without resorting to simulations at all.

5.2 Non-Simulationist Computer Use and Evidence

As I indicated above, scientists use simulations in epistemically significant ways. However, computer use in science can be epistemically significant even when it doesn't involve any simulations. Indeed, computer use can increase the attainability and reliability of evidence by helping data modeling and analysis, and computerized instrumental control, neither of which require the use of simulations.

5.2.1 Computer Use in Data Modeling and Analysis

Computers help scientists model and analyze data. As Suppes (1962, p. 252) observes, seldom if ever do scientists compare the predictions of a theory against sets of observation statements. Instead, they construct data models and compare the theoretical predictions with those models.

In order to create a data model, scientists first have to sift through large quantities of raw observations and measurements. Before the computerization of science, this sifting through was done manually, and sometimes by relying on nothing more than the experimenter's unexplained experience and intuition.

For instance, the lab notes Robert Millikan took during his Nobel-winning experiments to measure the unit charge are not merely long enumerations of measurement values and data points. They include a rather interesting torrent of commentary regarding the quality of each individual measurement as well as verdicts about whether those points should be reported in the published paper or not. If we don't take the role of the experimenter's intuition into account, it actually looks as if Millikan is guilty of cherry-picking his evidence to make it look neater than it actually was. (Goodstein 2010, pp. 33-34) However, what he did was actually a successful implementation of an implicit data model: he sifted through a mixed bag of measurements using his experience-born intuition regarding which measurement tokens "went wrong" due to the temperature and barometric pressure fluctuations in the lab. Millikan was well aware that not all measurements are created equal and the scientist in the lab must often make an educated guess about what is a valid measurement and what is merely an artifact. Understandably, those who didn't have this awareness even went so far as to blame Millikan of cherry-picking data. (*e.g.* Jackson 1984, p. 12)

Computers enable scientist do on a large scale what Millikan was able to do on a small scale. They can detect minute expressions of bias in large and chaotic sets of data points, letting us identify problems with equipment and experimental design.

They also have some potential in catching fake or tampered data, especially in fields where reproducibility is low and scientists do not expect replication attempts to be successful. It is a known fact that humans cannot generate true random numbers for they have a well documented systematic bias in number selection. The comparison depicted in Figure 5 between the sets of ‘random’ numbers humans come up with and sets random number generators produce is revealing. As the figure indicates, people have a bias for prime and odd numbers such as 7 and 17 and a bias against even numbers and numbers that look too typical such as 5 and its multiples.

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Figure 5. ‘Human bias in random number selection’, original figure online at:
http://bit.ly/1x48bZI

When a scientific study involves large amounts of manually generated fraudulent data as it more often happens in the fields where reproducibility is low, knowing the bias in question is potentially helpful. But without the use of computers it is a significantly time consuming task to verify whether a large data set is ‘cooked’ by hand or not. However, it is a trivial task for a computer program to conduct the same verification. In this regard, even as merely an anti-fraud measure, computerization of science clearly improves the reliability, therefore the quality of evidence in science.

Yet, computer use in data modeling and analysis cuts deeper than merely sifting through data and eliminating what is bad or fraudulent. Computers also play a positive role in contemporary data modeling by increasing the amount of information we can record and process as well as making it feasible to represent large quantities of data in a way that is accessible to human cognitive abilities. The cognitive enhancement in question is amplified by distributed computing projects like Astropulse and Folding@home.

The methodological contrast between the pre-computerization and post-computerization experiments in high energy physics is revealing. From the 50s through late 70s, processing the raw data from a high energy physics experiment involved going through large quantities of photographs of cloud, bubble or spark chambers, identifying by eye the interaction and decay patterns, and manually organizing common and unusual patterns to confirm or dis-confirm theoretical predictions, sometimes regarding previously unobserved particles or interactions. (Gallison 1987, p. 176) Today, these chambers are replaced by large scale electro-magnetic particle colliders but the hunt for new particles continues. However, one aspect of today's high energy experiments is remarkable: The sensors built into today's colliders collect terabytes of information per second. Had scientists tried to store such large amounts of information in an analogue medium like photographic film, filter and process it by human eye instead of the thousands state of the art computers located at CERN, they would have found themselves hopelessly overwhelmed by the logistic impossibility of the task. This clearly shows that computers make previously infeasible experiments and observations possible.

Another example illustrates how computers enable scientist to create intuitive representations of data models. Cytoscape, an open-source data-modeling software, makes it possible to represent interdependencies and interactions between a large set of factors, like different types of molecules found in a living cell. Without tools like Cytoscape it is impossible for a scientist specializing in the physiology of—let's say one—gene to have an understanding of how that physiology is affected by second and higher-order interdependencies and interactions with other genes. (Cline 2007)

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Figure 6. A Cytoscape model of the interactions between protein-coding genes, some of which are implicated in hereditary increased risk of heart disease. Image courtesy of personal friend Ahmet Raşit Öztürk, original image online at: http://bit.ly/UubX1P

The epistemic impact of Cytoscape on scientific research is very difficult to overstate. It has been used to varying degrees in over 1300 PubMed research articles since its launch in 2003.[41] Moreover, Cytoscape has been helping scientists overcome a logistical barrier in biological and behavioral sciences that has troubled philosophers and scientists alike: the cognitive infeasibility of modeling truly integrated systems.

Among those who worry about the infeasibility of modeling truly integrated systems, there are William Bechtel and Robert Richardson (as well as Jerry Fodor). According to Bechtel and Richardson, systems fall into two main categories: simple and complex. In simple systems parts are almost completely independent objects, they are usually qualitatively similar to each other and they interact minimally. An example of a simple system would be an inert gas stored in a closed container. In such a system, gas atoms collide with the walls of the container and occasionally with each other, but their dispositional properties such as the ability to undergo elastic collisions will be preserved. Simple systems can usually be modeled by means of direct localization mostly through analytic heuristics (Bechtel & Richardson 1993, p. 24-25).

Complex systems are divided into two sub categories: component and integrated. In component systems, parts are often qualitatively distinct and can interact with each other to some extent. However, their dispositional properties are preserved during such interactions. Therefore, it is usually possible to model them using the heuristic methods available to contemporary scientists (Bechtel & Richardson 1993, p. 25).

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[41] online at: http://1.usa.gov/1nirk9u

A familiar theoretical example of a full-blown component system would be a massively modular mind. On this model of mind, the mind consists of encapsulated processors. These modules are exposed to the influence of each other only through data bottlenecks and they lack plasticity. Therefore, though the modules interact with one another, their interaction is somewhat limited. Thus, it is possible to break the behavior of a massively modular mind into a number of area-specific subtasks and perhaps associate these subtasks with certain neural correlates in organism's central nervous system, thus producing a localized model.

On the other hand, in truly integrated systems such as living cells or actual mammal brains, where the interaction between parts is not minimal, we face serious difficulties in modeling. Part of the difficulty is due to the absence of functional localization in such systems. However, an equally serious issue, which has been resolved by Cytoscape, had been the enormous size and complexity of the data models that are needed to model the system itself.

The size complexity of the required data model increases (often exponentially due to the fact that the system to be modeled is integrated) as the size of the data set increases, which creates a logistical barrier that cannot be overcome by non-computerized science. But, thanks to tools like Cytoscape scientists today can faithfully represent data concerning integrated systems and construct visual data models. This doesn't go all the way toward resolving the problem Bechtel and Richardson worry about because there remains plenty of room for skepticism about representing the functional architecture of the system without localization. However, Cytoscape is still a giant leap forward that could not have been foreseen before the advent of open-source computing, the Internet and crowd-sourcing. In this regard, even only as far as data modeling and analysis is concerned, computerization of research entails an epistemic difference between the scientific theories of today and the past.

Admittedly, so far the advances in methodology we have looked at don't involve the emergence of a new source of evidence. The computer use we have covered so far merely improves the reliability and expands the boundaries of the evidence that can be gathered through analytic, observational and experimental methods. However, as we shall see in Section 5.4, computer use in science can achieve much more.

5.2.2 Computer Use in Instrumental Control

The second way in which non-simulationist computer use increases the attainability and quality of evidence in science concerns instrumental control.

Successful completion of some experiments or observations demands levels of precision that are simply beyond what can be achieved or maintained without computer use. For instance, the positioning and control of a scanning tunneling microscope (the instrument that is used, for instance, to experimentally confirm the simulation-generated predictions about the properties of Fermi surfaces, which I shall discuss in Section 5.3.1) requires precision measurable in picometers, a unit that is one trillionth of a standard meter.

> Optimum functioning of an STM device requires tip-to-sample position control with picometer precision, a rough and fine positioning capability in three dimensions, a scanning speed as high as possible, and also, preferably, simplicity of operation. These requirements have to be satisfied in the presence of building vibrations with up to micrometer-size amplitudes, electric noise, thermal drift, creep and hysteresis of piezoelectric translation elements and other perturbations. (Bai 2000, p. 63)

These precision requirements are so great that they are simply beyond what can be done with even the best analog instrumental control.

The same is the case with many other experimental or observational instruments that are in use in contemporary science including MRI and fMRI machines (Filler 2010), modern TOKAMAKs[42] and telescopes and spacecraft.[43]

However, the clearest example of a logistical barrier overcome by the use of computers in science is VLBI (very-long-baseline interferometry). VLBI is a technique in radio astronomy, which uses computers to synchronize and coordinate the data collected from geographically separated radio-telescopes. The most well-known and largest-scale application of this technique is the VLBA, or the Very-long-baseline Array.

The VLBA consists of ten radio-telescopes, built on geographically separated sites spanning over 5000 miles. The array allows scientists to make astronomical observations and cosmological measurements that are not only unprecedented but also rich in evidential value. Examples include detailed radio images of dying stars, supernovae and faraway galaxies and quasars, and measurements relating to cosmic background radiation, dark energy and the expansion of the universe, all of which are clearly evidentially significant.

Computers play several vital roles in the VLBA project, including data collection, storage, analysis and modeling. However, for our purposes in this section the most significant role they

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[42]online at: http://bit.ly/1nWn76R

[43]online at: http://bit.ly/1pdZfj3 and http://1.usa.gov/1rMTZUK

play is instrumental control. Each one of the ten telescopes produces large quantities of data. In order to form a radio image with high resolution, the data needs to be synchronized and combined using computer controlled atomic clocks and GPS systems in real time. In other words, without the use of a global computer network, VLBA and the observational evidence it produces would have been impossible.

Therefore, the use of computers in instrumental control also warrant the thesis that computerization of various fields of science improves the epistemic status of the dominant theories in those fields by increasing the attainability and reliability of the traditional sources of evidence.

5.3 Evidential Use of Classical Simulations

So far, the examples of computer use we have looked at indicate that computerization of science can increase the attainability and quality of evidence even without the use of simulations. However, this is not the only way computers can make epistemic contributions to the fields of science in which they are used.

Another way in which computer use increases the availability and reliability of traditional sources of evidence is through the use of classical simulations, where “classical” is to be understood as “non-genetic”, whereas genetic simulations constitute a sub-type I shall cover in Section 5.4.

There are two kinds of classical simulations: theory-driven and experiment-driven. Classical theory-driven simulations, are purposefully designed computer programs that aim to perform the computational tasks that are implicated or demanded by the theory. They fall into two sub-types: numerical simulations and representational simulations.

Typical classical numerical theory-driven simulations solve mathematical problems that arise from the theory, like finding the eigenvalues of a complex Hamiltonian or computing an integral function that is not approachable by analytical or heuristic approaches. In Section 5.3.1, we will look at a numerical theory-driven simulation that was successfully used to predict the behavior of heavy fermion superconductors.

Representational simulations are rarer in comparison, but not uncommon. They don't just solve a mathematical puzzle; they stand in a representational relationship with phenomena. As a result, representational simulations have working parts that can be mapped on to physical or biological systems, or at least parts of such systems. In Section 5.3.3 we will look at a series of astrophysical representational simulations that provided evidence for the NICE Model of Solar System evolution. Crowd-sourced protein-folding research project Folding@home is another well-known example of classical representational simulations.

Classical experiment-driven simulations on the other hand, are used to resolve various issues born out of experimental science such as experiment design, and calibration and standardization of instruments. They can again be divided into numerical simulations and representational simulations. In Section 5.3.2 I will offer a case study involving a classical experiment-driven representational simulation, which was instrumental in TOKAMAK reactor design.

Classical theory-driven and experiment-driven simulations both enable scientists overcome logistic barriers that would otherwise be insurmountable. Therefore, they greatly increase the attainability and quality of evidence that come from traditional sources.

This claim contradicts the pessimism I attributed to Winsberg, who argues that simulations cannot play any significant epistemic role in science due to their fictional status and semi-autonomy from theory. So, let's look at particular instances of simulations, which indicate that Winsberg underestimates the evidential potential of simulationist computer use in science.

5.3.1 Case Study: A Numerical Simulation Providing Evidence for the Q-PIM

Computer simulations can be used in overcoming logistical barriers in science. This certainly true for some classical theory-driven numerical simulations used in condensed matter physics.

One example illustrates the point especially well. It involves a numerical simulation driven by a theoretical quasi-particle interference model of heavy fermion superconductors. In order to see how the simulation in question helps scientists overcome a previously insurmountable logistical barrier, we need to have a basic understanding of heavy fermion superconductors and quasi-particle interference models.

Strictly speaking, a superconductor is a material (traditionally, a metal with a fairly regular crystal structure and recently a non-metal composite) whose electrical resistance drops to zero when cooled below a certain temperature. However, this definition applies to virtually all solids. Virtually all solid materials lose their electrical resistance when cooled to near-zero in absolute temperature scale. This is why in practice the word "superconductor" usually refers to high-temperature superconductors, which lose their electrical resistance at relatively high temperatures.

When superconductors contain non-magnetic impurities (atoms that don't belong to the normal crystal lattice), the superconductive characteristics change dramatically. When such an impure superconductor is lowered below the superconducting temperature, the flow of electrons follow certain patterns called “Fermi surfaces” that emerge from the electron screening vectors, which can be classically understood as the coulomb forces between the orbital and conducted electrons. Figure 7 illustrates (a) a three dimensional computer generated map of Fermi surfaces and (b) its cross-sections in a heavy-heavy fermion superconductor with non-magnetic impurities. The arrows indicate the electron screening vectors that create and shape Fermi surfaces.

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Figure 7. Fermi surfaces in a superconductor with non-magnetic impurities. (Akbari *et al*, 2011, p. 2, original figure number is 1)

According to the Hubbard theory of conduction, electrons ‘hop’ between conduction atoms following patterns that are called “electron bands.” In an ordinary conductive material, these bands represent arrays of short consecutive ‘hops’ which lead the conducted electron to lose its energy in the form of electromagnetic radiation as it gets captured by the destination atom. That’s why an ordinary conductor has electrical resistance and needs a continuously applied electric field to maintain a current. In a superconducting material, which is described by the KONDO lattice model, these bands form continuous Fermi surfaces that allow electrons to ‘hop’ an arbitrarily large number of conduction atoms at a time without losing their energy. This is why superconducting materials have no measurable electrical resistance. So, the Hubbard theory combined with the KONDO lattice model provides a complete explanation of superconductivity.

Physicists can experimentally determine the shape and dynamical properties of a superconductor’s Fermi surfaces by physically cleaving the conductor at multiple locations and scanning the cross-section with a scanning tunneling microscope. Interestingly, in contrast with the explanatory success of the accepted theoretical model of superconductivity and detailed experimental studies of Fermi surfaces, predicting what the experiments will reveal about the exact shape and dynamic behavior of Fermi surfaces in an impure superconductor has been extremely difficult until recently.

The nature of the problem resembles the n-body problem in classical physics. The electrons inside the conductor, orbital or conducted, exert non-negligible coulomb forces on their peers. (The force in question is a function of the inverse square of the distance between two individual charges.) As the electrons move around, the magnitude and direction of these forces change dramatically for each electron. Since Fermi surfaces are created and shaped by these forces, it becomes very difficult to infer a prediction about the shape and behavior of Fermi surfaces from the accepted theoretical model.

Given the direction I have been pushing, you might guess that at the end of the story a computer simulation saves the day and makes it possible to infer predictions from the theory. You would be correct. However, why your guess was correct is a bit more complicated than you might imagine. The development of theory-driven simulations that managed to yield predictions from the Hubbard theory and the KONDO model was contingent on the invention of another theoretical framework called quasi-particle interference models.

In 1956 Soviet physicist Lev Landau independently co-discovered the Fermi-Landau Liquid theory (the other co-discoverer was the Italian physicist Enrico Fermi). Among the mathematical methods Landau developed alongside the theory, there was a novel way to represent physical systems consisting of many atomic or subatomic components. This novel way allowed him to use the statistical information that is available from macro measurements on the system to generate a mathematical object. This object, which contained information about an entire system, looked oddly familiar. It looked like a wave function. This was a surprise, because wave functions are typically used to represent much smaller systems, often particles or small ensembles of particles, such as atoms or electrons. In this picture, it made sense to call the odd mathematical object a "quasi-particle model" of the system, since the entire system was treated as if it was just one particle.[44]

It gradually became clear to the condensed matter physics community that the quasi-particle model was more than just a mathematical novelty. If and when the eigenvalue of the quasi-particle Hamiltonians could be determined, physicists could predict certain geometric and dynamical characteristics of Fermi surfaces. However, typical quasi-particle Hamiltonians contain so much information that it is practically impossible to calculate the eigenvalues by hand, since matrix representations of quasi-particle Hamiltonians contain thousands of rows and columns.

This is where theory-driven numerical simulations enter the picture. As a segment of a research project that is currently underway, physicists[45] from UIC have recently developed a classical numerical simulation of quasi-particle interference models of heavy fermion supercon-

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[44]For the technically curious, here is a simplified but still intelligible illustration of how a quasi-particle model of a superconductor is constructed: Suppose for the sake of simplicity, all the free electrons in a superconductor are positioned on a straight line like beads on a string, whose rough distribution can be inferred by making macro measurements on the superconductor. Each free electron is pushed by all of its peers as well as the electrons that are bound to atoms, either to the right or to the left, with a force inversely proportional to the square of the distance between the electrons. Therefore, each electron has at least one equilibrium position on the line, where the net force acting on the electron would be zero because at that position all the coulomb forces would cancel each other out. Now, let's imagine we name each electron from left to right, say "$e_1, e_2, ..., e_n$," and represent the distribution of electrons by plotting the initial distance between the actual position of each electron and its nearest equilibrium position.

That, of course, wouldn't look anything like a wave and this is not just an aesthetic problem. Non-continuous functions are mathematically unwieldy. But, we can remedy that problem by connecting the point values with straight lines and applying a Gaussian to render it differentiable. The end product is a quasi-particle model.

Almost all useful quasi-particle interference models are much more complex than the toy example described above, which explains why we need to computers to use them to make successful predictions.

[45]Including colleague and personal friend John van Dyke at The University of Illinois at Chicago, who kindly shared his back-then unpublished results with me in spring 2013.

ductors to generate the Hamiltonians in question and compute the eigenvalues. The computational task involved is highly demanding (they book supercomputer time to produce results in a reasonable time-frame) but they are now able to successfully predict the geometrical properties of several heavy fermion superconductors. (Allan *et al* 2013, p. 471)[46]

The example shows that theory-driven simulations enable scientists overcome logistical barriers such as the computationally demanding task of inferring experimentally testable predictions from a quasi-particle model of a superconductor.

It also demonstrates one of the reasons why Winsberg's concerns about semi-autonomy and discretization are unfounded. After all, the numerical simulation at hand is merely an instrument that helps scientists overcome a previously insurmountable logistical barrier by performing a calculation that is implicated by theory. Scientists construct the simulation, and solve the discretization problem by tweaking the temporal and spatial resolution of the simulation until the results the simulation yields conform to the experimental findings. Any resemblance, overlap, dependence or lack thereof between the simulation and theory are therefore irrelevant, as long as the calculation the simulation performed is implied by the theory that is being tested. In other words, it is not epistemically relevant whether simulations are fictions or not. Winsberg is wrong; simulations can speak about the truth, or at least, empirical adequacy of theories, by not only extending the domain of theoretical modeling as Winsberg acknowledges, but also by

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[46]Some of the results are still unpublished as of March 2014.

enabling scientists to test those models in ways that would have been impossible without the use of simulations.

In this regard, computer use increases the attainability of experimental evidence through theory-driven simulations. Hence, the Fermi-Landau Liquid theory coupled with the KONDO Model and the Hubbard theory satisfies the computerization condition of the exempting defeater I proposed in Chapter 4. Since Fermi-Landau liquid theory is the dominant theory in the field of condensed matter physics, which is a stable and efficient field of science, it is exempted from the Pessimistic Induction in its entirety.

5.3.2 Case Study: Representational Simulations of TOKAMAK Reactors

There are many successful implementations of theory-driven simulations in several fields of empirical sciences, however most of them are clustered in physics and chemistry. So far the sub-field of condensed matter physics was the most successful in harnessing the power of theory-driven simulations to increase the attainability of experimental evidence. The same point applies to experiment-driven simulations. One example is the use of computer simulations to design experiments involving TOKAMAK reactors. TOKAMAKs are experimental fusion reactors that trap a fusible plasma by strong magnetic fields within a torus shaped reactor (Hence the name TOKAMAK, which is an anglicized acronym for "toroidal chamber with an axial magnetic field" in Russian.[47])

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[47] online at: http://bit.ly/1lxHiYI

This is a difficult task, which explains why there hasn't been a commercially viable TOKAMAK reactor, in part because it is very difficult to predict the behavior of plasma in the reactor. A sustainable reaction requires the plasma to remain dense and hot (this is called "high recycling plasma"), which demands effective real-time manipulation of the magnetic fields trapping the plasma, which will inevitably contain impurities and interact with the reactor walls. This is why computer models are vital in designing TOKAMAK experiments as well as the reactors themselves.

> It is a matter of course that the establishment of the high recycling plasma depends also on the design of the divertor. The distance between X-point to the divertor plate, plate orientation to the magnetic flux surface and divertor configurations (i.e. open or closed divertor) conspicuously affect the plasma behaviour in the divertor. In addition, both plasma-surface interactions and atomic processes do play important roles. It is, for these reasons, ardently required in the design of the divertor to evaluate the edge/divertor plasma behaviour under a realistic modelings. (Ueda & Tanaka 1990, pp. 106-7)

Examples like this show that classical experiment-driven numerical simulations enable scientists overcome logistical barriers such as the computationally demanding task of designing experimental fusion reactors. These reactors help scientists test otherwise-untestable predictions of theoretical models in plasma physics. In this regard, classical experiment-driven simulations as well as their theory-driven counterparts increase the attainability of experimental evidence.

This again indicates that Winsberg's blanket pessimism about the evidential role of simulationist research is unwarranted. Why Winsberg's pessimism is unwarranted here is however, different from why it is unwarranted in the case of the heavy-fermion superconductor simulation we studied in the previous subsection. In that theory-driven simulation, scientists obtained evidence for a bundle of theories by creating a numerical simulation whose findings can be verified by experiment. In the experiment-driven simulations involving TOKAMAKs that we have just looked at, the evidential benefit is not specific to a particular theory but to an entire field. However, still we have a theory-independent way of confirming that the simulations help; they help because they help scientist build better reactors.

Therefore, we can conclude that the epistemic status of present plasma physics community is superior in comparison to the epistemic status of past generations. This doesn't imply that a particular theory in the field satisfies the computerization condition. After all, it may be the case that the evidence obtained from the reactor designs produced by the simulation we looked at is recalcitrant to the dominant theories in plasma physics, or inconclusive. Still, there is room for cautious optimism that a closer look will reveal at least one theory that satisfies the computerization condition.

5.3.3 Case Study: Classical Representational Simulations and the NICE Model

It is not only the numerical simulations that can help scientists overcome logistical barriers. Representational simulations too make important evidential contributions to science.

A representational simulation is a computer simulation whose components are meant to correspond to parts of a natural system. A numerical simulation, such as the quasi-particle simulation we examined in Section 5.3.1, doesn't have to 'correspond' to the system under study in any ordinary sense of the word. It could be merely a tool for solving a mathematical equation that is key to inferring a prediction from a theory. A representational simulation experiment on the other hand, is made with a referential intent. In this regard, the difference between a numerical and a representational simulation is born out of whether the simulationist scientist had an instrumental or referential intent.

Although representational simulations are sometimes called “synthetic experiments,” this use is misleading. The scope of representational simulations is much wider than the traditional experimentation. Representational simulations could be used to predict the behavior of relatively small systems that we can experiment with such as a medium sized solid object or very large scale systems that are logistically or physically impossible to experiment with such as astronomical objects.

In 2005, a team of scientists published a series of studies in *Nature* that finally solved one of the long standing mysteries in the evolution of Solar System and confirmed the predictions of a theoretical model describing the orbits of giant planets, which is called the “NICE model.”[48](Staab 2007) The studies provide very illuminating examples of successful large scale representational simulations.

The mystery these representational simulations solved is called the Late Heavy Bombardment (LHB). The LHB was a brief and sudden burst of a large number of asteroid and comet impacts on rocky planets and satellites in the inner Solar System. Until the LHB, which happened around 700 million years after the formation of the Solar System, the frequency of asteroid impacts followed a non-increasing pattern. However, as evidenced by impact-melt rock samples recovered from the moon (see Figure 8, where there is a sharp increase in impact-melt

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[48]The model was named after the French city Nice, where it was first proposed.

rocks around the time of LHB)[49], this changed very suddenly with the LHB as large numbers of asteroids hurled down to the inner Solar System.

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Figure 8. Cumulative lunar impact mass over time demonstrating LHB (Gomes *et al* 2005, p. 468, original figure number is 3)

For a long time the cause of the LHB was a mystery because we didn't know what could have triggered such a sudden and large scale event. Then the NICE model came along and proposed two hypotheses that could jointly explain LHB: In the early Solar System, the orbits of giant planets in the outer Solar System were much tighter and the disk of asteroids and comets that

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[49] The samples were recovered during the Apollo missions and dated using isometric dating. (Tera *et al.* 1974, Cohen *et al.* 2000) We are fortunate to have a natural satellite in this regard, because Earth has continental subduction which erased all terrestrial evidence from those early impacts.

form today's Kuiper Belt were much closer to the Sun. However, the interactions between the disk on one hand and Jupiter and Saturn on the other gradually changed the eccentricity and period of their orbits. As the mean motion resonances of Jupiter and Saturn reached a factor of 1:2 around the 700 million year mark, the resulting orbital resonance forces slung out the ice giants Neptune and Uranus (Staab 2007). Then the gravitational wells of one or more of these ice giants plowed into the disk and scattered millions of asteroids and comets in every-which way. This resulted in the LHB and the reformation of a much less compact Kuiper Belt at a much farther orbit.

Of course, it is one thing to have a theoretical model that could explain a natural phenomenon if it were true, and it is another thing to have evidence for the truth of the model in question. Until recently, scientists didn't have evidence that can back up their hypothesis that a tighter orbital configuration of the giants would eventually sling out Neptune and Uranus to scatter the disk at the right time.

The difficulty in confirming the NICE model was entirely logistical and it was simply beyond what is possible without computers. Although one could manually do the calculations for the eventual slingshot effect on Neptune and Uranus by the orbital resonance forces from Jupiter and Saturn, until recently the calculations required for confirming the claim that the shift in the mean motion resonances required for the 1:2 ratio could have resulted from the interactions between millions of comets and asteroids forming the disk and the gas giants were impossible to do with meaningful precision.

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Figure 9. Snapshots from the representational simulations that show how the expanding orbits of Jupiter and Saturn could have scattered the earlier Kuiper belt and triggered LHB. Snapshot (a) indicates the initial configuration of the Solar System. Snapshot (b) shows the orbital resonance crossing of Jupiter and Saturn. Snapshot (c) shows how the resonance crossing destabilized the orbits of Neptune and Uranus. Snapshot (d) shows the eventual state of the original belt objects, scattered in every direction. This is evidence for the NICE model, as they demonstrate that the NICE model retrodicts the LHB. (Gomes *et al* 2005, p. 467, original figure number is 2)

This changed when evidence came from a series of 16 representational simulations that overcame the logistical barrier. 8 of these simulations were done to understand the relationship between the timing of the crucial 1:2 mean motion resonances crossing and the location of the disk. They confirmed the theoretical expectation built into the NICE model that there would be "a strong correlation between the location of the inner edge [of the disk] and the time of the 1:2 [mean motion resonances] crossing." (Gomes 2005, p. 467)

Then another 8 representational simulations confirmed that some of the orbital configurations of the gas and ice giants that are consistent with the NICE model produced the ∼700 million years delayed 1:2 mean motion resonances crossing that is sufficient for the LHB. In other words, since we know the timing of the LHB from the isometric dating of moon rocks, these 16 simulations not only gave us meaningful constraints on the possible configurations of the original disk, but also confirmed the theoretical intuition that the NICE model could explain the LHB.

The logic of the evidence is clearly hairier than what a naive empiricist understanding of science tells us what evidence should look like. After all, there is an interplay between the NICE model and the representational simulations, where the theoretical model provides an initial rough framework for the simulations to be produced. Then the simulations fill out the gaps in the theoretical model and show that when those gaps are filled in various ways, the theory can successfully retrodict an event we know to have occurred.

The situation is analogous to what Galison discovered about experimentalist science. A picture where experiment is completely autonomous from theory is as naive as a picture where there is no autonomy whatsoever. Similarly, there is an interplay between successful simulationist practice and theory it engages. Because of the historical nature of the NICE model, there are inevitable gaps in our knowledge pertaining to the facts such as the exact locations and mass distribution of the Solar System bodies some 700 million years ago. But a demonstration that these bodies could be configured in such a way to result in the 1:2 mean motion resonances crossing and the LHB should increase our confidence in (*i.e.* our subjective probability assignment to) the NICE model.

In this regard, there is no reason to think that the gaps in our theoretical models necessarily constitute a problem for the evidential significance of representational simulations. Winsberg, who is otherwise insightful, overlooks this crucial interplay between theory and simulation, and dismisses the possibility of simulated evidence because it fails to conform to a rather naive picture of confirmation where theory logically entails unambiguous observational statements, which are to be unambiguously confirmed by observation and evidence. In reality, simulationist and theoretical practices are semi-autonomous from one another; they complement and inform each other in some cases whereas they offer evidence for or against the results of one another in some others.

Another interesting corollary of our study of the representational simulations confirming the NICE model has to to with robustness, which is understood as the agreement between different simulated models. (Parker 2011, p. 580) When you simulate a system (such as the early Solar System) where there are ineliminable unknowns such as the precise distribution of objects, in principle you can generate a multitude of simulations that fill out those unknowns in different ways. If you fail to get the same results from all or even most of those simulations, that means the results aren't robust with regards to the ensemble, as they aren't corroborated by the ensemble of simulations in question.

This is certainly the case with the 16 simulations we looked at in this section. Although the 16 simulations agree among themselves, they wouldn't agree with every possible alternative simulation outside the ensemble. There are ways in which the early Solar System bodies could be configured which wouldn't result in a 1:2 mean motion resonances crossing at the right time—if ever—and therefore wouldn't trigger the LHB as we know it.

One might think that the absence of robustness in this particular sense discredits the claim that the representational simulations we studied in this section can provide any evidence for the NICE model. However, this is would be a mistake; robustness is not a necessary condition for evidential significance.

The motivation for making this mistake, I suspect, comes from a temptation to think of robustness as the simulationist's analogue of reproducibility in experimental science. Reproducibility is rightfully considered the gold standard in experimental science. When you repeat an experiment and fail to get the same results, this should make you worry that the original result was a fluke, or that there is something wrong with the experimental control. Similarly one might think that robustness is a necessary condition for reliability of simulated results; when you 'repeat' the simulation and fail to get the same results, shouldn't you conclude that the original results were a fluke?

You shouldn't because the analogy between simulational robustness and experimental reproducibility is a bad analogy. The analogy breaks down at a crucial point: Experimental reproducibility presupposes *repeating* the experiment in a setting that is by and large the same as the original experiment. The only difference between attempted repetitions is supposed to be replacing the sample with new samples to rule out sampling error. But even that 'difference' is superficial; all samples are supposed to be the same in the sense that they are all intended to be representative (and typically randomly selected)[50] subsets of the population. Simulations with different parameters don't fit this model of repetition; the differences between them are substantial.

Naturally, sometimes we are justified in taking the disagreeing results from substantially different simulations of the same phenomenon as evidence against the results in question. However, there are exceptions such as the retrodictive simulations we have looked at in this section. When a set of simulations are coupled with a background theory that informs us that an event has taken place at a specific time (*e.g.* the LHB happening about 700 million years ago) and a theoretical model that is consistent with the known facts and parsimonious (*e.g.* the NICE model), we are warranted to ignore the disagreement between the 16 simulations that can predict the LHB and the rest that can't. The decision to trust the 16 simulations in question is not based on an arbitrary choice about how to fill out the gaps in the theoretical framework that is being tested by those very simulations. The decision is based on the recognition of the

[50]see Section 3.2.1 for a discussion of representative non-random sampling.

fact that these 16 simulations *do not complete* the NICE model. What we have evidence for is not the un-gapped version of the NICE model. What the simulations do is to show that the theoretical intuition behind the NICE Model is correct. When the gaps are filled out by making historically and physically plausible assumptions, the model retrodicts LHB.

In this regard, these 16 simulations provide reason to up the previous subjective probability assignment we had for the NICE model. This is not necessarily confirmation, but it is indeed evidence. Therefore, a strong and unqualified robustness is not a necessary condition of evidential significance of a representational simulation.

The situation however is different when we look at predictive simulations, such as long-term climate models. There, the gaps are wider and due to the absence of a marker like LHB, which we know to have happened, decisions on how to fill out the gaps in our theoretical models are arbitrary. Since we don't know how the distant future is going to turn out, we don't have a way of sifting through the pool of non-robust simulations, as we can in the case of the 16 simulations we analyzed in this section. In such cases, pessimism about the predictive success of representational simulations appear warranted, and demanding robustness as a necessary condition for evidential significance is prudent.[51]

In brief, as the representational simulations demonstrate, which were possible thanks to the vast computational resources today's computers can offer, scientists use representational simu-

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[51]The corollary I have defended here (*i.e.* "robustness is not a necessary condition for evidential significance") complements Wendy Parker's observation that robustness is not a sufficient condition for evidential significance either. (Parker 2011, p. 589) Therefore, contrary to the mistaken view of seeing robustness as the simulationist analogue of experimental reproducibility, we should instead acknowledge that robustness is neither a necessary nor a sufficient condition for evidential significance.

lations to obtain evidence for an astronomical model that would have been otherwise impossible to obtain. Once again, this supports my claim that computer use in science enables scientists overcome logistical barriers. Equally importantly, the NICE model satisfies the computerization condition. Assuming that the NICE model is situated in stable and efficient field, it is completely exempt from the Pessimistic Induction.

In the next section, I will argue that at least one particular type of computer use (i.e. Genetic Simulations) promise us even more. Genetic simulations are autonomous from theory to even a greater extent than the sort of autonomy we encountered in our study of a classical simulation in Section 5.3.1. Since genetic simulation is also methodologically distinct from other sources of evidence, it is a novel and substantial source of evidence, evidence *ex silico*.

5.4 Evidence *ex Silico*

Genetic programming allows the programmer to tap the power of artificial selection to make a computer autonomously generate a numerical simulation. In science, genetic simulations are employed to generate numerical simulations that can achieve what cannot be done by classical theory and experiment-driven simulations.

The process of creating a genetic simulation starts with a small number of randomly or manually created initial programs called “seeds”. These seeds then are subjected to an iterative algorithm. During each iteration cycle the individuals in the current population of programs are evaluated based on their success at correctly predicting the values contained in the training set, which contains experimentally determined data. At the end of each iteration cycle each individual program has a chance of producing a mutant off-spring, and where more successful programs are more likely to reproduce. (Tomassini 1995; Muntean 2011) In cases where the generation is successful[52] scientists obtain at least one numerical simulation that can be used to make reliable predictions and retrodictions outside the training set.

The epistemic ramifications of genetic simulation use in science are immense, because such simulations are epistemically autonomous from theory in a substantial way. The way a genetic simulation successfully predicts or retrodicts phenomena is by and large independent of the theory whose implications are being tested with the help of the simulation. After all, the simulation is not purposefully designed by a scientist to correspond to or mirror a theoretical model, unlike some representational simulations and some numerical simulations.

An impressive example of genetic simulation use in physics, which illustrates why it is a new source of evidence, is happening at the CERN lab at this very moment. One of the seven large scale experiments that are currently underway at CERN is called the Large Hadron Collider Beauty (LHCb). In LHCb, scientists are investigating charge parity violations in b-hadrons, particularly in B decay events following non-proton-proton collisions. An average run of LHCb produces terabytes of data.

The filtering in question is done in two steps called "triggers." The first trigger (called L0) filters out all proton-proton interactions. L0 "makes use of the fact that particles from a B decay have a higher transverse momentum with respect to the particle beam axis (pT) than

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[52] Since genetic algorithms are heuristic as opposed to classical, they are not guaranteed to produce results. But they often do. (Tomassini 1995)

particles coming directly from the primary proton-proton interaction"[53] which comprise the overwhelming majority of the collisions that take place in the collider. The second level (called HLT) employs two tools to sift through the remaining candidates: an array of sub-detectors whose readings are compared to the array closest to the collisions and a genetic simulation that has been trained to determine if given bits of information are coming from a B decay event. HLT throws away every collision on which both the sub-detectors and the genetic simulation don't agree.

Producing the genetic simulation is computationally non-trivial, though updates aside, it is a one-time event. However, using the genetic simulation to process the data gathered during the experiment requires immense computational resources. Even "after [initial] filtering [...] a very large amount of data still remains. 35 gigabytes - equivalent to 8 DVDs worth of information - is fed every second into 2,000 state-of-the-art computers, located deep underground at the LHC site. These machines select interesting events to save for analysis, further trimming the 1 million events per second to a more manageable 2,000."[54]

This feat would not have been possible without computers. However, that's not the crucial point. The crucial point about HLT is that it juxtaposes experimental evidence that is produced by the sub-detector array with the evidence that the genetic simulation yields. They have to agree, or the event gets thrown out. This egalitarian treatment of the verdicts from the genetic

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[53]online at: http://bit.ly/1oeLXUp

[54]online at: http://bit.ly/1oeLXUp

simulation and the experimental result is an indication that they have comparable evidential significance and they are methodologically distinct from one another.

The case of genetic numerical simulations make the potential epistemic significance of computer use in science crystal clear. Since they make reliable predictions and retrodictions, they allow us to test the predictions of our theories without resorting to analytic, observational or experimental evidence.

That's why it is warranted to treat the genetic simulations as a novel and substantial source of evidence and demarcate their results as evidence *ex silico*. Since evidence *ex silico* can and does help confirm or falsify our best scientific theories (as in the case of the Standard Model), we should conclude that the researchers in this field not only possess a tool for increasing the attainability and reliability of evidence they obtained from traditional sources, but they also have access to a different source of evidence. In other words, **(iii)** is not true for theories for which there is evidence *ex silico* such as the Standard Model. The epistemic status of such current scientific theories is different from—and superior to—the epistemic status of any theories of the past. Therefore, we are warranted to believe that the Standard Model, like the Fermi-Landau Liquid theory and the NICE Model, is here to stay.

However, it is important to remember that even non-genetic simulations and other uses of computers do provide evidence by increasing the attainability and reliability of the evidence from the traditional sources, which is sufficient to overturn **(iii)** as far as some theories are concerned.

5.5 Conclusion

In Chapter 1, I made a conjecture. Premise **(iii)** of the epistemic formulation of the Pessimistic Induction is not universally true. There are theories in contemporary science whose epistemic status is superior to the epistemic status of their predecessors.

In Chapter 4, I argued that showing that a theory in contemporary science simultaneously satisfies four conditions is sufficient to establish the conjecture. One of these four conditions was the computerization condition: Computerization of the field hosting the theory has improved the quality of the available evidence or diversified the sources of evidential support the theory enjoys, making it unique among all the theories ever dominated that field.

With the goal of establishing the conjecture in mind, I dedicated the present chapter to establishing the claim that some theories of contemporary science satisfy the computerization condition via three routes.

First, computer use increases the attainability and quality of evidence by helping data analysis and modeling on the one hand and instrumental control on the other. Neither of these has to involve the use of simulation, yet they are every bit important in showing that computerization can help exempt theories from the Pessimistic Induction. After all, certain experiments and observations are simply impossible without computerized data analysis or instrumental control.

Second, classical simulations increase the attainability and quality of evidence by allowing scientists test their theories in unprecedented ways. As we have seen in Sections 5.3.1 and 5.3.3, simulations can help theory testing directly with or without representing.

Finally, certain recent developments in high energy physics suggest that a certain sub-type of computer simulations present a novel and substantial source of evidence: evidence *ex silico*. As we discussed in Section 5.4, the operationalization of LHCb experiments suggest that scientists treat genetic simulations as if they are methodologically on par with observational and experimental evidence. Since by their nature genetic simulations are also completely autonomous from theory, their use indicates that LHCb taps a novel and substantial source of evidence that was not available to the previous generations of scientists.

In this regard, Winsberg's pessimism regarding the evidential potential of simulationist science is unwarranted. However, the chief claim of this chapter is stronger than that. Computer use in science, with or without simulations, with or without *ex silico*, provides grounds for optimism regarding certain theories. We have at least three current best theories that satisfy the computerization condition, probably many more: The Fermi-Landau Liquid theory, the NICE model and the Standard Model. All three are dominant theories in stable fields that also seem to satisfy the efficiency condition. Provided that there are no other grounds for pessimism about these theories, optimism regarding all three of them is warranted. They are exempt from the Pessimistic Induction. Within the scope of the fields they are situated, they are probably the best theories that evidence will ever warrant. We have *prima facie* justification to believe that they will not share the sad fate of their predecessors; they are here to stay.

In the next and last chapter, I will turn my attention to the efficiency condition and identify those fields in which no theory is likely to unequivocally satisfy it. This will help us determine the limits of optimism that can be warranted by the exemptionist strategy in the age of simulationist science.

6. LIES, DAMNED LIES AND STATISTICS: LIMITS OF OPTIMISM

In Chapter 1, I conjectured that premise (iii) of the epistemic formulation of the Pessimistic Induction is not true of every current best theory. In Chapter 4, I argued that when four conditions are satisfied by a theory, that theory is completely exempt from pessimism. The key among these four conditions were the computerization condition and the efficiency condition. In Chapter 5, I have documented in reasonable detail that some current best theories in physics satisfy the computerization condition. In the present chapter, I will turn my focus on the efficiency condition, which is unequivocally satisfied by a scientific theory only when the field hosting the theory has been efficient and not wasteful in utilizing increased research capacity resulting from the exponential growth and computerization of science. I am particularly interested in when and why the efficiency condition fails to obtain unequivocally for some theories in contemporary science. Understanding these conditions will help us see the limitations of the exemptionist strategy and see the limits of optimism that is warranted by the recent computerization of science.

As Fahrbach (2011) argues successfully, there is an undeniable global trend of exponential growth in the demographics of science. However, as I have pointed out in Chapter 4, he is wrong to assume that the exponential growth of the number of active scientists will surely lead to an exponential growth in the quantity of evidence. Contrary to Fahrbach's assumption, increased research capacity can go to waste through inefficiency of research.

There are several ways in which research capacity may go to waste. The first two among them are potential hazards that threaten virtually any field of empirical science: copious repetition and vicious theory-ladenness. Fortunately, these two, which I shall call "easy problems," can be—and often are—effectively nullified in most cases. However, there are also 'hard problems' that are more tightly connected to specific fields, and they cannot be countered as effectively: popular but unhealthy testing practices, low experimental reproducibility, pervasive research misconduct and retraction.

In the present chapter, I will look at these easy and hard problems, with the purpose of determining which areas of science, if any, suffer from them significantly. This is important because knowing the extent to which a field suffers from these problems will also help us know to what extent the field managed to utilize the increased research capacity and yielded evidence in favor of the dominant theories it hosts. The knowledge in question is important because it is the key to understanding the limits of optimism that can be warranted by the exempting defeater I proposed in Chapter 4.

In this chapter, I will argue for a thesis that can be stated roughly as follows: due to the pervasiveness of hard problems in most fields of behavioral and health sciences, very few—if any—theories in these fields can be expected to satisfy the efficiency condition. However, there is room for optimism as far as physical sciences are concerned, where hard problems are often not present or effectively nullified.

In various places throughout this chapter, I will do some broad brushing, especially when I am making generalized comparisons between physical sciences on the one hand, and behavioral and health sciences on the other. In reality however, there are sub-fields of physical sciences which seriously suffer from the hard problems, such as the sub-field of organic semi-conductors which we will look at in Section 6.2.2. There might also certainly sub-fields of behavioral and health sciences where the hard problems aren't as pressing as they are in other sub-fields, and upon close inspection we might discover that some theories in these sub-fields might satisfy the efficiency condition.

Still, there is some methodological sense to my broad brushing. What appears to be a crude broad brushing helps highlight a fact about the nature and causes of the failures of efficiency. The fact that sub-fields plagued by the hard problems such as organic semi-conductors are in the minority among the physical sciences whereas they are in the majority among behavioral and health sciences suggests that there is something about the subject matter of these fields that make it harder for them to be efficient.

In other words, what looks like an offensive blanket generalization targeting whole disciplines of science is actually a sympathetic acknowledgment of difficulty of doing science in these areas. In this regard, that no or almost no theory in a field of science unequivocally statisfy the efficiency condition is not a reflection on the character, intelligence, creativity or even the success of the scientists in that field. Failures of efficiency are, more often than not, triggered by the hard problems that are directly or indirectly connected to the nature of the phenomena that is studied by particular fields. The phenomena physical sciences typically engage are easier to isolate, observe and intervene into within a controlled environment than the phenomena studied by behavioral and health sciences. Physical phenomena are structurally less complex than psychological, social and biological phenomena. That's why physical sciences typically have an easier time remedying the hard problems whereas behavioral and health sciences typically tend to get bogged down by them. Everything I say in this chapter for and against large collection of sub-fields must be read with an awareness of this point.

6.1 Easy Problems: Repetition and Vicious Theory-Ladenness

Research capacity may end up being invested into copious repetition[55] of past research. For instance, if during the next solar eclipse we repeat Eddington's 1919 experiment and find the trajectories of starlight to be altered by the gravitational field of the Sun in the same way Eddington observed, that would not increase the quantity of evidence we have for the theory of relativity.

Second, vicious instances of theory-ladenness of evidence can lead to ineffective utilization of increased research capacity. Most theoretical arguments, observations, experiments and even some simulations involve a degree of theory-ladenness. For instance, the use of measurement instruments inevitably import some theory. In some cases, the resulting theory-ladenness is vicious, which renders the outcome of the measurement evidentially worthless.

We have already discussed an example of vicious theory-ladenness in Chapter 4: testing a hypothesis about how much a coil suspended between the poles of a magnet would deflect for a given current by using an ammeter. The example involved vicious theory-ladenness because an

[55] When done in moderation, repetition of past research can sometimes improve the quality of evidence by demonstrating repeatability. However, as far as the research supporting accepted and well-established theories is concerned, by assumption the point of diminishing returns from repetitions must have already been reached.

ammeter is also a coil suspended between the poles of a magnet. In other words, the hypothesis that is being tested is also the hypothesis whose truth warrants the use of the instrument.

Fortunately, not all theory-ladenness is vicious like Chalmer's example. When taken in isolation optical microscopy might presuppose an optical theory about how the microscope works.[56] Yet, this does not taint the evidence we acquire through the optical microscope in part because we are gathering evidence not regarding a theory of optics, but presumably regarding a theory of microorganisms. Another reason why the theory-ladenness of optical microscopy is epistemically unproblematic is that there are other instruments (*i.e.* the electron microscope and the tunneling microscope) which do not work by the principles that govern the operations of the optical microscope. When the evidence obtained through theoretically co-independent measurements converge, theory-dependence is not vicious (Chalmers 2003, Hacking 1983). In this regard, we might safely assume that theoretical arguments, observations, experiments and simulations improve the quantitative component of the epistemic status of a field when they do not involve vicious theory-dependence.

In virtually every field of science scientists are very successful in preventing the potential waste of research capacity that is presented by copious repetition and vicious theory-dependence. For instance, the reward mechanism in science strongly discourages copious repe-

[56]Even this is not quite obvious. Under a reliabilist conception of measurement, it is not clear why one would need any theoretical commitment to use instruments to gather evidence. For a long period of history, people used their eyes to gather information about the nature despite the fact that they had no understanding of the optical and neurological underpinnings of vision. In this regard, it might be enough that the instrument is reliable to provide evidence; we might not need a theory to explain and verify its reliability.

tition. In those fields where reproducibility of results are typically high, this leads to efficiency without undermining the reliability of findings. Moreover, scientific training in virtually every field of science puts a great deal of emphasis on identifying and remedying instances of vicious theory dependence.

In this regard, the easy problems are indeed easy. Although it is conceptually possible for them to undermine the efficiency of science, in practice there are mechanisms in place to protect science against them.

However, it's hard to speak with the same confidence about the hard problems, which are not always remedied effectively. When an individual field is plagued by hard problems to a serious extent, it is impossible for any theory in that field to unequivocally satisfy the efficiency condition. In the next section, we will look at the manifestation of such hard problems and explore the limits of optimism.

6.2 Hard Problems and The Limits of Optimism

In Chapter 1, I claimed that not all theories are created equal. The same thing can be said about the individual fields hosting those theories. Some fields of science are more efficient in actualizing their research capacity than others. Some others are bogged down by certain problems to the extent where it is impossible to argue that any theory within such fields can unequivocally satisfy the efficiency condition.

In most cases, a theory's failure to unequivocally satisfy the efficiency condition can be traced back to the social, methodological and metaphysical situation of the field hosting that theory. The failure in question is often a causally more complex bussiness than the failures of the other three conditions. In particular, the efficiency condition is not unequivocally satisfied by theories which are situated in fields where there are pervasive methodological problems, low experimental reproducibility, and high incidence of retraction and research fraud.

This unsightly complexity may give the impression that the practitioners in those fields are failing as researchers especially if the theory in question is a dominant theory in a stable field. However, the fact that the efficiency condition does not unequivocally hold for any theory in a field does not at all suggest that there is no respectable science being done in that field. Actually, in almost all the fields whose theories fail to unequivocally satisfy the efficiency condition, there are many hard-working and clear-headed scientists who are doing top-notch research. The problem is not that there is no good research going on. In some fields, admittedly there are serious methodological issues that bother even the researchers working in that field. However, in other cases the problem is not even caused by factors that are within the control of scientists. Sometimes research findings are unreliable for reasons that have to the with the nature of the phenomena scientists investigate rather than inherent problems with their methodological practices. These qualifications must be kept in mind when reading the following remarks.

When these 'hard problems' in a field are wide-spread and far-reaching enough, they will inevitably taint the evidential quality of the majority of findings in that field. In other words, if a substantial amount of research in a field is based faulty or counterproductive observational, experimental or simulationist practice, it does not matter if there are islands of reliable evidence in that field. The efficiency condition will fail to hold unequivocally for all or almost all theories in that field. When that is the case even dominant theories in the field will not be protected from the Pessimistic Induction by the exemptionist strategy.

6.2.1 The NHST and Related Methodological Problems

There are various methodological problems plaguing the fields of economics, psychology, social sciences and neuropsychology that are so pervasive that their presence makes it unlikely for any theory in these fields to unequivocally satisfy the efficiency condition. The first and foremost among these problems is the unhealthy reliance on and pervasive misapplication of the statistical method called the "Null Hypothesis Significance Testing" (NHST). The shortcomings, potential pit-falls and actual harmful impact of the practices associated with NHST have been independently documented and illustrated many times by the researchers in the aforementioned fields as well as philosophers of science, epistemologists and statisticians (Rozeboom 1960; Bakan 1966; Meehl 1967, 1978, 1990; Cohen 1962, 1994; Lyken 1991; Gigerenzer 1993; Ziliak & McCloskey 1996, 2004, 2008; Bennett et al 2009) but the problem seems to persist.

In addition to the methodological problems that directly result form the over-reliance on and mis-application of NHST, the fields in question also suffer from issues concerning the reliability of the commonly employed experimental designs and the peer-review process that indirectly aggravate the problems related to the NHST. These issues include publication bias for positive results, false-positive-prone research methods such as discovery oriented research and commercial-interest-driven testing by independent teams. (Ioannidis 2005; Smith 2005) Moreover, in some fields like psychology and fMRI-based neuropsychology reproducibility of research findings is typically low and there is a growing and well-justified concern that the majority of the published research findings in these fields are false positives. (Carpenter 2012) Finally, recent studies (Steen 2011a, 2011b; Budd 1998, 1999) show that both publication retraction and research fraud are on a steep rise in health sciences.

NHST can be construed as a function whose input consists of a set of objects with two variables (a sample), the empirically measured values of those variables (data) and the experimental hypothesis (H_1) that there is a correlation between those variables in the population from which the sample was drawn. Formally construed, the output of the function is the probability (the p value) of the data provided that the null hypothesis (H_0) is true. Crudely put, H_0 is the negation of H_1.

The p value is computed by assuming that the sample is taken from a—typically much larger—population where the values of the variables are normally distributed and there is no correlation between them (hence, H_0 is true) and calculating the probability that the researchers randomly selected a sample which gives the false impression that there is a real correlation. The p value then is compared with the significance intervals conventionally set at $(< 0.10), (< 0.05), (< 0.01)$ or (< 0.001). If the p value is within the conventionally determined significance interval, the effect hypothesized by the experimental hypothesis of the study (*i.e.* H_1) is declared to be statistically significant relative to that interval.

Here is an illustration of how this is done. Suppose researchers are interested in studying the relationship between two variables such as traveling and IQ scores. They come up with an experimental hypothesis H_1, which is the proposition that people who have visited Chicago do not have average IQ. In this scenario, the null hypothesis H_0 ends up being the negation of H_1; the people who visited Chicago are average.

The researchers then find participants drawn from the population—ideally—at random. The group of participants (*i.e.* the sample) consists of 9 people who have been to Chicago. Then the participants perform a task that measures their IQ. Suppose the test yields the following IQ measurements for each participant: 120, 110, 124, 142, 104, 97, 88, 123 and 127, where the sample mean is 115.

Ex hypothesi, IQ scores of an entire population is normally distributed with a mean of 100. The questions one might use the NHST to answer is the following: How likely is it that the above average mean observed in the sample is a fluke? What is the probability of getting a 115 mean in a sample of this size assuming that the mean IQ score of all those who visited Chicago is actually 100?

In order to answer this question, we need to compute the distance between the population mean of 100 (*i.e.* μ_0) and the sample mean 115 (*i.e.* $\bar{x}$) as a measure of the standard error of the mean, SEM (*i.e.* $\sigma/\sqrt{n}$):

$$z_{stat} = \frac{\bar{x} - \mu_0}{\sigma/\sqrt{n}}$$

In the case of the IQ test in question, let's stipulate that SEM is 5, a typical value for the Wechsler test for instance. Then we get,

$$z_{stat} = \frac{115 - 100}{5} = 3$$

To convert the z_{stat} into the p value, which is the answer to the question "What is the probability of getting a 115 mean in a sample of this size assuming that the mean IQ score of all those who visited Chicago is actually 100?" we need to look at a normal distribution graph (see Figure 10) and calculate the area under the curve that falls under the right hand side of 3 standard deviations from the mean. In this case, this area is (0.0013). That is our p value; the probability of drawing 9 individuals whose average IQ score is 115 from a population whose mean is 100 is (0.0013). In the technical language of NHST, this p value indicates "statistical significance" for ($p < (0.05)$), where (0.05) is the commonly accepted threshold for significance.

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Figure 10. Standard IQ distribution, which is a bell curve where 68% of all population falls within 85 and 115. Original figure online at: http://bit.ly/bi79fj

NHST that I have just illustrated and its variants, are commonly used as the sole data analysis method in psychology, social sciences, (Cohen 1994, Lyken 1991) economy (Ziliak & McCloskey 1996, 2004, 2008) and neuro-psychology (Klein 2010).

However, as I stated earlier, there are serious methodological problems associated with NHST. The problems related to the use of NHST have been documented and illustrated many times by the researchers in the aforementioned fields (Rozeboom 1960, Meehl 1967, 1978, 1990, Cohen 1962, 1994, Ziliak & McCloskey 1996, 2004, 2008, Bennett *et al* 2009) but the problem seems to persist.

There are four serious problems associated with applications of NHST. The first problem is that the practitioners in those fields sometimes confuse the p value with the probability that H_1 is true given the data. For instance, someone who commits this mistake would conclude that the probability that Chicagoans aren't average is (0.0013), which isn't really what the NHST reveals. Misinterpreting the p value in this way is clearly an elementary mistake but it is made surprisingly often, especially by the audience of scientific studies. Even experts admit that they are not immune to it (Cohen 1994).

More frequently—and with more far-reaching implications,—the p value is the only thing that is reported in a study. This reveals a crucial shortcoming of the method because the p value itself has very little empirical meaning. After all, without assigning an accurate and precise prior probability to H_1 representing a genuine/non-accidental correlation we cannot infer the probability of H_1 is true given the data from the p value. This technical shortcoming translates in practice into a vast ocean of reported correlations whose scientific significance cannot be empirically judged. (McCloskey & Ziliak 2008)

A third problem with the method originates from the simple fact that for any two genuinely independent variables in a naturally occurring population, it is immensely probable that H_0 is false of that population. To see why this is the case, again consider the illustration above. If you measured the individual IQ scores of everyone in the US, there will almost certainly be a tiny difference between the those who have been to Chicago and those who haven't, one way or another. Even if it were possible to use NHST to rule out H_0 and to strengthen our confidence in H_1, doing so is often not an accomplishment that will contribute to the advancement of science.

This last point is very revealing about a crucial difference between the typical fields within physical sciences on the one hand, and behavioral and health sciences on the other. Generally speaking, the researchers in fields like physics, chemistry and some areas in biology are seldom satisfied by estimating the probabilities that two variables are correlated. On the contrary, their chief objective is almost always to measure, predict and model the exact nature of the correlation in question. For instance, imagine that after much experimental effort a physicist declares about an elastic coil "When you pull on it, it gets longer." (Tukey 1969, p. 80) In such a surreal scenario, the colleagues of this naive physicist would shrug and demand the equation that would tell them with how much force you need to pull the coil in order to make it $x\%$ longer than its original length. However, in those fields where NHST is the only or the major data analysis method, the empirical inquiry stops at the p value.

The fourth problem originates from the efforts to remedy the third one, which again highlights a telling disanalogy between the physical sciences versus other fields. In order to avoid the blatantly pointless exercise of the null hypothesis rejection, most researchers construe H_0 not simply as the claim that there is no correlation between the variables. Instead, they construe it directionally: as the claim that there is no positive—or alternatively, no negative—correlation between the variables. For instance, instead of taking the null hypothesis to be "those who have visited Chicago have the same IQ as those who didn't", they take it to be "those who have visited Chicago have lower IQ than those who didn't."

This looks like a good idea, for unlike the 'point' (*i.e.* non-directional) null hypotheses, directional null hypotheses are not almost always false *a priori*. But the looks are misleading, and the proposed remedy has grave implications about the fields which heavily rely on NHST.

To demonstrate these implications, Paul Meehl (1967, p. 109) invites us to consider the following scenario: Suppose we have a population where there are n variables that can be studied, which implies that there would be $n!/[(n-2)! \times 2!]$ possible pairings of these variables. We randomly generate a set of hypotheses consisting of one hypothesis for each possible pairing. For each hypothesis in this set, there is exactly one corresponding directional null hypothesis. Now, since the set is randomly generated the prior probability of an arbitrarily selected such directional null-hypothesis to be false is (0.5). Suppose, we design experiments to test these hypotheses by NHST. Also suppose, we are so good at designing experiments, the probability of Type I and Type II errors (failing to reject the directional null hypothesis when it is false, and incorrectly rejecting the directional null hypothesis when it is true) are both close to (0).

This would entail that even with our randomly generated (hence, presumably inferior quality) hypotheses, our findings will end up being true about 50% of the time. (Meehl 1967, p. 110)

This is bad news, which becomes clear again when we compare it to the situation in physical sciences. For instance, in physics, theories do not make claims about bare correlations. They rather make specific predictions, such as exactly how much a metal coil will elongate when you pull it with n Newtons of force. As a consequence, the better the experimental set up gets, the more accurately you can measure how well the prediction meshes with the real phenomenon. As Meehl puts it using Popperian terminology,

> [I]n physics the effect of improving precision or power is that of decreasing the prior probability of a successful experimental outcome if the theory lacks verisimilitude, that is, precisely the reverse of the situation obtaining in the social sciences. (1967, p. 113)

In other words, the better the experimental design becomes, the more accurately a physical theory must represent the world in order to pass the test of the experiment. Otherwise, its predictions will be proven wrong by measurements with increased accuracy and precision.

Compare this with the hypothetical scenario where all you do is to conduct NHST on randomly selected directional null-hypotheses. We know it *a priori* that it is higly unlikely for Chicagoans to have exactly 100 mean IQ. Therefore, either the Chicagoan mean IQ is slightly higher than the population, or slightly lower. The better designed and more effective your experiment is, the easier it will be to detect the minutest differences between Chicagoan mean and the population mean, which is 100. Suppose you guessed randomly that the Chicagoan mean is higher. As precision and accuracy of your measurement increases, the chances of the effect predicted by your randomly selected experimental hypothesis being statistically significant will converge to (0.5). If you do science like this you will be right half the time no matter what just like a broken clock that is correct twice a day.

In other words, in disciplines where there is a heavy reliance on NHST, the better experimental design becomes, the easier it gets for a randomly generated (hence, presumably inferior quality) hypothesis to pass an empirical test.

The lesson to take home is brief and grim: Whichever way one looks at this difference between the physical sciences and the hypothetical scenario in question, the situation does not look good for the fields that heavily rely on NHST. (Meehl 1967, p. 113)

Unfortunately, most published research findings in economics, behavioral sciences, health sciences and imaging-based neuroscience fit within this grim picture. So, excluding some atypical sub-fields, there are strong reasons to believe that the efficiency condition does not hold unequivocally for any theory in these fields, unlike the physical sciences whose sub-fields typically do not have an unhealthy reliance on NHST. Therefore, despite the growth of research capacity resulting from the exponential demographic growth in science and extensive utilization of computers in research, we shouldn't expect a general accumulation of evidence in favor of the dominant theories in the fields heavily relying on NHST, for most of the research effort fails to yield evidence.

In addition to these methodological problems, which directly result form the NHST, there are parallel concerns about the reliability of the commonly employed experimental designs and the peer-review process.

The first among these concerns there is the publication bias for positive results. In those fields where negative results are not news, there is a bias towards publishing the studies that report correlations rather than lack thereof. A combination of career pressure and the bias in question in turn tends to create a strong incentive for opting for the experimental designs which make positive results easier to obtain. (Goldacre 2012) So, the bias in question not only hinders the objectivity of the peer-review process but also skews the experiment design. (Ioannidis p. 697, 2005)

Another problem is so-called "discovery oriented research" where large quantities of data are 'mined' to discover correlations. Naturally, the prior probabilities one should assign for the hypotheses claiming the reality of these correlations are typically very low. Combined with over-reliance on NHST, these studies often produce vast numbers of statistically significant correlations, which are very likely to be false. (Ioannidis p. 698, 2005)

Yet another problem is the prevalence of biases that originate from personal, professional or commercial interests. Even when there is no deliberate fraud, these biases taint the findings by influencing hypothesis selection, data analysis and experimental design. (Ioannidis p. 698, 2005)

Finally, there is a methodological problem which is sometimes intentionally exploited by those with financial interests in seeing some positive results published. Testing by several independent teams in conjunction with the publication bias for positive results creates the false impression that there are real relations where there is only noise. After all, if a pharmaceutical company hires twenty 'independent' research teams to investigate the efficacy of a drug the company invested heavily on, at least one indisputably positive result is likely to emerge even if the drug is not much better than placebo. (Ioannidis p. 698, 2005)

As Ioannidis points out, we can infer the following alarming corollaries from these observations: The more skewed the experimental design looks (smaller sample sizes, smaller effect sizes, less stringent selection criteria for the investigated relations, overly flexible data interpretation) the less likely the findings are to be true, and the greater financial and professional interest are, the less likely the findings are to be true. (pp. 697-8, 2005)

Again unfortunately, most of the research findings in economics, behavioral sciences, health sciences and imaging-based neuroscience fit one or both of these corollaries, which means that most of the research findings in these fields are probably false. (pp. 699, 2005) The only good news is these corollaries do not seem to apply to most findings in the physical sciences. This is again consistent with my previous observation that there are strong reasons to believe that the efficiency condition does not unequivocally hold for the aforementioned fields but it holds for at least some theories in physical sciences.

In addition to these general methodological problems, there is one problem that is localized to behavioral sciences and imaging-based neuroscience. The logic of the problem is very strikingly articulated by Allen Newell in his paper, 'You Can't Play 20 Questions with Nature and Win'. (1973)

Newell first observes that at any given time, experimental psychologists study at least dozens of inter-related phenomena. Then he argues that the absence of a unifying psychological paradigm makes it near-impossible to see "how it all fits together":

> We never seem in the experimental literature to put the results of all the experiments together [...] We do [...] relate sets of experiments. But the linkage is extraordinarily loose. One picks and chooses among the qualitative summaries of a given experiment what to bring forward and juxtapose with the concerns of a present treatment. This aspect of our current scientific style is abetted by our tendency [...] to case the results of experiments in terms of their support or refutation of various binary oppositions. Thus, what is brought forward from an experiment is supposed to be just such qualitative summaries. Innumerable aspects of the situations are permitted to be suppressed. Thus, no way exists of knowing whether the earlier studies are in fact commensurate with whatever ones are under present scrutiny, or are in fact contradictory. (p. 298)

This observation appears to have a surprising implication: It seems to imply that no theory in experimental psychology (and in neuroscience where the scientists also seem to 'play 20 questions with nature') can satisfy the dominance condition of the exempting defeater I proposed earlier.

However, this implication should be rejected. There is plenty of wiggle room about how exactly we should understand the word "dominates," and we can plausibly claim that some theories in contemporary behavioral sciences and neuroscience may be dominant in a sense that matters for our purposes.

Admittedly, in psychology or neuroscience there is nothing like the Standard Model of particle physics, the Oxygenation Theory of Combustion of chemistry, or the Theory of Natural Selection of biology. However, the comparison is misleading. After all, the problem is not that in these younger science there are no widely accepted theoretical and experimental hypotheses. In fact, there are plenty of them such as Piaget's model of psychological development. In this regard, one may concede that some contemporary psychological theories deserve the status 'dominant' in the sense that they are widely accepted even if they lack the unifying character of the dominant theories in physical sciences. So, we can agree with the methodological worries Newell raises without making any concessions about the domination condition.

Yet, the methodological worries about the impossibility of seeing "how it all fits together" are sufficient to raise a further suspicion about whether the dominant theories of psychology and imaging-based neuroscience can satisfy the efficiency condition. Because of the chaotic situation, most of the research done in these fields would be hard to interpret, to put it mildly. Again, in order to drive the point home we can draw yet another apt contrast between the hard-sciences and the fields in question. But I think it is hardly necessary at this point.

In conclusion, we have bad news and good news. The bad news is, various methodological problems I reviewed in this section give us serious reasons to believe that the efficiency condition does not unequivocally hold for economics, behavioral sciences, health sciences and imaging-based neuroscience. The good news is, these methodological problems are by are large absent from the physical sciences or at least they are not present as severely.

6.2.2 Low Experimental Reproducibility

The reproducibility of research findings differ between fields, not only because of the differences in methodology, rigor and utilization of technology, but also because of the differences between the subjects that are studied. In those fields where the studied phenomenon is complex and the outcome of the experiments and observations can be influenced by a large number of factors that are difficult to control, it is natural to expect low experimental reproducibility. Right the expert opinion fears (Grens 2014) and empirical evidence suggests (Fanelli 2010) reproducibility is particularly low in experimental Psychology, fMRI based behavioral neuroscience and some areas of biological sciences whereas it is relatively high in most areas of physical sciences.

This is a problem since low reproducibility means wasted research capacity. After all, if the findings cannot be replicated, most of the research effort will not yield evidence.[57] However, this is only the tip of the iceberg. Actually, low experimental reproducibility in a field promotes a culture in which negative results are considered uninteresting, unimportant and even unpublishable. It is this resulting culture that is uninterested in negative results more than the low experimental reproducibility itself that is most detrimental to the epistemic status of the theories in a discipline.

There are various fields that exemplify this grim culture very aptly, especially the fields of behavioral and health sciences. However, I will use an example from a field of physics instead to illustrate the fact that the culture of anti-negative bias can take root in even in the so-called hard-sciences: organic semi-conductor research.

In September 2002, the world of physics was taken by surprise by an investigation assessing allegations made against a seemingly brilliant young physicist Jan Hendrik Schön of the Bell Labs. Before the report and the allegations preceding them, Schön was considered by many a future Nobel Laureate. He was almost unbelievably productive, "producing on average one research paper every eight days" between 1998 and 2001 (Goodstein 2010, p. 99), most of which

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[57]Findings that are not replicable are epistemic anectodes; they are by definition private pieces of information. Perhaps some of them indicate truly, but from a public point of view it is impossible to distinguish those that indicate truly from the false ones. Scientific justification however, is essentially public. That's why unreproducible findings aren't evidence.

were published in prestigious journals such as *Science*, *Nature*, *Applied Physics* and *Physical Review*. The results he reported were often groundbreaking, such as successfully inducing for the first time ever the Quantum Hall effect and superconductivity in organic material. But his most impressive result was the alleged single-molecule transistor, an achievement that would revolutionize electronic computers if it could be mass produced. (Goodstein 2010, pp. 98-99)

Then some of Schön's colleagues started getting suspicious with how neat his results were. Startled by how perfect Schön's graphs looked, professor Paul McEuen from Cornell University spent a whole night printing Schön's graphs on transparencies and superimposing them on one another. His efforts led him to an alarming discovery. Graphs supposedly depicted distinct data sets consisting of measurements with different material had identical noise.[58] Not only that but also the only difference between some graphs—other than the name of the studied material—was the fact that the axis of one graph was a multiple the same axis of another graph, in one occasion merely multiplied by -1. (Beasley *et al* 2002, E-9–E-11)

The findings of the ensuing investigation sent shock waves throughout the world of empirical science as they revealed that Schön had committed extensive research fraud. Although he admitted to only fudging the data a little "to make it look more compelling," his actual crime was more severe. The fact that he didn't have any raw data recorded on his computers and his lab notebooks as well as the fact that many of his graphs look cloned with identical noise and identical curves clearly indicate that he made up large chunks of data. (Beasley *et al* 2002)

[58] From personal communication with McEuen.

One should wonder how such a glaring and extensive case of research fraud in the limelight of cutting edge physics managed to take more than three years to catch. The answer has to do with low reproducibility that is particular to the field of organic semi-conductors and the systematic bias against negative results that stems from it.

What the Bell Labs investigation report said on the issue is revealing:

> "Attempts to reproduce the samples in Murray Hill have not been successful, in particular the crucial deposition of the dielectric for gating carrier concentration. Outside workers appear to have been unable to reproduce results. Samples are said not to survive long enough in air to be remeasured, or are subjected to destructive experiments." (Beasley *et al* 2002, D-11)

Goodstein makes a similar observation:

> [W]as Schön working in a field in which results are not easily reproduced? He was. Results in the field of semi-conductor devices are notoriously sample-specific, depending crucially on the skill and luck of the person who prepares the sample. Failure to reproduce any given result in any given sample is not considered proof of anything. Nobody could prove Schön had cheated just by attempting to replicate a particular result and demonstrating that it didnt show up in a particular sample. In fact, until McEuen and others started noticing duplications in the published data, no one complained about the results. (2010, p. 102)

Goodstein's observation that "Failure to reproduce any given result in any given sample is not considered proof of anything" is crucial. The fact that the experts in a field don't consider a failed replication "proof of anything" means that failed replication attempts are inherently unpublishable. It's not very hard to imagine what this would do to the evidence produced in the field in question. The literature of the field—even the prestigious peer-reviewed publications—will be full of 'interesting' studies reporting bold positive results though in reality such results are by and large flukes, false positives even when there is no fraudulent intent involved. After all, even if someone tried to replicate a positive result and failed, his failure wouldn't make the press.

The problem I have just alluded to is known as the "file drawer problem"; in fields where reproducibility is low due to the nature of the studied phenomena, failed replication attempts aren't news. Therefore, they are unpublishable. As a result, only positive results get published and negative results are put in to 'a file drawer,' away from the eyes of the public and closed to the access of even the experts in the field.

As the following two cases illustrate, the file drawer problem is not a hypothetical issue. It is the menace of especially behavioral and health sciences, where reproducibility is almost universally low.

In 2010, a precognition study conducted by Prof. Daryl Bem at Cornell University reported statistically significant results in 8 out of 9 experiments where the subjects were given tasks to measure their abilities in "precognitive approach to erotic stimuli and precognitive avoidance of negative stimuli; retroactive priming; retroactive habituation; and retroactive facilitation of recall." (Bem 2011, p. 407) In less technical terms, the results indicated that the subjects can foresee the future or do things that would entail reverse causation. The study was published in the respectable *Journal of Personality and Social Psychology.*

What happened next is very striking, described very colorfully by Ben Goldacre:

> [M]ost of the people who read [the study] said "Okay, well, fair enough, but I think that's a fluke, that's a freak, because I know that if I did a study where I found no

> evidence that [people] had precognitive powers, it probably wouldn't get published in a journal." And in fact, we know that that's true, because several different groups of research scientists tried to replicate the findings of this precognition study, and when they submitted it to the exact same journal, the journal said, "No, we're not interested in publishing replication. We're not interested in your negative data." [T]his is already evidence that in the academic literature, we will see a biased sample of the true picture of all of the scientific studies that have been conducted. (Goldacre 2012)

The situation in pharmaceutical research is even worse than experimental psychology due to a serious conflict of interest resulting from the relationship between private funding and medical journals. Here is how Richard Smith who for 25 years served as an editor of the prestigious *British Medical Journal* describes this conflict of interest:

> A large trial published in a major journal has the journal's stamp of approval [...] It will be distributed around the world, and may well receive global media coverage, particularly if promoted simultaneously by press releases from both the journal and the expensive public-relations firm hired by the pharmaceutical company that sponsored the trial. For a drug company, a favourable trial is worth thousands of pages of advertising, which is why a company will sometimes spend upwards of a million dollars on reprints of the trial for worldwide distribution. The doctors receiving the reprints may not read them, but they will be impressed by the name of the journal from which they come. The quality of the journal will bless the quality of the drug. (Smith 2005, p. e108)

In this environment, a journal editor who turns down a clinical trial study reporting a positive result turns down "upwards of a million dollars" revenue for the journal they represent. Even for journals that are supposedly 'non-profit,' for all intents and purposes this is an offer one cannot refuse.

On the flip side of the coin, there is no financial benefit in publishing a clinical trial study reporting a negative result. In fact, it is a loss overall, for it diverts away editorial resources that can be invested on the more lucrative path Smith describes. But even if the journals were willing to publish clinical trial studies reporting negative results, private funding behind such studies often discourages or even bars researchers—via the legal framework of intellectual property rights—from submitting their negative findings for publication.

The result in both cases and in countless many others, is the file drawer problem, which is especially severe in fields where reproducibility is low.

The fact that the problem is very serious—and is being taken very seriously by the active researchers in the aforementioned fields,—is clear from some recent developments. For instance, leading psychologists recently initiated two projects (*i.e.* PyschFileDrawer.org and Open Science Collaboration), which were motivated in part by these long-standing concerns about low reproducibility in their field. (Carpenter 2012) These projects both aim at systematically going through large volumes of published psychology research and trying to replicate the reported findings.

The results of the OSC are not public yet, however as of July 2014, PychFileDrawer.org has made a small but depressing sample publicly available. Out of the findings of 33 studies that were repeated, only the findings reported by 9 have been successfully replicated, whereas the rest are either exclusively unreproducible (20), or yielded ambiguous results (4) with at least one successful and one failed reproduction attempt.

Combined with the unhealthy reliance on NHST that I have discussed in the previous section, the file drawer problem can render large quantities of research in a field evidentially vacuous. In other words, we seem to have no choice but to conclude that at least in the disciplines like experimental psychology and pharmaceutical science where low reproducibility and the file drawer problem persists, the efficiency condition does not unequivocally hold for any theory, which disqualifies the theories in these fields from being exempt from the Pessimistic Induction. Pessimism about those theories is warranted by the revolutionary history of science. We should expect new revolutions where these theories will be abandoned or substantially revised.

6.2.3 High Incidence of Retraction and Research Fraud

A third group of hard problems that threaten the efficiency of research in several fields stems from high incidence of retraction and research fraud. Recent studies on retraction and research fraud (such as Steen 2011a, 2011b, Jones 2009, Budd *et al* 1998, 1999) show that both are on a steep rise in health sciences.

For instance Budd *et al* (1998, 1999) examined 235 cases of retractions of studies that were initially published through PubMed database. They observe that about a third of retractions are due to "evident scientific misconduct" and roughly an equal amount is due to "error." The rest is retracted either due to the failures of reproduction or without any public reason. Steen (2011b) on the other hand, classifies the reasons for retraction of over 2000 (742 of them from PubMed database) studies he examined as "error" (73.5%) and "fraud" (26.6%). (p. 249)

This seems to be inconsistent with Budd's (1999) one-third ratio of evident scientific misconduct, but the two findings actually agree. Steen's notion of fraud is narrower than that of Budd *et al.* Steen (2011b) defines "fraud" as "either data fabrication or falsification", and he categorizes plagiarism, data duplication and lack of public justification for retraction as cases of error. (p. 250) On the other hand, Budd *et al* (1998, 1999) include plagiarism and data duplication in the category of "evident scientific misconduct" as well as fabrication and falsification of data. In this regard, "evident scientific misconduct" includes not only outright fraud, but also lesser offenses.

Steen's (2011b) data indicate that the total number of papers retracted per year increased sharply over the last decade. The rate of increase in retractions is greater than the rate of increase in total number of publications. More terrifyingly, there is an increase in the delayed retractions, which happen years after the publication. In 2000 the longest delay before a retraction was 8 months, whereas it is 108 months in 2010.

Moreover, the incidence of retractions due to fraud increased sharply and it was coupled with a lesser increase in the retractions due to scientific mistakes. However, the fact that more papers were retracted in the last year than in 2000 alone is not enough to establish an increase in attempted fraud. After all the journals may be policing themselves more effectively. (p. 250) But the fact is that the incidence of fraud related retractions per year has increased a fifteen-fold between 2000 and 2010.

Steen (2011b) proposes three possible interpretations for the evidence. First, the ratio of fraudulent to honest research is actually on the rise. This seems plausible given the fact that "the number of papers retracted for fraud increased more than seven fold between 2004 and 2009 [and d]uring the same period the number of scientific papers retracted for scientific mistake did not even double." (p. 251) However, this disparity may also due to the change in the standards of categorization.

The second possibility is that there is no real increase in the incidence of fraud and the observed increase is due to more intense efforts to catch fraudulent results.

The third possibility is that since the sample size is small, the perceived increase could be random noise. This is very unlikely, because in comparison to past studies such as Budd *et al*, there has been a steady and clear increase in the retractions (253 between 1966 and 1997 *vs* over 2000 in the last decade alone).

Another interesting aspect of the retraction phenomenon is how quietly they are announced. Among those Budd *et al* (1998, 1999) looked at, 235 retracted papers were cited 2034 times after their retraction. Only 19 of these citations notified the reader of the previous retraction. Moreover, among the 742 PubMed retractions that Steen (2011a, 2011b) studied, 30% of retractions were not indicated in any way by the journal.

My own investigations not only have revealed parallel results but also gave me the chance to compare the retraction and research fraud in biological sciences and physical sciences. After all, in order to justify my thesis that the efficiency condition holds for at least some theories in physical sciences while it doesn't unequivocally hold for any theory in several fields including health sciences, we need a comparison.

In order to determine the comparative trends of retraction and fraud in leading scientific publications, I went through all issues of *Science* published between January 1971 and January 2011 and analyzed all the published retraction notices, associated retracted articles, investigation reports and—in some cases—legal documents. During this period, a total of 67 research articles published in Science were officially retracted. Most retraction notices offered an explicit reason for retraction (51). In 16 cases, the reason was not explicitly stated in the retraction notice. However, in 11 of these cases I was able to determine the reason for the retraction by reading the relevant research ethics investigation reports or through personal communication with the authors of the retraction notices. In 5 cases, I was not able to determine any clear reason for the retraction. In 21 cases the reason was fraud, in 31 cases it was a non-fraudulent scientific error and in 9 cases it was non-replicability of the findings. In 19 cases, the retracted study was a physical science study (physics, chemistry, astronomy, material science, geology) in 46 cases it was a biological science study (biology, medicine, pharmaceutical science) in 1 case it was a behavioral science study (Psychology, fMRI based behavioral neuroscience, Sociology) and in 1 case it was a climate science study. The mean delay before retraction (the number of days between the publication of the retraction notice and the publication of the retracted article) was 856.

Now, the global trends: The number of retractions per decade increased from 6 in the decade of 1982-1991 to 48 in the decade of 2002-2011. The increase in fraud-related retractions was even more spectacular, they increased from 2 in 1992-2001 to 19 in 2002-2011, with less steep but still significant increases in other reasons for retraction. The mean delay before retraction also increased from 554 days in 1982-1991 to 934 days in 2002-2011.

Finally, let's look at how physical sciences compare to the biological sciences. The retractions in biological sciences grew by the same factor as the retractions in physical sciences. However, this is clearly a fluke since 12 out of 19 of all retractions in physical sciences trace back to the now notorious case of Mark Schön, who was working in the field of organic semi-conductors,

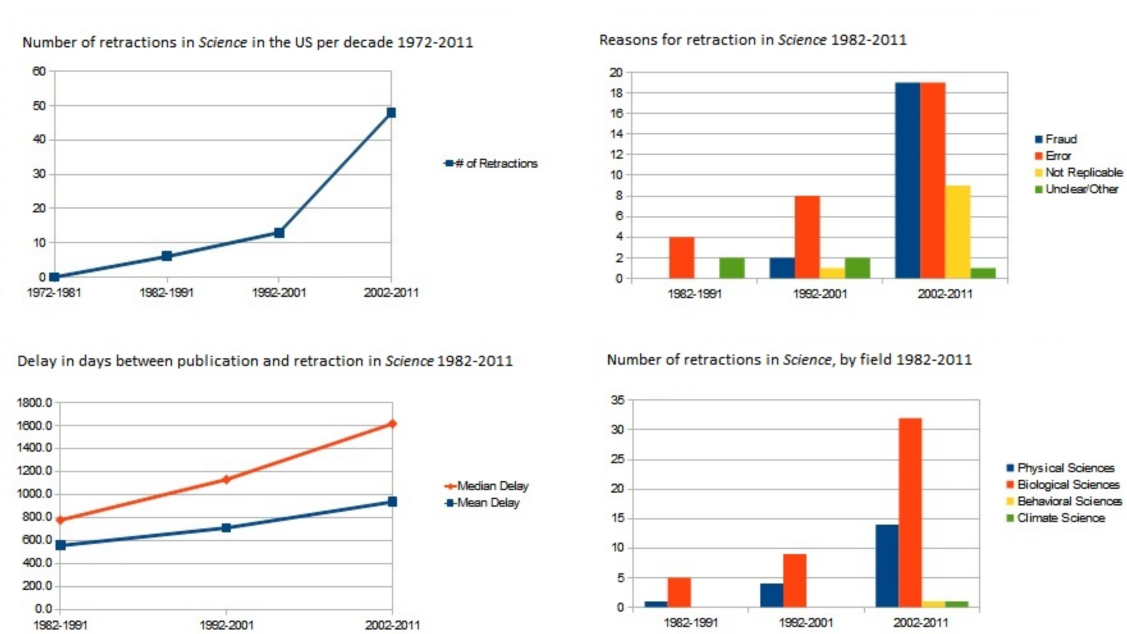

Figure 11. Retraction statistics in *Science*.

which is an unusual area of physical sciences in the sense that the results in it have extremely low reproducibility (Goodstein 2010, p. 102). In other words, the fact that there remain only 7 retractions in physical sciences that are not traceable to the Schön scandal is a testament to how healthy physical sciences are in general both in comparative and absolute terms as far as research retraction and fraud are concerned. However, the rise of retractions in biological sciences cannot be accounted in a similar way. There the problem is systemic rather than localized.

This is in agreement with Smith's and Ioannidis' analyses that the combination of low reproducibility, anti-negative bias, the file drawer problem and commercial conflicts of interest render large chunks of the results in those fields that suffer from these methodological problems evidentially suspect, to put it mildly.

Therefore, we seem to have no choice but to conclude that the efficiency condition doesn't unequivocally hold for these fields. They lie outside the scope of the optimism that can be warranted by the exemptionist strategy.

6.3 Conclusion

In the present chapter, I investigated the problems that might prevent a scientific theory from satisfying the efficiency condition, which requires that the field hosting the theory has been efficient and not wasteful in utilizing increased research capacity resulting from the exponential growth and computerization of science.

I looked at two types of problems: easy and hard. Easy problems—copious repetition and vicious theory-dependence—are potential problems that could in principle threaten the efficiency of almost all areas of empirical science. Fortunately, they can be and are effectively nullified in most cases. However, hard problems found in specific individual fields cannot be countered as effectively. Whereof one encounters methodological problems born from unhealthy reliance on NHST, low experimental reproducibility, pervasive research misconduct and retraction are pervasive, thereof one must remain cautiously pessimistic.

Though there are indeed exceptions to this, generally speaking there are significant differences in terms of efficiency between physical sciences on the one hand, and behavioral and health sciences on the other. In particular, typically sub-fields of physical sciences tend to suffer less from the hard problems in comparison to the typical sub-fields of behavioral and health sciences. Therefore, it is unreasonable to expect any theories in the typical sub-fields of behavioral and health sciences to satisfy all the exemption conditions unequivocally. However, there is room for optimism as far as most sub-fields physical sciences are concerned, where hard problems are often not present, again with some noted exceptions such as the sub-field of organic semi-conductors.

7. CONCLUSION

The chief objective of this manuscript has been to critically engage the Pessimistic Induction and show that a limited yet substantial fom of optimism is warranted in the face of the revolutionary history of science.

I called the strategy I used to defend the optimism in question "exemptionism," which conceded that the Pessimistic Induction is generally successful, but claimed that there are identifiable exceptions to it. The evidence today's scientists have for some current best theories is in some cases significantly better than the evidence their predecessors had for their best theories. Such current theories whose epistemic status is better than their predecessors are *completely exempt* from the Pessimistic Induction.

I identified four conditions that are jointly sufficient for a theory to have such a superior epistemic status:

- Domination Condition: The theory currently dominates its field; there are no other serious contenders.
- Stability Condition: The field has been stable for long enough that the exponential increase in research capacity has led to an unprecedented accumulation of potential evidence in favor of the theory.
- Computerization Condition: Computerization of the field has vastly increased the quantity and quality of evidence produced in the field and diversified the sources of evidential

support the theories in that field enjoy, making the current dominant theory unique among all the theories ever dominated that field.

- Efficiency Condition: The field has been efficient and not wasteful in utilizing increased research capacity resulting from the exponential growth and computerization of science.

When these four conditions are simultaneously and unequivocally satisfied, we have what I called an "exempting defeater" against the epistemic formulation the Pessimistic Induction. Optimism concerning the theories that unequivocally satisfy these four conditions is warranted. Provided that there are no grounds for pessimism other than the Pessimistic Induction, such theories are probably the best and final theories in their relative domains of science. We have *prima facie* justification to believe that they will not be abandoned or substantially revised during a future revolution.

Among the four conditions, perhaps the most interesting is the computerization condition. A general analysis of computer use in science which I offered in Chapter 5 suggested that at least some of the current dominant theories of stable and efficient sciences—Fermi-Landau Liquid theory in condensed matter physics, the NICE Model in Solar System cosmology and the Standard Model in particle physics—do indeed satisfy the computerization condition. Therefore, they are completely exempt from the Pessimistic Induction. I believe that a similarly successful case can also be made for some other theories in physical sciences and a smaller set of theories in biological sciences, such as the Theory of Natural Selection and perhaps even General Relativity though I left that as an unexplored conjecture.

By its nature, exemptionism is necessarily limited in scope. Its limits, which I explored in Chapter 6, leave a substantial number of contemporary theories outside the protection of the exempting defeater. Especially those fields that have been unstable, that couldn't employ computerized research to its full potential and that are bogged down by serious methodological problems are left entirely outside the scope of exemptionism as I defend it. In other words, no theory in such fields can be protected against the epistemic formulation of the Pessimistic Induction by the exempting defeater. I explained to a reasonable extent why very few if any theories of behavioral and health sciences can be expected to unequivocally satisfy the efficiency condition.

In other words, the exemptionist strategy delivers precisely what it promises: It saves some of our current theories from the Pessimistic Induction in their entirety, while it doesn't save anything else. Some theories, in particular those which unequivocally satisfy the four conditions, are probably here to stay, though they might have been born from a revolutionary history of science that is nothing but a graveyard of abandoned theories. The optimistic belief that they are the best theories in their individual fields that evidence will ever warrant is not refuted by the Pessimistic Induction because the evidence we have for them is nothing like we have ever had before. Other theories however cannot be saved in their entirety by the exemptionist strategy. Although some among them could turn out to be stayers, that would be a case of doxastic luck on the part of the optimist.

The upshot of the whole effort therefore, is a mixture of optimism inspired by the scientific golden age we are fortunate to be witnessing and an epistemic humility dead theories teach us.

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APPENDIX

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