Perception of Risk:
Disaster Scenarios at Brookhaven

by

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Chapter 1: Introduction

Multiplicity

It is a venture of discovery, to be made with a collection of hundreds of academics, hundreds of millions of dollars, and the drive for knowledge of the origins of the universe. It is a descent into madness, providing crazed scientists in white lab coats the power to destroy mankind with the press of a button. It is a scientific quest to elucidate the statistical physics of quarks and gluons. It is a pathway which may lead to monumental revolutions in the study of quantum levels. It is a newsworthy story.

Each of these narratives have been told to describe the exact same event: an experiment that was to be done in the Relativistic Heavy Ion Collider (RHIC – pronounced "rick") at Brookhaven National Laboratories. In 1999, gold nuclei were to be smashed together at such high velocities as to break the atomic particles (protons and neutrons) into their component parts (quarks and gluons).

When this proposed experiment was featured in an issue of the popular science magazine *Scientific American*, one man wrote a letter to the editor, citing a fear that this experiment would create a mini-black hole which would end mankind. This letter was published, and the fear soon multiplied into three disaster scenarios which would end mankind and possibly alter the fundamental structure of the universe as we know it. These disaster scenarios were:

1. Creation of a black hole.
2. Transition to a lower vacuum state.
3. Formation of a stable, dangerous strangelet which would convert all surrounding matter to a different form of matter known as strange matter.

Each of these disaster scenarios made its way into a media frenzy about the experiment at Brookhaven. The subject of much of the frenzy: will the physicists at Brookhaven destroy the universe?

This frenzy spread worldwide, with newspaper articles as far as Australia being published, websites being created devoted to the topic, talk radio shows discussing the scearnios, and even a segment aired on a television news show hosted by Peter Jennings.

The Experiment: A Crash Course in Sub-Atomic Physics

The feared experiment was to be done on a newly built ion collider in Long Island. The experiment itself was a collision of two gold nuclei (also called heavy ions due to a nucleus' lack of electrons). The novelty of this experiment as opposed to other experiments performed at other colliders is that RHIC was the first machine built which was capable of colliding entire heavy ions, rather than merely smaller particles.

Two beams of gold nuclei are raced around a 2.4 mile track at nearly the speed of light in opposite directions. If done properly, the nuclei will break apart in a special way. The conditions during the collision can reach more than 1 trillion degrees above absolute zero. The protons and neutrons, the atomic particles that make up the nuclei, "melt" and for a brief instant, the subatomic particles which compose the protons and neutrons are liberated. These subatomic particles are known as quarks and gluons.[1] When the collision occurs and these particles are liberated, the physicists at RHIC were hoping to find a quark-gluon plasma. As the term implies, this is a "soup" composed of quarks and gluons which had not already coalesced and formed particles such as protons and neutrons.

Why study this? Physicists believe that around 13 billion years ago, when the big bang occurred and the universe as we know it was created, initially a quark-gluon plasma had existed. As the universe cooled due to the universe's expansion, these quarks and gluons combined to form the larger atomic particles that we learned of in high school chemistry. It

[1] Quarks are now considered the fundamental building blocks of matter, and they come in six varieties (up, down, top, bottom, strange, and charm). The quarks are held together by "passing" gluons between each other, almost like baseball players passing around a baseball. This force is known as the strong force.

will hopefully give physicists insight into both nuclear and particle physics, cosmology, and astrophysics.

RHIC

The Relativistic Heavy Ion Collider itself has a long history, leading up to its first experimental use in the year 2000.[2] Originally conceived of as the ISABELLE project, this particle collider was to have the innovative property of "smashing" two beams of particles moving in opposite directions, rather than the traditional single beam colliding with a stationary target. In 1978, once money had been requested and granted from the United States government, the building of ISABELLE began. Due to technical problems compounded with the building of the now-defunct Superconducing Supercollider (SSC), the ISABELLE project had been stopped.

The idea to build a collider at Brookhaven was revived in 1984 by Brookhaven physicists—converting the tunnels of ISABELLE into the tunnels of RHIC. And in the year 2000, the first experiment was conducted.

However, this experiment was not to be reached until after an extended media debate, outlined briefly above, over the safety of the experiment had been settled. That is what this work is to address.

Risk

I will use this debate, both from a literary standpoint and from a scientific standpoint, to understand the concept of *risk* meaningfully. This event, the dispute over the safety of RHIC, will be the medium through which I will analyze the properties of risk. I divide this work into four main themes.

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[2] Much of the text from this section is taken from the BNL website: **Error! Bookmark not defined.** (Accessed 12 December 2002).

First, I will understand what is meant by the term *risk* at all. After all, can we merely define risk with a number, or do we have to include feelings and values? Is it an objective fact or a subjective opinion? Risk will also be seen to be a travelling entity, moving from one locale to another. In this particular case, I follow the introduction of the Brookhaven risk (destruction of mankind) as it weaves from medium to medium. I also consider *why* the risk spread to certain places that it did, such as conspiracy websites.

Second, I try to understand risk as a calculated entity by studying an independent report commissioned by Brookhaven to analyze each of the three disaster scenarios. I compare this report to a technical report written in 1942 by Edward Teller considering another disaster scenario—the ignition of the atmosphere by the first atomic bomb. I do this to find similarities in the scientific consideration of catastrophic events. How is one supposed to calculate with credibility events which have such uncertainty involved and such unlikelihood of coming to be?

Third, I discuss what happens to risk as it travels. I write about how a single risk mutates as it appears in one medium versus another, and why this happens. This is done via a case study of myriad newspaper articles written about the Brookhaven disaster scenario. From studying the discourse *within* these articles, we come to understand how one is constrained by both mathematics, language, and pressures from within the newspaper industry.

Lastly, I briefly discuss the physicists' response to the negative media coverage. What articles did they not agree with? What steps did they take to avert the public relations disaster?

In the next four chapters, I will not resolve the debate over the catastrophic effects of Brookhaven's experiments (by now, the debate has long been settled with RHIC performing many collisions and with me and you still here, alive and well). I will not accuse the media of being alarmist, nor make the claim that the Brookhaven physicists acted in the best manner possible. In the beginning of this chapter, I outlined this story told from different vantage points. I also am going to be telling a narrative. The way I tell it, though, is of an adventure, tracking risk from its inception – it is a story of information, mutation and appropriation between media of all forms (persons, newspapers, magazines, journals, television, email, etc.). It questions the role of scientific and media objectivity and enmeshes the two conceptions in a complicated way. By telling the narrative in this way and by working from the four themes outline above I hope to generate a more complete understanding of risk, not as a single, computable quantity, but rather as a value-laden construction generated and subsequently influenced by scientists and the media.

Chapter 2: The Propagation of Risk

Introduction

The March 1999 issue of *Scientific American* included an article titled "A Little Big Bang."[3] The article provided a layperson's introduction to the Relativistic Heavy Ion Collider (RHIC, pronounced "Rick") and a planned experiment which had the potential to create a quark-gluon plasma. This state of matter has not been seen since the early universe, billions of years ago, around the time of the big bang.

From this single article sprung a world-wide concern that the world was about to be destroyed – by this single physics experiment which was supposed to create a plasma that was to last a mere 10^{-23} seconds! It would seem that "A Little Big Bang" was part of a cultural phenomenon that could be described as a big bang of its own, in this case, an explosion of alarm and fear that resonated with the media, laypersons, and scientists.

In this chapter, I consider the "big bang" this experiment itself produced. By examining the events succeeding the publication of the original *Scientific American* article, which lead to notion that the world could be destroyed by scientific research, I will introduce the idea that facts, and in particular *risks*, travel. In this case, the risk that the destruction of the Earth (in fact, even the entire universe) was possible from the RHIC experiment moved from printed locale to printed locale, and from person to person. I trace this movement in the months following the March 1999 issue of *Scientific American*, examining how and where this risk traveled, and how, on a basic level, the presentation of risk shifted also in this movement. In the next chapter, I will explore one document that released a particular presentation of risk – via a report released by physicists, and in Chapter 4, I explore this *tranformation* of risk in more detail by using collected newspaper articles as a case study.

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[3] Madhusree Mukerjee, "A little big bang," *Scientific american* **280** (March 1999): 60-65.

Risk as a Fact

In order to discuss risk meaningfully, the concept, vague in itself, needs to be examined in a theoretical framework. Many social theorists have considered the topic of risk – most notably Ulrich Beck. Although their works are wonderful analyses of risk, they will not be utilized extensively. This is a result of the two major themes which guide this work: (1) the spread of a *catastrophic* danger which was given virtually no chance of occurring by any expert in the field (as opposed to many of the risks considered by Beck) and (2) the presentation and appropriation of risk in a single context.

More useful to understanding risk as a travelling entity is looking at risk from the viewpoint that the risk is a form of fact. The American Heritage Dictionary defines a fact as:

1. Knowledge or information based on real occurrences
2. a. Something demonstrated to exist or known to have existed
   b. A real occurrence; an event
   c. Something believed to be true or real.[4]

Following a strict reading of these definitions, *risk* would lie at the periphery of what it is to be a fact.

However, even with this broad definition of fact, a problem immediately arises. Is a risk *real*, or is a risk something *believed to be real*? Is it a mere objective probability – if such a phrase even has meaning – or is it more? I contend that in our framework of risk, we must divide risk into two types: an objective risk and a perceived risk. An objective risk exists, independent of human perception – inherent in any system lies a risk (a certain chance) of something happening. For example, inherent in a nuclear power plant lies some risk of a nuclear disaster – deriving from human error, complex interactions of different control systems, terrorist attack, etc. No single person could assign a probability to *every* situation – how can one objectively and exactly quantify the probability of a terrorist attack? So

Chernobyl, with or without anyone's perception of the dangers, contained a risk. We are only able to see that a risk was present with hindsight, as with the Chernobyl disaster.

It is perceived risk that determines how we think about these events – and it is where value systems get involved. The term risk (read perceived risk) has almost become a technical replacement for danger. As social theorist Mary Douglas says

> 'Risk' [traditionally] is the probability of an event combined with the magnitude of the losses and gains that it will entail. However, our political discourse debases the word. From a complex attempt to reduce uncertainty, it has become a decorative flourish on the word 'danger.'[5]

Understanding if an event is risky has become a wholesale determination of if an event is dangerous. Depending on one's personal views of nuclear safety, fear of terrorism, knowledge or lack of knowledge about nuclear radiation, etc., one will generate a perceived risk to decide whether a nuclear power plant is *safe*. A culture which embraces nuclear technology will generate different opinions on the safety of nuclear power plants than a culture that rejects nuclear power. Experts similarly have a perception of risk – generated from their analysis of the data (also subject to cultural influences). Their analysis will be built on their academic training – which in some way influences their views and the way the analysis is conducted. Which data should be used and which should be rejected? What is relevant material to include in the risk analysis and what is not? Is the method of calculating the risk right? And when making policy recommendations based on these risks, how does one quantify the value of a particular outcome. What is the value of one death or the existence of one species – how do you quantify that in a risk analysis?

It is by viewing risk in this manner (as a fact) and deconstructing it into objective and perceived risks that risk becomes a meaningful concept. Suddenly we are confronted with many types of risks – one person may see an event as risky while another may see the same

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[4] "Fact," *The American heritage dictionary of the English language*, Fourth Edition, Houghton Mifflin Company

event as safe. Also, it makes explicit how risk becomes *personalized* – and people come to understand risk in different ways – derived in some large part from the information (data) that each is presented with. Utilizing this loose interpretation of "fact" will allow for latitude when considering risk as a traveling entity. For the rest of this work, we will only be considered perceived risks – as objective risks are inscrutable.

Risk as a ***Risk-In-The-World***

With risk now understood at its core as a valued-laden belief extrapolated from knowledge, it can now be placed in a proper theoretical context. I employ anthropologist Joseph Dumit's concept of a "fact-in-the-world." Introduced in "A Digital Image of the Category of the Person,"[6] this concept was used to underscore the notion that facts do not lie in vacuo but rather are generated and propagated by the cultural surroundings – from mainstream movies to the research laboratory. Facts lie within the world they are generated in, and they travel and shift in this world, as they move from person to person and medium to medium. As Dumit eloquently elucidates, "Facts have to find us; we have to hear of them or read them, and we have to incorporate them as facts."[7] The broad focus of Dumit's paper is on the ways people learn, utilize, and incorporate facts.

With this conceptual framework, and our relaxed view of "fact," we can view the Brookhaven risk as a "risk-in-the-world," akin to Dumit's "fact-in-the-world." Important to Dumit, as it will be to us, is the idea that facts have to spread to be consequential. Bruno Latour, social theorist and founder of Actor-Network Theory, discusses the traveling life of

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(2000)

[5] Mary Douglas, *Risk and blame* (Routledge: New York, 1992): 40.

[6] Joseph Dumit, "A digital image of the category of the person," in *Cyborgs & citadels: Anthropological interventions in emerging sciences and technologies,* eds. Gary Lee Downey and Joseph Dumit (School of American Research Press: Santa Fe, 1997): 83-102.

[7] Dumit, "A digital image of the category of the person": 86.

facts: how they are born, sustained, and mutated as they travel from actor to actor (both human and institution) in a network. He describes this process in *Science in Action*:

> To picture the task of someone who wishes to establish a fact, you have to imagine a chain of the thousands of people necessary to turn the first statement into a black box and where each of them may or may not unpredictably transmit the statement, modify it, alter it, or turn it into an artefact. How is it possible to master the future fate of a statement that is the outcome of the behaviour of all these faithless allies?[8]

This notion of establishing a fact (the risk associated with the Brookhaven experiment) is one of the overarching themes of this work, and throughout this chapter, as well as others, we will see how this risk, created by the Brookhaven experiment, does act as a black box, mutating with multiple constructions of this risk, travelling in network of physicists, journalists, and lay persons. But more important to note in Latour's notion of fact is their total constructedness and traveling nature.

With the dispersion of a fact comes a wider audience – which has to decide how to interpret the fact. The audience does not automatically accept the fact. Rather, after being presented with the fact, the audience has to interpret the fact. Should the fact be believed? Should it be considered nonsense? In either case, the reader will be forced into a situation where he has to appropriate the fact in some way into his worldview.

Dumit, in the same article as above, coins the phrase "objective self-fashioning" to describe a similar appropriation process:

> The objective self is an active category of the person that is developed through references to expert knowledge and invoked through facts. The objective self is also an embodied theory of human nature, both scientific and popular... Objective self-fashioning is how we take facts about ourselves—about our bodies, minds, capacities, traits, states, limitations, propensities, etc.—that we have read, heard, or otherwise encountered in the world, and *incorporate* them into our lives.[9]

It must be clear here that Dumit is writing about incorporating brain scan images and what they can tell about human nature into a notion of the self – he was *not* writing about incorporating risks into our everyday lives. But I find the underlying principle relevant in the

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[8] Bruno Latour, *Science in action* (Harvard University Press: Cambridge, 1987): 104.

study of risks: people alter their view of the world with the knowledge of risks. They incorporate knowledge of risks in their everyday activities and beliefs (instead of Dumit's incorporation of facts into a view of the self). We will term this incorporation "risk appropriation." Often new studies are released stating that some vitamin or other can reduce the risk of cancer. Those that believe these studies are liable to alter their food intake. In the same vein, we will see how those persons who accept (even partially) that the Brookhaven experiment could destroy the world ended up reaffirming or altering their views on the role of science and scientific progress.

In addition to the lay-reader in a wide audience who is presented with the "risk-in-the-world" is the author who presents the risk *in some fashion* to the world. The author is charged with the responsibility of presenting an interpretation of the risk – based on his or her research. The author has choices which affect how the audience reads the piece – who should be quoted, what words should be used when describing the risk, how much of the article should be devoted to the idea of risk versus the experiment itself. By making these choices (however consciously or unconsciously), the author is crafting his or her own version of the risk associated with an event (in our case, Brookhaven's experiment). Thus, the author is forced to conduct research on the feasibility of the danger and incorporate this information with his or her worldview *and* the article. It isn't merely the lay-readers who are faced with "risks-in-the-world" – those who spread the risk are also faced with the same dilemma.

In the rest of this chapter, I intend to chronicle the birth and propagation of the Brookhaven National Laboratory's risk. In particular, I examine printed material in the form

[9] Dumit, "A digital image of the category of the person": 88-89.

of newspaper, magazine, and online articles, and consider briefly how the authors portrayed and spread the perception of risk.

Groundwork

Building the Relativistic Heavy Ion Collider in Long Island was first proposed in 1984. With a tunnel already excavated from a previously abandoned particle accelerator (ISABELLE) at the Brookhaven National Laboratory, RHIC was granted funding. Building began in 1991 and the first experiment, the collision of gold ions, was started on June 12, 2000. Between these two events, though, lay a massive roadblock. It was believed by some that the proposed experiment would possibly destroy the universe.

This idea was first sparked by a March 1999 *Scientific American* article detailing RHIC and the experiment.[10] *Scientific American* is a popular science magazine that publishes articles on almost every branch of science, from biology to computing to physics. The article resulted in a letter to the editor in a later issue detailing concerns over the experiment.

The writing in the March 1999 article was not geared towards particle physicists, but rather to its lay followers. Contained within the pages was an introduction to the world of the subatomic – quarks, gluons, and quantum chromodynamics (QCD) – as well as a description of the experiment and its goals. The potential importance of the experiment to experimental and theoretical particle physics was listed:

> Theorists are exhorting experimenters to search for disoriented chiral condensates (resulting from the tiny region of space at the center of the collision being unsure of how to pair its quarks and antiquarks); violation of charge parity (a symmetry normally believed to be obeyed by the strong force); and innumerable other hypothetical phenomenon. And if that isn't enough, Stöcker [physicist] maintains that a quark-gluon plasma is not the simple "free gas" of common conception but a complex, interacting system that in many ways resembles the hadronic phase following it. So the hunt for a smoking gun could be doomed from the start.
>
> The experimenters seem unfazed by the barrage. "I leave the arguments that start to sound like, 'How many angels can dance on the head of a pin?' to the theorists," shrugs Barbara Jackson, an experimenter at the State University of New York at Stony Brook.[11]

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[10] Madhusree Mukerjee, "A little big bang,": 60-65.
[11] Madhusree Mukerjee, "A little big bang,": 64.

The details about each potential scientific discovery is not as important to understand as the *way* these potential discoveries were presented. The experiment's possible outcomes were presented as uncertain but they held the capability of revolutionizing the theory girding one area of high-energy particle physics. In fact, one possibility was highlighted above the others:

> Perhaps the most intriguing possibility is the appearance of a strangelet: a quark droplet with many strange quarks. Strange quarks should be plentiful in the quark-gluon plasma and could hypothetically coalesce, along with up and down quarks, into this object. Although finding a strangelet—at least as exotic a state of matter as the quark-gluon plasma itself—would be thrilling, questions remain about whether it would be stable enough to reach the detectors.[12]

We will see how this statement would soon be revisited in the July issue of *Scientific American*, in a letter to the editor that would be the start of a media frenzy predicting the end of the universe.

The March 1999 article lay the groundwork for the ensuing events, by allowing for *Scientific American* to be the forum where the initial risk was generated.[13] As Dumit states, "Facts have to find us." *Scientific American* was the opportunity that allowed the facts to "find us" – it lay the groundwork which was to generate the initial concern about the experiment.

The Birth of Two Risks

In response to the original article outlining the RHIC experiment, reader Walter Wagner wrote a short letter to the editors at *Scientific American*. Wagner received a bachelor's degree from the University of California, Berkeley in 1972, getting a major in botany/biology

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[12] Madhusree Mukerjee, "A little big bang,": 64. It might be helpful to note that strange quarks are very similar to the 'regular' quarks that make up ordinary matter – the term "strange" is a purely scientific term.

[13] It must also be noted that more factors than a single magazine article are involved in laying the groundwork for the larger debate over risk and Brookhaven National Laboratory. The question that must be asked is: why did this particular experiment at a particular moment in time create such an outcry at Brookhaven (and not at some other particle collider or at some other time). I can only speculate from my research, but I would say confidently that two principle factors are the negative environmental record of Brookhaven and fact that the year was 1999 – close to the end of what many considered the millennium (with Y2K and other millennial fears rampant, the millennium fever could have allowed for newspapers to pick up the nearly apocalyptic-esque story).

and a minor in physics.[14] In this letter, Wagner discussed Hawking's postulate of the presence of miniature black holes in the moments after the big bang. He explains his concern:

> If [a mini black hole] were created near a large congregation of mass and if it started absorbing that mass before exploding, the black hole could reach a relatively stable half-life and thus continue to grow. If this happened on the earth, the mini black hole would be drawn by gravity toward the center of the planet, absorbing matter along the way and devouring the entire planet within minutes.
>
> My calculations indicate that the Brookhaven collider does not obtain sufficient energies to produce a mini black hole; however, my calculations might be wrong. The only way to determine the energy density at which a mini black hole would be created as an intermediary step to the type of explosion depicted in your article is to build a collider and do the experiment. Is the Brookhaven collider for certain below the threshold?[15]

With these sentences, Walter Wagner had introduced a risk into the world – the risk of the production of a mini black hole. This risk has a *physical* resting place in the world – within the pages of *Scientific American*. My claim is that this physical resting place allowed for people (authors and lay persons) to "appropriate risk" into their minds.

It should be noted that Wagner admitted that his calculations indicate that there is no threat of a miniature black hole, but he did not have complete faith in his numbers. Rather than ask for experts to verify his *theoretical* calculations, though, Wagner demanded *experimental* proof that no black hole would be created. He demanded a new collider be built as a stepping stone between RHIC's predecessors and RHIC itself.

In response to Wagner's concerns, Frank Wilczek of the Institute for Advanced Study responded in the same issue:

> Scientists must take such possibilities [of catastrophic disaster] very seriously – even if the risks seem remote – because an error might have devastating consequences. In the case of the Brookhaven RHIC, dangerous surprises seem extremely unlikely. First, nuclear collisions with larger energies take place regularly as cosmic rays rain down on our atmosphere – so if a disaster were possible, it would have already occurred. Second, related regimes have been explored in detail, and so we have substantial evidence that our theoretical framework for understanding what will happen is reliable.

[14] Wagner's varied intellectual history can be found on the website for the World Botanical Gardens (**Error! Bookmark not defined.**, a Hawaiian project spearheaded by Wagner himself. The characterization of Wagner as a "physicist" was common in subsequent newspaper articles.

[15] Walter Wagner, "Letters to the editor," *Scientific american* **281** (July 1999): 8.

> Although we cannot calculate the consequences in complete detail, we can distinguish credible from incredible scenarios.
>
> The idea that mini black holes will be formed, as Wagner suggests, definitely falls in the latter category. The energy densities and volumes that will be produced at RHIC are nowhere near large enough to produce strong gravitational fields. On the other hand, there is a speculative but quite respectable possibility that subatomic chunks of a new stable form of matter called strangelets might be produced (this would be an extraordinary discovery). One might be concerned about an "ice-9" type transition, wherein a strangelet grows by incorporating and transforming the ordinary matter in its surroundings. But strangelets, if they exist at all, are not aggressive, and they will start out very, very small. So here again a doomsday scenario is not plausible.[16]

In this response, Wilczek relayed his expert opinion, debunking the doomsday scenario proposed by Wagner. At the same time, however, he also introduced a new risk into the world: strangelet production. He called the production of strangelets a "respectable possibility" and raises the specter of "ice-9," alluding to Kurt Vonnegut's novel *Cat's Cradle*.[17]

In this novel, one character secretly devised a new form of ice called ice-9 which was solid at room temperature. Ice-9, when it comes in contact with ordinary water, converts it into ice-9. Similarly, certain strangelets (if they exist) would have the same property—when they come in contact with ordinary matter, they could convert it into strange matter. Wilczek did not really provide ample evidence in his letter discounting the strangelet scenario. In any case, by raising the notion of dangerous ice-9-esque strangelets, another risk had been born.

In an attempt to remove one risk from public concern, Wilczek inadvertently generated another risk. Cultural theorist Mary Douglas, who has written extensively about the concept of risk in societies, has noted this phenomenon. She states "Disputes about risk have become endemic and self-generating... Protecting against one category of risk exposes another."[18] In this instance, the dispute was over the risk of mini-black holes, and Wilczek, in an attempt to protect the RHIC project against public concern, voiced another concern

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[16] Frank Wilczek, "Letters to the editor," *Scientific american* **281** (July 1999): 8.
[17] Kurt Vonnegut, *Cat's cradle* (Delta: New York, 1998).
[18] Douglas, *Risk and blame*: 14.

about strangelets that was later turned into a reason to give doubt to the safety of the experiment – to give the experiment a risk associated with it. Wilczek's letter shows how risk can be self-generating.

It also should be pointed out that there is a divide between the theoretical and the experimental. A question is raised: what type of evidence is acceptable when evaluating a risk? Wagner obviously did not have complete faith in the theoretical – or at the very least, in his ability to use the theoretical to predict future events. Wilczek, on the other hand, utilized both experimental and theoretical evidence in his argument to dismiss Wagner's concerns. As this risk starts to propagate, this question will be asked again.

Newspapers Take Notice: The Unfolding of Risk

Within the same month as the letters to the editors came the initial newspaper article describing the catastrophic disaster scenarios. Jonathan Leake wrote "Big Bang Machine Could Destroy Earth" [19] for the *Sunday Times (London)*, published on July 18, 1999. This publication brought what could be called an aside in a science magazine into the mainstream press.

So why *did* Leake pick up this story? And how did he go about researching it? Leake stated that what prompted the story was "Brookhaven's decision to set up a committee to investigate the risk – and then to lie about its existence."[20] The committee that Leake referred to was a group of four physicists called by John Marburger, director of the Brookhaven National Laboratory, to study the experiment's potential for catastrophic consequences. Leake expounded:

> I spoke to John Marburger... and he said that the lab was not at all concerned about the risk and was taking no action. Then I spoke to Bob Jaffe who had just received a letter from Brookhaven asking him to serve on the committee assessing the risk. I went back to Marburger who then admitted to the committee's existence and said it had 'not been important' to tell me. I think an internationally

[19] Jonathan Leake, "Big bang machine could destroy Earth," *Sunday Times London* (18 July 1999): D7.
[20] Jonathan Leake, Personal Correspondence (26 November 2002).

> renowned laboratory calling in some of the world's best physicists to sit on a committee deciding if an experiment could go so badly wrong is a very valid story – especially when they cover up their actions.[21]

What Leake stressed is the appearance of a cover-up by Marburger, and how that made the story newsworthy. However, the article itself did not mention Leake's belief in a cover-up.

The article itself presented a serious group of scientists investigating credible disaster scenarios – scenarios with a "real risk." However, this conflicts greatly with Robert Jaffe's explanation of the risk. Jaffe (Director of MIT's Center for Theoretical Physics) was appointed head of the investigative committee comprised of four people. He stated that even before he was asked to serve on the committee that "No scientist who understood the physics thought that this experiment posed the slightest threat to anybody."[22] I asked Jaffe that if the threats were not credible, then why have the committee in the first place? He replied that "it was an attempt to take seriously the fears of science that they [the public] don't understand, and to try to provide them some reassurance that things they don't understand are not being controlled by a conspiracy." Ironically, it was this fear of conspiracy (of Marburger's "cover up") that led for the *Sunday Times (London)* article to be drafted in the first place.

Many reporters had contacted Jaffe, but he, in particular, remembered talking to Jonathan Leake.

> I thought the guy was totally irresponsible. He talked to me on the phone and promised me that, well, we kept discussing the issue of likelihood and I tried to give him quite a bit of background on how to discuss unlikely situations. And he kept saying to me, can you say this will never happen? And I said as a scientist I'm never going to say anything is never going to happen; its not part of our vocabulary. And I would say that it is extraordinarily unlikely, and he said well that means it could happen. Well, no, that means that it is extraordinarily unlikely.[23]

This is a dramatic illustration of the divide between the layperson and the scientist. Part of the disconnect is a matter of semantics. Is it true that "extraordinary unlikely" is the same as

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[21] Jonathan Leake, Personal Correspondence (26 November 2002).
[22] Robert Jaffe, Interview (22 November 2002).

"could happen"? Do these terms portray the same thing? Is one more accurate than the other? What are the limitations of words, and in fact, what are the limitations of numbers, when describing risk? We consider these questions in detail in Chapter 4. What is important to note here is the difference in the two stories – one told by the reporter and the other told by the physicist. As the risk traveled from Leake to the readership of the *Sunday Times (London)*, this risk was not the same risk as conveyed by physicist Robert Jaffe. The transmission of risk was not perfect (e.g. interpretation by the receiver was not the same as was intended by the sender). This will be a common trend in the rest of the spreading of risk.

However, from this single *Sunday Times (London)* article sprang forth a multitude of other articles in widely circulated newspapers. Most of the subsequent articles made *some* reference to the original *Sunday Times (London)* article. The subsequent articles were printed in such renowned and widely-read newspapers as *The New York Times*, *The Washington Post*, *The Ottawa Citizen*, *The Gazette (Montreal)*, and *Sydney Morning Herald*. In fact, *The New York Times* had a total of *four* articles on this story. *The New York Times* is seen by many as a gatekeeper for news, since it is so well-respected. Publication in important, established newspapers made the topic newsworthy.

This spreading was like a cascade, originating from the *Sunday Times (London)* article, allowing for the RHIC risk to travel worldwide. Besides many major and local newspapers picking up this story, a number of magazines, scientific journals, radio programs, and television programs all jumped on the RHIC disaster scenarios bandwagon. Scientific articles were written in *Physics Letters B*, *Physics Today*, *The Bulletin of the Atomic Scientists*, and *Science News*. Conspiracy websites found the subject a fresh and rich fodder for theories about John

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[23] Robert Jaffe, Interview (22 November 2002).

F. Kennedy Jr.'s plane crash. There was even a discussion of the story on "ABC World News Tonight with Peter Jennings" (on October 28, 1999).

I consider the *Sunday Times (London)* article, as well as the letters to the editor in *Scientific American*, to be "nodes" for the risk to travel through.[24] Following the paths that the risk spread, in many cases, involved in some way these two nodes. Almost all subsequent references to the RHIC disaster scenarios cited at least one of the two nodes – and incorporated many of the facts and evaluations of risk from these articles.

Public Mistrust – Conspiracy

One extreme symptom of public mistrust of science is the conspiracy theory. In the mainstream press, an anecdote was commonly told: Someone had called the Brookhaven National Laboratory and asked if RHIC had caused a black hole to swallow John F. Kennedy Jr.'s plane on July 16, 1999 – the same day RHIC was being tested for the first time.[25] This "black hole theory" was not the only version of the story. In fact, on July 21, 1999, *Wireless Flash*, a news service specializing in "off-edge pop culture news and entertainment content,"[26] sent out a news release stating

> The official word is that the accelerator is being used to recreate conditions just moments after the universe's 'Big Bang' but others think the device is an experimental weapon. Theorist Ru Mills, (Editor in Chief Rumor Mill News), says it's possible the particle beam was set off deliberately to obliterate the plane and its passengers, which explains why investigators have found no major wreckage.[27]

From this sprang more mainstream newspaper articles, about this conspiracy, taking a more skeptical tone. On the same day as the Wireless Flash news release came an article in a

[24] My concept of "nodes" is similar to social theorist Bruno Latour's conception of "obligatory passage points" in networks of scientific actors. In this view, actors in a field create passage points that one *must* cross in order to gain credibility. In this case, I see a similarity between that and the obligatory works that must be cited in the traveling of an idea. For more on "obligatory passage points," see Bruno Latour, *Science in action* (Harvard University Press: Cambridge, 1987): 181-182.
[25] The experiment itself was not conducted until about a year later, on June 12, 2000.
[26] Wireless Flash, the "pop culture wire for media professions" can be found online at http://www.flashnews.com/. This particular news release is dated July 21, 1999.
[27] **Error! Bookmark not defined.** (Accessed 20 January 2003).

British newspaper website, which enumerated the conspiracy theories surrounding Kennedy's plane crash.[28] In the article, the author Patrick Barkham states that "Some interpretations are predictably outlandish" and then goes on to list RHIC fears, UFO abduction, and US Navy cover-up stories.

Another conspiracy website said that RHIC

> is giving rational, well-employed physicists all over the world some very bad nightmares, and in fact a *London Times* article from July 18 cited a number of these scientists as literally besides themselves with apprehension over potentially inconceivable awesome consequences of this experimentation

adding that "The actual times of the experiments [tests] of the experiments conducted that day were kept strictly secret."[29] The former quotation was contradicted by the actual *Sunday Times (London)* article, which didn't cite a single scientist who was apprehensive about the RHIC experiments. The latter quotation, along with the focus of the webpage (which is "missing"—thus presumably destroyed—radar data), demonstrates the fear that secrecy entails for some members of the public. In this view, experts keep the public from fully understanding the truth of matters – they are portrayed as "mad scientists." The overarching fears of scientific conspiracy and cover-up, especially by the government, have had legitimate groundings in American history (eg. Tuskegee experiment). Therefore, these concerns cannot simply be discarded as wholly irrational – as even the irrational may have a grounding in the rational. These notions of conspiracy are a prime example of "risk appropriation" – where readers incorporated the Brookhaven risk into their worldview and developed a conspiracy theory out of it.

Online Articles – Mad Scientists vs. Credible Researchers

However, it isn't only conspiracy websites dealing with UFOs and government cover ups that include words of apprehension over the Brookhaven physicists and their ability (and

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[28] **Error! Bookmark not defined.** (Accessed 20 January 2003).
[29] **Error! Bookmark not defined.** (Accessed 20 January 2003).

power) to perform the experiment. Fred Moody was a writer for abcnews.com and is the author of two technology books. His columns dealt with science and technology and appeared every other week on abcnews.com. For this Brookhaven incident, Moody devoted two articles: the first presenting his views and the second as a response to all the criticism he received on the first.

The first article, "Atlas Shrugged," appeared online on September 14, 1999[30] (only a few months after the initial concerns were voiced) and espoused the dangers of creating a black hole. Moody first heard about the black hole scenario through an email from friend and "eccentric physicist" (traveling of risk) and then did some more research – stumbling across the July 1999 *Scientific American* letters to the editor and the *Sunday Times (London)* article. Note that we have tracked the journey the risk took – and in fact it went through both nodes (*Scientific American* and *Sunday Times (London)*).

He proclaimed that the *Sunday Times* article "editorialized against the experiment which it considers frighteningly dangerous." However, the article never explicitly railed against the experiment, and in fact stated multiple times that the probability of something bad happening was "infinitesimally small."[31] This is a translation of risk (which we will consider in Chapter 4). This fact would be highly obscured from someone who had read only Moody's piece, and not the original British article.

Moody's opinion on the experiment is one of apprehension of the scientific community. He presents a view of science as a generator of unintended consequences, listing antibiotics (causing resistant bacteria) and the internal combustion engine (causing pollution)

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[30] **Error! Bookmark not defined.** (Accessed 26 January 2003).

[31] In fact, Jonathan Leake, the author of the article, said that he did not intend to make the article alarmist and specifically included quotations from physicists stating the risks were small for that reason (Personal Correspondence, 26 November 2002). This is another prime demonstration of the transmission of knowledge – and how one original portrayal of risks gets lost through "risk appropriation." In other words, Moody read Leake's article and appropriated the risk in a way differently than Leake had intended.

as evidence. And he finds human destruction inevitable – destruction resulting from scientific hubris: "we are essentially playing at being God – an unforgivable offense, punishable... by annihilation." He finds scientists to be a powerful, unchecked minority, playing a role that goes beyond their scope as scientists and humans.[32]

Moody then was prompted to write another article, published online at abcnews.com, defending the first one.[33] Why? The original article manufactured a backlash from the scientific community – with critics accusing Moody of panicking many readers needlessly. Moody's second article supports this claim, as it does cite people who were frightened to the point of asking "How can we stop the Brookhaven thing? Petition, injunction" and "What day in November will the experiment be? Macabre as it may be, I thought it would be fun to have a party the night before."[34]

Moody defended his article from both the scientific and panicked community by simply stating "I have long experience in readers misinterpreting my prose, motives and morals, but I'm consistently surprised at being taken far more seriously than I take myself."[35] In other words, the piece was originally meant to be inflammatory – not to make the point that the specific RHIC experiment was truly bringing forth the end of the world, but rather to underscore the notion that the technological hubris of the scientist could bring forth a real end-of-the-world scenario (and the RHIC experiment is perhaps a harbinger of that possibility).

[32] Mary Douglas comments on this idea of a powerful few and risk: "the generalized tendency of humans turns out to be quite the other way, not naturally timorous but rather overintrepid and difficult to persuade of the reality of dangers. But if the dangers in question are thought to be inflicted by a powerful minority (the industrialists) on a helpless majority, the sense of a subjective immunity is not evoked. The difference is that the attitude to risks inflicted by others is political... Risk perception may not be the issue at all, but indignation at bamboozlement and exploitation." Quoted in Mary Douglas, *Risk acceptability according to the social sciences* (Russell Sage Foundation: New York, 1985): 33-34.

[33] **Error! Bookmark not defined.** (Accessed 26 January 2003).

[34] Ibid.

[35] Ibid.

This second article has a twofold importance. First, it shows that risk is a battlefield (see Chapter 5) – it gets debated within and without the scientific community. It is the outcome of this battle which allows or disallows action to take place – and as such, the battle is important. Second, it shows risk as a travelling entity. Besides positing the risk in a popular media forum, with the potential for others to read and write about the risk, the second article has demonstrated how potent this forum is; Moody received letters from scientists and laypersons from his first article, enough to justify writing an addendum article. The risk traveled from Moody to readers. Those that read the article took the information in it away with them in some form or another. They believed all of it, believed none of it, or believed some of it.

There was another RHIC article posted on an Internet website that garners a sizable audience, **Error! Bookmark not defined.**. The article, titled "Big Bang Machine Gets Down To Work,"[36] was published on June 14, 2000. This is almost an entire year after the *Scientific American* letters to the editor, which introduced the risk to the world. The article was written to inform the audience about the first images of particles colliding in RHIC – after the experiment had been performed. One section of the article, however, dealt with the set of catastrophe scenarios, but cited two scientists who dismiss the scenarios. A PHENIX email news list (PHENIX is one of the detectors used to detect particle collisions at RHIC) sent out a link to this story to the Brookhaven personnel on the list with the commentary that the article was "At last – a balanced and reasonable news report on RHIC."[37] This is in gross contrast to the Moody article, which received numerous objections from scientists. In the next chapter, we will explore the clash between scientific and outside communities in more detail – the msnbc.com article was one of the few that the scientific community found to be

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[36] **Error! Bookmark not defined.** (Accessed 26 January 2003).

fair in its description of the risks associated with the RHIC experiment. The rest, like Moody's piece, was viewed as irresponsible journalism.

Conclusion

In this chapter, we have seen risk as being born and travelling from locale to locale, shifting in its tone and form. The RHIC risk was born out of a concern voiced in one magazine article and gained popularity through a prominent article published in the *Sunday Times (London*). From that article onwards, there was a media cascade. Newspapers all around the United States (and even in other countries) picked up on this story and spread their version of the Brookhaven risks to their own audience. Other forms of media, including websites, also jumped on the Brookhaven bandwagon, printing both conspiracy-based and fact-based versions of the story.

The author of each article had a choice on how to present the risk – based on the information they were presented and their own personal convictions. Not all authors presented the same view of the risk. This is to be expected. Some were physicists, some were science-writers, and some had very limited knowledge about the physics girding the experiment. Each author held different beliefs about science progress. Some daring authors had an alarmist attitude regarding the risk while others presented a negligible risk. This dramatic difference in presentation occurs for various reasons – and we will consider these reasons and differences in more detail in Chapter 4, where we examine various newspaper accounts of the Brookhaven experiment and associated risks.

This version of the spread of the Brookhaven risk does more than show that the risk became a popular media subject burgeoning from a pair of letters to the editor in a popular

[37] **Error! Bookmark not defined.** (Access 26 January 2003).

science magazine. It demonstrates the transmission capabilities and malleability of risks, of facts. We are, in the media, presented risks as facts, which according to Dumit,

> typically imply relationships between things that are not bound to time and space and culture; they simply are. But facts are not untethered. They are facts-in-the-world. One task is to understand how the meanings of facts change—how we are never handed facts but are continually faced with facts-in-the-world and continually judge their status and relative worth for ourselves. Facts are bits of mastery in expert culture. Expert culture is about being extremely knowledgeable about a very few things. We each know very little about most things, and in their entirety the facts are beyond reach.[38]

This is a broad view of facts as socially constructed entities. In characterizing risks as types of facts, we can see how Dumit's view of facts comes to life with the Brookhaven risk. It is apparent that the risks are presented in different ways in different media. And it is also apparent that the risks do not lie in vacuo. They are presented to the wide audience by travelling from locale to locale, and thus have become "risks-in-the-world." These risks are to be appropriated (and perhaps used) by the reader (as in Moody's piece where one reader wanted to create a legal injunction against the laboratory) or by other authors in the writing of their own articles. Through these "risk appropriations", we have seen on a basic level how the risk does not take on one single form, but rather is presented in a multitude of different ways. The risk cannot be considered, then, a single risk. Instead, it must be viewed as a series of different risks based on the same situation, which in their travels, shift in meaning. It is in this way that we can consider risks as socially constructed – based in some fundamental sense on the authors' beliefs.

This is a key concept – because it brings risks away from the normal probability interpretation into the social realm. Harking back to the french fry analogy – we saw that risk was more than a mere number. It is an *interpretation of facts*. People have a hand in shaping risks in the ways they interpret the data, whether the data comes from newspaper articles, scientific articles, or interviews. (This evaluation of the data, then, becomes the risk.) As the

[38] Dumit, "A digital image of the category of the person": 86.

information about the "risky" situation travels, so does that particular interpretation of the data.

Chapter 3: Calculations and Scientific Presentations of Risk

Introduction

On June 10, 1979, a report titled *LA-602: Ignition of the Atmosphere with Nuclear Bombs*[39] was declassified by the United States government. The report addressed the concern that an atomic or hydrogen bomb might instigate a self-propagating nuclear chain reaction in the atmosphere. Needless to say, this report is a risk analysis of the destruction of humankind made possible by an advance in technology. This is similar to a report released on September 28, 1999 by the Brookhaven National Laboratory discussing the various disaster scenarios conjured by the use of the Relativistic Heavy Ion Collider (RHIC).

In this chapter, I consider both reports side-by-side. Though differing in their content and historical contexts, both grapple with the presentation of catastrophic risk from the *scientific* perspective. By examining both scientific analyses of catastrophic risk together, I hope to understand risk in a richer way. With these two reports I will show that the *presentation of risks is intimately related to risk itself.* In fact, the presentation of the risk, in some sense, partly defines the risk. (In chapter 4, I discuss how and why the presentation of risk translates from medium to medium). Through the examination of both reports of risk, the methods and conventions used by scientists when analyzing risk will be exposed. By picking apart what calculations are done and how they are presented, one can get a better sense of how the scientific community understands catastrophic risks.

History of LA-602

In Berkeley in the middle of the summer of 1942, Robert Oppenheimer was working with highly regarded physicists Edward Teller, Hans Bethe, Felix Bloch, Emil Konopinski, and John H. Van Vleck, among others, on the early stages of a fission bomb. At this time, Teller and Konopinski were working together in an effort to prove that a fission bomb could not be used to ignite a thermonuclear reaction in deuterium[40]. Teller stated:

> we tackled the job of writing a report to show, once and for all, that it could not be done.... But the more we worked on our report, the more obvious it became that the roadblocks which I had erected for Fermi's idea were no so high after all. We hurdled them one by one, and concluded that heavy hydrogen could be ignited by an atomic bomb to produce an explosion of tremendous magnitude. By the time we were on our way to California... we even thought we knew precisely how to do it.[41]

However, Teller pointed out that reactions other than between deuterium and deuterium were possible and that either a fission bomb or a hydrogen bomb might ignite the earth's oceans or atmosphere. In other words, with his voiced concern, Teller brought a catastrophic risk into the world. Oppenheimer was affected enough by this potential risk to request further calculations.

Teller's concerns did not resonate with the other physicists in Oppenheimer's group, with Bethe commenting "I didn't believe it from the first minute."[42] Bethe went through Teller's calculations and discovered unjustified assumptions; Teller was soon convinced by Bethe's arguments. In Richard Rhodes' *The Making of the Atomic Bomb*, one physicist recounted the way with which the risk was dealt:

> together with new difficulties, new solutions emerged. The discussions became fascinating and intense. Facts were questioned and questions were answered by still more facts.... A spirit of spontaneity, adventure, and surprise prevailed during those weeks in Berkeley, and each member of the group helped moved the discussion towards a positive conclusion.[43]

The final report *LA-602*, written by Teller, Konopinski, and Marvin for Oppenheimer, documented the logic used to come to the conclusion that fission and hydrogen bombs would not ignite the atmosphere.

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[39] E. Konopinski, C. Marvin, and E. Teller, *LA-602: Ignition of the atmosphere with nuclear bomb* (14 August 1946). Declassified, housed by the U.S. Department of Energy.
[40] Deuterium is an isotope of hydrogen which contains one proton and one neutron in its nucleus.
[41] Quoted in Richard Rhodes, *The making of the atomic bomb* (New York: Touchstone, 1986): 416-417
[42] Rhodes, *The making of the atomic bomb*: 418
[43] Rhodes, *The making of the atomic bomb*: 419

Basic Arguments in LA-602

The central question considered in *LA-602* was whether a self-propagating chain of nuclear reactions could be started with the detonation of either fission or thermonuclear bombs. The answer, given in the abstract, was clear: "It is shown that, whatever the temperature to which a section of the atmosphere may be heated, no self-propagating chain of nuclear reactions is likely to be started."[44] More important, though, to this discussion of risk is to understand *how* scientists determined the risk was negligible.

In order to quantify the risk, the authors first determined that the cause of the potential risk, at the most elementary level, was high temperatures which could create a sustained reaction between atomic nuclei in the atmosphere. Reactions between two nitrogen nuclei, as well as nitrogen nuclei and protons, were the only reactions considered because of nitrogen's property as the most unstable common constituent in air, thus making it more likely to be involved in chain reactions.

Various nitrogen-nitrogen reactions were listed, and the reaction with the second most energy released was taken to be standard for all nitrogen-nitrogen reactions. This reaction, $N_7^{14} + N_7^{14} \rightarrow Mg_{12}^{24} + \alpha + 17.7$ Mev, had been chosen over the reaction which releases 26.7 Mev of energy because of the infrequency of that reaction and the fact that the energy released in that reaction was in the form of gamma rays[45] (which would not propagate a chain reaction).

Two different assumptions were made at this point, early in the report, and both were examined concurrently in all subsequent calculations; both assumptions were for the geometric cross section for nitrogen encounters. The first one stated that the geometric

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[44] *LA-602*: 2.
[45] A form of high energy electromagnetic radiation.

cross section[46] for nitrogen encounters was a constant 2 barns (denoted σ_k), while the second assumption stated that the geometric cross section for nitrogen encounters was exponential under some fundamental threshold and a constant 2 barns beyond that threshold (denoted σ_G). With these two assumptions, parallel calculations were made. So, for example, if one calculation utilized the cross section of nitrogen, two results were given: one in terms of σ_k and the other in terms of σ_G. These two assumptions were made because at that time *the cross section for nitrogen encounters was not known from observations*. To mitigate this lack of knowledge, the authors adopted the notion that a *geometric* cross section could be utilized instead.

The nitrogen-proton reaction was discounted as unable to produce significant amounts of energy for three reasons: (1) the cross section of the proton-nitrogen reactions should be similar to nitrogen-neutron cross sections which have been measured (and at peak value are only about one-eighth of a barn), (2) the energy yield from the proton-nitrogen reaction is only about one-eighth of that found for nitrogen-nitrogen reactions, and (3) the low abundance of protons that would be available to produce these reactions.

The rate of energy production, calculated from the two geometric cross section assumptions, was considered for nitrogen-nitrogen reactions. It follows that the rate of energy *loss* in nitrogen-nitrogen reactions should be considered also. The main source of

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[46] A cross section is the probability that an interaction will occur between a projectile particle and a target particle. Physicist Rudolf Peierls explains this in Rhodes, *The making of the atomic bomb*, on 282: "For example, if I throw a ball at a glass window one square foot in area, there may be one chance in ten that the window will break, and nine chances in ten that the ball will bounce. In the physicists' language, this particular window, for a ball thrown in this particular way, has a 'disintigration cross section' of 1/10 square foot and an 'elastic cross-section' of 9/10 square foot." The unit of measure for cross section is the barn, roughly equivalent to the size of a uranium nucleus, 10^{-24} cm^2.

energy loss results from Bremsstrahlung[47] (denoted $(dE/dt)_B$ in the paper). A "safety factor" S was thus defined:

$$S = |dE/dt|_B \;/\; (dE/dt)_G$$

where $(dE/dt)_G$ was the rate of energy production by nitrogen-nitrogen reactions based on the second assumption. As long as the energy loss was greater than the energy production, no ignition point existed. In other words, if S was less than 1, then more energy was being produced than lost in each reaction, so a chain reaction had the possibility of occurring. A plot of S was generated and contained a minimum value of approximately 1.6. Propagation of the reaction throughout the entire atmosphere was deemed unlikely.

The original low safety factor of 1.6 was not "safe" enough for the authors. What if there had been some mistake or unwarranted assumption in the analysis? A second analysis, independent of the first, was performed. The goal now was to determine how large a volume would need to be heated up, and to what temperature, to allow for a sustained chain reaction in the atmosphere. In this analysis, the energy would transfer from one atom in the atmosphere to the next via 15 Mev α particles[48] (these particles were chosen because they are the most energetic particles produced in the air). An "energy-loss cross section" per atom was calculated to be about 3.5 barns, which corresponds to a mean free path in air of 57 meters. Thus, "[o]nly a sphere of 57 meters radius can retain a substantial part of the energy produced. At least such a volume must be heated in order that the thermonuclear reaction be sustained."[49] Heating such a volume to a nuclear temperature of 10 Mev would require, at best estimates, 1.5×10^6 kg of fissionable material, or an 8-meter-radius sphere of liquid deuterium.

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[47] Bremsstrahlung is radiation produced by decelerating electrons, since it is known that energy is emitted by an accelerating particle (proportional to $1/m^2$, where m is the rest mass of the particle).
[48] Alpha particles (α) are helium nuclei with two protons and two neutrons.

One last caveat was made: instead of the nitrogen-nitrogen reactions which produce 17.7 Mev of energy, the authors noted that if most of the reactions in the air took the form N + N → O + C + 10.6 Mev, the radius needed to be heated would be merely 7 meters, and much less fissionable material or liquid deuterium would be needed. This was due to the short ranges of the oxygen and carbon nuclei (being much more massive, they do not travel as far as alpha particles in reactions). In this case, the only "saving grace" was that only about 60% of the energy (10.6 Mev rather than 17.7 Mev) was produced in each nitrogen-nitrogen reaction, which increased the minimum safety factor *S* from 1.6 to 1.6/0.6=2.67. This meant that even if a lot of oxygen and carbon had been produced, the prior calculations done assuring that more energy would be lost than produced were made even stronger.

The final conclusion from the entire set of analyses was stated less strongly than in the abstract:

> There remains the distinct probability that some other less simple mode of burning may maintain itself in the atmosphere. Even if the reaction is stopped within a sphere of a few hundred meters radius, the resultant earth-shock and the radioactive contamination of the atmosphere might become catastrophic on a wold-wide scale. One may conclude that the arguments of this paper make it unreasonable to expect that the N + N reaction could propagate. An unlimited propagation is even less likely. However, the complexity of the argument and the absence of satisfactory experimental foundations make further work on the subject highly desirable.[50]

From this conclusion, it is apparent that there was a hesitation on the authors' part to make a definitive statement ruling out any sort of chain reaction.

History of Review of Speculative "Disaster Scenarios" at RHIC

With the publication of Walter Wagner's letter to the editor in the July 1999 issue of *Scientific American*, the risk that "mini black holes could be created by smashing a proton into an antiproton with enough energy... devouring the entire planet within minutes"[51] was introduced to the world. Frank Wilczek, then at the Institute of Advanced Study, responded

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[49] *LA-602*: 12
[50] *LA-602*: 17

in a letter dismissing Wagner's black hole speculations, but tacked on an additional risk: "there is a speculative but quite respectable possibility that subatomic chunks of a new stable form of matter called strangelets might be produced... a strangelet grows by incorporating and transforming the ordinary matter in its surroundings."[52] With this reply, Wilczek had introduced another risk into the world.

With the media frenzy that followed, the Brookhaven National Laboratory was receiving negative attention from the public. John Marburger, the director of BNL at the time, had been in communication with Robert Jaffe, the director of the Center for Theoretical Physics at MIT. Jaffe said:

> I was giving advice to Marburger in conversations on the telephone... So in the spring of 1999 I was talking to Jack [Marburger] a few times trying to help give him scientific advice for dealing with conversations with people like Walter Wagner... And then in the early summer, he asked me if I would chair a committee that would do a more formal review.[53]

Besides selecting Jaffe, a pioneer in the theory of strange matter, as head of the committee, Marburger also asked Frank Wilczek, Wit Busza, and Jack Sandweiss to serve as members. Wilczek, a theorist, was chosen by Jaffe and Marburger to "pay the wages of his sin since he's the one that started this all with his letter," while Sandweiss and Busza were experimenters who were chosen because they were "very responsible, thoughtful, contributing members of the community who [took] the time to work on this because they have such a deep respect for Brookhaven."[54]

The committee communicated in groups of twos and threes, mainly on the telephone. Wilczek and Jaffe were in charge of the calculations, which were then checked by experimentalists Busza and Sandweiss (who, in addition, provided practical estimates). There was mainly agreement on the work done in the committee, according to Jaffe, and a general

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[51] Walter Wagner, "Black holes at Brookhaven?" *Scientific american* **281** (July 1999): 8
[52] Frank Wilczek, "Black holes at Brookhaven?" *Scientific american* **281** (July 1999): 8
[53] Interview with Robert Jaffe (22 November 2002)

attitude of bemusement at the topic being worked on. As Jaffe explained in an interview, "No scientist who understood the physics thought that this experiment posed the slightest threat to anybody."[55]

The committee's work started during the spring of 1999 and was completed that summer. The final report was six pages long with 20 pages of appendices and references. Titled *Review of Speculative "Disaster Scenarios" at RHIC*[56], it was released to the general public on September 28, 1999, and was later expanded into an article published in the *Reviews of Modern Physics* in October of 2000.[57]

Basic Arguments in Review of Speculative "Disaster Scenarios" at RHIC

The authors began the report by noting both the fear of a "catastrophic process with profound implications for health and safety" and the purpose of the report, which was to "address the safety of each of the speculative 'disaster scenarios'"[58]. The three scenarios considered in the paper were (1) the formation of a black hole or gravitational singularity, (2) initiation of a transition to a lower vacuum state, and (3) the formation of a stable stranglet which accretes ordinary matter. The conclusion from the report was listed in the introductory paragraph, stating that these disaster scenarios were "firmly excluded."

The black hole scenario was dismissed by stating that the strength of gravity in the RHIC environment is 10^{-22} for classical effects and 10^{-34} for quantum effects (where 1 represents gravitational effects as the nuclear force). In simpler terms, the nuclear force is many orders of magnitude stronger than the other forces which acting at the atomic scale. The derivation for these numbers was included in Appendix A and pointed out that the

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[54] Interview with Robert Jaffe (22 November 2002)
[55] Interview with Robert Jaffe (22 November 2002)
[56] W. Busza, R.L. Jaffe, J. Sandweiss, and F. Wilczek, *Review of speculative "disaster scenarios" at RHIC* (28 September 1999). The report can currently be found at the RHIC webpage (**Error! Bookmark not defined.**)
[57] R.L Jaffe et. al. "Review of speculative 'disaster scenarios' at RHIC," *Reviews of modern physics* **72** (October 2000): 1125-1140.

strengths of gravity were *bounded* by these miniscule values, because many other factors which would further resist the collapse into a black hole (such as momentum and electric charge) were not taken into account.

The vacuum instability scenario was explained in layperson's terms. A vacuum (empty space) can exist in various states, and some physicists (even though "certainly nothing in our existing knowledge of the laws of Nature demands it"[59]) have conjectured that the vacuum in the universe now is metastable. If this were true, metastability implied that something could cause the vacuum to change states. The evidence against this came primarily from cosmic rays. Cosmic rays are particles of high energies, travelling in outer space, and they have collided numerous times in the history of the universe.

To quantify this, the authors of the report relied on a paper by Hut and Rees to estimate the number of cosmic ray collisions which Earth could have "experienced" (i.e. noticed the effects of). The formula was

$$N \sim 10^{47} \, (f(\mathrm{A})/f(\mathrm{Fe}))^2 \, (56/\mathrm{A})^{2.7} \, (100 \text{ GeV} / \mathrm{E})^{3.4}$$

where 10^{47} is the number of iron-iron collisions at a center of mass energy exceeding 100 GeV/nucleon, N is the number of collisions, $f(\mathrm{A})$ is the fractional abundance of a nucleus with atomic mass A in high energy cosmic rays, and E is the energy per nucleon (proton or neutron) in the center of mass frame. The authors were forced to estimate $f(\mathrm{A})$ due to a lack of experimental measurements on the fractional abundance of elements in cosmic rays heavier than iron with energy greater than 100 GeV/nucleon; they used gold at lower energies because a good amount of data had been taken. The resulting value for the number of collisions N, even if $f(\mathrm{A})$ had been off by orders of magnitude, "is far greater than the

[58] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 1
[59] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 2

total number [of collisions] anticipated at RHIC."[60] Thus, if the universe were to switch states of vacuum because of high energy particle collisions, it would have done so by now.

Lastly, and covered in the most depth in the report, was the notion that strange matter could be created in the collision. Again, this scenario was dispelled. Four conditions, each emphasized as unlikely, were listed that would have to be met simultaneously in order to allow the creation of dangerous strange matter[61]. If it existed, dangerous strange matter would turn all in contact with it to strange matter also. This theoretical framework is based on the theory of quantum chromodynamics (QCD).

But even if these theoretical arguments concerning strange matter were unconvincing to readers, the authors included natural "experimental" evidence: cosmic rays colliding with the Moon. The authors used an argument similar to the vacuum scenario: if 10^{28} collisions between iron nuclei (a reasonable approximation for gold nuclei) have occurred in the history of the Moon, and at most 2×10^{11} gold-gold collisions will occur at RHIC in 10 years, then "production of one dangerous strangelet at RHIC would lead us to expect production of order 10^{17} on the Moon over its lifetime."[62] In other words, strange matter would have already appeared on the Moon if it were manufactured in RHIC-style heavy ion collisions, and if the strange matter produced was "dangerous," the Moon as well as Earth would have been converted into strange matter by now.

The last matter the report deals with is a paper by Dar, De Rujula, and Heinz, published in *Physics Letters B*, a highly-respected physics journal.[63] This paper made weak assumptions regarding the production of strangelets that Jaffe (and the other authors) felt

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[60] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 3

[61] Even if strange matter existed, only stable, *negatively* charged stranglets would be dangerous – positively charged strangelets are innocuous. An atom which came in contact with a negatively charged strangelet would be absorbed by the strangelet, converting it to strange matter.

[62] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 5. Strangelets are small lumps of strange matter.

inappropriate and "ad hoc." Still, in Appendix C, this extreme view was considered. According to Dar, et al., the probability *p* that one dangerous strangelet will be produced at RHIC was of the order of 5×10^{-12}. This value was used to calculate the number of dangerous stranglets produced per cubic centimeter in the galaxy: $10^{-41}p$. In addition, Dar, et al., used the fact that 10^{57} cm^3 of matter are used in the formation of a star. This meant that the probability a star would end up with a dangerous strangelet in it was approximately $10^{16}p$ – and this star destruction would be similar to a supernova explosion. Comparing $10^{16}p$ to the observed rate of supernova explosions show *p* to be less than 10^{-19}, a far cry from Dar, De Rujula, and Heinz's value of *p* as 5×10^{-12}. Thus, to the investigative committee called by Marburger, there was a large safety margin for RHIC collisions.

Presentation of Risks: Methods of Assessment

Perceived risks do not exist independently of human perception. The ignition of the atmosphere was not a risk-in-the-world until Edward Teller made it so by voicing his concern to the gathered physicists at Berkeley. If Teller or another physicist had never been struck with the idea, or if he had never passed it on, the destruction of the atmosphere would not have been a possibility in the physicists' minds. Similarly, if Walter Wagner and Frank Wilczek had never communicated their concerns (via the letters to the editor in *Scientific American*), the Brookhaven risk-in-the-world would have never been "born" or spread. Thus, necessarily, a risk-in-the-world is a human construction which travels.

In this way, the introduction of risk to others becomes a *risk-in-the-world*. This initial conception of risk then migrates from person to person, medium to medium, and with each appropriation, the risk is translated and transformed. The risk, originally a single conception, becomes multiple conceptions of the same event. In this chapter, we view risks not as they

[63] A. Dar, A. De Rujula, and U. Heinz, "Will relativistic heavy-ion colliders destroy our planet?" *Physics letters B*

are appropriated by scientists and researchers themselves (see Chapter 5 for a discussion of the scientific community's view of the Brookhaven risk), but how they are presented in texts written by scientists.

I chose to juxtapose *LA-602* with the investigative committee's review of the RHIC disaster scenarios for two primary reasons. First, this juxtaposition will show the shared tactics used by some scientists when investigating and presenting their research on catastrophic risks. Second, by examining both texts, the effect of having differing audiences will further our understanding of how scientists present their view of risk. The truly social nature of risks will be revealed.

Both reports are an attempt to dismiss, or at least downplay, the danger of catastrophic disaster. The scientists had already made their calculations and come to their conclusions before writing the final report. The reports themselves provided a means for the scientists to concretize their convictions by *presenting* their views about the "true" risk in detonating a fission bomb or colliding high energy gold ions. The underlying implication in both reports is that these reports are backed up by the authority of science, and thus the conclusions drawn should be believed.

The authority of science, though, does not mean that the science underlying the reports is exact and completely understood. As social theorist Theodore Porter elaborates:

> Mapping the mathematics onto the world is always difficult and problematical. Critics of quantification in the natural sciences as well as in social and humanistic fields have often felt that reliance on numbers simply evades the deep and important issues. Even where this is so, an objective method may be esteemed more highly than a profound one. Any domain of quantified knowledge, like any domain of experimental knowledge, is in a sense artificial.[64]

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470 (December 1999): 142-148.

[64] Theodore Porter, *Trust in numbers: The pursuit of objectivity in science and public life* (Princeton University Press: Princeton, 1995): 5-6.

We will see the ways that these evaluations of risk, these mapping of mathematics onto the world (or perhaps mapping the world onto mathematics is more appropriate), are artificial (read: constructed via scientific procedures to mitigate uncertainties).

Both evaluations of risk involve a number of unknowns and uncertainties. In *LA-602*, these include: knowledge of the processes which deal with energy production rate and radiative loss rate in reactions, the atmospheric reactions to consider which could allow for a chain reaction, the cross section for nitrogen-nitrogen reactions, and the purely nuclear scattering of alpha particles by nitrogen. Brookhaven's report contained uncertainties in: theoretical knowledge of the stability of the current vacuum state, the measurements of fractional abundance of elements heavier than iron in cosmic rays with energy greater than 100 GeV/nucleon, experimental evidence of the existence of stable strange matter, the "bag constant"[65] of strange matter, and the knowledge of mechanisms for strangelet production in high energy heavy ion collisions.

These unknowns and uncertainties are scientific gaps which are *necessary* for the creation of risk, because they allow for non-predictability (however small) in the outcome.[66] If the gaps in knowledge were not present, a risk would not be a risk, but a certain danger. However, when dealing with a field, replete with bizarre phenomena and extreme calculations, like particle physics, certainty is rarely present – in the theories, experimental results, and even in the fundamental structure of the theoretical model.[67]

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[65] The report explains, "The overall energy scale of strange matter is determined by the confinement scale in QCD [Quantum Chromo Dynamics] which can be parameterized by the 'bag constant.'" (Busza et. al, *Review of speculative "disaster scenarios" at RHIC*: 13). More technically, it is the difference in energy densities of the vacuum state near quarks and gluons (particles passed between quarks) and the vacuum state absent of quarks and gluons.

[66] According to Mary Douglas, "Knowledge always lacks," quoted in Mary Douglas, *Risk and blame* (Routledge: New York, 1992): 9.

[67] One of the basic tenets of quantum mechanics is that *all* calculations in modern physics are necessarily probabilistic.

These unknowns and uncertainties *had* to be addressed in order for the authors to present a comprehensive analysis. Otherwise, these reports would appear as indefinite. The three ways these uncertainties were addressed were common to both reports: a heavy reliance upon estimates and approximations, a combination of theory and experimental evidence to fill in gaps of knowledge, and the use of a "worst case scenario" analysis. The end result of these tactics was a seemingly complete and convincing examination of the scenarios.

Both in the theoretical and experimental realms, gaps in knowledge or complication of calculation create a reliance on estimates and approximations. For example, in the Brookhaven report, the authors had no data concerning the fractional abundance of elements heavier than iron in high energy cosmic rays. To bypass this, the authors utilized the fractional abundance of gold at low energies, which is known well. Then, the lower energy values were extrapolated "to higher energies using standard scaling laws, which agree excellently with available data."[68] In Teller's report, many approximations were made. For example, to bypass not knowing the cross section for nitrogen encounters, an approximation of the geometric cross section was made. In addition, in the formula describing the energy loss due to Bremsstrahlung, an approximation was made for the relativistic correction factor $1+E/mc^2$, amended in a footnote:

> The correct formula gives a more rapid increase of radiation with energy than given here. The added radiation is difficult to take into account, especially since the exact amount depends on screening effects... In any case, the additional radiation is negligible until the electron temperature surpasses 0.7 Mev. But it will be seen that such electron temperatures cannot be expected until nuclear temperatures ~40 Mev are reached.[69]

In other words, an approximation was used for ease, but the authors showed this approximation was valid because of later conclusions drawn. In these examples, justification

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[68] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 8.
[69] *LA-602*: 9

for the approximation had been provided; this is because these approximations and estimates were *judgements*. The authors had a choice – what form of approximation to use, what degree to approximate to, or even to approximate at all. This choice, and their beliefs in the validity of the choice, formed the authors' judgement.

Two more examples, both of which are not involved in calculation of the overall risk, involve the application of formulae that are not valid for variables outside certain regimes. The authors of *LA-602* used the *non-relativistic* formula for Compton loss for a temperature of 400 Kev, justifying it by calling it "a crude estimate."[70] In the same fashion, the authors used Chapman-Jouguet relations for the velocity of a detonation wave[71], even though they stated explicitly "It is not clear that the Chapman-Jouguet relations is applicable in the present case."[72] In these examples, the scientists were acknowledging that the use of approximation was questionable – this is their *judgement* of the approximation.

These judgements, besides creating what Porter calls an artificial domain of knowledge, provide credence to the results:

> A decision made by the numbers (or by explicit rules of some or other sort) has at least the appearance of being fair and impersonal. Scientific objectivity thus provides an answer to a moral demand for impartiality and fairness. Quantificiation is a way of making decisions without seeming to decide.[73]

The numerical approximations kept the analysis quantified, and this assisted in preserving the appearance of objectivity. Quantificiation (formulae, numbers) swept the idea that choices were made by physicists under the proverbial rug: judgements were no longer judgements but certainties, created via a rule-bound scientific edifice that provided a systematic way to get rid of unknowns (approximations, extrapolations, simplifications).

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[70] *LA-602*: 16. This Compton loss refers to the inverse Compton effect, which is when net energy is transferred from a high energy electron to a photon.
[71] The Chapman-Jouguet theory describes a detonation wave propagating with a certain velocity.
[72] *LA-602*: 16
[73] Porter, *Trust in numbers*: 8.

Another tactic used by the authors of both reports was juggling both experimental and theoretical evidence; when knowledge of one was lacking, the authors often turned to knowledge of the other. This occurred most frequently in the Brookhaven report, because knowledge of strangelets was so limited. The knowledge of strange matter was confined to the theoretical, since "experimental physicists have searched unsuccessfully for stable or quasi-stable strangelets over the past 15 years."[74] Theoretical calculations did not exclude the possibility for stable strangelets outright, so the authors turned to "natural" experimental evidence with high energy heavy ion collisions on the Moon to discount the possibility of RHIC producing stable strangelets. Such "natural" evidence (in this case, with the rate of supernova explosions) was also used to discount Dar, De Rujula, and Heinz's paper. In *LA-602*, experimental results for values such as energy released in certain reactions and various cross section values were used *within* a theoretical framework. These values, derived from experiment, were plugged in theory-based equations. In sum, there was a mixing of *kinds of knowledge* being brought to the table, which was a tactic used in the reports to create a more complete picture of the risk.

Lastly, and most striking in both reports, was the use of a "worst case scenario" type of analysis. This feature of the analysis was stressed throughout the entirety of both reports. This type of analysis involved choosing, when uncertainty existed, the "worst case" value which made sense physically. For example, it was believed that only 1% of a fission bomb's energy would be available to produce a change in temperature, but *LA-602* assumed the "worst case" – that the all the energy produced went to raising the air temperature. The report was explicit about this method: "Because of the uncertainties in the knowledge of these processes [energy production and loss], the policy should be adopted of exaggerating

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[74] Busza et al. *Review of speculative "disaster scenarios" at RHIC*: 14

the dangers at any point which appears at all questionable."[75] This was done, so in the report when the safety factor *S* had a minimum of 1.6, that was merely a very robust *lower bound* for the true safety factor, because of the "worst case" assumptions built in the calculations. The Brookhaven report was less direct about explicitly stating their "worst case" approach, but made the method clear with statements such as "[e]ven if this estimate were off by many orders of magnitude," "[w]e are being extremely conservative by choosing the largest possible mass and the smallest possible distance scale defined by the collision," and "to be conservative we will usually take $\gamma = 2.7$." Jaffe and the other authors of this report found that the true "worst case" limit, even more drastic than their conservative estimates, was provided by the Dar, De Rujula, and Heinz paper, which abandoned some of the authors' most basic assumptions about strangelets. In the report, though, even this extreme "worst case" limit was shown to not provide substantial evidence for the creation of dangerous strangelets.

The worst case analysis was used to show something was *not probable*; it was an effective technique because the uncertainties were eliminated with exaggerated "educated guesses." When considering an event with dire consequences such as the destruction of the Earth, calculating the risk cautiously is important. However, with the "worst case" approach, not only is safety demonstrated, but there is an additional invisible, incalculable buffer zone of safety provided. This creates a sense of additional safety in the evaluation of risk for the reader, even though it was not quantified.

Presentation of Risks: Audience

The presentation of risk alters based on the audience intended to read the risk. Obviously, if an audience is not fluent in the erudite science being examined, or cannot

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[75] *LA-602*: 3

logically translate a probability into meaning in the real world, then a scientific explanation of risk is lost on them. In that case, the authors presenting a risk must turn to other tools to explain the risk (see chapter 4). However, in the case of *LA-602* and the Brookhaven report, the audiences were informed. *LA-602* was written for physicists. In fact, the document was classified, and kept from the general public's eyes, until 1979. In contrast, the Brookhaven report was written both for physicists *and* the general public. The technical version of the report, for the expert audience, was written and published in the *Reviews of Modern Physics* later. The Brookhaven report was written to be a convincing reply to Walter Wagner and the unbridled media frenzy:

> I think [the report] was an attempt to take seriously the fears of science that they [the public] don't understand, and to try to provide them some reassurance that things they don't understand are not being controlled by a conspiracy.[76]

To put it another way, the Brookhaven report was used partially to allay public fears about the abuse of modern science and technology.[77] The difference in audience and purpose affected the way these two reports were written.

The primary difference between the two reports is the fundamental structure. The Brookhaven report consisted of six pages of explaining why one could consider the risk negligible. It was then followed by eighteen pages of appendices, fleshing out some of the actual mathematics and higher-level physics used to derive the general conclusions listed in the report itself. The information in the appendices, being relegated to the end of the report, was implicitly deemed peripheral. The audience need not understand the appendices to understand the scientific conclusions drawn regarding the danger of the RHIC experiment. In fact, the black hole disaster scenario was given only five sentences of discussion in the

[76] Interview with Robert Jaffe (22 November 2002)
[77] Sheila Jasanoff has found in her analysis of risk management that "Techniques such as cost-benefit analysis and risk assessment make it easier to reassure critics within and outside government that policy decisions are

report itself, excluding Appendix A. On the other hand, *LA-602* did not contain appendices, and conclusions were drawn in the text only after the physical and theoretical evidence had been presented.

Another difference between the two reports lay with the scientific explanation of phenomena. *LA-602* did not define physics terms like "cross section," "Bremsstrahlung," "Hankel functions of the first kind," and "relativistic Maxwell distribution." The intended audience, physicists, already knew and understood what these terms meant. On the other hand, the Brookhaven report defined such terms as "past light cone" and "strangelets" in the report itself (this was not so in the appendices)[78]. Also, non-technical words were used in place of a more technical explanation, such as with the phrase "stopping power." Lastly, the Brookhaven report constantly made analogies to more commonplace phenomena:

> The total center of mass energy (E_{CM}) of gold-gold collisions at RHIC will exceed that of any existing accelerator. But E_{CM} is surely not the right measure of the capacity of a collision to trigger exotic new phenomena. If it were, a batter striking a major league fastball would be performing a far more dangerous experiment than any contemplated at a high energy accelerator.[79]

> Physicists have grown quite accustomed to the idea that empty space – what we ordinarily call 'vacuum' – is in reality a highly structured medium, that can exist in various states or phases, roughly analogous to the liquid or solid phases of water.[80]

This use of analogy made abstract physics concepts accessible to those not trained in the field. The difference in audience shifted what and how the authors wrote about physics.

The differences in audience manifested themselves in the conclusion of the risk; the risk in *LA-602* was presented cautiously while the risk in the Brookhaven report was presented as definitively absent.

> There remains the distinct probability that some other less simple mode of burning may maintain itself in the atmosphere... the complexity of the argument and the absence of satisfactory experimental foundations makes further work on the subject highly desirable.[81]

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being made in a rational, nonarbitrary manner." Quoted in Sheila Jasanoff, *Risk management and political culture* (Russell Sage Foundation: New York, 1986): 30.

[78] The appendices assume a higher level of physics understanding, with physics formulae and terms frequently utilized without much (if any) explanation.

[79] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 1

[80] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 2

> It is indeed fortunate that they [dangerous strangelets] *will not be produced* at RHIC.[82] [emphasis added]
>
> Our conclusion is that the candidate mechanisms for catastrophic scenarios at RHIC are firmly excluded by existing empirical evidence, compelling theoretical arguments, or both. Accordingly, we see no reason to delay the commissioning of RHIC on their account.[83]

This difference in the presentation of risk could be attributed to a difference in the opinions of the two sets of authors. However, I believe this is not entirely the case. In an interview, Robert Jaffe, head of the committee assigned to review the Brookhaven risk, stated that as "a scientist I'm never going to say anything is never going to happen; it's not part of our vocabulary.... I would say that it is extraordinarily unlikely."[84] But in the report, as seen above, it was stated that strangelets would *not* be produced, rather than they were highly unlikely to be produced. In addition, advice was offered about the doing of the experiment at RHIC. Because of this, I believe the authors of the Brookhaven report were attempting, in addition to providing a scientific presentation of risk, to allay public fear and further scientific understanding. (This discontinuity between statistical probabilities and assurances, especially in the news media, will be examined in greater detail in the next chapter.)

Conclusion

The presentation of risks is intertwined with the perception of risks – thus it is important to understand the form that the Brookhaven report took when it was deployed into the larger social realm, to be appropriated and translated. It is this reason that the presentation of risk is important to study. I chose to consider both the Brookhaven report and the Teller report together in order to provide a better sense of how catastrophic risks are dealt with in the scientific community – the techniques used to analyze the nature and probability of the risks. As I have shown, these include approximations and estimates, the

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[81] *LA-602*: 17
[82] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 19
[83] Busza, et al. *Review of speculative "disaster scenarios" at RHIC*: 1

juggling of both theoretical and experimental evidence, and the use of a "worst case scenario" analysis. Overall, these techniques are a series of assumptions built into the evaluation of risk, but these assumptions are made explicit in the text. These techniques are used to overcome gaps of knowledge and make the analysis more convincing. Lastly, I showed how scientists change their presentation and evaluation of risk based on the intended audience.

In short, I hope to have shown that risks are not mere conceptual entities "out there," but rather have a physical place in the world. In this chapter, we have seen their physical manifestation in scientific reports. Risk is not a true "risk" until it is perceived and communicated to others. Scientific reports are one way these risks are communicated. The scientific presentation of risks works in two ways: it demonstrates the scientific view of the risk *and* has the potential to alter the way the audience perceives the risk.

[84] Interview with Robert Jaffe (22 November 2002)

Chapter 4: Translation of Risk

In an earlier chapter, I introduced new terminology regarding risk. I coined the phrases "objective risk" and "perceived risk" in order to discuss the Brookhaven experiment meaningfully. Recall that an objective risk is the inherent probability of danger associated with any action or system. This type of risk exists independently from human perception. On the other hand, we used the term perceived risk to describe the *belief* in a danger associated with any action or system. Unless explicitly stated, all risks discussed in this chapter should be presumed "perceived risks."

In the previous chapter, the Brookhaven risk was analyzed by a set of physicists. These physicists were forced to deal with a risk analysis which was reminiscent of Pascal's wager[85] – a miniscule chance of terrible destruction and an overwhelming chance of nothing happening. This risk analysis was an attempt to quantify an objective risk – and as such, became a perceived risk. This quantification was the physicists' means of understanding the "dangerousness" of the experiment. It was the physicists' perception of the risk, after analysis, which was given to the public, in the form of their report. And as we saw, the risk diffused.

The manner in which this risk was presented and shifted as it diffused into the larger social context will be explored more completely in this chapter. To do this effectively, a set of articles from large newspapers will be examined in a case study. The articles will be viewed from the framework outlined by sociologist Dorothy Nelkin, who has concluded from her

[85] Pascal's wager is a pragmatic argument created by Blaise Pascal, a seventeenth century mathematician, for the belief in God. It goes as follows: one can either believe in God or not. If one rejects the existence of a higher being and He exists, one is damned, and if one rejects the existence of a higher being and He doesn't exist, then nothing happens. On the other hand, if one accepts that there is a higher being and He exists, one has a chance for inifinite happiness, and if one accepts that there is a higher being and He doesn't exist, then nothing happens. So given a choice of accepting the existence of God or not, it is better pragmatically to say there *is* a God because you are playing the safer bet, outcome-wise.

research that scientific journalism is bound by forces acting among the scientific, journalistic, and readership communities.

> Most journalists working for the daily press are constrained by competition, deadlines, budgets, and the need to cover complex subjects within limited space and time. They must attract and hold the attention of readers, and they must develop an angle that will define their writing as news. These constraints, as much as the culture of scientific journalism, contribute to the character of contemporary reporting on science... they pose special problems for those reporting on the complex, uncertain, and often slowly evolving events that characterize science news.[86]

Though some readers and physicists laid blame on journalists for "alarmist" or scientifically inaccurate pieces, this labeling does not answer *why* the alarmist tone or inaccuracies were present. This is one question that will be answered in this chapter.

On another note, we saw that in the previous chapter, the physicists on Brookhaven's investigative committee used equations as a primary technique of expressing risk. However, for a risk to be expressed to a lay-reader, one who knows little or nothing about theoretical and experimental particle physics, a risk must be expressed using the power of language. The fact that newspaper writers do not include equations and numbers in expressing a risk plays a vital role in how a journalist chooses to express a risk. Here is where the power of language becomes so important: a language of risk has to be created by non-experts and experts alike to paint a portrait of risk for their audience. This point will be explored in detail.

After the emergence of the story in the mainstream press, the portrayal of risk in the newspaper articles was done in a multitude of ways. The focus of the story provided the reader with an implicit view of the risk. If an article started out by considering the scientific intention of the Brookhaven experiment and relegated the discussion of the disaster scenarios to the end of the article, the author was sending a wholly different message of risk than if the story began with a lengthy discussion of risk and relegated the discussion of the

[86] Dorothy Nelkin, *Selling science* (W.H. Freeman and Company: New York, 1987): 111.

merits of the Brookhaven experiment to the end. Also, literary techniques used to bolster claims made in the article play a key part in a reader's understanding of the risk. These include analogies and references made in the piece, as well as quotations used. Examining the quotations used provided an interesting view into the author's intentions in the piece, because the author chooses which quotations to place in the story, where to place them in the story (in the beginning of the story or at the end), and from whom to take the quotations themselves. Each of these techniques listed provides clues as to the author's intention in portraying the risk. Thus, at the heart of it, this chapter is concerned with the connections between language and risk, and its intent is to elucidate the mutability of risk as it diffuses in the news media.

(Don't) Judge an Article by its Headline

Stories involving discussion of the "disaster scenarios" at Brookhaven came out en masse in newspapers around the world beginning in the summer of 1999. Each newspaper article discussed the Brookhaven experiment and risk in different ways. Some authors focused the story on the danger of the experiment. Others focused on the experiment itself. Others focused on RHIC and BNL. Still others mediated between the three.

We are given a glimpse of the different focuses merely from the headlines: "Big bang machine could destroy Earth," "Physicists fear big bang replicator project unsafe," "Earth-shattering experiment a test of faith," "Brookhaven ion collider clears first big hurdle," "No danger from strangelets," "Scare stories and mysteries of quarky behavior," "The biggest bang of all," "Defusing fear of 'big bang' machine," "Experiment ignites fear of black hole," "A black hole 'ate' JFK Jnr / Science," "The New York strangeler," "Frankenstein, call your office," "Birth of the universe, in Long Island," and "Apocalypse how" (to list just a few)[87]. A headline such as "Big bang machine could destroy Earth" is likely to garner a different initial reaction from "No danger from stangelets." Even from these different headlines, one can immediately see the immense power that a few choice words can have on a reader. This is a theme that will emerge again and again in this chapter.

All of these headlines have a form of *story inflation* occurring within them. Sociologist Dorothy Nelkin has studied science journalism and notes this phenomenon: "While most journalists try to avoid a sensationalist and titillating style, they tend to magnify events and to overestimate if not sensationalize their significance."[88] Many of the headlines written about Brookhaven were almost apocalyptic! This inflation did not only occur in headlines; in many cases, it was carried into the article itself.

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[87] Included is a list of articles that I surveyed in my view of risk. Jonathan Leake, "Big bang machine could destroy Earth," *Sunday times (London)* 18 July 1999; Jonathan Leake, "Physicists fear big bang replicator project unsafe," *The Ottawa citizen* 18 July 1999: D7; Jonathan Leake, "Strangelet days indeed, ion collider could destroy Earth," *The Austrailian* 19 July 1999: 8; Simon Benson, "Earth-shattering experiment a test of faith," *The daily telegraph* 20 July 1999: 11; Earl Lane, "Story is out of this world," *Newsday* 21 July 1999: A8; Valerie Cotsalas, "Brookhaven ion collider clears first big hurdle," *The New York times* 25 July 1999: LI4; Suzanne Farrell, "Re-creating creation at BLN," [sic] *The Suffolk times* 29 July 1999; "No danger from strangelets," *The San Francisco Chronicle* 3 August 1999: A18; Malcolm Brown, " Will Brookhaven destroy the universe? Probably not," *The New York times* 10 August 1999: F5; Ron Haybron, "Creating our own black hole is far-fetched," *The plain dealer* 29 August 1999: 9K; Curt Suplee, "Scare stories and mysteries of quarky behavior," *The Washington post* 13 September 1999: A11; Curt Suplee, "The biggest bang of all: Could this experiment end world? Scientists say no, but they're reviewing it," *The gazette (Montreal)* 18 September 1999: J8; Stephen Reucroft and John Swain, "Diffusing fear of 'big bang' machine," *The Boston globe* 11 October 1999: D1; Bruce Lambert, "Lab's chicken littles will be disappointed," *The New York times* 17 October 1999: LI2; Claire Guyan, "Experiment ignites fear of black hole," *The evening post (Wellington)* 15 November 1999: 22; Graham Philips, "A black hole 'ate' JFK Jnr," 5 December 1999: 180; John Markoff, "Dr. Frankenstein, please call your office," *The New York times* 19 March 2000: Section 4, Page 1; John Markoff, "Frankenstein, call your office: Visions of computerized utopia giving way to predictions of doom," *The Gazette (Montreal)* 19 March 2000: A6; "Who'll be left to say sorry?" *The evening post (Wellington)* 10 June 2000: 4; Mark Sappenfield, "Birth of universe, in Long Island," *The Christian science monitor* 15 June 2000: 1; "The truth of the matter," *Sydney morning herald* 24 June 2000: 5; Call Hall, "Peering into black holes," *The San Francisco chronicle* 17 July 2000: A8; Michael Cooper, "The long shadow of science past; Long Island labs, on the defensive, struggle for community confidence," *The New York times* 20 August 2000: 33; "The New York Strangeler," *The economist* 2 December 2000; Earl Lane, "Colliding with fiction / Report further dispels fears of Brookhaven ion test disaster scenario," *Newsday* 12 December 2000: C3; Corey Powell, "Apocalypse how," *The Ottaway citizen* 30 December 2000: B1; Guy Gugliotta, "A dipolmatic addition to scientific equation; Brookhaven's Marburger tapped as adviser," *The Washington post* 2 July 2001: A15; George Johnson, "Physicists strive to build a black hole," *The New York times* 11 September 2001: F1; Martin Rees, "Saturday review: A brief history of the future," *The guardian (London)* 29 December 2001; Jim Merritt, "Long Island's tall tales; Get to the truth behind some of our grand delusions," *Newsday* 31 March 2002: G8.

Of course an article's focus is not solely contained in its headline. (In fact, the headline is usually not written by the journalist at all). Rather, it is by looking at the body of the article that the true "spin" of the story is revealed. Does the author discuss the merits of the Brookhaven experiment? Does the author discuss the end-of-the-world predictions in the lead paragraph? What analogies and references are made? Who is quoted and what quotations are used? As mentioned before, an author has choices in what to include *and what not to include* in an article. I will show later in the chapter that the focus of an article plays a key role in the *appropriation of risk* by the reader.

With this in mind, a newspaper article cannot be viewed as a mere purveyor of facts, but a point-of-view, which reflects the author's understanding of the facts. By patching selected facts together in a piecemeal fashion into an article, an author is creating a synergy between these facts. The sum total of the selected facts, placed together according to the author's wishes, is greater than the individual facts themselves. With this synergy, a point-of-view is revealed.

Numbers vs. Words

Let me start off by introducing two passages from the same article:

> This time the setting is Brookhaven National Laboratory in New York, where scientists have constructed at 2.6km circular tunnel for an unprecedented experiment in nuclear physics. About now they are accelerating gold particles to 99.995 percent of the speed of light and crashing them into each other. This will create temperatures of more than a trillion degrees – 10,000 times hotter than the sun. Under these conditions matter will "evaporate" into a soup of sub-atomic particles, replicating the conditions that occurred a millionth of a second after the formation of the universe.
>
> Brookhaven took the threat seriously enough to set up a team of physicists to assess the risk and while the project has now been cleared, one scientist on the committee said he believed the risk, though tiny, could not be ruled out.[89]

This journalist did not shy away from using numbers in the article – in fact, in the first passage, the journalist scattered impressive numbers ("trillion degrees," "99.995 percent the

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[88] Nelkin, *Selling science*: 19.
[89] "Who'll be left to say sorry?" *The evening post (Wellington)*.

speed of light," and "10,000 times hotter than the sun") freely. However, in the second passage, the passage dealing with the risk of the experiment, there were no numbers. Recall that in the previous chapter, the BNL investigative committee primarily used numbers to analyze the risks associated with the gold-collision experiment. The scientists used numbers to elucidate the risk, the science journalist did not. This difference in presentation has an effect on the readership.

As mathematician John Allen Paulos writes:

> ominous is the gap between scientists' assessment of various risks and the popular perceptions of those risks, a gap that threatens to lead either to unfounded and crippling anxieties or to impossible and economically paralyzing demands for risk-free guarantees.[90]

The appropriation of risk has consequences – as with the woman in the second chapter who wanted to throw an end-of-the-world party, with Walter Wagner who wanted a new ion collider built, to be on the safe side, and with those who saw in the risk a government conspiracy.[91] Why, then, did almost all of the journalists in the collection of articles I surveyed avoid utilizing numbers and resort to using ambiguous quotations to describe the risk?

The physicists on the investigative committee shied away from producing a single probability for disaster. In fact, because the worst case assumptions were made, a single probability for disaster would be impossible to compute. Because of this indeterminacy, science journalists were not given a concrete notion of the danger of the experiment to latch onto for their articles. In fact, the indeterminacy itself gives rise to a notion that there is some form of danger – computable or not. Furthermore, the physicists' report itself is not an easy read for someone who is not formally trained in the higher-level sciences.

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[90] John Allen Paulos, *Innumeracy* (Hill and Wang: New York, 1988): 4

[91] The effects of science media on the public and on public policy is discussed in more detail in Chapter 5 of Nelkin, *Selling science*: 70-84.

The journalists then were forced to turn to interviewing physicists to clarify and simplify their views of the risk. Even in the second passage in the introduction to this section, the actual evidence from the risk analysis is not presented, but instead, this evidence is swapped for an authoritative opinion. This phenomenon has been noted in other cases by Dorothy Nelkin:

> Often the press portrays science as the arbiter or judge of technological perils. Scientists, even when interested parties in disputes, are viewed as the source of authoritative evidence and definitive solutions. Yet, with some notable exceptions, we seldom read about the scientific issues involved in risk disputes or the methods of risk analysis. Thus we are left with no basis for making meaningful judgements about competing claims.[92]

In other words, science journalists sometimes relegate scientific evidence to a lower status than a quotation from an eminent scientist. This certainly held true in the Brookhaven case, where almost all articles (regardless of whether they presented the experiment as dangerous or not) did not discuss in detail any of the evidence that the BNL committee used to reach their conclusions.

The Language of Risk

However, the journalists cannot be blamed for not including *all* the scientific evidence, as this would be impossible. In addition, journalists have a responsibility to their readership. Any mention of the formula in the Brookhaven report used to compute cosmic ray probabilities would have been beyond the scope of most readers. As a result, the newspaper authors had to turn to the *language of risk* to compensate.

I have compiled a list of words and phrases regarding the Brookhaven risk that were employed by the newspaper authors in my survey of newspaper articles.

Table 1

tiny but real risk	less likely	could be disastrous
the risk was tiny but could not be ruled out	the risk is exceedingly small	the probability of something unusual happening is not zero

[92] Nelkin, *Selling science*: 54.

the chances... were infinitesimally small	astonishingly unlikely	could destroy the earth
could create a black hole	could be disastrous	could also have catastrophic consequences
small risk	SMALL risk	could alter the structure of anything nearby
extremely unlikely	miniscule risk	could create some sort of... catastrophic reaction
the chances 'are about the same order of possibility as if I were to win the lottery'	unlikely to the level of the most absurd thing you could imagine	not possible
might create an unforeseen catastrophe	might fundamentally alter their surroundings	could result in the formation of a black hole
the odds are infinitely small	no chance	the experiments... will not blow up the world
could destroy the Earth	chance was... infinitesimally remote	could start a chain reaction
probably not	may be on the verge of destroying the world	no one is quite sure what these collisions might spawn
might have created a world-devouring monster	might go one to annihilate the ordinary matter around them	might nucleate a tiny black hole
far-fetched	not plausible	the experiment will not tear our region of space to subatomic shreds
could theoretically create a black hole	not credible	these things just don't happen
there is no question that it is not going to happen	the end of the world is near	we think not
current theories let us guess... but calculations are difficult and our ideas uncertain	calculations suggest that this won't happen	probably negligible
the world... will not repeat, not---come to a cataclysmic end	might accidentally create a black hole	no credible mechanisms for catastrophic scenarios at RHIC
the dire predictions 'are firmly excluded'	will not cause the creation of a new universe	It will not be the end of the world as we know it
the end of the world might be nigh	could spell the end of the world	could accidentally create a black hole
unlikely... but the possibility couldn't be ruled out	Trillions and Trillions to one	highly unlikely
few things can ever be proved to be absolutely impossible	the very concept of risk needs reassessing when all of existence is said to be at stake	infinitesimally remote
could create an exotic form of matter	the world destruction was not imminent	has dismissed the idea
there was a tiny chance	could end up consuming all existence	it is not possible
should not be a matter of concern	poses no danger	widely dispelled by scientists
ruled out by fundamental principles of physics as well as by some empirical evidence	far-fetched and implausible	pretty much impossible

It is important to include an almost excessive number of these phrases so that the sheer diversity of statements *about the same situation* could be seen explicitly. One cannot help but be confronted with the contradictory nature of these statements. How can an event which "is not possible" and have "no chance" of occurring also be an event which "could end up consuming all existence" or "might nucleate a tiny black hole"?

It is the ambiguity and profusion in the language of risk that partially helps give rise to these contradictions. The words "might," "could," "is," "not," "no," "small," "miniscule," "impossible," "tiny," etc. are the tools which the author has to craft a notion of risk from. Each of these terms has their own connotations associated with them. "Might" and "could" connote a serious possibility – rather than discounting a possibility.

On the other hand, what do the words "tiny" and "miniscule" connote? Do they invoke a sense that the experiment is safe? Therein lies the limitation of risk – without a number, there is no reference. These words are left open-ended, for the reader to extrapolate a meaning. "Tiny" could just as well mean a chance of 1 in 100 as it does 1 in 100,000,000. "Tiny" and similar words do not answer the question that any mathematician or physicist would be asking – tiny *compared to what*? This is where "risk appropriation" enters the picture; extrapolating from the article, the reader is forced to give "tiny" a meaning in order to fit it into his or her worldview.

However, even if very large or small numbers had been included in a description of risk, the "risk appropriation" from one person to another can be completely different. This view was published in a recent *Boston Globe Magazine* article "Playing with Billions."[93] The author, D.C. Denison, claims that there has been a large influx of numbers of the same magnitude as a billion that has occurred as of late – from new scientific advances and large

[93] D.C. Denison, "Playing with Billions," *The Boston globe magazine* 2 December 2002: 20-23, 29-32.

companies – and there is gap between the numbers themselves and an understanding of their magnitude. She cites mathematician John Allen Paulos who believes

> It's very difficult to get a visceral feel for [large numbers like a billion]. There aren't many instances of this kind of big number in people's lives, so they can't compare them. The bigger the number, the greater the potential that people make misinformed or gullible assumptions.[94]

In other words, there is no frame of reference for a lay-reader to associate very large or very small numbers in. Cognitive scientist Douglas Hofstadter agrees with Paulos, believing that the people tend to space out when numbers get big. He elaborates

> It's an ongoing battle between a scientific mentality and a 'don't care' mentality. Increasingly when we talk about [large numbers like a billion], it's important to have a sense of what we're talking about.[95]

So by including a phrase "Trillions and Trillions to one," as one article did, to express the chance of disaster could make no difference in a reader appropriating this risk than a phrase stating "billions and billions to one" or even "thousands and thousands to one." The concept of these near-infinitesimal chances differs significantly in the math world, but to the lay reader, all three chances may be read equivalently.

Mathematicians and cognitive scientists are not the only academicians who have given thought to the importance of language involving risk. Social theorist Mary Douglas describes the utilization of the term *risk* itself:

> The language of risk is reserved as a specialized lexical register for political talk about the undesirable outcomes. Risk is invoked for a modern-style riposte against abuse of power. The charge of causing risk is a stick to beat authority, to make lazy bureaucrats sit up, to exact restitution for victims. For those purposes *danger* would have once been the right word, but plain *danger* does not have the aura of science or afford the pretension of a possible precise calculation.[96]

We will see in Chapter 5 that risk can be a weapon employed in political or scientific battles.

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[94] Paulos, as quoted in ibid, on 22.
[95] Hofstadter, as quoted in ibid, on 22.
[96] Mary Douglas, *Risk and blame* (Routledge: London, 1992) : 24-25

However, there is a more general problem when working with numbers (not only very large or very small numbers) for a large lay-audience: there is a gross lack in understanding of the meaning.[97] Dorothy Nelkin expounds:

> Few journalists or their readers can judge if numbers are meaningful or accurate, or if sampling techniques or research methods are appropriate. Furthermore, explanations in science often defy common sense. As a journalist puts it, "The news part is not hard to get right.... It is the mechanism by which something works that is hard to grasp and to simplify..." Yet complexity requires such explanations; readers will need background material if they are to understand the significance of scientific events.[98]

Besides a lack in understanding, Nelkin refers to the difficulty in simplifying the complexities of scientific events. She discusses the press coverage about the ozone layer: with many competing claims, oftentimes writers didn't know how to understand the intricacies of the issues and evidence, so they merely presented competing viewpoints on the controversy.[99] This is akin to what some journalists did with the Brookhaven controversy – instead of presenting competing evidence, they presented competing claims. Some journalists also find that presenting both sides of an issue, no matter how small a population the minority constitutes, is being journalistically fair because it is presenting a story objectively. However, the primary difference between the ozone and Brookhaven situations is that in the Brookhaven situation, there were not many arguing that the end of the world was possibly nigh because of the RHIC experiment (while in the ozone controversy, there are quite a few, creating a vocal oppositional minority).

Perhaps, and I am admittedly speculating, this is one of the factors which led to the large flood of articles upholding the notion that physicists were seriously endangering the

[97] I am admittedly, in this analysis, simplifying the situation. Mary Douglas states that "Risk research has uncovered many conundrums and paradoxes. It has found that 'the public' definitely does not see risks in the same way as the experts. The gap between lay and expert opinion has given rise to [many subdisciplines]" in Douglas, *Risk and blame*: 17. In particular, and considered in many analyses of risk, is the psychological reasoning for this gap – from a lack of understanding in probability to irrational fears underlying risk understanding.
[98] Nelkin, *Selling science*: 125-126.
[99] Nelkin, *Selling science*: 56-7.

world. Even though most credible physicists were decrying the Brookhaven risk, some newspaper reporters, in their limited time and resources, were forced to rely on newspaper articles that came before it and conversations with physicists. Most of the newspaper articles had specific references to the initial *Sunday Times (London)* article and Walter Wagner's *Scientific American* letter to the editor, both of which lent some credibility to the fear. On the other hand, the Brookhaven report and the physicists on the committee strongly denounced the fear. From this, perhaps some journalists saw controversy – and instead of trying to present rationale from both sides why there was/wasn't a risk, they decided to present the risk as being in question. In this form, the end-of-the-world danger becomes a possibility, dependent on the outcome of the debate.

Literary Techniques for the Masses

So if numbers are not often used by reporters when expressing risk, then how *is* risk relayed, besides with vague terminology? This obstacle is overcome by the use of analogy, descriptions, the use of references (both historical and literary), and humor. The effect of each of these on the reader may be unintended; they each contribute to forging the focus of the article.

In one article, a black hole was said to be "like water going down a drain"[100] and in another, the experiment itself was said to "rip the particles apart in a microcataclysm."[101] In addition, there was talk of stranglets which would "gobble up the entire planet"[102] These descriptions simplify the explanations of the science – reduce the complexity for readers. However, Dorothy Nelkin has noted that:

> The language of science is intended to be precise and instrumental. Scientists communicate for a purpose – to indicate regularities and aggregate patterns, and to provide technical data. In contrast, journalist language has literary roots. Journalists will choose words for their richness of reference,

[100] Haybron, "Creating our own black hole is far-fetched," *The plain dealer*.
[101] Sappenfield, "Birth of the universe, in Long Island," *The Christian science monitor*.
[102] Haybron, "Creating our own black hole is far-fetched," *The plain dealer*.

> their suggestiveness, their graphical appeal. They are likely to prefer a "toxic dump" to a "waste disposal facility."[103]

In other words, the visual images that the above phrases conjure up are potent. They are something that a non-scientist can relate to. They are descriptive. They can be visualized. They are a simplification of events. As such, the descriptions are a transformation of the "scientific fact" into a "literary fact."

References to both historical events and literary texts also helped set the tone in some articles. In a few articles, the "ignition" of the atmosphere by the atomic bomb concern was spelled out. [104] Drawing attention to this particular event has a two-fold effect. First, the ignition of the atmosphere was taken seriously by serious scientists – scientists who built the atomic bomb. So by association, these articles were implying that the Brookhaven fear was also taken seriously by serious scientists (even though we have seen that was, for the most part, not true). Second, the atomic bomb is an image that has cultural resonances today. The horror of the mass destruction that it brought to so many lives, and the fear it strikes, cannot be separated. By harking to this image, the journalist (either overtly or implicitly) is reflecting the horrific effects of scientific advances.

In addition to making reference to the atomic bomb, articles also referenced Ice-9 from Kurt Vonnegut's *Cat's Cradle*, the movie *The Blob*[105], and the fable of the Sorcerer's Apprentice[106]. The Ice-9 reference was described in Chapter 2. The other two references also allude to the dangers of science (with the blob being purple alien goo which eats everything in sight, growing in size, and the sorcerer's apprentice being a neophyte sorcerer

[103] Nelkin, *Selling science*: 177.
[104] In Benson, "Earth-shattering experiment a test of faith," *The daily telegraph*; Haybron, "Creating our own black hole is far-fetched," *The Plain Dealer*; "Who'll be left to say sorry?" *The evening post (Wellington)*.
[105] In Suplee, "Scare stories and mysteries of quarky behavior," *The Washington post*. The blob is a 1958 horror film.
[106] In Farrell, "Re-creating creation at BLN," [sic] *The Suffolk times*. This was originally a poem written by Johann Wolfgang von Goethe, author of *Faust*, and later turned into part of the Disney cartoon *Fantasia*.

unsuccessfully attempting to wield his master's immense power). These references were different ways to get across the same image: the dangerous power that lay in the hands of scientists.

The notion of "mad scientists," scientists who abuse technological power, was countered in some articles by scientists themselves (and the journalists who chose to use these quotations). These scientists employed another literary technique in their speech: humor.

> By one estimate, about 10 trillion collisions with more energy and higher mass than RHIC's gold nuclei have occurred on the surface of the moon alone since it formed. And Earth's satellite has not been formed into strange matter. "Or green cheese," Jaffe noted.[107]
>
> The [BNL] report is expected soon, and few doubt that the panel will dismiss the apocalyptic concerns. "The really important question is whether we'll destroy the universe before or after we win the Nobel Prize," joked RHIC Project Director Satoshi Ozaki.[108]
>
> The United States Secretary of Energy, Bill Richardson, referred to the debate when he spoke at the formal dedication of RHIC this month. "Much as been written about this project, some of it alarmist," he said. "But let me be clear: Turning on this machine will not cause the creation of a new universe that will swallow our own. It will not be the end of the world as we know it... And even if it was, at least we shouldn't have to worry about the Y2K problem any more."[109]
>
> "A bunch of us were sitting drinking beers one night, trying to figure out a good practical application for RHIC," says BNL physicist Jeffery Mitchell, 37, of Rocky Point. Here's what they decided: "There are always an odd number of socks when they come out of the wash, so there's got to be some black hole that swallows up all the socks," Mitchell says. "So we came up with a way to study this in the laboratory."[110]
>
> In turn, the notion that such a particle would pose any catastrophic risk to the planet is "preposterous," Jaffe said. It is "unlikely to the level of the most absurd thing you could imagine," he said. "It's more unlikely that a spaceship is going to land in the middle of Texas, and that aliens are going to come out and tell us that the New York Yankees are all aliens."[111]

I see the use of humor as having multiple effects. First, the scientists' humorous quotes make the scientists themselves appear as common, everyday people, in addition to being experts. These *people* would not want to destroy the world, as opposed to an abstractly portrayed "mad scientist." Second, humor shows the scientists as so unconcerned with the

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[107] Suplee, "Scare stories and mysteries of quarky behavior," *The Washington post*.
[108] Suplee, "The biggest bang of all," *The gazette (Montreal)*.
[109] Lambert, "Lab's chicken littles will be disappointed," *The New York times*.
[110] Merritt, "Long Island's tall tales," *Newsday*.
[111] Lane, "Story is out of this world," *Newsday*.

threat of black holes or strangelets or vacuum instabilities that they can joke about it. Third, it makes the science "more palatable" for the audience to accept.[112] Fourth, it shows the experiment as not dangerous. If the principal investigator of the investigative committee, the RHIC project director, or the Secretary of Energy each dismisses the concern with such aplomb, what real credibility could there be to the concern?

To Write or not to Write

Still, after all this analysis of the articles, I am left wondering why some journalists chose to present the Brookhaven situation as being potentially risky, in spite of the fact that experts in the field were crying out "RHIC is safe" in interviews and scientific publications. Why did one author chose to use the quotation "It is astonishingly unlikely that there is any risk *but I could not prove it*"[113] (emphasis added) in one article while in another author chose "a doomsday scenario is not plausible"[114]? Even barring these different ways to present the Brookhaven risk, even the existence of a newspaper article itself lends legitimacy for a concern. The message being sent is that *this topic has been researched by a journalist and approved for publication, so this is a topic worth thinking about.*

This story spread rapidly, in many different forms with many different focuses. In fact, a reference to it popped up recently in an article discussing the building of a different particle accelerator.[115] I can only conjecture as to why this phenomenon occurred. First, science journalists have the problem of their work as not being considered newsworthy.[116]

[112] Nelkin, *Selling science*: 119. Nelkin makes the contention that science is easier to swallow for the public if it is humorous or curious. In this particular controversy, it is hardly debatable that the Brookhaven disasters are curious and intriguing. Some physicists also used humor when talking about the disaster scenarios with journalists.
[113] Leake, "Physicists fear big bang replicator project unsafe," *The Ottawa citizen*.
[114] Lane, "Story is out of this world," *Newsday*.
[115] Discussion of the Brookhaven event was included in an article about building the Large Hadron Collider at CERN in Johnson, "Physicists strive to build black hole," *The New York times*;
[116] As explained by Nelkin, "Science writers also compete with political writers for space and complain of problems convincing editors that science is as newsworthy" in Nelkin, *Selling science*: 113.

Thus, if some sort of controversy is involved, whether it be between scientists or between the public and scientists, legitimacy is bestowed upon the story. The story can no longer be considered a filler-piece, but rather an important news story. Second, the story was timely (coincident with Y2K and other millennial concerns.) In fact, quite a few of the articles I surveyed included discussions of other ways that humans could technologically destroy themselves. There was even one article printed on December 30, 2000 that listed twenty different ways Armageddon could actually occur.[117] In addition, the differences *within* the articles themselves, the many differing focuses, may be attributable to the various external forces that science journalists are subjected to, cited in the introduction. These factors in conjunction with each other might provide part of an explanation as to why this story gained such popularity and why the Brookhaven risk gained some credibility with the journalistic community, even with the opposition of scientists.

Conclusion

Dorothy Nelkin's book *Selling Science* has formed a framework for this chapter – considering newspaper articles as items of discourse necessary to be subject to analysis. However, these items of discourse are created out of social, cultural, and literary constraints, and must be viewed as such.

Hopefully in this chapter I have demonstrated the ways that the science media portrays risk and how these different ways all have different effects for the reader. Some of these techniques were story inflation, the use of potent and/or ambiguous words to describe risk, and various literary devices. Because of these techniques, many articles were written on the same event, but all were differing in their analyzing the risk. The authors of each article were presented with *choices* – what terms to use, whom to interview, what quotations to use,

[117] Corey Powell, "Apocalypse how," *The Ottawa citizen*.

in what order to place these facts, etc. These choices all come together to forge a point-of-view. This point-of-view then enters the mainstream media for others to read, and once again, risks are propagated. Risk spreads.

Chapter 5: Scientists Talk Back

Introduction

Quarks and gluons. To many, these terms are likely candidates to be used in sequel of Lewis Carol's "Jabberwocky." However, to the scientists involved in the RHIC project, these terms are not fantasy-based, coined for the amusement of readers. To these scientists, contained in these terms (and in the RHIC experiment) lay answers to fundamental questions about the universe. As one physicist/journalist stated about the actual purpose of the experiment in the midst of the media frenzy: "We worry about the structure of the atomic nucleus and the forces that govern it to achieve a better understanding of the whole universe itself."[118] The RHIC experiment was not a small high school physics experiment, repeating tests of the known. The machine itself took 8 years to construct and cost around $700 million. It involved over 1,000 collaborators from nearly 90 institutions, and was exploring, among other things, the early state of the universe.

In the course of this work, the scientific analysis of the Brookhaven risk has presented in depth, as well as the press coverage of this risk. On the other hand, the question of the scientific community's reaction to this coverage remains open. The reaction of the individual scientist to the press coverage and his or her subsequent action is the focus of this chapter. In addition, I consider a break *within* the scientific community regarding the scientific underpinnings of the disaster scenarios. In this characterization, the scientific community cannot be seen as a single, homogeneous, objective entity, but rather as a divided, political, public entity.

To make this characterization of the scientific community, I will tackle four different themes. First is of the scientific reaction to the press coverage. Second is the action taken by

Brookhaven to counter the negative press. Third is the difference between "natural" and "unnatural" science. Fourth is the scientific dispute in the middle of the public relations disaster. These themes will broaden our understanding of the concerns in the scientific community. Our technical understanding of the investigative committee's report of the risk will be complimented by the scientific community's reaction.

"Good" Article, "Bad" Article

The physicists working on the Brookhaven experiment, expectedly, were not pleased with the initial negative media attention caused by the *Sunday Times (London)* article, as well as some subsequent articles that researchers working on the Brookhaven experiment described as "adverse"[119] and "unfortunate."[120] I found a bulletin board for the PHENIX physicists online, and on this electronic archive, a series of postings chronicling the physicists reactions to the press coverage. (PHENIX is one of the four detectors to be used in the experiment at RHIC.) It is with this archive in addition to discussions with involved physicists that I hope to untangle exactly what qualities in news articles allowed each to be categorized (by the scientific community) as either a responsible or irresponsible article.

To do this properly, I will initially consider news articles considered "good" by writers to this PHENIX list (and, later, focus on criticisms). In one posting, Brant Johnson, an employee in the physics department in Brookhaven, wrote of two "well balanced" stories: a *Newsweek* story and a *New York Times* story.[121] Johnson was led to these stories by his wife and a colleague in the high energy and nuclear physics directorate (at Brookhaven). As an aside, we can note that media perspective about the Brookhaven experiment is travelling via

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118 Stephen Reucroft and John Swain, "Diffusing fear of 'big bang' machine," *The Boston globe* 11 October 1999: D1.
119 This is archived online **Error! Bookmark not defined.** (Accessed 13 April 2003).
120 **Error! Bookmark not defined.** (Accessed 13 April 2003).
121 **Error! Bookmark not defined.** (Accessed 13 April 2003).

both face-to-face contact (from wife to husband, and physicist to physicist) *and* via electronic contact (from bulletin board posting to physicist).

These two articles have one highly-noticeable property in common: in the first few sentences, the Brookhaven risk is categorically dismissed. *Newsweek* characterizes the experiment in a similar fashion to many alarmist pieces, but does so ironically. This characterization, replete with impressive numbers and colorful yet scientific terminology, is couched at both start and finish by the explicitly stated notion that the world is safe from Brookhaven:

> This is probably not the way the world ends: sometime this fall, researchers at Brookhaven National Laboratory will tap a few commands into a computer terminal, bringing their new particle accelerator – the Relativistic Heavy Ion Collider, or RHIC – up to full power. Atoms of gold – heavy enough to cause some real quantum fireworks – will course around two nearly circular, 2.4 mile "racetracks" in opposite directions at 99.9 percent the speed of light. The nuclei will smash into each other, exploding at a temperature 10,000 times hotter than at the center of the sun. For a hundred trillionths of a trillionth of a second, conditions will mirror the universe immediately after the big bang. From that brief genesis, though, a new universe will not be born. It won't grow, and it won't destroy the pre-existing universe, one we know and love. No Apocalypse, no Big Goodbye.[122]

The *New York Times* article begins with a similar sentiment – but manifests it differently. This piece implies that the BNL media alarmism is merely a method of generating "science news":

> Amid the summer news doldrums, what could be more invigorating than a warning that physicists may be on the verge of destroying the world? Luckily, this and similar alarms can be taken with a large grain of salt...[123]

This is furthered in the article, which later goes on to say that "every time a new nuclear accelerator begins operating, dire warnings have ensued," citing similar opposition to Fermilab's Tevatron as an example.[124]

Two other articles are also cited as "balanced" by physicists posting on the bulletin board – a *Washington Post* newspaper article[125] and a *MSNBC* online news article.[126] Not only

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[122] Adam Rogers, "The big bang is back," *Newsweek* 16 August 1999: 40-41.
[123] Malcolm Brown, "Will Brookhaven destroy the universe? Probably not," *The New York times* 10 August 1999: F5.
[124] Some feared that colliding protons and their anti-particle, the anti-proton, at the Tevatron would generate huge amounts of energy that would create a small tear in space-time – which would later expand.

do these dismiss the dangerousness of the Brookhaven experiment, but each explains *why* the danger does not exist (in contrast to most of the other news articles examined in the previous chapter, where quotations from physicists were often used as proxy for scientific explanations).

Even though physicists working on the PHENIX detector were lauding some news pieces, they certainly were not without gripes towards others. In particular, many were highly critical towards Fred Moody's *ABC News* piece (discussed in Chapter 2). In this piece, Moody warned his audience of the problem of having scientists with hubris holding too much power. One said that the Moody article was "notable only for a near-record density of physical and logical fallacies."[127] The piece was seen as irrational and incorrect – just like Brookhaven fears. Regardless, this particular physicist still recommended that others take action:

> Dear PHENIX Colleague:
>
> As Dr. Steadman of the DOE points out in the included message, the unfortunate and completely erroneous report posted to ABCnews.com [link removed] has, together with its loaded, prejudiced "instant poll" question
>
> "Should potentially dangerous experiments, like the one at Brookhaven, be allowed to proceed?"
>
> created a potentially serious problem for RHIC. While one would expect that the presence of excellent scientists in the administration with superb nuclear physics credentials (Dr. Moniz of DOE, Dr. Bond of OSTP) will guarantee an accurate briefing statement, it can't hurt to also submit your answer to the poll question.
>
> The fact that the response is limited to Yes/No, and that the question is prejudicial, is of course offensive, but one could justify overlooking this by the disclaimer printed immediately below it:
>
> "Not a scientific poll; for entertainment only "
>
> May you live in interesting times,
> Bill [128]

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[125] **Error! Bookmark not defined.** (Accessed 13 April 2003) – in Curt Suplee, "Scare stories and mysteries of quarky behavior," *The Washington post* 13 September 1999: A11
[126] **Error! Bookmark not defined.** (Accessed 13 April 2003) **–** in **Error! Bookmark not defined.** (Accessed 13 April 2003).
[127] **Error! Bookmark not defined.** (Accessed 13 April 2003).
[128] **Error! Bookmark not defined.**

This message is one of the first posted which indicated a major concern by the physicist of negative media attention. According to Brant Johnson, who had initially posted a link to the *ABC News* article, the Moody piece created a "flood" of calls to the White House *and even the United States president was to be briefed on the situation*. More than that, it shows a fear of negative public opinion – to the point of asking physicists, who don't even agree with the framing of the poll question on Moody's site, to vote (of course, "for entertainment only").[129]

Brookhaven's *official* response came in the form of a press release from John Marburger, the director of the Brookhaven National Laboratory.[130] The significance in this lies in the fact that Brookhaven felt so much damage was being done to the laboratory by this article opposing the RHIC experiment that it *had* to respond to it with more than the independent committee initially called by Marburger. In fact, Pete Genzer, in the Office of BNL Media and Communications, received over 300 email messages after the *ABC News* article, and went as far as to contact Stephen Hawking to dispel the notion that he agrees with Moody (some of Hawking's theories were cited in the Moody piece)![131] The RHIC experiment was no longer a "fluff piece"; because of the worldwide exposure, the article had become newsworthy and the scientists had to respond in kind.

Physicists contacted, including even Robert Jaffe, said that initially there was an attitude of bemusement and chuckling. Eventually, though, some felt fear that the entire project could be shut down. One graduate student working on the PHOBOS detector at RHIC during this frenzy said that at the time

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[129] Brant Johnson expounded on this poll: "the question is crazy, because it assumes that RHIC is dangerous. But you know how these polls are interpreted... This is becoming very serious" (on **Error! Bookmark not defined.** (Accessed 13 April 2003)).

[130] John Marburger, "Statement of ABCNEWS website article on RHIC," [BNL Press Release] 17 September 1999.

[131] Some physicists working at the University of California, Davis on the RHIC detector STAR created a webpage to provide their views on the danger and give links to "good" articles. On this webpage, they included

> there was a real fear that something bad could happen to the project (ie NOT because of a black hole, but because some politician would decide to 'save the world' and shut the project down).[132]

The effect that the perception this experiment has on the actual performance of the experiment will be seen a bit more in the next section. Out of this fear of the project being placed in jeopardy sprung action.

Public Relations

From the publication of the initial *Sunday Times (London)* article, BNL had been on guard. Four days after publication, Bill Zajc, a physicist working on the PHENIX detector, reminded other researchers on the BNL laws governing talking with the press and suggestions on how to do it. These "common sense suggestions" (his words) are:

1. Know who you are talking to. If asked to comment by a reporter, check his or her credentials. If queried by e-mail, try to understand who the correspondent is.
2. Quantitative statements of risk are a losing game. Terms that physicists often use, such as "very small, but not zero" can and will be misinterpreted.
3. If speaking for attribution in a print article, you have the right (i.e., should demand) that the reporter read back to you the precise quote [sic] that will be used.
4. It is much harder to get approval like #3 for television or radio interviews. Be careful!
5. When you write an e-mail reply, you do have the chance to express your thoughts in the manner you like. However, e-mail is also notorious for its inability to convey the context (emoticons won't make it into the Times), and also it is a medium of zero impedance (i.e., your mail can be forwarded endlessly).[133]

These suggestions imply the possibility of news reporters misreading (or, to read cynically, purposefully misinterpreting) scientists' views. (This fear most likely stemmed from the quotations used by Jonathan Leake in his *Sunday Times (London)* piece.) Even early on, the physicists at BNL were attempting to curb the negative publicity that eventually was to spread around the globe and be part of a briefing for the president of the United States. Still, even before Zajc took action, physicists had been acting carefully when talking with reporters.

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an email sent by Genzer to the flood of email messages he received – and in this email is where the Hawking discussion is made.

[132] Email correspondence with Patrick Decowski (15 December 2002).

At least one physicist Leake spoke to in his research was wary of misinterpretation. Robert Jaffe (the head of the investigative committee called by BNL) says of his interview:

> we kept discussing the issue of likelihood and I tried to give him quite a bit of background on how to discuss unlikely situations.... And we talked about what it meant that something has the probability of a part to 10^{14}. I tried many different ways to get him thinking about this. One example, an example I used, is to take a kilometer of beach on Cape Cod, which is a thousand meters long and a hundred meters wide, and you can calculate there are possibly 10^{14} grains of sand. And so I'm going to tell you that I'll give you $10 for every grain of sand that you put in your mouth, but one grain of sand somewhere on this kilometer length, 100 meter wide beach, that one will kill you and every other one we'll pay you $10. And this guy honestly said to me he wouldn't put any sand in his mouth. Which is absolutely crazy, but it shows you the way people think about risks. I would have stuffed my mouth with sand! These numbers like 10^{20} and 10^{30}, they don't mean anything to them, and because of that, we have a very hard time explaining the likelihood.[134]

Besides illustrating the frustration that physicists have with explaining very large numbers to non-physicists, as well as differences in opinions of the rationality/irrationality of taking a low-probability risk, this passage demonstrates one way that scientists attempt to speak with non-scientists. Jaffe was cautious in his discussion with Leake, attempting to give accurate information. He tried to give a physical, understandable, counterpart to the risk involved in the Brookhaven experiment – via analogy. His attempt was to act a mediator between scientific mumbo-jumbo and the lay person. However, Jaffe's only quotation in the article was:

> Professor Bob Jaffe... said he believed the risk was tiny but could not be ruled out. "There have been fears that strange matter could alter the structure of anything nearby. The risk is exceedingly small but the probability of something unusual happening is not zero."[135]

If Jaffe truly found there was no danger to the experiment, why then wouldn't he say there was no risk – and that the probability was for all practical purposes zero? This would make his view come across a lot stronger, and has less of a chance of being misinterpreted. This, again, ties in with the themes of the language of risk and science. As Jaffe put it, "as a

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[133] **Error! Bookmark not defined.** (Accessed 13 April 2003).
[134] Robert Jaffe, Interview (22 November 2002).
[135] Jonathan Leake, "Big bang machine could destroy Earth," *Sunday Times (London)* 18 July 1999.

scientist I'm never going to say anything is never going to happen; it's not part of our vocabulary."[136]

As discussed in Chapter 4, however, science reporters like Leake are constrained by deadlines, the complexity of an issue, pressure to present both sides of an argument, and a limited space to condense a lot of material. One can argue that Jaffe's quotation is not a fair representation of what he was intending to say, but one can also easily argue that Jaffe's quotation was what Leake himself understood of what Jaffe was saying. Leake did admitted that he would *not* have eaten the sand, in Jaffe's analogy, which demonstrates a lack of understanding by Leake to what Jaffe was attempting to relay about the miniscule risk at RHIC.

Besides individual conversations between reporters and physicists, Brookhaven also attempted to inform the public of the risk in the form of two official press releases. This is common practice in universities and large laboratories:

> Although individual scientists sometimes promote their own work, more often they rely on their institutions to disseminate information to the press. Most major research universities employ public relations professionals (called news officers or public information officers) or outside media consultants to publicize the work of their science faculty and, thereby, to enhance the image of their institutions.[137]

These two press releases were written on the defensive. The first was released on September 17, 1999 and written by the director himself, commenting on Moody's *ABC News* article. Moody's piece had generated so much publicity that Brookhaven was forced into action. The second was released a few weeks later on October 6, 1999 and summarized in a few paragraphs the investigative report discussed in Chapter 3. Both press releases were one method the laboratory had to spread their version of the risk (or non-existence of risk, in its

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[136] Robert Jaffe, Interview (22 November 2002). Thomas Ludlam, in the physics department at Brookhaven, agrees, being quoted as saying "This is the way good scientists think... In all probability nothing bad will happen, and in a practical sense it won't happen. But it's not a zero probability." (quoted from **Error! Bookmark not defined.** (Accessed 13 April 2003)).

case). The investigative committee's report itself was also another way for Brookhaven to spread their view of the risk – and as such, it was placed initially on the Brookhaven homepage (now it is housed prominently on the RHIC homepage).

Last to consider is the role of outside individuals acting within the media onslaught. This fear was characterized by one researcher:

> there was a real concern that some political who would like to get (re)elected could play on the public fear (and brookhaven's poor reputation at that time) to shut down the whole project. [sic][138]

The Evening Post (Wellington), *The Christian Science Monitor*, and *MSNBC* all reported that Walter Wagner, composer of the Scientific American letter to the editor which sparked the media frenzy, filed a federal lawsuits to stop the RHIC experiment.[139] So this fear had come to fruition. Luckily for these physicists, according to these sources, Wagner's request was denied. The $700 million Relativistic Heavy Ion Collider was saved.

Natural vs. Unnatural

In the coverage of this media frenzy – by both scientists and journalists – a sublte distinction was being drawn within discussions about the experiment: an unwritten question was being asked about the boundaries of nature. These boundaries test the question of who has power (the power to alter the natural). If the Brookhaven experiment was a novel test of nature, and a questionable activity, then credence would be bestowed on the nay-sayers, characterizing the physicists as mad scientists who have the ability, power, and irrationality to destroy the world (for example, see Fred Moody's piece). On the other hand, if the

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[137] Dorothy Nelkin, *Selling science* (W.H. Freeman and Company: New York, 1987): 138.
[138] Email correspondance from Patrick Decowski with author (15 December 2002).
[139] "Who'll be left to say sorry?" *The Evening post (Wellington)* 10 June 2000; Mark Sappenfield, "Birth of the universe in Long Island," *The Christian science monitor* 15 June 2000: 1; "Big bang machine gets down to work," *MSNBC News* 14 June 2000 (can be found at http://www.msnbc.com/news/314049.asp#BODY).

Brookhaven experiment was seen as replicating natural phenomenon, as being in-step-with rather than dangerous to the universe, then the experiment has no reason to be stopped.[140]

Mary Douglas presents a relevant view of the connection between nature and risks in her book *Risk and Blame*. If one were to take the experiment as natural as a given, then Douglas would place the experiment into two different thought processes – two different myths about nature that people have come to accept. The first views nature as "robust" which all perturbations to nature will "work out for the good." Nature, in this myth, "encourages bold, individualistic experimentation, expansion, and technological development." The second views nature as "fragile" with the smallest alteration affecting the landscape dramatically.[141] Even though Douglas used these myths to classify natural resource ecologists, I believe they are useful when considering the Brookhaven experiment. The first view is akin to the physicists' characterization of the experiment as "natural" while the second view is akin to those who described the experiment as "unnatural."[142]

Two particle physicists who wrote an article for *The Boston Globe* exemplified this view, even going as far as to provide nature with its own agency by making the claim "the experiments that the scientists at RHIC will be doing are similar to the ones that Nature itself has been doing."[143] Marburger and the Media and Communications Office at BNL all latched on to this idea. Many of the quotations from these people in newspaper articles underscore this notion: if the disaster scenarios were credible and scientifically accurate, the

[140] This type of debate, at essence arguing over the "natural" versus the "unnatural" has appeared in other debates over risk. The most notable is in the genetically modified (GM) food debate – where some argue that genetic modification is a non-risky activity, merely a method of speeding-up natural evolution, while others argue that that genetic modification is altering the process of nature, and thus possesses great risk for mankind.
[141] In one of her earlier works, Douglas espouses a related view of change which goes hand-in-hand with this myth of nature: "Suppose the changes we fear are involuntary because they are irreversible: once set in motion, they continue on inexorably until they cascade out of control. Irreversible changes are explosive and unstable, each deviation growing larger and larger until the environment is so latered it can never return to its original state" in Mary Douglas, *Risk and culture* (University of California Press: Berkeley, 1982): 21.
[142] Mary Douglas, *Risk and blame* (New York: Routledge, 1992): 263.

moon would have been destroyed by now (and us with it) since similar "experiments" (collisions) had been occurring via cosmic rays for millions of years. In fact, the Brookhaven report examined in Chapter 3 even said that the universe had performed these "natural 'experiments'" before.[144] Besides providing empirical evidence for the safety of the RHIC experiment, the use of the term "experiment" and employing of the terms natural/nature fashioned the RHIC experiment as something that was normal, and would merely examine a robust Nature.

On the other hand, articles were written which characterized the experiment as venturing into the unknown, creating collisions of higher energy than had even been reached by previous accelerators (Walter Wagner's letter highlighted this concern). The experiment had been characterized as recreating creation, and producing something that had not been seen for eons (the quark-gluon plasma):

> The ions in the two tubes will travel in opposite directions to increase the power of the collisions. When they smash into each other, at one of the several intersections between the tubes, they generate miniscule fireballs of superdense matter with temperatures of about a trillion degrees – 10,000 times hotter than the sun. Such conditions are thought not to have existed – except possibly in the heart of some dense stars – since the Big Bang that formed the universe between 12 billion and 15 billion years ago.[145]

This passage characterizes the experiment in very unnatural terms. The experimental setting is described (tubes and all), and it does not involve some natural process, but rather some forced process created by high-technology-based machinery. The rarity of the event and the uncertainty regarding the knowledge surrounding it ("except *possibly* in the heart of some dense stars" [emphasis added]) is presented to the audience. Colorful terms like "plasma," "fireballs" and "superdense matter" evoke a visualization of a supernatural experiment. And

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[143] Reucroft and Swain, "Diffusing fear of 'big bang' machine," *The Boston globe*.
[144] W. Busza, R.L. Jaffe, J. Sandweiss, and F. Wilczek, *Review of Speculative "Disaster Scenarios" at RHIC* (28 September 1999): 5. The report can currently be found at the RHIC webpage (**Error! Bookmark not defined.**)
[145] Leake, "Big bang machine could destroy Earth," *Sunday times (London)*.

the astronomical numbers mentioned (the temperature even put in context of the sun) definitively place the experiment outside of the norm for a layperson. The experiment is unnatural – venturing into territories that have not been explored. It is this fear allows Moody's article about technological hubris and risk to have meaning for his readership. The scientists are no longer rational human beings investigating nature. Rather, it could be claimed by this camp, they are playing god by altering it.

Scientific Disunification

The scientific community vehemently defended the RHIC experiment – and different scientific papers were written regarding the disaster scenarios. One grew out of the investigative committee's report, and another was written by physicists at CERN. It is these two articles that I consider, since the differences in them demonstrate a breach within the scientific community.

Both came to the same conclusion – that the disaster scenarios were firmly excluded from the experiment's outcome. However, the methods used to derive this conclusion provided a difference in scientific opinion. This difference in method was eventually to cause a small rift in the scientific community – for non-scientific reasons.

In the latter published article, written by the investigative committee members, the authors wrote:

> Dar *et al.* (1999) [the physicists at CERN] have made an extreme model of strangelet production, where production is completely confined to central rapidity. We know of no physical motivation for this assumption. On the contrary, what we know about particle production in heavy-ion collisions argues against such a model... Although we find such a model impossible to justify on any theoretical grounds, we will use this rapidity distribution when we review the work of Dar *et al.* (1999).[146]

The authors, especially Jaffe, found this model (and other assumptions) to be contradicted by experimental evidence. Even beyond this criticism, the authors found the model "ad hoc"

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[146] R.L Jaffe et. al. "Review of speculative 'disaster scenarios' at RHIC," *Reviews of Modern Physics* **72** (October 2000): 1125-1140 on 1136-1137.

as these assumptions were even more "worst case" than the ones in the initial report released by the investigative committee. However, Jaffe and his committee still decided to use the CERN physicists assumptions in their paper, performing a series of parallel calculations, one based on his assumptions and the other based on their assumptions, just as Teller had done decades earlier in *LA-602*. This had to be done – as Dar, De Rujula, and Heinz had made even looser assumptions than Jaffe and his colleagues – in keeping with the worst case scenario. But the question that is raised, and that Jaffe feels was violated by Dar and his colleagues, is where does one draw the line for a worst case assumption? His disdain for these assumptions was made clear in an interview:

> Dar, De Rujula, and Heinz had an idea that they wanted to play with about how strangelets would have behaved in the early history of the universe, and like many of my theory colleagues, they were infatuated with their idea. Their idea provided no useful information if our assumptions about strangelet products were correct, so they wrote a paper in which they assumed very unlikely ad hoc mechanisms, things that most scientists who know the subject would consider outrageous. But if you make that assumption, then the bounds that we derived evaporated, and that opened the door for the bounds that they were interested in discussing to become the operative bounds.[147]

In other words, Jaffe saw his research as being undermined by the Geneva-based physicists. More than that, he saw the CERN paper as an opening for reporters to latch on to the notion that the Brookhaven report had credible physicists disagreeing with it:

> people like Wagner... they latched on to the Dar, De Rejula, and Heinz paper and said "look legitimate scientists disagree with your premises." Furthermore, it turns out that Dar De Rejula, and Heinz's idea has a flaw in it, a rather obvious flaw, so their proposed limit is not valid. So if you believed everything that you see in the literature, they demolished our limit and then we demolished their limit, and so Mr. Wagner can say "look there are no limits."[148]

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[147] Robert Jaffe, Interview (22 November 2002).
[148] Robert Jaffe, Interview (22 November 2002).

To Jaffe, the scientific community was not putting on a face of solidarity in a time when public opinion was questioning the validity and safety of its work.[149] The scientific accuracy of the articles was not the only concern Jaffe held; his concern was of scientists themselves providing the ammunition to those attempting to put a metaphorical cog in the RHIC experiment.

Conclusion

Scientists working on the RHIC experiment had a choice. They could either ignore the initial negative press generated from the *Scientific American* letters and the *Sunday Times (London)* article or they could respond. Scientists and friends lead other scientists involved in the project to various news articles. Suggestions were provided by one physicist to his colleagues on how to deal with reporters. Articles which misrepresented the situation were criticized within the Brookhaven circles. To those outside Brookhaven, physicists spoke to reporters, attempting (sometimes in vain) to give a scientific yet understandable view of the risk (as being a non-risk). Brookhaven's Office of Media and Communication had two press releases written dismissing the risk. And the investigative committee's report dismissing each disaster scenario was put on the Brookhaven website, for reporters and lay persons to access – with the report itself geared away from the high energy particle physics community and towards the layperson

The physicists at Brookhaven were afraid – not of a strangelet transforming matter around it or of a black hole suddenly devouring the solar system. They were scared of bad publicity and generating public fear. After years of construction, hundreds of people, and hundreds of millions of dollars, this single bout of bad publicity could shut down the experiment (Walter Wagner acted in this climate). At some level, through their actions, they

[149] In one unofficial and online poll held on MSNBC, in two days after being placed online, 22% of the voters

showed an understanding that the scientific knowledge is constructed as Sheila Jasanoff contends

> partly in accordance with norms internal to science, such as empirical testing and peer review, but partly also in accord with external social interests, including the political interests of particular scientific communities.[150]

But even with the scientific community somewhat worried about the negative publicity, debates took place about the actual science behind the risk analysis of the disaster scenarios – namely between the investigative committee (Jaffe, Busza, Sandweiss, and Wilczek) and a group based in Geneva (Dar, De Rujula, and Heinz). The major concern that Jaffe had over the debate, perhaps even overriding his concern with the correctness of the scientific analysis, was the breach the differing analysis provided, which reporters could exploit – the scientific disunification which gave credibility to the notion that scientists do not know how to understand the Brookhaven risk completely.

Brookhaven physicists did not passively take the brunt of the negative publicity. They actively worked to counter the publicity in the small ways they could – by voting in online polls and talking to reporters. They understood that public opinion of their experiment, and of scientists, were in some small sense, at stake in this debate.

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believed that "the potential risks are worrisome" (data recorded in **Error! Bookmark not defined.**).

[150] Sheila Jasanoff, *Risk management and political culture* (Russell Sage Foundation: New York, 1986): 70.

Chapter 6: Conclusions

I started researching this topic without a clear understanding of the subject matter at hand. I had only been informed by the newspaper articles I had read, and I initially had believed that a real crisis in modern science was at hand: physicists were being provided, by their knowledge, with uncontrolled power – no real checks and balances. I chose to pursue the topic to see if we, meaning all humanity, had crossed yet another threshold of knowledge allowing for world destruction. My focus originally was to be on power relations, rights, and morality, while risk was demoted to a secondary concern.

As my research progressed, I found a vastly different story unfold, the story told in this work. I began questioning the entire notion of knowledge, of fact, of objectivity – not in a philosophical but rather cultural sense. I found that knowledge is culturally informed, that risk is a cultural construct.

The story of the disaster scenarios at Brookhaven does not have a single ending point. The experiment was performed in 2000 and the world did not end with a whimper nor a bang. But does the story really stop then? Even in 2001, newspaper articles were referencing the story. Other proposed particle colliders took in account this media disaster to avert having their own media disasters. And in 2003, the RHIC website still has the investigative committee's report on its homepage. A residue of this event was still present after the experiment.

It could be argued that this analysis is not complete. It considers the presentation of risk and happily ignores the cognitive aspect of risk. Moving back one more step in our tracking of risk, what causes an interpretation of risk in the minds of people?

This deficit was due to three major reasons. First, pragmatically, I did not have the time or resources to seek out and interview many scientists, reporters, or laypersons about their personal views. With this, I could have started noticing patterns in understanding about risk, as other researchers have. Second, many of the existing analyses about risk from the cognitive standpoint rely on the misunderstanding of basic elements in probability theory; they take a highly rational approach to the subject, finding misunderstandings in mathematical relationships which lead to misinterpretations of risk. This leads to my last reason, and most important: the risk that I was considering in this paper is a catastrophic risk. In a sense, it was a paragon of a perceived risk. Because of this, the risk that spread in the media was not based on mathematical fallacies as much as on cultural fears and literary presentation. In fact, as we saw, mathematics played an almost negligible role in the newspapers' coverage of the story.

This work extends the literature on risk. I utilized the works of only a few theorists (primarily Mary Douglas and Dorothy Nelkin). Much of the work written on risk, notably Ulrich Beck, focuses on risk as a cultural and social construct. However, these works for a large part assume that risks are created by some group or other and then simply *are*. The analysis takes risks from creation to cultural battles. The story that took place in-between these two was left out. Rarely did I see any mention of how risks are portrayed (via literary methods) nor how risks diffused to the audiences where they then are prime to be in the social arena for debate (to where the risk becomes part of a social consciousness). It is this gap I attempted to fill with this analysis.

In these pages, we have come to understand risk as an extremely powerful and loaded term. While unpacking the term, I noted that risk has to be examined as a twofold entity – an objective risk and a perceived risk. And as objective risks are by definition inscrutable, this work deals with risk as a *risk-in-the-world*. Using the Brookhaven National Laboratory as our case study, I attempted to extrapolate some general properties of perceived risk. The properties of this perceived risk were teased out in Chapters 2, 3, 4, and 5. I enumerate them below:

Properties of (Perceived) Risk

1. It travels in certain paths, somewhat determined by cultural factors (e.g. what is considered newsworthy? does this affect our ability to perform the experiment? is this another technological fear?)
2. It preys on cultural fears
3. People appropriate risks and fit them into their worldview (risk appropriation)
4. It is generative and shifts form as it travels (in one sense, a single risk traveling becomes many risks)
5. It arises out of scientific uncertainty which must be mitigated by *judgements*
6. It is constrained by the profusion of ambiguous words (the language of risk) and shaped by literary techniques
7. It is used as a weapon in a battlefield over politics, funding, social acceptance, etc.

The multifaceted view of risk as a traveling entity, as a piece of mutating information, was provided.

The vantage point we took was of information traveling. Earlier, in Chapter 2, I invoked social theorist Bruno Latour's Actor-Network Theory. I will now revisit it. In *Science in Action*, Latour propounds:

> A statement is thus always in jeopardy, much like the ball in a game of rugby. If no player takes it up, it simply sits on the grass. To have it move again you need an action, for someone to seize it wand throw it... At any other point, the trajectory of the ball may be interrupted, deflected or diverted by the other team... The total movement of the ball, of a statement, of an artefact, will depend to some extent on your action but to a much greater extent on that of a crowd over which you have little control. The construction of facts, like a game of rugby, is thus a collective process.[151]

This passage sums of many of the views that we have uncovered about risk. Risk needs to be "picked up" in order to spread (otherwise, it stagnates). Those who pick it up are those with some interest in controlling it (either the scientists, the media, opponents of Brookhaven) and these groups are, in their attempt to control it, by definition working against each other

[151] Bruno Latour, *Science in action* (Harvard University Press: Cambridge, 1987): 104.

in some way. And most important, noted by Latour, is the action of the "crowd" – which I interpret as the society at large. The cultural values, which guide who cheer on certain "teams," these are the real controllers of risk. They are the underlying force guiding the path of the risk as well as the determination of the teams.