SCIENCE, CONFLICT AND THE DEVOTIONAL ARTIFACT: A SOCIAL CARTOGRAPHY OF THE TURIN SHROUD CONTROVERSY

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Abstract

This dissertation comprises an historical case study of a scientific controversy concerning the provenance, authenticity and material attributes of a Catholic devotional artifact known as the Shroud of Turin, an ancient linen burial cloth imprinted with a faint image of a man who bears the marks of a Roman crucifixion. Beginning with an account of a 1978 scientific inquiry into the shroud carried out by a team of American scientists, this project traces the charged epistemic disputes that unfolded among scientific and non-scientific actors over the question of the shroud's historical status, which endured for years following the inquiry. It is argued here that scientific disputes concerning a devotional artifact lend distinctive insights into how we might comprehend the conditions underlying epistemic conflict, the agential role of material objects in configuring and circumscribing controversy, and the reasons why closure in controversy is complicated by scientists' reverence to the socio-material ambiguities of the devotional artifact itself. It is further argued that the epistemic and political disputes among shroud scientists were compelled by contenders' commitment to both the instrumental and intrinsic value of imparting a credible scientific narrative of the Turin shroud for the broader social milieu.

Dedication

For my mother, with absolute love.

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Introduction

This dissertation found its inspiration in a peculiar controversy. In December 2002, the Royal Ontario Museum held a six week long exhibit of the James Ossuary, a stone chest bearing an Aramaic inscription translated as "James, son of Joseph, brother of Jesus." The exhibit followed on the heels of a claim, published in the *Biblical Archeology Review,* that the ossuary was an authentic relic, thus making it one of the earliest known material objects to reference the historical Jesus. Not surprisingly, the ROM exhibit drew considerable public and professional interest and even inspired modest veneration from some visitors (Gatehouse, 2005).

Shortly following the exhibit, the authenticity of the ossuary came under intense scrutiny by an international community of scientists and antiquities experts who claimed that the inscription was a forgery crafted by an unscrupulous antiquities dealer. The ROM was censured for heedlessly and opportunistically exhibiting a relic whose provenance had not been established by legitimate scientific techniques (Gatehouse, 2005). A highly publicized controversy involving scientists, forensic experts, historians and antiquities scholars ensued from the initial forgery charge. The authenticity disputes included a seven year legal battle waged among scientists and archeologists in an Israeli court. In March, 2012, judge Aharon Farkash ruled that the prosecution did not provide sufficient evidence that the inscription on the ossuary had been forged, and the antiquities dealer, Oded Golan, was acquitted of the charges (Friedman, 2012).

A long tradition of scholarship in science and technology studies (STS) has called attention to the theoretical importance of scientific controversy as a social phenomenon.

Indeed, it is through conflict within and beyond scientific communities - the struggles over meaning, resources, expertise, power, and "contested values"-that we are provided the surest insight into the social dynamics of knowledge production, and its implications for social worlds beyond the boundaries of science (Nelkin, 1995, p. 445). Controversy, as Harry Collins and Trevor Pinch (1979) have noted, "highlights social processes with particular clarity" (p.238).

Controversies concerning the authenticity of devotional relics are marked by complexities, tensions and struggles that are both consonant with, and distinct from, the kinds of conflicts that have comprised the analytical interests of science studies scholars thus far. To date, however, the field has paid modest attention to this kind of controversy, despite the unique insights it brings to our understanding of both the social relations of science, and the underlying conditions and dynamics of controversy. The conflicts waged among scientists over the provenance and authenticity of objects of devotional significance thus invite deeper analytical and theoretical clarity.

I began with the James Ossuary story because it inspired the empirical themes of this dissertation. Specifically, I began to take a deep interest in the constitution, effects and implications of the work scientists do when constructing, defending, and competing over evidentiary claims about historical objects of disputed origin, and particularly artifacts bearing religious significance. The James Ossuary controversy compelled me, moreover, to recognize the sociological importance of the devotional artifact as a *contested terrain,* where several epistemic and ideological positions intersect and collide. What, then, does a sociological lens bring to bear on the meaning and implications of the

socio-political struggles and epistemic conflicts waged among scientists over such objects? What constraints, negotiations and tensions are inherent in, and unique to, this kind of scientific controversy? How and *why* do scientists become involved in such disputes, and what is at stake for science in waging battle over the historical status and possible authenticity of a religious artifact? What are the consequences of scientists' involvement in determining the provenance of a religious object?

At the outset, many of these questions seem to invite straightforward answers. We can rightly assume that when the stakes of a dispute among social actors involve legal or intellectual custodianship of an object, a territory, or an idea, the motivating factors are rooted implicitly in the interests (social, political, professional, ideological or otherwise) of disputants. Often resolution rests upon how effectively such competing interests are mobilized, the relative credibility of disputants, or the strength of resources deployed by them. Where disputes are grounded in matters of epistemological consequence, resolution relies not only on the potency of evidence, but the persuasiveness of its rhetorical articulation within competing domains of expertise (Callon, 1999; Golinsky, 2005; Latour & Woolgar, 1986; Latour, 1987; Nelkin, 1995).

As I will demonstrate, the provenance disputes waged among scientists over a religious artifact are indeed compelled by political and ideological interests of disputants. Moreover, the veracity of competing evidence claims do indeed rely on the persuasiveness of their articulation in various communicative forums. The strength of rival evidentiary claims rests, too, on the strength of allies assembled in laying claim to competing truths. But I suggest that scientific controversies concerning objects of faith

are motivated and sustained by more than what a reductivist analysis of their socio-political dimensions might reveal. Before I elaborate on this position, I will introduce the empirical focus of this work.

Science, Controversy and the Devotional Artifact

This project concerns the disputes among scientists and non-scientific actors that have erupted, and continue to be waged, around a Catholic artifact known as the Turin shroud, an ancient linen burial cloth imprinted with a faint image of a man who bears the marks of a Roman crucifixion.[1] Believed by pro-authenticity advocates to be the funeral shroud used to enfold Jesus Christ's body following the crucifixion, the Turin shroud is considered to be one of the most venerated religious artifacts in Western culture (Meacham, 1983).[2] Among its more fervent adherents, the Turin shroud is a "miracle cloth" - a divinely wrought testament of the resurrection. Skeptics, however, have long claimed that the Turin shroud is simply one among hundreds of objects that have comprised a thriving economy of spurious religious relics throughout history (Drews,

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[1] My use of the term "devotional" artifacts generally refers to objects of spiritual import to Western and Abrahamic faith communities. Although the Turin shroud is described interchangeably as an "artifact" and a "relic", I choose the former as a more precise term of reference for my purposes. While the Turin shroud is revered by some pro-authenticity adherents as a "sacred" object, it has not been formally consecrated by the Roman Catholic Church as a holy object.

[2] The Turin shroud is woven from Near-East Mediterranean basin-flax and measures fourteen feet long by three feet wide. The image on the cloth is often referred to as a "scorch" mark for lack of an adequate descriptor (Oxley, 2010). The cloth contains markings resembling bloodstains at the head, wrists and feet, and scourge wounds indicative of a Roman crucifixion. It is believed to be the burial shroud used by Joseph of Arimathea and Nicodemus to wrap Jesus Christ's body following the crucifixion, as described in the Gospels of the New Testament. Although the shroud is owned by the Catholic Church and has been integrated in the Church's religious celebrations throughout history, its veneration is not limited to members of the Catholic faith.

1984). As such, the shroud is marked by an extensive legacy of debate and often vehement conflict over questions of its provenance and authenticity.[3]

As with many religious antiquities of indeterminate origin, conflicts over the shroud's authenticity long pre-date contemporary disputes, most notably among historians and theologians.[4] Three points of contention have endured throughout the shroud's history. First, its historical chain of title is continually contested among scholars. While several theories concerning the shroud's earliest history have been proposed by historians, no consensus has been reached among shroud scholars.[5] The second recurring point of contention concerns the question of image formation. Throughout its history skeptics have attributed the shroud image to the "clever skills" of an artist, and several techniques have been employed to reproduce the image in an effort to substantiate what is commonly referred to as the "human artifice theory." Finally, the third and perhaps most controversial issue of contention concerns the shroud's exalted rank as the genuine burial cloth of Jesus of Nazareth. Attempts to "prove" the shroud's authenticity have typically involved correlating the physical characteristics of the image (i.e., the location of the

[3] The question of the shroud's provenance refers specifically to whether or not the cloth originates from the time of Christ's death. The question of authenticity refers to whether or not the shroud is the genuine burial cloth of the historical Jesus.

[4] Archeologist William Meacham (1983) notes that, "centuries before science cast the issue in a totally new perspective, disputes over the authenticity of the Shroud involved eminent prelates and provoked a minor ecclesiastical power struggle" (p. 283).

[5] It is generally accepted among historians that the Turin shroud was brought to Lirey, France, circa 1355 by a French knight named Geoffrey de Charney. See Appendix A for a brief account of the early history of the shroud.

wounds and scourge marks) to scriptural accounts of the crucifixion.[6] Skeptics refute such claims as thoroughly misguided, and suggest that they have had implications for the shroud's misapprehension as a genuine artifact of the crucifixion (Drews, 1984).

Specific to my interests is the fact that science has inserted itself into a longstanding conflict concerning the shroud's historical status and material properties. Contentions among scientists have centered largely on conflicting evidence claims concerning the shroud's chemical and organic components, and the true age of the linen. As this study will show, following a 1978 study of the shroud by a team of American scientists known as the Shroud of Turin Research Project (STURP), the shroud became a subject of intense controversy both within and beyond the scientific community.[7] Disputes during this phase of the conflicts rested primarily on the "human artifice theory" – the question of whether the shroud image was made by a human body or was an artistic rendering.

In 1988, the shroud was famously demystified by a team of scientists who carried out a series of comparative radio-carbon dating tests and concluded that the shroud was a 14th century relic. While this finding settled the matter for many skeptics, it also ignited a new phase of disputes among scientists who challenged the reliability and ethics of the 1988 carbon dating tests. Authenticity debates have continually surfaced as new evidence

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[6] Certainly one of the most frequently cited studies of the shroud is that of surgeon and anatomist Pierre Barbet, who published a series of forensic experiments in 1953 in a book entitled *A Doctor at Calvary*. Barbet's interests focused on the manner of death of the subject of the shroud, and his experiments centered on the crucifixion markings on the cloth (in particular, the direction of blood flow on the wrists and feet) (Barbet, 1953). Barbet's work is often evoked among pro-authenticity advocates as "proof" of the shroud's authenticity.

[7] Hereafter referred to as "STURP".

claims drive the contending discourses in one direction or another. Recently, a dispute among shroud scholars emerged around the claims of Italian organic chemist Luigi Garlaschelli, who publicly announced in 2009 that he was able to reproduce the shroud image using medieval techniques (Greene, 2009).

Thus alongside the contemporary scientific controversies concerning cold fusion, the curative potency of Vitamin C, the effects of fluoridation in water, causal theories of AIDs, and the apposite place of creation science in public schools (to name a few), an equally complex and antagonistic controversy has raged among scientific and non-scientific actors in recent history, yet it has gone relatively unnoticed by controversy analysts. The conflicts waged over the provenance of the Turin shroud find common ground with most scientific controversies that have captured the interest of science studies practitioners. As this study will demonstrate, disputes among shroud scientists rested not simply on the strength of their competing evidence, but on the relative capacities and strategies deployed by opposing actors and institutions to enforce the legitimacy of their evidence claims. The tactics and relations of conflict underlying this controversy thus evoke analytical themes familiar to controversy studies. Contrarian science, dissent, suppression, boundary work, relations of power, and contests of credibility all find relevance in this case study, and will be considered in some depth (Delborne, 2005; 2008, 2011; Fujimura & Chou, 1994; Gieryn 1983, 1999; Shapin 1995; Martin, 1991, 1999, 2008).

Like most socio-scientific controversies, the epistemic and political struggles around the Turin shroud have transcended the fictive boundaries of science itself. The

shroud controversy has embodied the entanglements of a diverse array of social actors whose interests and interventions have had a considerable bearing on the dynamics and vicissitudes of the disputes. This case study will demonstrate, therefore, that when the site of epistemic struggle involves an unconventional object of scientific interest (specifically, one of no apparent scientific value), the character of the disputes, in some ways, does not deviate appreciably from other scientific controversies that have been the focus of intellectual interest in science studies.

But this is only a starting point. The conflicts precipitated by the Turin shroud are also importantly distinct from conventional scientific controversies. The object of contestation, a religious artifact, bears a cultural meaning and a place of relative importance in the social landscape that differs markedly from that of techno-scientific objects. The shroud is ostensibly venerated as a sacred artifact by a large population of devout followers. Simultaneously, this artifact has drawn considerable secular and scientific interest, owing in large part to its puzzling characteristics. Most importantly, however, the provenance/authenticity disputes waged among scientists over this artifact do not offer the same material outcomes or implications as do the conflicts that erupt over techno-scientific objects.[8] That is, there are no medical innovations to be had, no technological designs prevailing over others, and no public policies implemented by the closure of disputes. I propose, however, that there is intellectual value in accounting for

[8] The Turin shroud is perhaps best understood as an "ideotechnic" object. In archeology, "ideotechnic" objects refer to material artifacts that signify the "ideological rationalizations" of a social system or cultural group (Binford, 1962, p. 219). Religious relics and other symbolic objects are designated as such because they function as a means by which social actors are enculturated into the "symbolic milieu" (p. 219).

the conditions, impetus, and endurance of scientific disputes waged over objects of religious significance. Accordingly, the aim of this case study is to illustrate the value of extending the empirical and interpretive lens of controversy studies and to account for the conflicts that manifest around seemingly marginal objects of contention. In what follows, I examine how disputes involving a devotional object lend new insights into how we might understand the conditions underlying epistemic conflict, the agential role of material objects in shaping and circumscribing controversy, and the reasons why closure in controversy is complicated by what I call scientists' "secular reverence" to the material ambiguities of the devotional artifact.

As this case study will demonstrate, within the various phases of the shroud controversy, the points of contention among shroud scientists have centered on determining how scientific interventions on the artifact should properly clarify and resolve its ambiguities. Indeed, the quest to assign a credible interpretation to the shroud often served to affirm science's authority to achieve this understanding, as well as the *necessity* and *obligation* of science to do so. My account of these disputes over the shroud's provenance, however, is focused not simply on the question of *how* the conflicts were waged, or *who* prevailed in them, but more importantly *why* scientists entangled themselves in this curious and contentious endeavor at the outset. I argue that the impetus for the protracted tensions among scientists over questions of the shroud's provenance cannot be readily reduced to instrumental interests alone. Rather, the endurance, complexity and vitriol of the epistemic and political disputes among scientists entrenched in this controversy were compelled by their commitment to both the instrumental and

intrinsic value of imparting a credible account of the Turin shroud for the broader social milieu.

Methods: A Social Cartography of Controversy

The attempt to document any large-scale scientific controversy is a methodological trial for the social researcher. Controversies are complex and confounding precisely because "they are the crucible where collective life is melted and forged: they are the social at its *magmatic* state" (Venturini, 2010, p.265). Like most scientific controversies, the Turin shroud controversy is circuitous, unpredictable, and indeed confounding in its complexity. It has comprised rather atypical entanglements of social actors (e.g., clerics, scientists, faith groups and skeptics); institutions (e.g., churches, palaces and scientific laboratories) and material objects (e.g., relics, religious art, microscopes, human blood and X-rays). The conflicts I describe here are recurrently settled and re-ignited, waged in contemporary social life, but entrenched in an extensive historical backdrop of similar convolutions and tensions. And we cannot forget that the Turin shroud, the key material actor in this story, is swept along the currents of these disputes and inscribed along the way with multiple narratives of truth. Each iterative layer, like new stains on the cloth, make closure or purity of explanation that much less likely.

Given these interpretive challenges, my methodological approach takes guidance from the empirical principles of social cartography, a program of analytical techniques adapted from the conceptual tenets of Actor Network Theory. As implied by its

theoretical counterpart, a social cartography approach attempts to account for and interpret the conflicts of collective life as complex networks of socio-material actors "where alliances and opposition transform recklessly" and where the most heterogeneous relationships are forged (Venturini, 2010, p.262). Analytic priority is therefore placed on the heterogeneity of human and material actors embedded in a particular conflict.

Social cartography does not simply "map" the boundaries of dispute but examines the intersections of heterogeneous actors who make up the controversy, and whose presence or absence bears markedly on the dynamics of the conflicts. Deploying several observational lenses, the social cartographer attends to the intricate relations among humans, artifacts, and technical objects, and considers how these intersections have implications for the vicissitudes of the disputes. The analyst is urged to consider as well the "dispersed discourses" of the dispute under question, as well as the "thick mesh of relations" surrounding the controversial statements and their implications for animating the debates (Venturini, 2010, p. 266).

Finally, social cartography attends to the ideological meaning actors assign to the conflict, and to the desire to find stability in closure. Specifically, what reasons do actors attribute to their positions, and what "vision of the world" do they assume might be achieved by it (Venturini, 2010, p. 266-267)? This methodological framework is appropriate and useful to the exploratory nature of this study and its empirical distinctiveness. As noted previously, scientific controversies involving devotional relics have not been attended to in any empirical depth in science studies, and consequently there is rather modest methodological guidance for unraveling their complexities and

singularities.

My account and interpretation of the Turin shroud controversy draws upon several sources of data. I examined a selection of peer reviewed articles published in scientific and technical journals, in which evidence claims about the shroud have been disseminated and debated, as well as a selection of articles published in popular science journals such as *Nature* and *Science*. Given that the epistemic disputes intersected with other disciplinary forums, I extended my analysis to academic literatures located in disciplines such as anthropology, history, and biblical archeology. My analysis also focused on a selection of first hand narrative accounts of the shroud controversy written by shroud scientists and published for mainstream consumption, as well as secondary accounts written by members of the shroud science community. Finally, I examined mass media representations of the shroud disputes as they unfolded following the 1978 STURP inquiry in Turin.

The relativist analysis of scientific controversies advocates a symmetrical treatment of conflicting claims and positions in a dispute, assigning no epistemological priority to the legitimacy of the claims themselves, and no primacy to the relative authority of their proponents (Scott et al., 1990). This methodological principle, originating in the Strong Program, represents a response to the positivist tradition, whereby objectively factual scientific claims in controversies are set against less authoritative, and by implication, distorted knowledge (Bloor, 1976). In keeping with the relativist program, I employ a symmetrical approach in this study, to the extent that I attend to the shroud debates with no investment in the veracity of any of the opposing

claims. My analysis is not concerned with the relative legitimacy of authenticity claims themselves, but with the ways in which conflicting evidence is assembled, deployed, contested and competes for primacy within the controversy itself.

Theoretical Frameworks

This project draws from diverse theoretical sources in science and technology studies. My analysis of the STURP inquiry and its outcomes is guided broadly by a social constructivist framework, and draws specifically on theoretical insights concerning discursive representational practices in science. Both literary and visual representational practices are key to the production of contemporary scientific knowledge about the shroud, as well as the scientifically-mediated transformation of this artifact into a comprehensible object. Accordingly, I delineate the theoretical significance of STURP scientists' social, epistemic and discursive practices and their implications for how the material properties of the Turin shroud were made legible and comprehensible.

My analysis of the controversy that followed the STURP inquiry is in dialogue with the theoretical insights offered by controversy studies and related schools of thought in STS. I also seek, however, to illuminate how elements of the shroud conflicts are not fully comprehended by the conceptual frameworks of controversy studies. This work is also informed, therefore, by theoretical perspectives concerning conflict as a social phenomenon that have not been taken up in great depth in controversy studies. Specifically, I draw from the theoretical work of Georg Simmel in order to delineate the complexities and paradoxes of the shroud conflicts beyond their ostensible antipathies,

struggles and competing interests. I apply Simmel's ideas of "convergent" and "divergent" currents that underlie the relations of conflict among social actors, as well Simmel's broad conception of conflict as a positive (or constructive) form of sociation.

I also take theoretical guidance from the approach offered by the work of Victor Turner (1974), who conceptualizes large-scale conflict among social actors in terms of what he calls a "social drama", or the "aharmonic or disharmonic process arising in conflict situations" (p. 37). Turner's conceptual framework finds particular relevance to the large scale public controversy that preceded the radiocarbon dating of the shroud in 1988.

Outline of Chapters

Chapter One comprises an overview of the foundational and emerging scholarship concerning epistemic disputes within science, as well as the interpretive accounts of scientific controversies that encompass the broader political, ideological, and ethical concerns of both scientific and nonscientific stakeholders. Along with attending to the ways in which the various schools of thought in controversy studies inform this case study, I discuss how the shroud controversy finds a constructive place in the literature. In this chapter I also engage the theoretical perspectives of Georg Simmel and Victor Turner and discuss how their approaches to comprehending the social relations of conflict lend valuable conceptual clarity to the disputes described in this work. These theoretical frameworks, which have thus far found scarce expression in controversy studies, offer a useful means by which controversy analysts might interpret the impetus, character and dynamics of conflict among social actors.

Chapter Two comprises an empirical account of the Shroud of Turin Research Project (STURP) inquiry that took place in Turin in 1978. My specific analytical focus concerns how the Turin shroud was translated into a problem of scientific consequence, and how evidence claims concerning the shroud's material composition and provenance were assembled, represented, legitimized and defended by STURP scientists. As such, my guiding empirical question asks: when scientists intervene on the devotional object, *how* do they do so, and *what* is revealed by their interventions? Situating the analysis within a constructivist framework, I focus on the symbolic, epistemic, social and political relations and practices of STURP scientists, and demonstrate how the Turin shroud was not simply interpreted by scientists, but was *captured and re-constituted* by their interventions. I argue that the STURP team's evidentiary representations of the shroud had implications for precipitating the contemporary controversy over the artifact's historical status and material constitution.

In Chapter Three I explore the controversy known as "The Great Blood Dispute" that arose among shroud scientists following the 1978 STURP inquiry, which involved two conflicting evidentiary claims concerning the shroud's material composition (i.e., whether the shroud was composed of human blood or paint). I argue that the blood dispute was underlined and driven by disputants' commitment to both the instrumental and intrinsic value of delivering a truthful scientific interpretation of the shroud to the broader social milieu, as well as their belief in science's cultural authority and obligation to do so.

In Chapter Four, I explore a controversy that raged alongside the Blood Dispute which concerned the question of the shroud's historical origins. Here the contentions among scientific and non-scientific actors were focused around the utility of radiocarbon dating as the appropriate technique for settling the longstanding question of the shroud's provenance. I explore the radiocarbon dating conflicts with an analytical focus on how and why scientists and other social actors sought to call forth definitive evidence of the Turin shroud's provenance, and more importantly, why this pursuit precipitated such antipathy and struggle. I argue that the question of the shroud's provenance emerged as a "priority of narrative" dispute, where the truth of the shroud's historical status was contingent upon who would shape and govern the correct scientific narrative of the artifact. I will also demonstrate how contenders in the radiocarbon dating controversy articulated the role of science in matters of religious consequence for the broader public.

In Chapter Five I take account of how the results of the radiocarbon dating of the shroud rendered a final verdict concerning its historical origins. My specific analytical focus in this chapter examines how and why the radiocarbon dating outcomes precipitated a collective resistance on the part of members of the shroud science community, who denounced the validity and ethics of the radiocarbon dating venture. I argue that the post-closure dissent reveals the extent to which science embraces uncertainty as intrinsic to the creative repertoire of its practices and interests.

A Note on the Value of Unorthodox Ventures

As indicated earlier, the Turin shroud controversy is an underexplored cultural phenomenon that invites deeper scrutiny. I have taken up the task of applying a sociological lens to the science of devotional objects in order to comprehend the distinct complexities and constraints that underlie scientists' encounters with decidedly unorthodox scientific pursuits. The theme of unconventional or marginal science has typically been attended to in science studies by situating the analysis within rather ephemeral binaries (e.g., science versus pseudoscience, "rejected" versus "accepted" knowledge). From the outset my approach has avoided characterizing shroud science as a "fringe," "pseudo" or "marginal" (and by implication inconsequential) scientific interest. To do so, I suggest, would be to tell an incomplete story. To be sure, shroud science has often been in tension with the stern conventions of orthodox science. While much of the work carried out by shroud scientists has appeared in several legitimate peer-reviewed scientific journals, and while the study of the shroud has appealed to the expertise and interests of numerous scientific fields, shroud science is often considered unworthy of a place in the province of legitimate scientific inquiry. As a focus of sociological interest, however, the science of devotional artifacts and its concomitant discontents is a rich terrain of inquiry, and offers a novel perspective on the social relations and entanglements that emerge around the contested objects of our socio-material world.

Chapter One: Conceptualizing Scientific Controversy

Introduction

Studies of scientific controversies find common ground in the notion that controversy discloses the critical encounter between science and the broader social world (Jasanoff, et al.,1995). As an enduring intellectual concern in science and technology studies, controversy in the social relations of science has invited a diverse range of empirical and theoretical perspectives. In this chapter I engage with some of the foundational and emerging STS scholarship concerning scientific controversies, beginning with an account of the literature that imparts an interpretive understanding of the internal disputes waged within the scientific community over competing epistemic claims. I follow with a synoptic overview of the relevant scholarship concerning public scientific controversy - the large scale disputes that extend beyond the localized epistemic divisions among scientists to encompass the broader ideological, ethical, and political interests of both scientific and non-scientific stakeholders.

In this chapter I also attend to two theoretical frameworks for understanding the social relations of conflict that have thus far found a modest presence in the scholarship, but which are useful in drawing out the nuances of controversy. I engage Georg Simmel's conceptual insights concerning conflict as a positive (or constructive) form of sociation, and discuss how Simmel's theoretical interpretation of conflict among social actors has potential for enriching an understanding of the Turin shroud disputes. I follow with an account of Victor Turner's theoretical interpretation of large-scale public conflicts or "social dramas," and discuss its analytical utility in the context of this case study.

In many ways, the Turin shroud controversy falls in line with the controversies I discuss here. As I will demonstrate in the following chapters, the disputes waged among shroud scientists over competing forms of evidence resemble the shape and tenor of the intellectual disagreements that erupt among scientific actors in any dispute that has scientific consequences. When the shroud conflicts developed into a larger scale controversy involving a heterogeneous array of actors and institutions, the disputes were fuelled by vested interests, conflicting values and competing forms of authority claiming entitlement to disclose a factual account of the shroud. As such, the shroud controversy has been waged with as much intensity, vitriol and complexity as any of the disputes that have ignited over issues of moral, political and epistemic consequence.

What is distinct about this controversy, however, is that the object that gave rise to such protracted discord is anomalous to the conventional objects of contention in science. Moreover, the resolution of the Turin shroud's provenance offers little in the way of concrete social or political implications. There are no innovations, cures, or public policies at stake in settling the ambiguities of a devotional object. Yet it is precisely these distinctions that invite analytical attention. Despite the fact that the Turin shroud is of negligible scientific value, it is within this controversy that we see science in full blown action, where scientific actors not only assert their intellectual authority to demystify the ambiguous objects of our social world but find themselves committed to an ethical obligation to do so. It is also within this controversy that we see an object of little instrumental worth to science being rearticulated as one of critical epistemic importance

to both science and the public, particularly within the phase of dispute concerning the question of the shroud's historical origins.

My broad aim in this chapter thus seeks to highlight how a scientific controversy waged over the ontological status of a religious object finds a constructive place in the scholarship. While I will demonstrate how the conceptual themes in this literature inform this case study, I also discuss how the singularities of the Turin shroud controversy raise new questions concerning the character, underlying conditions and implications of scientific conflicts concerning ideotechnic objects. I suggest that there is analytical value in extending the interpretive lens to controversies that seem, at the outset, marginal to the concerns of controversy studies, inasmuch as such controversies disclose both consistencies and paradoxes not fully addressed by the existing scholarship.

Dissent in Science

Science is marked by a long history of conflict among its practitioners, who have waged acrimonious battles over competing truths and intellectual claims to discovery (Barnes, et al., 1996; Kuhn, 1962; Merton, 1957, 1973). As R. Allen Harris (1990) notes, "science was born of strife" (p. 31). As such, its history of "fierce dialectics" represents the "cornerstone of science and the modern world" (p. 31). Granting that conflict is intrinsic to science, how have science studies practitioners construed its character, utility and effects?

Robert Merton (1957, 1973) took into sociological account the basis and function of dissent in science, particularly as it pertains to what he called the "sordid disputes" over priority of discovery (p. 635). Arguing that priority disputes are not merely rooted in

the egotism of "contentious men" who compete for professional primacy, Merton proposed broader grounds for the eruptions of contention in science (p.638). Dissent is rooted more broadly in acts of conformity to the moral authority of science's institutional norms. The "ethos of science" constitutes:

> ...that affectively toned complex of values and norms which is held to be binding on the man of science. The norms are expressed in the form of prescriptions, proscriptions, preferences, and permission. They are legitimized in terms of institutional values. These imperatives, transmitted by precept and example and re-enforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience, or, if one prefers the latter-day phrase, his super-ego. Although the ethos of science has not been codified, it can be inferred from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed toward contraventions of the ethos. (Merton, 1973, p. 269)

According to Merton, conflict over matters of priority is in large measure a consequence of the institutional normative system of honorific and material rewards granted to those who generate original intellectual products. Much like sanctions, rewards are deeply embedded in the cultural value system of science and account for the vitriol and endurance of conflict among its practitioners. As Merton notes:

> ...the social organization of science allocates honor in a way that tends to vitiate the institutional emphasis upon modesty. It is this, I believe, which goes far toward explaining why so many scientists, even those who are ordinarily men of the most scrupulous integrity, will go to great lengths to press their claims to priority of discovery... or more specifically and more completely, great concern with the goal of recognition for originality can generate a tendency toward sharp practices just inside the rules of the game or sharper practices far outside. (Merton, 1957, p. 649)

Merton's typology of the ethos of science has since come under critique in science studies for its failure to correspond to the reality of science's institutional structure, value

systems or practical aims (Collins & Restivo, 1983; Hess, 1997). As David Hess (1997) notes, Merton's typology is better construed as an idealization, or a "legitimating ideology" to which scientists might find recourse in the midst of controversy, as well as a prescription for how scientists "should behave ideally" (p. 57).

Harry Collins and Sal Restivo (1983) have similarly challenged Merton's idealized characterization of science, arguing that conflict in science is not predicated on any static, overarching normative scheme. Dispute, they argue, is premised on the organizational *variability* of the scientific community. Indeed, they propose that both the "greatest scandals" of science's history and the most intensive periods of intellectual creativity were precipitated by major transformations in the organizational conditions of science itself. As they note:

> There is no enduring set of norms that guides the behavior of scientists. What does endure is the activity of scientists (and related types of intellectuals) pursuing rewards such as wealth, fame, and the power to control the flow of ideas and to impose their own ideas on others. The organization of the scientific community determines the nature of the reward system. Under some conditions, ideas are considered most useful when held as secret resources; they can then be the basis for prestigious cults or used as weapons in competitive situations. (Collins & Restivo, 1983, p. 200)

While conflict is not, as Merton argued, rooted in an overarching regulatory structure, some scholars have argued that conflict nonetheless inheres in the social relations of science and constitutes a productive element in science's epistemic practices. T.S. Kuhn's *The Structure of Scientific Revolutions*, perhaps one of the most influential contributions to the post-positivist shift in science studies, argues that conflict is exemplified by a transition from one governing paradigm to a new scientific world view,

and a "displacement of the conceptual network through which scientists view the world" (Kuhn, 1962, p.101). Kuhn employs the metaphors of "crisis" and "revolution" to indicate not only the shift to a new epistemic paradigm, but the tensions provoked by the incommensurability of one emerging paradigm with its antecedent. Such tensions are endemic to normal science and contribute fundamentally to its dynamics. As Kuhn writes:

> ...scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of nature to which that paradigm itself had previously led the way. In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution. (Kuhn, 1962, p. 91)

More recently, R. Allen Harris (1990) has argued that conflict in science, and in particular the "deeply rhetorical" character of disputation, is the fundamental means by which scientists "forge" their truths (p.5). As he notes, dissent is "immensely productive. Science is a knowledge-generating activity, and knowledge is too precious a commodity to lay fallow for long. Rhetoric-reasoned exchange, as well as bickering, back-stabbing, barking disputation- is the life's blood of science" (p. 29). Dispute in science is not characterized simply by rational critique but by its rhetorical tendencies. As Harris notes:

> Rhetoric suffuses normal science so completely that it is impossible to find even the smallest corner without suasion, argumentation, topoi-without the nuclear ingredients of rhetoric with which we manage symbols, achieve consensus, and make knowledge. Normal science is unimaginable without agreement, and agreement is unimaginable without rhetoric...as in all other domains, then, rhetoric has two principal reflexes in scientific argumentation, to achieve consent, and to galvanize dissent. (Harris, 1990, p. 14-15)

In his article "Raising Questions: Philosophical Significance of Scientific Controversies", M.P. Silverman (1992) argues that conflict brings into sharp relief the fact that science, as a "creative human activity", is replete with uncertainty, passion, presupposition, ambiguity and intersections of philosophical and theoretical belief systems (p. 164). The criteria by which a conflict over competing theories is resolved has less to do, argues Silverman, with the relative strength of one theory over another. Instead, resolution is often the result of the "intrinsic beauty and simplicity of one theory" whose practical value is only later taken into consideration (p. 167). The "suggestive power of mathematical beauty," for example, accounts, in part, for the prevalence of geocentric world views in science (p. 168).

Silverman further contends that conflict in science is premised on an adherence to scientific belief systems that often exhibit the characteristics of religious faith. As he notes, despite the fact that "the road from experiment to result can often be sufficiently tortuous and entangled that no conclusion can be reached," the belief in the capacity of objective experimentation to resolve even the most anomalous phenomena is an "article of faith" to scientists (p. 176).

In *Science in Action*, Bruno Latour (1987) attends to scientific conflict in terms of how it exemplifies the contingency of scientific fact production. The "dissenter" in Latour's analysis engages in practical and rhetorical mechanisms in order to contest and dismantle a scientific claim. Specifically, the dissenter who doubts the veracity of a scientific claim enrolls an array of resources and allies in an effort to "untie what attaches the spokesmen and their claims" (p. 79). The dissenter builds counter-laboratories,

conducts counter-experiments, renders the opponent's claim subjective, translates the interests of allies, and entices the opponent's allies away. By illustration, Latour offers an account of the bitter dispute between Louis Pasteur and Felix Pouchet that erupted around the question of spontaneous generation (i.e., whether or not micro-organisms generate in germ free conditions). Arguing that controversy entails an array of human and non-human actants, Latour describes how Pasteur's micro-organisms "betray" him when Pouchet's counter-experiments shows them to behave counter to Pasteur's claims.

In their analysis of the politics underlying the controversy over the etiology of AIDs, Fujimura and Chou (1994) argue that conflict in science is fuelled not only through competing styles of scientific reasoning, but through what they call contending "styles of practice" that are deployed by disputants in laying claim to a theory. Styles of practice "stress the historically located collective efforts of scientists, technicians, administrators, institutions, and various 'publics' as they build and sustain ways of knowing" (p. 1032). From this perspective, the controversy over the etiology of AIDs was waged in terms of "laboratory" versus "epidemiological" styles of practice, and adjudicated through "different languages of verification" (p.1022).

Thomas Gieryn's (1983) concept of boundary work in science describes the strategies of demarcation deployed by scientists in the name of preserving science's epistemic authority. Boundary work refers to the "pragmatically driven" mechanisms by which scientists articulate and reinforce the distinction between science and "non-scientific intellectual activities" (p. 782). Within the cultural space of science, boundary work operates as a mechanism of sanctioning and social control, whereby science

"displays its ability to maintain monopoly over preferred norms of conduct" (Gieryn, 1999, p.16).

In the midst of dispute, dissenters are often subjected to expulsion on the basis of their non-conformity to the orthodox conventions of scientific practice. Boundary work leveled against dissenters, or "imposters" often takes the form of attacks against dissenters' intellectual credibility, their objectivity, and their commitment to the normative criteria of scientific practice. Often disputants find recourse in contests of credibility, whereby demarcations between the orthodox of "good science" versus heterodox of "bad science" are rhetorically asserted (Gieryn, 1999).

Gieryn's boundary work approach thus foregrounds how the cultural authority of science is tactically mobilized and asserted, particularly during episodes of controversy. In his historical account of the 19th century phrenology controversy in Edinburgh, for example, Gieryn demonstrates that the rejection of phrenology as a legitimate practice was predicated on divergent ways of "mapping out" the boundaries of science and defining what could legitimately reside in its cultural space. Critics and supporters of phrenology "could agree that science was a valued space, capable of surrounding claims with cultural authority, but the extent to which a contested science fit depended on reconfiguring these boundaries" (Gieryn, 1999, p. 130).

Much of the scholarship concerning conflict in science has explored the extent to which social interests play a considerable role in the dynamics and trajectory of disputes over scientific fact claims and are "reflected in the language and conceptual structure" of controversies (Mendelsohn, 1989, p. 97). Donald MacKenzie (1978) advanced the

tradition of "interest analysis" in his interpretation of the historical Karl Pearson and George Udny Yule controversy. While the Yule/Pearson dispute itself was explicitly rooted in the question of how to effectively measure statistical association, the conflict was ultimately fuelled by the cognitive and social interests of the disputants. Specifically, Pearson's work was grounded in his commitment to the ideology of eugenics and Fabian socialism. By contrast, Yule, who was indifferent, if not repelled, by the eugenics movement, was invested in preserving the interests of the declining conservative class. MacKenzie's tentative conclusions suggested, therefore, that the statistics controversy was driven more broadly by conflicting class interests (MacKenzie, 1978).

Steven Shapin (1982) similarly took up the idea of social interests in his historical account of the Edinburgh phrenology disputes that took place in the early nineteenth century. At issue in the dispute was the question of whether phrenology could claim any legitimacy as a scientific diagnostic tool. Shapin argues that the direction and shape of the phrenology conflict was grounded in the broader class struggles that were occurring in Edinburgh at the time. Gieryn (1999) has argued, however, that while social interests certainly served as one way by which the legitimacy of phrenology was sustained or rejected, the issue was largely contingent on how disputants constructed a "rhetorical map" of science within the "culturescape" of early-Victorian Edinburgh. Thus the "mapping out the boundaries for science became a prerequisite for understanding the meaning of phrenology and its pragmatic implications for one's life and interests" (p. 130).

Critiques of the interest analysis approach have centered largely on its failure to

resolve the "imputation problem," or the question of the extent to which "thought or belief can be attributed to social classes or other formations as a consequence of their particular interests" (Barnes, 1977, p. 45). Critics have charged, moreover, that interest analysts assert an untenable causal link between large-scale class interests and the small-scale motivations and practices of social actors embroiled in intellectual conflicts. Critics have been skeptical, therefore, about the plausibility of scientists being influenced by "exogenous" interests, and argue that interest analysis reduces actors to "interest dopes" (Cantor, 1975; Hess, 1997).

But the question of how intellectual disputes within science are grounded in wider sociopolitical interests has not been entirely abandoned in the controversies scholarship. Everett Mendelsohn (1987) employs a variation of the interest analysis in his revisionist account of the Pasteur-Pouchet spontaneous generation controversy. Mendelsohn suggests that while the epistemic issue at stake concerned the development of the biological sciences, the debate itself was entrenched in a struggle among the intellectual elite of French society around two competing philosophies: materialism and idealism. As such, the disputes between Pasteur and Pouchet (later revived by John Tyndall and Thomas Henry Huxley) were prefigured by "overtly philosophical, religious and political elements" (p. 113). Mendelsohn argues, therefore, that interests are not "accidental intrusions into an otherwise ordered rationality of science" (p. 121). Instead, both narrow intellectual interests and broader philosophical, metaphysical and societal interests are "natural element[s] in scientific debate" and bear significantly on the contours and closure of scientific disputes themselves (p.121).

Brian Martin's (1999) extensive empirical work on dissent in science considers how mechanisms of suppression act to safeguard the interests of those who are threatened by heretical scientists. The suppression of dissent, typically manifested by acts of censorship, blacklisting, sanctioning, dismissal, and ritual degradation, provides a "direct link between power inside and outside science and what is accepted as scientific knowledge" (par. 66). Dissidents are deemed "heretics" whose contested views are rearticulated as deviant and antithetical to the conventions of orthodox science. As he explains:

> Heresy is in effect created by the response of orthodoxy: by attacking certain views, those views become delineated as beyond the pale. Heresy and dissent can be said, then, to be socially constructed; their status as forms of ideological challenge is not inherent in knowledge claims but depends on the way they are treated by the orthodoxy. (Martin, 2004, p. 714)

The suppression of the heretical scientist serves to protect the particular interests of dominant groups who bear a stake in promoting and protecting the dissemination of knowledge. Martin shows, for example that the suppression of dissent has frequently been deployed to safeguard the economic interests of industry. Scientists who have produced studies linking pesticides with cancer, for example, have received serious sanctions imposed in the commercial interests of the petrochemical industry (Martin, 1999). Similarly, controversy over the promotion of nuclear power and the ensuing suppression of its critics reflects efforts to advance both private interests and state power. Thus as Martin explains, "several factors support the case for a linkage between a system of power and a pattern of suppression: interests in a particular stance on the issue in question; a challenge to the interests; a key role for dissident experts in supporting the

challenge; and direct or indirect use of power to attack some of the dissidents" (Martin, 1999, par 67).

Recent scholarship has examined how expression of dissent in science - the resistant actions that contravene the vested interests of a dominant group- constitutes a singular form of concerted intellectual activism. Delborne's (2008) study of the controversy over the presence of transgenic DNA in Mexican maize elaborates on the concept of "dissident science" as a significant mode of political resistance on the part of dissenting scientists.[9] Dissident science operates in deliberate resistance to the conventions of scientific dispute and communication, or what is generally referred to as "agonistic " engagement. Agonism ascribes to the normative routines of epistemic dispute in science, where opponents "adhere to the rules of the game" by engaging in rationalist debate over methods and interpretation (p. 515). Agonistic engagement, while often deeply adversarial, purports to be free of political or ideological interests, and reproduces the "assumed boundaries between politics and science" (p.514). Conversely, scientists engaged in dissident action do so with explicit political motives, and thus combine "intellectual struggle with social action" (p. 515). In particular, dissident science exposes the predilections of orthodox science, disrupts the imperatives of scientific freedom and autonomy and lays bare the political motivations of the fictive boundaries asserted by mainstream science.

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[9] The controversy concerned two scientists working in the field of agricultural biotechnology who discovered that transgenic DNA had 'introgressed' into landraces of Mexican maize. The findings "call[ed] into question institutional and technical capacities to control the movement of GM crops" and challenged the dominant scientific ideas about the capacity of crop genes to spread into other geographic areas (Delborne, 2008, p. 517).

Although Ullica Segetrale's (2000) interrogation of the sociobiology debate does not reflect an interest-analysis, the main argument emerging from her interpretation speaks to how the controversy was compelled by disputants' politicized ambition to advance their ideas of what constitutes "good science" (Segstrale, 2000). Specifically, Segestrale argues that proponents of the sociobiology position (particularly that promoted by E.O. Wilson) engaged in "politics by scientific means," or the promotion of a conservative political agenda disguised as sound science. Conversely, opponents who wished to promote anti-adaptationist arguments engaged in "science by political means", a strategy that entailed calling attention to the moral and political implications of sociobiology in order to advance an original scientific position. While the sociobiology debates were explicitly concerned with the legitimacy of sociobiology, the controversy was implicitly compelled by the competing political agendas of disputants.

Thus far this overview has engaged the question of how epistemic conflict among scientists has been problematized in the science studies scholarship. Insofar as the Turin shroud controversy embodies both internal contests of evidence among scientists, as well as large scale disputes involving multiple stakeholders, it is useful to delineate the existing interpretations concerning the impetus and conditions of dissent among scientific practitioners. As discussed, the "fierce dialectics" that characterize the relations of science are largely compelled by sociopolitical interests and differential power relations, and are enacted through various modes of dispute (e.g., boundary work, conflicting styles of reasoning, contests of credibility, and suppression of dissent). As this case study will show, the internal conflicts among shroud scientists embody variations of these tactics of

dispute. But I suggest that theories of scientific conflict do not fully account for why scientists might engage in protracted epistemic battle over an object of non-scientific consequence, one whose resolution offers negligible gains. In the context of the shroud controversy, the conflicts among scientists are not rooted solely in instrumental aims; they are complicated by deeper commitments to the artifact itself, and to scientists' sense of the inherent value of the struggles over its interpretation.

One theoretical approach to conflict as a sociological concern that has found scarce expression in controversy studies is that of Georg Simmel (1955), whose contemplations of the productive capacity of social conflict and its sociological value are worth considering in relation to both internal scientific disputes, and large scale controversies. In *Conflict & The Web of Group-Affiliations*, Simmel argues that conflict in social life, or the "convergent and divergent currents" intrinsic to social units are intersected with, and fundamentally productive to, the flow and movement of the life process (p. 15). Conflict is fundamentally a "form of sociation" and is, as such, sociologically positive (p. 13). As he writes:

> If every interaction among men is a sociation, conflict – after all one of the most vivid interactions, which, furthermore, cannot possibly be carried on by one individual alone – must certainly be considered as sociation. And in fact, dissociating factors – hate, envy, need, desire – are the causes of conflict. It breaks out because of them. Conflict is thus designed to resolve divergent dualisms; it is a way of achieving some kind of unity, even if it be through the annihilation of one of the conflicting parties. (Simmel, 1955, p. 13)

Indeed, absolute harmony and unification in social life is not only empirically implausible, argues Simmel, but constitutes a stagnant form of social relations, lacking

any change or movement.[10] In keeping with the dialectical mode of analysis that underlies much of his work, Simmel argues for the value of ambivalences in social relations, such that both antagonism and harmony hold together and ultimately constitute social relationships. The sociological significance of conflict rests in these unique tensions - conflict comprises "mixtures of antithesis and synthesis, superordination of one over the other, mutual restrictions as well as intensifications" (p. 87).

Simmel argues for example, that in the context of scientific disputes over the establishment of a scientific truth, "the *contrast* between unity and antagonism is perhaps most visible", primarily because the conflict is waged in terms of "relentless objectivity" (p. 40). In other words, the terms of scientific dispute are based only on the objective facts of the dispute - subjective factors do not enter into the debate. As such, the dispute is often extremely hostile. As he notes, "...any yielding, and polite renunciation of the merciless exposure of the adversary, any peace prior to the wholly decisive victory would *be treason against that objectivity* for the sake of which the personal character has been eliminated from the fight" (p. 40, emphasis mine).

Simmel points also to legal conflict, where disputing parties, even if engaging in vitriolic contests, are unified by a higher order (i.e., the law). Enmity is bound by the normative structure of legal disputation, and the terms of disputes are absolute, objective and often merciless. But there is always an element of unity in these conflicts, even if it is

[10] As Simmel notes, "the society of saints which Dante sees in the Rose of Paradise may be like such a group, but it is without any change and development; whereas the holy assembly of Church Fathers in Raphael's *Disputa* shows if not actual conflict, at least a considerable differentiation of moods and directions of thought, whence flow all the vitality and the really organic structure of the group" (Simmel, 1955, p. 15).

of a narrow kind. In legal disputes, the element of narrow unity is signified by the fact that parties are equally subordinated to the law which acts as a "higher social order" that "binds the norms and procedures" of the dispute (p. 36). As such, disputants in legal conflict "mutually recognize that the decision is to be made only according to the objective weight of their claims; they observe the forms which are unbreakable valued for both; they are conscious that they are surrounded in their whole enterprise by a social power which alone gives meaning and certainty to their undertaking" (p. 37). Scientific conflict rests similarly on a "broad basis of unities and agreements between enemies" (p. 37).

I propose that Simmel's intuitions offer an enriched understanding of the struggles that inhere in scientific communities and beyond them. At the outset, Simmel invites us to look past the manifest polemics, rhetorical wrangling and political convolutions of conflict to consider what rests beneath these antipathies. Importantly, he foregrounds an inexorable tension between the subjective elements of dispute among social actors (e.g., the vested interests of disputants), and the supra-individual aims of contenders. Moreover, Simmel makes visible an essential tension in dispute that gives an enhanced meaning to its endurance, impetus, vitriol, and its resolution.

Consider, for example, Simmel's position concerning the utility of conflict itself in social relations. As noted, Simmel argues that conflict does not impede the flow of social relations; dispute cannot be construed as a dysfunctional element in social interaction. Instead, it is the means by which social relations remain dynamic. As he suggests, conflicts are "by no means mere sociological liabilities or negative instances.

Definite, actual society does not result only from other social forces which are positive, and only to the extent that negative factors do not hinder them" (p.15).

Mikael Hård (1993) expresses a somewhat similar idea in his critique of the social constructivist analysis of harmony and consensus in socio-technical systems. Hård argues that conflict among social actors is frequently misunderstood in controversy studies as a "dysfunctional" phenomenon that impedes consensus and stability of sociotechnical systems. He suggests that the utility of conflict among social actors should be construed as an essential "causal factor in innovation, diffusion, transfer and application" (p. 409). He employs a conflict perspective to challenge the consensus orientation of constructivism, and contends that socio-technical systems are ultimately borne of social groups in conflict. Hård's position derives from a Weberian framework that suggests (much like Simmel) that conflict between groups is "a pre-requisite for society to be dynamic" and a means by which to preclude its stagnation (p. 410). Hård argues that:

> It may be the case that the ability of a society to favor technical change is a result of conflicts taking place in a large number of arenas and concerning not only monetary gains but also property rights and the control of production processes, recognition, and legitimacy, as well as political power and authority. The rate of technological change would be minimal in a society in which harmony and consensus prevail. (Hård, 1993, p. 418)

It is perhaps not that Simmel's conceptual work *unsettles* existing ideas about the meaning and implications of scientific dispute. Indeed, Simmel's ideas and abstractions about the nature of conflict are often *in dialogue* with some of the arguments raised by analysts concerned with the motivations of scientific disputes. Competition in science is, as noted in several studies explored earlier in this chapter, often carried out (implicitly,

explicitly or rhetorically) in the name of a higher purpose (namely certainty, truth, or consensus). As well, conflict in science is often waged with a unifying thread underlying the dispute. Segestrale's account of the socio-biology debates suggest as much when she draws upon a "symbiosis" inherent in the controversy itself. She notes that the sociobiology controversy was, "a situation where scientists on both sides in a controversy are doing their best to create *political* scandal in order to promote their *scientific* agendas. We would here have an extraordinary situation of *synergy* between the critics of sociobiology and their target – all while the world thought they were at each others' throats!" (Segestrale, 2000, p. 12).

Although my analysis of the shroud controversy is not fully situated in Simmel's theoretical scheme, I do take guidance from Simmel's insights, particularly in terms of how elements of the disputes among shroud scientists are simultaneously divisive and marked by converging interests. My purpose in drawing upon Simmel's interpretation of conflict is to move the analysis beyond a narrow focus on the antagonisms of the shroud disputes, and to consider the deeper social processes that account for the vitriol, endurance and underlying motivations of the conflicts.

Controversy, the Public and Social Values

Although the literature explored in the previous section focused principally on the underlying conditions and character of conflict as it is waged within scientific communities, there is neither a fixed demarcation between internal and external scientific disputes, nor any rigid division between the epistemic and non-epistemic factors that

ignite and sustain scientific controversies. Even controversies that appear largely confined to the esoteric concerns of science are in many cases bound up in broader social interests. As Brante and Elzinga (1990) note, scientific controversies "may be generated to a greater or lesser extent by internal or external factors. The dividing line between internal and external is [in] flux, conditioned by yet other criteria" (p. 39).

Crick and Gabriel (2010) employ a Habermasian framework of interpretation that sees scientific controversies not simply as "epistemological affairs concerned with the truth-status of knowledge claims made by scientists", but as pragmatic affairs that "deal with the practical relationship between means and ends" (p. 209). From this perspective, controversy arises from, and is *constituted by*, the ethical or political tensions in the public sphere. As they explain:

> ...on closer examination, what becomes clear is that public scientific controversies are not simply public reactions to internal debates conducted by scientists. Absent the droughts of the 1970s and the public anxiety over environmental sustainability, for instance, there would never have been a controversy over global warming in which millions of dollars were invested to either prove or disprove its existence. The point is that in public scientific controversies, ethical or political aspects do not simply lie ''on top'' of a scientific dispute. Quite the opposite. The ethical and political exigency of the moment is precisely the reason for publicizing—and perhaps even creating—the scientific dispute in the first place. Although the initial diagnoses of a problem might arise in the laboratory, public scientific controversies arise, as Habermas puts it, in the ''life-historical background of violated interests and threatened identities that the effects of deficient system integration are first experienced as pressing problems''...in short, a public scientific controversy is an ethical or political conflict which helps call into existence a scientific dispute that potentially has a direct bearing on its resolution... (Crick & Gabriel, 2010, p. 207, 217)

The field of controversy studies has foregrounded the ways in which social, ideological, ethical, moral, pragmatic and political concerns underlie the character and

resolution of scientific controversies. Indeed, science is the arena in which the "deeply contested values" of the wider social milieu come into conflict and negotiation (Nelkin, 1995, p. 445). Large scale techno-scientific controversies are "polycentered" affairs involving heterogeneous forums of participants who "converge and diverge" around manifold issues, and do so amidst significant uncertainties (Limoges, 1993, p.420). As such, much of the controversies scholarship has placed an interpretive lens on the intersection between science and the social problems in which citizens have a stake.

Scholars suggest that controversies involving science and technology have been compelled by a diminishing public trust in the authority of science and other social institutions, as well as a mounting concern that the imperatives of science and technology are often in conflict with the social, ideological and political values of citizens (Giere, 1987; Nelkin, 1987). Dorothy Nelkin (1987) has argued that controversies reflect the struggles to sustain "values, norms and political boundaries" in the midst of rapid scientific and technological change (p.284). As such, controversies embody ideological tensions in the broader social milieu around questions of risk, freedom of choice, equity, social justice, traditional values, and the social implications of techno-scientific knowledge.

While it goes beyond the scope of this chapter to carry out a comprehensive account of the broad range of scientific controversies that have been scrutinized in the science studies scholarship, I consider a few exemplary case studies that have elaborated on the implications of socio-scientific disputes waged among multiple stakeholders with diverse social, political and moral interests. Inasmuch as the shroud controversy has been

waged as a large-scale dispute involving diverse stakeholders, it is useful to take brief account of the principle insights and common themes that have emerged from the empirical explorations of the determinants, dynamics and outcomes of public controversies.

Peterson's and Markle's (1979) study of the Laetrile controversy foregrounded the political and ideological tensions between orthodox Western medicine and advocates of alternative cancer therapies.[11] While the Laetrile controversy initially began as a specialized knowledge dispute, it shifted into the sphere of legal and ethical debate concerning citizens' freedom of choice in seeking alternative treatments. Accordingly, the controversy signified a struggle between the "populist ideology" of Laetrile proponents vs. the "ideology of expertise" invoked by detractors (p.154).

A similar confrontation between alternative cancer treatments and orthodox medicine was represented in the Vitamin C controversy, which involved a dispute over claims of a correlation between Vitamin C deficiency and cancer. Evellen Richards (1988) has shown that the controversy entailed a deeply politicized struggle between proponents of vitamin C and mainstream cancer researchers concerning "control over the determination and evaluation of cancer treatments" (p. 685). Thus the controversy itself disclosed the complex social and political contingencies inherent in therapeutic evaluation of effective medical therapies, to the extent that the very notion of "efficacy" was "politically defined and defended" (p. 685).

[11] The controversy was centered around the claim that Laetrile, a substance found in the seeds of several fruits, can cure or control cancer (Richards, 1988).

Brian Martin's (1991) extensive study of the protracted and thus far unresolved controversy over the benefits of fluoridation in water represents a political, moral, ethical and value-based confrontation between scientists and several interest groups bearing different stakes in the dispute.[12] Martin argues that the controversy reveals the extent to which knowledge is inextricably "entwined with power struggles" and the political and economic interests of dominant stakeholders (p. 137). In this context, industrial corporations whose interests favored pro-fluoridation science influenced the debates by supporting "partisan activity" and providing the "political environment" that favored the pro-fluoridation position. As Martin argues:

> The problem is that disputes over scientific knowledge are intertwined with disputes over values. They also become involved with wider power struggles, and traditional political philosophy assumes that claims of scientific knowledge can be unambiguously adjudicated in a realm of science separate from ethics and politics. Arguably, science is always inherently bound up with systems of power. But, even short of this claim, it is certainly the case that science related to fluoridation is carried out in a situation where knowledge is entwined with power struggles. The traditional assumption that adjudication of claims based on scientific knowledge is separate from social decision making does not hold true. (Martin, 1991, p. 137)

It is also argued that there are key moral imperative underlying the fluoridation disputes. Edward Groth (1991) suggests that fluoridation is only nominally a scientific issue. Fundamentally the dispute centers on the moral implications of advancing social justice by providing mass treatment of the public fluoridated water, versus the implications of encroaching on individual rights to choose such interventions on the body

[12] The controversy involved two conflicting paradigms among experts in dispute over two issues: the relative benefits of fluoridation to prevent tooth decay, and the claim that fluoridation carries profound health risks, owing largely to the toxicity of fluoride (Martin, 1991).

(Groth, in Martin, 1991). The issue of fluoridation is, therefore, one of risks and benefits to public health, and is rooted in a clash of "value judgments" among citizens and experts.

Science has come into direct confrontation with non-scientific actors around the politics of human illness and therapeutic intervention. Steven Epstein (1996) has documented the intricate politics underlying controversies that have erupted over the treatment of AIDs and social actors' resistance movements against prevailing biomedical models and AIDs causation theories. Epstein's analysis informs our understanding of the extent to which various controversies around AIDs constitute struggles of credibility between scientists and lay persons, challenges to the orthodoxy of science, and a "struggle for "ownership of" and "democracy within" science (p. 29).

Controversies are also ignited by conflicting world views concerning fundamental questions of the origins of life. This is well represented by the persistent battles between science and various incarnations of scientific creationism. The controversy between science and creationism writ large can be generally described as a clash between the naturalistic theories of evolutionary science and creationism's religiously motivated contention that natural phenomena can be logically attributed to a supernatural causal force (Cracraft, 1982). Although creation science purports to be legitimately scientific, opponents argue that the creation science model is little more than a reformulation of natural theology which explains away objective evidence by recourse to "the agency of a divine being" (Gilkey, 1982, p. 68). More recently, the tenets of creationism have been

re-framed within the Intelligent Design movement, which takes inspiration from Biblical narratives but "conducts its business in the currency of science" (Fuller, 2007, p. 2).

The focus of analytical concern for science studies is not simply the epistemic legitimacy of scientific creationist claims themselves, but the political implications of the creationist movement as it competes for equal epistemic authority in society's institutions, and particularly within science education. Several high profile controversies have erupted in the United States, for example, over the question of whether creationist science is legally entitled to be taught alongside evolutionary biology (Fuller, 2007; Lambert 2006; Allgaier, 2012; Clark et al., 2007; Sturm, 1982; Shermer, 1991). As Shortell (2011) notes:

> As biologists and creationists debate about science education, the nature of human origins is the subject matter of the controversy, but functionally both sides are fighting to have public discourse reflect their conceptualizations, to define the context of this cultural conflict in a way that reflects their viewpoint. For creationists, public affirmation of the creation story of the Bible is a means of aggrandizing a theologically conservative Christian identity. For life scientists, recognition of the evolution account underscores the importance of scientific practice with the biologist as objective truth-seeker. The "culture war" is, in part, a conflict about status in the public sphere. (Shortell, 2011, p. 435)

Science studies scholars have also taken into account the implications of creationist science for the pubic understanding of science. Dorothy Nelkin (1982) conducted one of the earliest sociological inquiries into the continuous tensions between science and creationism, calling the conflicts a "dramatic expression of the renewed concern with moral and religious values...and the related ambivalence towards science" (p. 18). She argues that the influence of creationism in the broader society speaks, in part, to shifts in public trust in the "authority of scientific judgment" (p. 187). More recently

studies have explored how intelligent design is increasingly gaining endorsement by members of the public who find its tenets as plausible as naturalist explanations of the origins of life (Shortell, 2011; Park, 2001)

Gieryn et al., (1985) have interpreted the creationism disputes in terms of how the ideology of science seeks a professional monopoly over the "market" for scientific knowledge. Drawing this idea through the 1981 McLean creation science trial, the authors suggest that the conflict provided an opportunity for scientists to assert their exclusive cognitive and cultural authority to "decide the validity of knowledge about nature" (p. 401). As they note:

> Only credentialed professional scientists have the expertise and authority to define science, to evaluate the validity of a scientific theory and to prescribe curricula for public school science classes. Scientists at the trial erected a boundary between their science and the pseudoscientific religion of creationism. This boundary-work served the narrow, legal goal of removing creation science from biology classrooms in Arkansas public schools. It also served the broader, professional goal of excluding competitive but illegitimate providers of "scientific" knowledge whose interests and objectives are not consistent with those of the scientific community. (Gieryn et al., 1985, p. 405)

Although the scholarship explored thus far has taken up the problem of scientific controversies from various interpretive positions, the studies are unified by the idea that controversy, as a social phenomenon, exemplifies the complicated and deeply charged relationship between science and the social world. Indeed, Pinch and Collins (1993) argue that controversies are the ideal places to view "science as usual", inasmuch as the antipathies inherent in science become most visible.

Common to these accounts is that controversies are underlined and driven by moral, ideological, political, epistemic or economic concerns. I suggest, however, that by

taking into account the controversies that erupt over issues of seemingly marginal pragmatic, political, social or scientific consequence, we are afforded an opportunity to comprehend why such disputes emerge, proliferate and endure, and what sorts of values are being advanced or safeguarded in these disputes. Certainly, as I will demonstrate in the following chapters, the conflicts among scientific and non-scientific actors around the provenance of the Turin shroud are congruent with the ways in which controversy has been characterized in the literature. But there are elements of controversy that manifest differently in the context of the shroud disputes. To clarify this point, I'll briefly consider the core analytical issues raised by controversy analysts.

Interests: As this overview demonstrates, controversy is often driven and circumscribed by the vested interests of contenders. But a controversy waged over a religious artifact, which seemingly offers no practical gains, demands a deeper interrogation of whose ambitions and interests are being served. I will show that the vested interests of shroud scientists are decidedly complex. They include the obligation to impart a truthful scientific narrative of this artifact, a politicized interest in governing this knowledge, and an instrumental interest in demonstrating the role and capacity of science to interpret the enigmas of the artifact.

Emergent stakes: Scientific controversies are clearly waged because something of value is at stake. Thus the intensity, duration, and complexity of controversies are rooted in disputants' struggle for primacy over the issue or object of contention. The challenge of understanding the character of the shroud controversy is that the stakes of the dispute are

not readily apparent. Concurrently, the value of examining a controversy of this type is that we gain insight into how stakes *emerge* as the conflict proliferates and as more actors become entangled in the politics of the disputes.

Objects of dispute: Scientific controversies are ostensibly waged around a material object (e.g., a technology) a form of knowledge (e.g., the classification of a particular disease) or a practice (e.g., medical interventions on the body). To what extent, however, do the objects of dispute *shape* and *circumscribe* the controversy itself? The distinction of the controversy explored in this case study is that the devotional artifact is quite central to how the disputes among scientific and non-scientific actors unfold and why they are waged with such intensity. More importantly, I will show that a devotional object compels in scientific actors a deeper commitment to the purpose and value of the dispute than we might anticipate in conventional scientific conflicts.

Boundaries: By its very definition, controversies embody lines of demarcation. Within conventional public controversies, the fault lines are often drawn between science and a concerned public, experts and non-experts, between competing institutions, or between conflicting ideologies and values. Within internal scientific disputes, demarcations are cast between experts around contesting epistemic claims. Boundaries are also imposed by scientists against the perceived threats of dissenters. Thus when the analyst confronts a scientific controversy, it is necessary to identify where the boundaries are drawn. In other words, understanding the impetus, dynamics, complexities, idiosyncrasies, shifts and resolution of a controversy entails "mapping" the dispute, and identifying where the

boundaries are being cast among contenders and why. The shroud conflicts reveals ostensible lines of demarcation among contenders, but this controversy simultaneously defies a consistent "mapping' of fixed boundary lines, and is not readily construed in terms of any rigid polarities.

Controversy as Social Drama

Recently, controversy analysts have considered how the sequence, form and dynamics of public scientific controversies lend themselves well to a dramaturgical interpretation. Crick and Gabriel (2010) note that:

> ...an understanding of the arc of public scientific controversies requires a broadly dramatic rather than a narrowly epistemological framework of analysis... public scientific controversy is not an affair of fractured consensus about an abstract intellectual matter that is publicized over the mass media; it is a temporal legal drama in which citizens and publics dramatize common problems in order to redress them in parliamentary bodies where collective judgments must be made on the basis of common understanding. (Crick & Gabriel, 2010, p. 212, 217)

The actions and interactions of social actors entangled in conflict embody performative elements that can be understood as dramatic expressions. Delbourne (2010) argues, for example, that "scientists, their allies, and opponents engage in struggles not just over what is true, but who may validate, access, and engage contentious knowledge. Viewed through the metaphor of theater, *science is always performed* for an audience, and that audience is constructed strategically and with consequence" (p. 67).

As noted earlier, Victor Turner's (1974) social drama framework provides a novel and instructive means of construing public scientific controversies in terms of their dramatic phases and structures. Turner's analytical starting point is that conflict "seems to

bring fundamental aspects of society, normally overlaid by the customs of habits and of daily intercourse, into frightening prominence. People have to take sides in terms of deeply entrenched moral imperatives and constraints, and often against their own personal preferences" (p. 35). Large-scale conflict among social actors, Turner argues, embodies a processual structure. Where controversy studies has delineated the shape of a controversy in terms of its initial eruption, the entanglement of various contenders, and its closure, Turner interprets these phases of conflict in terms of "aharmonic" and "disharmonic" processes that unfold in four dramatic stages. The act that signals the eruption of otherwise stable social relations is referred to as the "breach" that disrupts the "regular, norm-governed social relations that occur between persons or groups within the same system of social relations"(p. 38). The breach constitutes the "symbolic trigger" that precipitates a mounting public confrontation. In Alan Gross's (1984) account of the recombinant DNA controversy, for example, the breach was signified by a letter printed in *Science* and *Nature,* wherein ten scientists advised the scientific community of the risks and hazards of recombinant DNA experiments, and emphasized the ethical imperative of imposing a moratorium on any further experiments. Such an act signified a "breach" inasmuch as it ignited a conflict among scientists who believed the moratorium recommendation was grounded in a moralizing agenda.

The breach is followed by a "crisis" phase, which can become "co-extensive with some dominant cleavage in the widest set of relevant social relations to which the conflicting or antagonistic parties belong" (Turner, 1974, p. 38). Indeed the crisis phase, as Turner defines it, describes the fundamental character of most public scientific

controversies. The enduring public battle concerning the legitimate place of creation science in public science education, for example, certainly speaks to a "dominant cleavage" in terms of conflicting world views concerning how we are to understand the origins of life. So too does the controversy over the etiology of AIDs highlight the cleavage between scientific authority and the politics of resistance among non-experts who bear a stake in the definition and treatment of disease.

Turner suggests that the crisis in a social drama is followed by a redressive phase, whereby society "adjudicates rival claims by means ranging from informal mediation to formal justice..." (Gross, 1984, p. 398). The function of redressive action is to restore social order and harmony, and ultimately bring about a "reintegration of the disturbed social group" (Turner, 1974, p. 41).

Beyond providing a conceptual map for tracing the progression and termination of public controversies, Turner's framework delineates the analytical significance of social actors' actions within the phases of a social drama. Specifically, each phase contains forms of rhetorical expression and symbolic action. The "breach" for example, is often expressed in altruistic terms, where the actor who instigates the conflict believes the act is carried out for the benefit of other parties. Turner's attention to the symbolic actions and rhetoric of disputants lends valuable insight into how the concerns and positions of disputants in a controversy are articulated, and how these rhetorical expressions function to move a controversy through its phases.

Turner offers a deeper comprehension of how dispute is underlined by a dramatic energy and form, and how, in fact, the aim of conflict is to restore social order. The social

drama metaphor provides interpretive value to studies of controversies insofar as the phases of conflicts can be understood as containing a functional value and an analytical significance. Breach, crisis, redressive action, and reintegration are more than simply categories – they express movement and process. Moreover, Turner asks us to attend closely to the language and actions of disputants in order to understand how rhetoric and symbolic action bear on the movement and resolution of disputes.

As noted, I employ the social drama framework to impart interpretive clarity to one phase of the Turin shroud controversy that I explore in chapter four. This particular episode is well informed by controversy studies in terms of how political tensions and differential relations of power underlie the dispute. However, I suggest that the social drama framework allows for a deeper comprehension of the role and function of the symbolic language and actions of contenders, and how they are critical to the ways in which the controversy ignites, develops and finds resolution.

Closure and Post-closure

What are the terms by which a controversy is settled? Under what conditions are conflicts re-ignited, and how are they re-configured? The traditional positivist standpoint argues that closure, at least within specialized scientific disagreements, proceeds by appeal to evidence and rational deliberation. This is otherwise referred to as the "sound argument closure," which presupposes that controversies in scientific communities remain relatively unencumbered by extra-epistemic factors (Engelhardt & Caplan, 1989; McMullin, 1989). The positivist framework is limited, however, by the idealized assumption that both conflict and resolution in science proceed unaffected by social or

political factors, and undistorted by the prejudices of disputants (Engelhardt & Caplan,1989). Indeed, closure often relies less on epistemological certainty than on "which side enjoys the benefit of the doubt in the face of scientific ambiguity" (Freudenburg et al., 2008, p.2).

The symmetry principle proposed by the Strong Program suggests that closure is not achieved by the primacy of sound argument and the appeal to facts, but by a complex array of social influences that favor a particular side (Barnes, et.al., 1996; Bloor, 1997; Martin et al., 1995).[13] As Engelhardt and Caplan (1989) note:

> Scientists do not engage in science as disembodied knowers...differences in views of proper scientific methods are often conjoined with membership in conflicting interest groups. The interest groups may be political, economic, or structured around special concerns of prestige and special personal investment...a concrete scientific controversy usually cannot be reduced simply to a controversy regarding facts and the proper warrants for drawing conclusions from evidence. (Engelhardt & Caplan, 1989, p.11)

Thus closure often relies on the processes of "successful persuasion at the level of scientists," or more broadly, the success of one group "in the political marketplace of contending interest groups" (Martin et al., 1995, p. 519).

The resolution of controversies, however, is often provisional, circumstantial and "imperfect" (Halfon, 2008). Specifically, closure in controversy does not inevitably proceed from consensus or the establishment of certainty. Indeed, in the case of public controversies involving issues of political consequence and public policy, political

[13] The Strong Program emerged in the mid 1970s from a group of researchers associated with the Edinburgh School. Its primary founders, David Bloor and Barry Barnes, proposed four principles to guide the empirical pursuit of scientific knowledge making: c*ausality* (studies of science should explain the causes of beliefs), *impartiality* (SSK practitioners should be impartial to the truth or falsity of knowledge claims), *symmetry* (the same causal factors explain both true and false beliefs), and *reflexivity* (Barnes, et.al., 1996; Bloor, 1997).

closure itself does not inevitably follow from scientific closure. Rather, political closure relies upon the ways in which "scientific certainty" is employed and articulated in subsequent debates in the public sphere. As Saul Halfon (2008) argues, "profound and complete closure from one perspective gives way to conflict and great uncertainty from another" (p. 297).

In Halfon's exploration of the Depleted Uranium controversy, for example, scientific closure around the question of the relative dangers of depleted uranium (i.e., the claims that depleted uranium does not emit harmful radiation) was achieved within public science institutions whose legitimacy was bound up in state support. The controversy did not, however, reach any resolution in the public sphere. That is, the issue of the relative safety of depleted uranium became a source of enduring contention among anti-nuclear activists who contested the claims of both government and scientific institutions. The resolution of controversy often relies, therefore, upon the nature of the issue at stake and the social groups entrenched in the dispute. As Nelkin (1995) suggests, where conflicts are waged over matters of moral consequence (e.g., disputes over animal rights or fetal research), closure is typically precluded by "conflicting visions" of disputants (p. 455).

But if a controversy resurfaces, what is its character? Bart Simon (1999), who chronicled the eruption and resolution of the cold fusion controversy, demonstrates that heterogeneous research movements often re-emerge and sustain themselves even after a controversy is declared unequivocally resolved or "dead" by the scientific community. The twelve month controversy around cold fusion ostensibly "died" when a majority of scientists agreed that the claims of nuclear fusion were "spurious and unfounded" (p. 62).

What followed was a recasting of cold fusion into a "pathological" science where scientists rendered the claims of cold fusion proponents into "fictions" or "artifacts" (p. 79). Consequently, scientists who engaged in cold fusion research were inevitably branded as pseudo-scientists.

Yet despite the scientific community's consensual view that cold fusion was untenable, several scientists have continued to engage in experimental work around new lines of inquiry, forming new research collectives and a "fledging" scientific field (though not one that has acquired any legitimacy in the mainstream scientific community). What follows closure, then, is not the inevitable demise of the losing side, but its reconfiguration into social groups whose epistemic legitimacy is established both within and outside of the "boundaries" of mainstream science. The controversy might be closed to some, but remains active to others.

By extension, Simon suggests that the original object of contention – in this case cold fusion - is neither "dead" nor "alive" but "undead." In other words, the "fictional artifacts" of the losing side do not cease to exist but linger as "technoscientific ghosts." Understood in this way:

> [Cold fusion] is neither dead nor alive, it is undead – like a ghost, a phantom or a spectre...the living are the real things and legitimate people of our social worlds, while the dead are the failed remnants (people and things) of old controversies which no longer exist and cannot act. The undead, however, do exist and can act, but their actions are mediated and constrained by their relations with the living...as a consequence, being undead should not entail being forgotten, being dismissed, or being unimportant, even in an explanation of the production of *scientific* knowledge. (Simon, 1999, p. 64-65)

The Turin shroud controversy has likewise resisted closure, notwithstanding the fact that the radiocarbon dating evidence of the shroud's provenance seemed to preclude any further debate. Within the post-closure phase of this controversy, it is the legitimacy of the radiocarbon dating evidence that comes under intense critique, such that two competing scientific discourses render the question of the artifact's historical status simultaneously resolved and undetermined.

Conclusion

My purpose in this overview has been to delineate the prevailing analytical and empirical concerns of controversy studies. While many of the conceptual themes of the scholarship find relevance to my case study, I propose that the shroud controversy reveals singularities and paradoxes that offer a distinctive understanding of the social relations of conflict among scientific and non-scientific actors, and thus makes a unique empirical and analytical contribution to the field. In what follows, I will demonstrate how the Turin shroud, as the object of contention, shaped and circumscribed the character of the controversy, disputants' relationship to the artifact itself, the lines of demarcations among social actors and the epistemic values and commitments of contenders.

Chapter Two: Capturing Artifacts: The Science of the Turin Shroud

Introduction

How did the Turin shroud come to be both a legitimate object of scientific interest and a catalyst for the protracted epistemic and political tensions among scientists? Before interrogating the complexities of the contemporary provenance disputes themselves, I wish to establish analytical clarity with respect to this question. To this end, I devote this chapter to an account of the 1978 STURP inquiry, the outcomes of which precipitated the shroud controversy.

As noted previously, the STURP inquiry involved twenty-four American scientists from diverse fields of expertise who travelled to Turin in 1978 to examine the physics and chemistry of the Turin shroud.[14] In my account of this inquiry, I draw upon a separate but essential set of empirical questions: how does science confront and interpret the devotional artifact, and what are the effects and implications of such interpretations? By what means, and against what kinds of constraints, was the Turin shroud made legible, analyzable and *comprehensible* by scientists? How were evidence claims

[14] The STURP team comprised a total of thirty-one members, twenty-four of whom participated directly in the Turin inquiry. The remaining seven members conducted post-Turin data analysis. The team's areas of expertise included: physics, biology, the chemical sciences, optics, spectroscopy, radiography, volcanology, meteorology, astronomy, nuclear physics, molecular physics, entomology, microscopy, botany, bacteriology, mycology, physiology, pathology, endocrinology, anatomy, immunology, hematology; and the analytical, inorganic, organic, biological, physiological, pharmaceutical, geological, and textile chemistries. In 1978, STURP members were affiliated with scientific institutions, military institutes, universities, medical centers and corporations including: Lockheed Corporation, U.S. Air Force Weapons Laboratories, Los Alamos National Scientific Laboratories, Nuclear Technology Corporation, St. Agnes Medical Center, Oriel Corporation, U.S. Air Force Academy, Sandia Laboratories, Santa Barbara Research Center, IBM, Western Connecticut State University, New England Institute, and the University of Colorado.

concerning the shroud's composition and possible provenance assembled, represented and defended?

My interpretation of the STURP inquiry is situated in a conventional social constructivist framework. Within science and technology studies, the social constructivist agenda has provided analytical space in which to examine the authoritative knowledge claims of science and their pre-eminence in the social world. Its theoretical and empirical achievement rests in demonstrating the complex processes by which closure or "black boxed" knowledge is accomplished within domains of techno-science (Latour & Woolgar, 1986; Winner 1993). At the outset, constructivism rejects the notion that the imperatives of rationality and autonomous logic alone dictate the assembly and soundness of techno-scientific knowledge. Constructivism thus unpacks the content of scientific knowledge, thereby disclosing the constellation of interests, negotiations, choices, conflicts and contingencies that comprise the social logic of knowledge construction.

As an interpretive lens, social constructivism is essential to understanding how material artifacts are constituted by the symbolic, epistemic, social and political relations and practices of scientists. I am mindful, of course, that science studies practitioners have raised considerable misgivings in recent years about the tenability of social constructivism as a fruitful explanatory framework. Indeed, constructivism is often regarded as a "moribund" method of analysis (Restivo, 1995, p. 95). The term itself, it is argued, has come to lack specificity, to the extent that its deployment as an explanatory scheme offers little more than the idea that phenomena are rooted in social relations

(Hacking, 1999; Sismondo, 1993, 1996). Langdon Winner (1993) has drawn attention to the ways in which constructivist scholars have failed to attend in sufficient depth to the political and ideological implications of the things they purport to be constructed. Bruno Latour (2005) has argued that social constructivism, in its very use of "the social" as the central category of analysis, proceeds from a weak point of departure. The social is not the context, he argues, but the end product, or "effect" of heterogeneous associations among things that are not themselves social (Latour, 2005).

Despite what appears to be a post-constructivist mood in science and technology studies, I suggest that a constructivist approach provides the greatest clarity with respect to the analytical aims of this chapter in particular. Specifically, the constructivist agenda is prominent in my account of the STURP inquiry, inasmuch as I seek to understand the complex socio-material practices by which the shroud was initially translated into a scientific object, interpreted, represented and made comprehensible through the epistemic interventions of STURP scientists. Indeed, the Turin shroud, I argue, was not simply interpreted, but *captured and re-constituted* by the STURP team's material and epistemic interventions in ways that transformed, importantly, how it would come to be represented, understood, inevitably contested and continually *re-made* as the object became a site of contention among multiple social actors. I employ the term "capturing" as a way of conceptualizing the efforts of STURP scientists to secure and stabilize a particular representation of an artifact whose meaning has always been ephemeral.

My analysis of the STURP inquiry takes into consideration the communicative resources deployed by STURP in both formal publications and first-hand narratives as

key to understanding how the shroud was discursively represented in particular ways. Scientists persuade in myriad forms. Scientific discourse embodies literary techniques and other communicative strategies that "reveal" phenomena and speak authoritatively for the objects being represented (Golinsky, 2005). As Golinsky observes, "science's high epistemic profile in our culture is bound up with the notion that it produces knowledge of a higher degree of certainty than that yielded by purely human processes of persuasion" (p. 106). Shapin (1984) has similarly argued that literary technologies in scientific communication, the tactically persuasive "ways of speaking" about nature, serve to secure the convictions of audiences (p. 481).

Much of the work of the STURP inquiry involved interrogating the source of the image formation, as well as the organic components of the markings on the cloth. In doing so, STURP deployed visualization techniques that revealed latent features of the cloth. As noted previously, fundamental to the Turin shroud's singularity are the visual characteristics it embodies. The shroud's interpretation and veneration has long been rooted in what is purportedly revealed by the cloth (i.e., the image of a crucified man). The visual peculiarities of the shroud thus distinguish this object from other historical relics, and account for the enduring cultural and scientific interest in it, as well as its eminence as a "miracle cloth" in faith communities. As such, part of my analysis takes account of some of STURP scientists' visual products, though to a modest extent. I draw on the analytical insights in science studies concerning visual representational practices of science, which form part of the crafting of scientific knowledge.

My account and analysis of the STURP inquiry draws on discourses that emerged from the collective work of the STURP team, represented in two forms: a selection of formal peer-reviewed scientific articles published by members of STURP, and first hand narratives written by STURP members for public consumption. Before attending to the STURP inquiry, a brief sketch follows of the historical context from which the contemporary scientific interest in the shroud emerged.

Shroud Science: A Brief Historical Overview

The inception of "shroud science" began with a historically significant occurrence on the date of the shroud's public display in 1898. Turin was celebrating the 50th anniversary of the signing of the Statuto Albertino constitution of 1848. As part of the celebrations, the shroud was put on public display, and King Umberto I granted his approval for the shroud to be photographed for a public exhibition of sacred art (Wilcox, 2010). Secundo Pia, the amateur scientific photographer commissioned by the Shroud Commission to record the "vague and bizarre looking" stains on the cloth, took the first photographs of the shroud image (Tribbe, 2006, p.7).[15]

Using his own self-devised technique involving glass plates coated with a photosensitive emulsion, Pia discovered that his wet negatives revealed a positive image of light and shade in a clear rendering of a man's face. As such, the image on the cloth appeared to be a photographic negative. Pia further discovered that the bloodstains on the

[15] The imprint on the Shroud is faint and visible to the naked eye only from a distance of approximately six to ten feet (Tribbe, 2006). The most popular contemporary image of the Shroud is a photographic negative enhanced in sepia tone.

image appeared as positive, rather than negative images. Given that these visual peculiarities shaped the first body of questions that formed the basis of shroud science, Pia is credited with having inadvertently precipitated the era of scientific interest in the shroud, thus marking the artifact's transition from pre-scientific object of veneration to one of scientific appeal.[16]

The images produced by Pia's photographs inspired several theories of image formation in subsequent years. In 1902, two scientists of the Sorbonne, Paul Vignon and Yves Delage, conducted a series of experiments to test the longstanding human artifice theory.[17] Finding that any painted reproduction, when photographed, produced a distorted image, Vignon and Delage concluded that the image could not be an artistic creation. They proposed instead that the image was imprinted on the chemically sensitive cloth by ammonia-rich vapours emitted by urea-laden sweat (Morgan, 1983; Volckringer, 1991).[18]

The scientific community received Vignon's and Delage's findings with a mixture of interest and hostility, and a brief controversy erupted in the French Academy of Sciences.[19] The secretary of the Academy, organic chemist Marcellin Berthelot, initially

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[16] Pia's discovery tends to be framed as a moment of twofold significance, in that it marked both the inception of shroud science as a field, and a singular confrontation with the Divine. Popular narratives embellish Pia's discovery with descriptions of his astonishment in seeing the image materialize in his negatives: "as he lifted the first plate up to the dim red light for an initial visual check, he was so startled by what he saw that he almost dropped the plate" (Tribbe, 2006, p. 54). Another author writes, "what was clear, at least to Pia, was that Jesus had left not only his "photograph" on the shroud, but also a visual record of what happened to him in the bloody hours before his death" (Wilcox, 2010, p. 3).

[17] This refers to the theory that the image on the cloth was a painting.

[18] Urea is a waste product in the blood. It is excreted profusely when the body is in a state of extreme physical stress or trauma (Schwalbe & Rogers, 1982).

[19] Initial media responses were somewhat skeptical about the scientific legitimacy of Vignon's and Delage's findings, as evidenced in a 1902 report from the New York Times: "The strangest story of the week is that relating to M. Paul Vignon's amazing photographic experiments with the Holy Shroud,

refused to allow Delage to present his work to its members. Known as a vocal atheist who would later participate, in his capacity as France's Minister of Public Instruction, in secularizing the public schools in France in 1904, Berthelot finally agreed to permit Delage's presentation but refused to allow the vote of confidence to be taken by members of the Academy. Consequently, Delage's work was not included in the presentation proceedings. Berthelot's objections to Delage's work were premised on his strident conviction that scientific work containing religious references had no place in the Academy (Wilcox, 2010). Delage, insisting that he'd remained "faithful to the scientific spirit" in his work on the shroud, argued that the basis for these objections was entirely unjustified (Wilcox, 2010, p. 29). Claiming that his detractors "unfairly grafted on to this scientific question a religious issue", Delage responded to the censorship of his work in a letter to the editor of Paris's *Review Scientific*:

> In refusing to insert my note in the proceedings, it has been forgotten that this publication contains matters which are much more hypothetical – theories...corroborated by no experiment, and many others based upon arguments far more fragile than those brought forward here. But then there was no question of matters touching religion. There lies the difference...I consider Christ a historical person, and I see no reason why people should be scandalized if there exists a material trace of his existence. As to the question of [His divinity], I have said nothing, because I have nothing to say. (Wilcox, 2010, p. 29)

The Academy did finally accept Delage's experimental study for publication in the Academy's *Comptes rendus de l' Academie des Sciences* but emphatically refused to

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preserved in the cathedral at Turin, and the most astounding feature of that is *the seeming acceptance of the results as scientific by both the esteemed Lancet and the London Times...*" ("Holy Shroud Tested", 1902, p. 3, emphasis mine*)*.

publish the work if it made any reference to the religious significance of the shroud image itself or made any fact claims about provenance of the cloth (Weaver, 1980).[20]

In 1942, Jean Volkringer, a pharmacologist and professor of chemistry, published an independent study of the shroud in which he tested the vapourograph theory. Volkringer determined that the nature of the imprint was not a result of substances transferred to the cloth itself, but was instead a result of cellulose degradation. This is a chemical process whereby lactic acids from organic tissue, when transferred to paper, cause a break down of the paper's cellulose molecules, leaving a uniform, sepia colored imprint (Volkringer, 1991).[21]

Between 1969 and 1973, under the direction of Turin's Cardinal Michele Pellegrino, the Shroud was examined by a group of Italian scientists and textiles experts commissioned to conduct limited non-destructive tests of the cloth's properties and advise on methods of storage and preservation. The work of the commission included removing thread fragments and surface debris for laboratory testing. Findings from the thread samples, examined by textile expert Dr. Gilbert Raes, suggested that traces of cotton fibers on the linen thread were a variety known as *Gossypium herbaceum,* originating in the Middle East of the first century (Tribbe, 2006). The finding was rather consequential to the question of the shroud's provenance, in that it challenged the

[20] The "vapourograph" theory was later rejected when subsequent controlled experiments could not reproduce the same results. It was not, interestingly, the failures of replication that undermined Delage's and Vignon's claims. At the time, historians theorized that the shroud was one of several bogus "Passion relics". As such, the weight of the objections from historians well versed in the historical economy of spurious relics tended to undermine any scientific claims to the contrary (Drews, 1984).

[21] Although Volkringer's work failed to provoke much interest at the time, his theory of cellulose degradation would be re-visited by STURP chemists, who concluded that this was the process that produced the sepia-coloured image.

prevailing claim that the shroud was a 13th century European forgery (cotton was apparently unknown in Europe until AD 1350) (Rogers, 2008).

The nascent stages of shroud science received little to no serious attention from the broader scientific community, and with some exception, the disparate theories proposed by shroud scientists tended to fade into obscurity. Indeed, the early attempts at the scientific study of the shroud are marked by discrete and often desultory practices. Historian Robert Drews (1984) suggests that many scholars' wariness about treating the Turin shroud as a credible subject of intellectual inquiry has stemmed largely from a longstanding belief that the shroud is one of many religious hoaxes. Consequently, any serious consideration of a spurious relic would pose a professional risk to scholars in any discipline. One might speculate that the scientific community was perhaps less concerned with the spuriousness of the relic than with the overall value of shroud science relative to science's more exalted aspirations.

The Turin Shroud: from Devotional Object to Scientific Problem

The 1978 STURP inquiry sprang from the curiosity of an American physicist. In 1974, John Jackson, a member of the US Air Force Weapons Laboratory in Albuquerque, New Mexico took an interest in a photograph of the shroud and was particularly intrigued by the shadings, or "intensity points" on the image. Jackson wondered if these image intensity points on the cloth could be measured to determine a "cloth-body" distance. Such a measurement would suggest that the image was formed by the cloth being draped

over a three-dimensional form, a finding that would challenge the recurring human artifice theory.

Jackson brought a photograph of the shroud to the image analysis laboratory of Sandia National Laboratory in Albuquerque, where it was placed beneath a VP-8 Image Analyzer, a technology developed for the NASA space project for 3D topographical imaging (such as relief maps of the moon's surface). The image generated by the analyzer revealed a three-dimensional human form, signifying that the image was encoded with spatial data. Any painted, drawn or photographed image can be rendered into a three-dimensional relief, but the results are always somewhat distorted. The shroud's uniqueness rested in the fact that it was converted into a true three-dimensional relief, with no distortions. In other words, the image's "varying tones or intensity levels could thereby be seen not so much as true photographic light and shade but rather as encodings of the (still theoretical) body's relief in relation to its distance from the cloth at each related image point" (Wilson & Schwortz, 2000, p.36).

This discovery, though perhaps not one of scientific import, was sufficiently intriguing to Jackson to consider doing further research. To this end Jackson enlisted several colleagues from various scientific fields to weigh in on the image. On the strength of a growing, collective interest in the apparent anomalies produced by the VP8 images of the shroud, the STURP team, comprised of thirty-one scientists, was formed in 1977. The primary motivation for many scientists to enlist in the STURP project seemed, at least at the outset, to simply be scientific curiosity. In his narrative account of the STURP inquiry, chemist John Heller notes, "what in the world would make a theoretical

physicist...undertake an investigation of the Shroud...? The same thing that drives all good scientists – curiosity, which sparks interest" (Heller, 1983, p. 22). STURP member Ray Rogers similarly observed, "I had never heard of the relic. I had no interest in religious objects of devotion. I probably should have rejected any involvement in the Shroud of Turin project, but some curious observations appeared on the photographs of the cloth. I thought it would be a simple matter to identify paint, and I told a colleague at work, 'give me a Classical Scientific Method and 20 minutes and I'll have that thing shot full of holes'" (Rogers, 2008, p. v).

A scheduled public exhibit of the shroud in 1978 presented an opportunity for the team to examine the cloth on site in Turin. STURP members faced a heavy challenge at the outset: they would require official permission to access the shroud, and would need to justify their rationale to carry out direct experiments on the cloth. To this end, STURP members enlisted the support of the president of the Holy Shroud Guild, Father Adam J. Otterbein and its vice-president, Father Peter M. Rinaldi.[22] Following a 1977 conference in which scientists and clerics convened to deliberate the potential research implications of the VP8 photographs, STURP scientists were asked by Otterbein and Rinaldi to present the proceedings of the conference to members of Centro di Sindonologia in Turin.[23] The mandate of Centro, an offspring of a four hundred year old confraternity, is to maintain the proprietary rights to the intellectual study of the shroud. This is a somewhat symbolic custodianship. The Centro cannot officially grant or deny access to

[22] The Holy Shroud Guild is a Catholic American organization whose mandate is to promote the multidisciplinary study of the Shroud.

[23] Translated as "The Center for Shroud Studies".

the shroud. The organization functions as a mediator or "spiritual inheritor" of the shroud (Heller, 1983, p.52). Some have suggested that the implicit aim of the Centro is to censor any intellectual work that might cast doubt on the question of the shroud's authenticity (Heller, 1983).

In the name of diplomacy, and to preclude any political obstacles to the Turin inquiry, STURP members were advised to present their research proposal to the Centro and involve them in the process before seeking official permission from the archbishop-cardinal of Turin. STURP member John Jackson recalls:

> We were ill at ease, and the [Centro] seemed very suspicious and chilly towards us. But we had brought with us a bunch of the Proceedings volumes, and we passed them out. You could see that they were impressed...with the illustrations and equations and all, we made an impact. (Heller, 1983, p.52)

It is possible to interpret STURP scientists' efforts as a key moment in the shroud's conversion into a legitimate scientific problem. We are reminded here of Michel Callon's (1999) notion of *translation*, the conceptual device for understanding the strategic actions employed by scientists in the delimitation of a particular problem, and in the assembly of alliances and interests required to resolve it. Indeed, in the face of intractable custodians of a religious object and the delicate negotiations required to forge a *pathway* to the artifact, the challenges for the STURP team were multifold: problem definition, alignment of interests, achieving consensus concerning the import of scientific intervention, and persuading allies that specific actors, namely STURP scientists, were indispensible in the resolution of the problem. The STURP team could not proceed (they were, as Callon would suggest, "fettered") without the investment of two institutions (the

Holy Shroud Guild and the Centro). Neither could they proceed without "capturing" the interests of allies, and ensuring consensus that the phenomenon in question constituted a problem worthy of scientific intervention. Indeed, scientists' ability to lay authoritative claim to a particular problem is accomplished through strategies of persuasion, or "interessement," by which the explicit interests and concerns of non-scientific actors are defined, appropriated, and reformulated (Callon, 1999). This is revealed in STURP members' efforts to position themselves as the key authoritative body in the scientific interpretation of the shroud.

The meeting with the Centro proved successful. The STURP team, having secured the necessary alignments of all the key actors invested in the shroud, was granted official permission by Turin's Archbishop Anastasio Ballestrero to conduct non-destructive testing on the cloth. STURP scientists were informed that they would have five days immediately following the public exhibition to apply their experiments and data collection procedures. The research protocol demanded considerable financial resources for equipment and travel. As such, the STURP team faced the challenge of procuring the funding needed to support the thirty-one person expedition to Turin. This was no easy task. In STURP member John Heller's account, the team's endeavors were generally met by misgivings on the part of formal institutions and funding bodies. Corporations and foundations alike refused to fund a scientific inquiry that "might have religious significance in a secularist world" (Heller, 1983, p.62).

The team was denied government and university funding on the same grounds. For the sake of maintaining the objectivity of the research, the STURP team did not

accept funds from any church or religious organizations. Instead, members invested their own personal funds into the trip, and defrayed some of the costs by gathering financial support through charitable donations and honoraria given in lecture circuits. It would seem that both scientific curiosity, (which Heller suggests, "drives all good scientists"), and a measure of scientific hubris had much to do with the STURP team's willingness to invest in a venture that failed to find any formal endorsement or financial support. It is important to note as well that the STURP team's persistence was due, in part, to the fact that the opportunity to examine the shroud directly was a rare occurrence, given that shroud custodians have typically been wary of any physical handling of the cloth.

Makeshift Places of Production: Turin and the Significance of the STURP Laboratory

> What is this thing that causes so much emotional response? The easy answer is "It is a 'real thing.'" Both scientists and theologians can agree on that. We diverge when we begin to describe the "real thing"...it is not an insult to either science or religion to point out that there are fundamental differences between them. Science is deduced. Religion is revealed. They are both natural parts of our lives or mental processes. (Rogers, 2008, p. 3)

> The Shroud is not like the mythic Holy Grail...it is a real, palpable thing. Science is damn good at measuring things! That is our specialty. This cloth is made up of atoms and molecules which we can identify. Hubris or no, it is absolutely impossible that science will not come up with the answers. (Heller, 1983, p. 10)

Both Heller and Rogers express a conventional disposition in science: no matter what complexities or uncertainties an object poses for its interpretation, science can surely distinguish, draw out, draw *upon* and delineate its material properties. The shroud is, at least in Heller's and Roger's view, real, palpable, measurable and thus ontologically *available* for their objective interpretation. But what did the scientific interpretation of the

shroud entail, and what were the implications of this interpretation for its reconstruction? What did it mean to apprehend the "real object" and depict the Turin shroud differently? What contingencies and constraints were encountered, resisted and transcended, and to what extent are the products of this work constituted by such contingencies and constraints? Broadly, what *kind* of artifact would emerge from these practices?

In the previous section I suggested that the STURP team's success in achieving formal approval for the Turin inquiry was due not only to their strategic negotiations with its custodians, but also to their recasting of the shroud as a legitimate scientific problem.[24] As such, the shroud was shifted into the purview of STURP scientists' expertise. In this section I explore the STURP team's five day inquiry work in Turin, with a specific focus on the significance of the "place of production" – the makeshift STURP laboratory. My analysis here takes some moderate guidance from the insights of laboratory studies, a framework of interpretation that attends to the spectrum of symbolic, epistemic and cultural practices that characterize the laboratory setting (Knorr-Cetina, 1992; 1995). The STURP lab, I suggest, represents more than the provisional site in which scientists could carry out their interrogation of the shroud. Rather, I argue that the STURP lab is analytically consequential, in and of itself, for understanding how the shroud was made amenable to STURP members' rendering practices. Moreover, the STURP lab would come to signify the primary locus of authoritative evidence claims about the artifact.

On October 8th, 1978, the STURP team travelled to Turin with two objectives: to test the human artifice theory by examining the cloth for paint pigment, and to examine

[24] Indeed, STURP scientists worked in two directions. In one, they treated the shroud as a religious object, while in the other they constituted it as a scientific object.

the chemical properties of the cloth itself. Although STURP scientists initially wanted to conduct radio carbon dating to determine the approximate age of the cloth, the size of the samples required for this procedure would have caused too much damage to the cloth. The team was granted permission to conduct experiments on the condition that all tests were non-destructive.

This compromise with the shroud custodians is worth some brief analytical attention. Objects of scientific interest, and particularly natural objects, are typically altered, if not destroyed entirely in the course of scientists' experimental work. Michael Lynch (1988) describes this as the ritual practice of sacrifice in science that functions to "create and preserve an extended domain of temporal, spatial, and human significations" (p. 276). As he notes:

> Although in laboratory sacrifice these significations are not 'religious', they transcend the concrete limits of the naturalistic animal's corpse and disclose the onto-theological order of what Heidegger calls 'the mathematical'. (Lynch, 1988, p. 276)

While STURP scientists were not permitted to engage in any physical "sacrifices" of the shroud, certainly the rendering practices planned for the shroud signify a constrained kind of sacrificial ritual intended to transform the artifact into an "abstracted and purified" analytic object (Lynch, 1988, p. 279). Yet in this context, the scientific sacrifice was also inhibited by the circumscriptions associated with handling this sacred religious artifact.

In the months prior to their departure, STURP members developed a detailed research protocol organized around each team member's areas of expertise. The protocol

included strictly delegated timelines for experiments and data collection, detailed procedural and methodological protocols, and several technical applications.

The scope of the research required nearly eight tons of technical equipment and an enormous amount of physical space in which the equipment could be properly organized. STURP members required this space to create a makeshift laboratory in which the work could be organized into sub-disciplinary data collection work sites, and in which the research protocols developed by the team could be implemented within the allotted timelines. To this end, the team was assigned a set of suites in the Palace of the House of Savoy, the former home of the deposed King Umberto II, who at the time was the legal owner of the shroud.[25] The suites comprised a ballroom and six spacious rooms replete with marble statuary, coats of arms and classical frescoes on the ceiling. Armed guards stood in the courtyard below (Heller, 1983).

Due to the considerable time lost by an unanticipated confiscation of their equipment at Italian customs, the STURP team was under tremendous pressure to transform these rooms into the makeshift lab. I want to consider how one STURP member describes the organization of the STURP laboratory:

> Hammers and prise bars flew. Everyone knew what to find in what crate, for everything had been logged and catalogued... the ballroom was set up for repair, maintenance and logistical control...the windows were shuttered, blocked, and fitted with thick black plastic. The doors were baffled with black plastic so that no light could leak in...other rooms were set up as preparation rooms, and they were filled with gear. In these rooms equipment could be adjusted, tested and calibrated, and then brought into the Shroud test room... Ron London, a

[25] The Turin shroud was owned by the Royal Savoy family from 1464-1983. It was conferred upon the Roman Catholic Church by deposed King Umberto II of Savoy in 1983 (Wilson, 1998). See Appendix A for the historical custodianship of the shroud.

> radiographer who cheerfully clomped his way through the palace in cowboy boots, converted the lavatory into a darkroom so that X-ray and some other film could be developed on the spot. One by one the sinks were cannibalized. Tanks were installed, safety lights, water pipes, and electrical connectors were rigged...had the original palace designers seen these transformations, they would have thought the result straight out of Jules Verne. (Heller, 1983, p.100-101)

The manufacture of the STURP laboratory in the Savoy palace (or, to borrow from Latour (1999), the positioning of the lab in the "midst of a world untouched by laboratory science") is analytically noteworthy in several respects (p. 259). I suggest that STURP scientists' efforts and struggles to construct a functioning laboratory (in a decidedly unusual backdrop of armed guards and nervous museum officials) can be construed as more than a conversion of space for practical ends. Rather, these exertions signified a concerted *disciplining* of the environment in the face of unusual constraints (e.g., time and resource limitations, the presence of non-scientific actors, etc.) and in the face of considerable uncertainty (specifically, what was the thing they were to examine?). A long tradition of scholarship has elaborated on the theoretical significance of the laboratory, laying bare the idea that as a physical entity, the lab represents more than the material locus of knowledge production. It is the site wherein the credibility of scientific practice is affirmed, where the authority of science's epistemic practices are symbolically enhanced, where the social relations of production unfold in complex ways, where objects of analysis are not only intervened upon but *translated* and "politically and symbolically construed," where uncertainties and ambiguities are made "doable," and where epistemic effects are engendered by these practices (Knorr-Cetina, 1992, 1995, p.143); Latour, 1999).

Thomas Gieryn (2006) reminds us as well that laboratory walls enable scientists

to claim "exquisite control" over the objects of their work by affording a way to segregate the work from the "vicissitudes and promiscuities" of the external environment (p. 5, 6). The laboratory is thus a "privileged truth spot," or the bounded epistemic territory that contains the practices of science and lends symbolic authority to these practices (p. 5). It is not insignificant, for example, that the configuration of the STURP lab was designed to prevent any outside interferences (windows are "shuttered" with plastic, doors are "baffled," and light is effectively cast out). Gieryn refers to these uncertainties as "epistemic risks" that manifest in uncertain places (namely in fieldwork science). Epistemic risks represent the endless "distractions and contaminations" that force scientists to establish a laboratory as a means of *containing* nature (p.6).

Such containment and disciplining of space in the Savoy palace was, I suggest, importantly implicated in the process of the shroud's transformation into a workable object of the STURP team's epistemic practices. In view of the fact that STURP scientists were confronting a peculiar object in an uncertain environment, the space itself must be conditioned to properly *receive, contain* and *know* the object of study. We are reminded here of Latour's (1999) account of Louis Pasteur's work on the outbreak of bovine anthrax, and the implications of Pasteur's crude laboratory for claiming mastery over the disease and shifting the *scale* of the problem. That is, "disease," as an elusive entity, was effectively contained, dominated, named and *known* only through the obligatory point of entry of the lab (Latour, 1999). As Latour notes, "to go straight at anthrax, you should make a detour through Pasteur's lab" (p. 261). Likewise, if we wish to unravel the complexities of the shroud, it is necessary to pass through the makeshift

lab, where these complexities can be effectively contained and systematically organized. Thus, as Latour explains, "no one has ever seen a laboratory fact move outside unless the lab is first brought to bear on an 'outside' situation and that situation is transformed so that it fits laboratory prescriptions" (p. 272).

As the primary domain through which the complexities of the shroud would be revealed and provisionally resolved, the STURP lab became, both symbolically and materially, an "obligatory passage point" in the culture of shroud science (Callon, 1999; Latour, 1999). Moreover, the activities and practices of the STURP lab have tended to carry the most epistemic weight and authority, particularly in subsequent controversies over the shroud (a point I will elaborate on in later chapters).[26]

Confronting Uncertainty: Reconfigurations in the STURP Laboratory

Four STURP members recall the moment when the shroud was brought to the team:

> Down the long corridor came twelve men – six on each side – supporting at shoulder height a large piece of plywood covered in red silk. Everyone waited with bated breath, wondering how he or she would feel when the silk was pulled back and the Shroud was revealed. It was reverently placed down, and the senior Poor Clare nun pulled back the silk. Not one member of the team felt even a twinge of spiritual awe or emotion.
> Jackson: It looks like a cloth with funny stains on it.
> Jumper: the image is fainter than I thought it would be.
> Dinegar: The blood is a peculiar color.
> Gilbert: It looks the way I thought it would. (Heller, 1983, p. 107)

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[26] There is little doubt that the media contributed, to some extent, to shaping the symbolic authority of the STURP laboratory. In 1980, National Geographic published what was perhaps the most exhaustive report of the STURP expedition. In its 18 page pictorial piece entitled "Science Seeks to Solve the Mystery of the Shroud" STURP scientists are described as "pilgrims of science" examining the shroud with "white-glove care" (Weaver, 1980, p.737).

It is clear from the descriptions that the visual properties of the shroud were more confounding than they were revelatory. STURP members were, if not slightly disenchanted, certainly perplexed by the problem that lay before them. How is an object that engenders such uncertainty made acquiescent to the techniques of science? How is certainty wrested from ambiguity? How are the unique constraints presented by the artifact effectively transcended?

To be known and made knowable, objects of scientific interest must be made *amenable* to scientific intercession (Lynch, 1985). No less than any other object of scientific scrutiny, the shroud too required such a conversion. To be sure, this idea is straightforward. The assembly of knowledge about the properties and provenance of the shroud entailed, at the outset, a deconstruction, disciplining and translation of the artifact, such that uncertainty could be surmounted, specific constraints could be transcended, and "epistemic effects" could be engendered (Knorr-Cetina, 1999).

The shroud was mounted on a rotating metal table that had been specially designed in the U.S. by the STURP team. For the purposes of carrying out multiple, simultaneous and time-sensitive data gathering techniques on each of the image components of the cloth, scientists worked in small groups around the clock on a rotating basis for five consecutive days. Fixed and mounted in this way, the shroud became the "site" on which social interactions, coordinated epistemic practices, and divisions of labour unfolded. Michael Lynch (1990) observes that the collective rendering practices of scientists – the ways in objects are measured, coded, analyzed and made comprehensible – are, importantly, social processes. As he notes, such practices can be construed as "an

assembly line resulting in public access to new structures wrested out of obscurity and chaos. Instruments, graphic inscriptions, and interactional processes take the place of the 'mind' as the filter, serving to reduce phenomena of study into manageable data." (p.152)

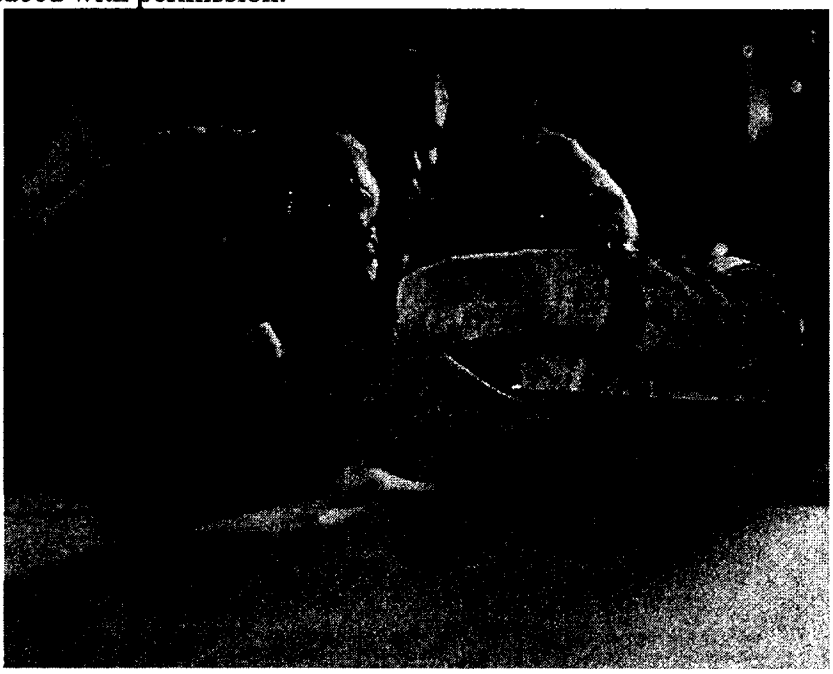
Such was the nature and outcome of the STURP team's collective, synchronized interventions on the shroud. As STURP member John Heller describes it:

> ...[STURP] swung into action with its carefully orchestrated program. The activity became much more measured than the set-up procedures, but was still highly intensive. Most of the time, one group was working at each end of the cloth while a third was carrying out a procedure in the middle...as the work progressed, the recorded results began to pile up. Everything that was done was logged...everything was photographed. Everybody was photographed. Every action by every person was photographed so that a complete documentary would be available of each individual's action, location of measurement, and instrumentation. A voice recording was made of every event so that there could be no question of what was done when, by whom, and how. There was an original plan to send some data back by satellite so that it could be mathematized in the home laboratories to determine if more measurements should be made...everything was stored. (Heller, 1983, p. 109; 115 -116)

Image One: Shroud is mounted on rotating table

Scientists employed a number of techniques to carry out multiple tests on the cloth, including: x-radiographs, image enhancement procedures, infrared photography, ultraviolet and visible reflectance spectroscopy, macroscopy, ultraviolet photography, tape sampling and microscopy. They identified test point areas by placing magnets on specific examination points, a technique the team called "mapping" of the image. Members extracted samples from the body image and non-image areas for subsequent analysis. The "mapping" of the shroud was an important technique that provided for hundreds of additional procedures associated with determining the shroud's chemical properties.

Image Two: STURP scientists examine underside of linen

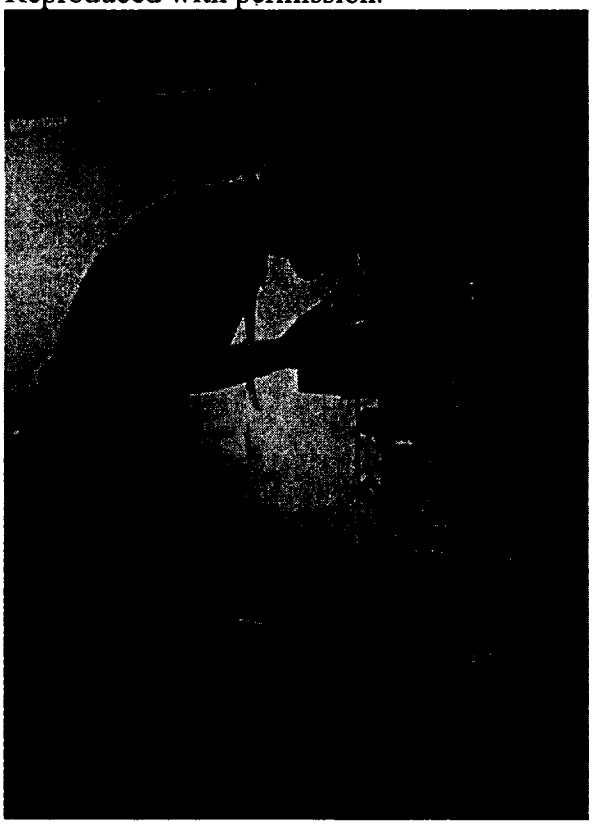


Understood in practical terms, the STURP team's techniques enabled a systematic series of non-destructive experiments and micro-photographing of test points under considerably limited time constraints. The practices of mapping, marking and "constituting graphic space" are indeed common in scientific practice (Lynch, 1985, p.41). But these are more than simply technical and methodological performances. The re-constitution of the shroud into a cartographical space effectively rendered the artifact acquiescent to STURP scientists' practices, and allowed for the imposition of order in the midst of ambiguity. Lynch (1985) notes that objects of scientific interest become visible and analyzable through the practice of "civilizing" or "disciplining" objects under study. An object thus:

> ...behaves in accordance with a programme of normalization. This does not mean that it fails to resist, or that its recalcitrance does not serve to adumbrate its objective news for science. It is to say that, when an object becomes observable, measurable and quantifiable, it has already become civilized; the disciplinary organization of civilization extends its subjection to the object in the very way it makes it knowable. The docile object provides the material template that variously supports or frustrates the operations performed on it. Its properties become observable-reportable in reference to the practices for revealing them. If the object was not compliant to such a programme, its attributed properties would be incompletely or 'unscientifically' observable. (Lynch, 1985, p.44)

The shroud was rendered into what might be construed as a topographical space on which cultivations, enhancements, transformations could unfold. In being subjected to these rendering practices, the shroud was increasingly "captured" and converted into a comprehensible object.

Image Three: Microphotographs of Shroud



At the conclusion of the five day testing period, the shroud custodians conducted closing ceremonies, said a prayer and restored the shroud to the reliquary. STURP members packed their equipment and data samples and returned home to begin the long process of analyzing their work. Though overwhelmed by the intensity of the Turin experience and the work to follow, members were confident that the data they extracted would provide findings of some consequence. As STURP member John Heller recalls, "whatever was to be discovered from all this analysis, there would be no lack of specific, reproducible evidence. Regardless of what the data showed, the meticulous nature of the experiments

would produce unequivocal data" (Heller, 1983, p.116).

In several ways, the assembly of practices and techniques carried out in the makeshift STURP lab do not deviate appreciably from the ways in which we have come to understand science "in action." In the face of constraints, uncertainties and exceptional settings, STURP scientists effectively created an environment in which contingencies and uncertainties could be reasonably governed. I have emphasized, for example, the analytical importance of the makeshift laboratory for understanding the process of establishing a socio-technical environment in which governance over an object is at least moderately accomplished. The STURP laboratory signifies more than just space – it is a *normative* region where outside "contaminants" (e.g., light and noise) could be diminished, and where the object of interest could be "received" by the scientific gaze.

The STURP lab also embodies, importantly, considerable symbolic authority in the broader narrative of shroud science. It is the place of epistemic privilege that lends credibility to the work and claims made therein (Gieryn, 2006). As noted, the STURP lab, and all of the social relations of production therein, has come to signify the primary site of authority, the obligatory passage point, with regard to evidentiary claims about the shroud.

I have drawn as well upon the significance of STURP scientists' techniques and technologies for effectively reconfiguring and translating the shroud into a comprehensible, scientifically workable object. Karine Knorr-Cetina (1999) observes that laboratory practices submit its objects of interest to a "social overhaul," whereby nature is reined in, contained, reconfigured, made workable and effectively "enculturated" by the

social order and practical operations of the lab (p.28).[27] Although Knorr-Cetina refers here to the re-constitution of natural objects, this idea finds relevance to the epistemic practices of the STURP lab. STURP members' interventions shifted the artifact out of its spiritual frame of reference and situated it in the makeshift culture of the lab, where it could be transformed into a site of multi-disciplinary practices and extractions.

The STURP lab contained, importantly, the social relations of production that shaped the outcomes of the inquiry. Accordingly, the decisions and negotiations embodied in the team's research protocol, the last minute changes and alterations STURP scientists made in the face of unexpected contingencies, the collective struggles against the constraints of time and resources, and the collective efforts to transform order from disorder were fundamental to the production of knowledge about the shroud.

Discursive Representations of the Turin Shroud

How did the "heterogeneity" of the STURP team's laboratory work shift to the "homogeneity" conveyed by their evidentiary representations of the shroud? (Law & Wittaker, 1988). What was captured and disclosed by STURP scientists' discursive depictions of the artifact? For two years following the inquiry in Turin, STURP scientists

[27] I acknowledge a limitation in applying the theoretical insights of laboratory studies to an historical case study. In its conventional application, the methodological distinctiveness of lab studies is that the research is carried out *in situ*. Accordingly, the lab studies method applies ethnographic scrutiny to the day-to-day mundane practice of science as it unfolds, thereby forestalling any reliance on "retrospective", temporally and contextually removed accounts of scientific activity (Woolgar, 1982, p. 484). My analysis of the STURP lab admittedly relies on secondary accounts of the Turin inquiry- the very thing that lab studies attempts to avoid. Although I am mindful of the fact that secondary accounts of the STURP expedition are circumscribed by the perceptions of the narrators, I am not entirely convinced that this precludes the application (with circumspection) of some key ideas offered by the lab studies framework.

held closed conferences and converted their data for publication in several technical and scientific journals. I single out for consideration a selection of excerpts from STURP scientists' formal publications. I lay particular emphasis on the communicative choices reflected by the descriptions of the STURP team's outcomes, as well as their implications for eliciting a particular comprehension of the shroud.

In a collection of peer-reviewed scientific and technical journals, STURP scientists disseminated several empirical findings concerning the chemical and organic composition of the shroud. Their tests yielded no evidence of any foreign material on the cloth that might suggest the image was a human rendering. No pigment particles, dyes, stains, inorganic elements, or other forms of painting media were detectable on the cloth. STURP members further asserted that the bloodstained areas on the cloth were indeed composed of human blood, and that the cloth likely enfolded a human form. None of these findings were explicitly linked to any provenance or authenticity claims per se, but they were a critical turning point in the longstanding debates around the human artifice theory.[28]

I want to first consider how STURP scientists introduced their rationale for their inquiry:

> The Shroud of Turin, believed by many to be the burial cloth of Jesus, has generated considerable controversy, but unlike some other controversial

[28] Perhaps not surprisingly, some media accounts amplified the implications of STURP's evidence, framing it often as a point in favour of authentication. In an article entitled "New Data Suggests Image on Shroud is Genuine", the New York Times reported that the blood evidence was "a crucial finding in the analysis of the shroud's authenticity" (Monagan, 1980, p.C3). The same article reported that "the scientists say they can neither prove the shroud to be a forgery nor account for how it was made, thus leaving the strong impression that it may be the real thing" (p.C3). STURP members were, in fact, under some media pressure to make a definitive statement attesting to the shroud's authenticity (Heller, 1983).

> subjects...the Shroud exists as a material object. It can be observed directly and objectively. The results of studies can be analyzed by scientific methods. (Schwalbe and Rogers 1982, p. 3)
>
> [The Shroud] is a relic, an object of faith. Ordinarily science remains detached from such objects, but in this case, the unusual quality of the image intrigues the scientific mind... Because the cloth has associated with it the mystery of its chemical origin and intrigue of its unique image, we can say at a very minimum it deserves study. Furthermore and perhaps the most compelling reason is the mystique (whether real or imagined) of the Shroud being associated with the religious and historic figure of Jesus. The question of its authenticity is worth investigating. (Jumper & Mottern, 1980, p. 1909)
>
> Regardless of the religious implications of the image, the physical characteristics of the cloth are subject to classical experimental techniques. (Accetta & Baumgart, 1980, p.1921)

If the work of shroud science is to be certified by the broader scientific community, it bears upon scientists to underscore, at the outset, their professional and personal indifference to the religious signification of the object. Clearly these preludes to their work represent STURP scientists' efforts to legitimize the scientific study of an object of faith, and to ensure readers that the outcomes were not compromised by any subjective distortions of the evidence. The shroud was, after all, an object inscribed with a specific and enduring set of faith-based interpretations prior to being re-defined by scientists. It was necessary, therefore, for scientists to shift the object out of its symbolic context. More importantly, however, it bears upon scientists to single out the attributes of this artifact that are objectively available to the senses – that is, attributes that are amenable to impartial scientific scrutiny and purification. The shroud was, as Schwalbe and Rogers noted, certainly controversial by virtue of its religious implications but nonetheless scientifically "observable" and thus within cognitive reach. Similarly, it

contained physical characteristics that were subject to scientific intervention and interpretation. Indeed, something that "intrigues the scientific mind" clearly invites such intervention.

In a multiply authored report published in *Archeological Chemistry* STURP members described their findings as follows:

> There seems ample evidence to explain all the red particles on the Shroud as originating from either the blood itself, the retting process, or, possibly, the water of the 1532 fire incident (c.f. above); we reject the hypothesis *(13- 15)* requiring that an artist had to have touched up an earlier image. We, therefore, strengthen our statement on the blood to say that all of what we see in the blood area is derivable from blood itself or its products. (Jumper et al., 1984, p. 462)

> ...[The] blood images present no mystery; all evidence suggests that the blood went on as one would expect for a cloth in contact with wounds or the normal secretions of such wounds. We therefore suggest that the blood images are the natural consequence of linen being in contact with wounds. If the blood images were made by contact with wounds, it follows that the cloth was used to enfold a body. We have independent evidence that the cloth was used in this way. The mapping function, which maps body-only image density to expected cloth-body distance...offers a consistent argument that the Shroud enfolded a human body shape. If we couple this argument with the testimony of forensic pathologists, we can say more: not only was it a human form, but further, it was a human body. (Jumper et al., 1984, p. 470)

In the same article the authors conclude the following:

> [The blood markings] are clearly made of blood, likely human; however, we cannot state absolutely that the blood is of human origin. It is in these blood areas that we [and others *(22)]* find proteins present of the types associated with blood serum. Finally, there is nothing unaccounted for in the blood areas that would lead one to suspect that anything but blood formed the blood images *(12).* We therefore do not agree that there has been an attempt to artistically enhance a "preexisting" blood image *(13-15);* nor do we feel that these are "painted" blood images. (Jumper et al., 1984, p. 467)

The potency of STURP's techniques and instruments to draw forth persuasive evidence and challenge the human artifice theory is also articulated in a report published in *Analytica Chimica Acta,* a 1980 article published in *Applied Optics* and a 1984 report published in *Archeological Chemistry:*

> The evidence seems to be sufficient to conclude that the Shroud "blood" areas are blood. The presence of protein, bilirubin, and albumin, the optical absorption and fluorescence characteristics of individual fibrils, and the iron concentrations determined by x-ray fluorescence, all support this hypothesis. This contradicts earlier tentative conclusions [59, 60] that were drawn mainly from the negative results of less sensitive tests. (Schwalbe & Rogers, 1982, p. 40)

> There has been no evidence found to suggest that the visible image results from a colored foreign material on the cloth. In this regard, the data are quite internally consistent. Microscopic studies have revealed the image to be highly superficial; the image resides in the topmost fibers of the woven material as a translucent yellow discoloration. No pigment particles can be resolved by direct Shroud observation at 50X magnification, nor can unambiguously identified pigment particles be found on the tape samples at 1000X. (Schwalbe & Rogers, 1982, p. 11)

> Positive chemical conversion to a fluorimetrically characteristic prophyrin species does confirm and give positive presumptive evidence for identification of the alleged blood areas on the Shroud of Turin as, in fact, containing blood. (Heller & Adler, 1980, p. 2744)

> Microchemical tests *(12),* mass spectroscopy *(16),* and laser microprobe Raman spectroscopy *(16)* all fail to show the presence of any added materials on the yellow body-only image fibrils to within their limits of detection. We conclude that no material has been added to these yellowed fibrils to produce the color *(12,16.)* (Jumper, et al., 1984, p. 455)

An initial reading of these excerpts suggests, perhaps, an unambiguous set of evidence claims about the physical properties of the Turin shroud, and what these findings might imply for its provenance and possible authenticity. The shroud contains no

evidence of human intervention, and bears several indicators that the image could have possibly been formed by a human body. Indeed, the image, once brought out of obscurity through scientific methods and instrumentation, "present[s] no mystery" at all" (it contains blood, and the blood flowed from wounds, the wounds are those inflicted, likely by crucifixion methods, on a human body).

But scientific language is neither neutral nor independent of the linguistic techniques by which a particular "reality effect" is achieved (Golinksy, 2005, p. 104). Communicative techniques in science employ modes of persuasion that strive for such an effect, thus allowing audiences to apprehend scientists' objects of interest through their linguistic representations. Latour & Woolgar (1986) call this the "essential congruence" between fact and the processes of "literary inscription" that evoke the facticity of a statement (p. 76).

STURP scientists' evidence, as it is disclosed in their technical accounts, is more than disengaged descriptions of outcomes. Instead, these outcomes may be interpreted as strategic methods of shaping the evidence in a manner that has implications for how the reader should now understand the artifact and its enigmatic features. The evidence settles (a least provisionally) what has been up to that point a considerable source of dispute in shroud history: the human artifice theory.

The shroud is also made increasingly comprehensible and intelligible through the strength of the STURP team's instrumentation and techniques. The mapping analyzer, referred to by one STURP scientist, provides a consistent body of evidence that the cloth contained a "form." STURP's techniques and instruments are not only critical to drawing

out the latent features of the artifact, but are essential to the artifact's *constitution*. Latour and Woolgar (1986) remind us of the importance of instruments and imaging devices in scientific practice (what they term "inscription devices"), to not only enhance the features of a phenomenon but to effectively *generate* that phenomenon. Drawing on Gaston Bachelard's concept of *phenomenotechnique*, Latour and Woolgar note that an entity intervened upon through techniques and devices embrace the appearance "of a phenomenon by virtue of its construction through material techniques" (p.64). Ian Hacking (1983) makes a similar observation, noting that "effects" of scientific experiments do not exist outside of the apparatus from which they were produced. That is, phenomena are effectively "created" by the tools and techniques of scientific experimentation (Hacking, 1983).

The STURP team's discursive representations of the shroud's material characteristics were also bound up in their visual renderings of the artifact. Visual representational practices entail the creation and use of visual and textual displays of phenomena (e.g., graphs, diagrams, equations, reports, photographs, and maps) that are requisite to how objects of science are rendered intelligible, analyzable and "revelatory" (Lynch, 1990; Latour,1990).Visual representations, coupled with text, serve to disclose, reconfigure, translate, reify and make comprehensible the objects of scientific interest. Pauwels (2006) notes, for example, that representational practices in science:

> ...[entail a] conversion of some kind; a process of inscription, transcription, and/or fabrication whereby the initial source (phenomenon, concept) is captured, transformed, or even (re)created through a chain of decisions that involve several actors (scientists, artists, technicians), technological devices, and normative settings. This complex process of meaning making has an important impact on

> what can be known and how, on what is revealed or obscured, and on what is included or excluded. (Pauwels, 2006, p.405)

Golinsky (2005) argues that the "visual impulse" constitutes the essence of the modern scientific prerogative to reveal what cannot be seen, thereby extending "ocular proof" to the senses (p. 145). Latour (1990) has argued that visual displays, or what he terms "inscriptions," effectively merge objects under study with the epistemic claims made about them. Moreover, graphic products of science allow the "confusion" of nature to be *contained* and *fixed*. Indeed, Latour has argued that inscriptions account in large part for the epistemic and cultural authority claimed by scientists and engineers, insofar as "no one else deals only with phenomena that can be dominated with the eyes and held by the hands, no matter when and where they come from or what their original size" (p. 45).[29] Inscriptions also provide the means by which natural phenomena may transcend time and space.[30] Nature, rendered into a two-dimensional visual representations (i.e., maps, photographs, graphic displays), can be moved, shuffled, reproduced and spread, such that "all the instants of time and all the places in space can be gathered in another time and place" (p. 45).

Visual representations in science are, fundamentally, *social objects*. They are inscribed with the collective and consensual act of construction and interpretation, as well as the cultural, technological factors and constraints inherent in these practices (Pauwels,

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[29] Latour (1990) has noted that science functions (that is, constructs, persuades, and argues) most effectively within manipulated spaces, where the scale of nature is reduced and recombined to enable its "mastery".

[30] In quite another context, Latour (1988) applies the notion of "regimes of representation" to imagery contained in religious art, where he argues that we are never afforded "direct access to the sacred" but are instead presented with representations containing a cascade of multiple mediations (p. 21).

2006). Selectivity and sense-making is a *collective* practice.[31] Scientists work in coordinated practice as "agents of mediation" to collectively transform the object of interest into legible data (Lynch 1990, p. 156). As Lynch explains:

> Such ordering of data is not solely contained 'in perception', but is also a social process – an assembly line resulting in public access to new structures wrested out of obscurity and chaos. Instruments, graphic inscriptions, and interactional processes take the place of the 'mind' as the filter, serving to reduce phenomena of study into manageable data. (Lynch, 1990, p.156)

The products of visualization practices themselves are thus importantly tied into, and reflective of, the division of labour, constraints and normative practices of laboratory work itself. Lynch (1985) suggests that the graphic images produced in scientific practice can be read as "idealized account[s]" of the labour itself (p.58). Thus the practices and their products embody the "social programme" of perception in science (p. 59).

The significance of visual representations rests in their communicative function and intent. What scientists want to disclose, and what the viewer is directed to see is

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[31] It is important to note that STS scholars have placed less analytical emphasis on individual cognitive processes and have attended in greater depth to the social conditions underlying the collective practice of visual representation in scientific practice, as well as the consumption of its products. It is argued that visual images in science are *social* and *political* products; they embody both the social practices of fact construction (i.e., abstraction, framing, disciplining, assembling, disassembling and reordering of the object) and well as the collective, interactional and inter-subjective processes of interpreting images rendered orderly and analyzable (Lynch, 1985, 1990; Amann & Knorr-Cetina (1990); Latour 1990). There is a wealth of emerging intellectual work, however, in the areas of cognitive science, the new philosophy of the mind, and neuroscience that underscores the complexity of the intersections among visual images, perception and embodied visual experience. Although the STS scholarship does not tend to be in dialogue with these schools of thought, they find relevance to discussions of visualization practices in science. In her book *Echo Objects: The Cognitive Work of Images*, Barbara Stafford (2007) argues that the perception of visual imagery and art is a deeply recursive process. The act of "seeing" imagery enables the viewer to comprehend the *process* of seeing. As she notes, certain forms of art "permit us to see the synchronizing cerebral processes involved in vision" (Stafford, 2007, p. 45). Zenon Pylyshyn (2003) challenges the picture theory of mental imagery and argues that our mental images do not correspond to visual representations. As he notes, "what can never serve as a theory of vision is a theory that says that vision creates a copy of the world inside the head" (Pylyshyn, 2003, p. 3). See also Edward Tufte (1997) *Visual Explanations: Images and Quantities, Evidence and Narrative*, Cheshire, CT: Graphics Press.

bound up in the choices and selections embedded in the visually rendered products themselves. It is not my aim here to provide a thoroughgoing analysis of STURP scientists' multiple visual products. It is useful, however, to take brief account of some of the ways in which the descriptions of STURP scientists' visual renderings of the shroud impart an "optical consistency" that serves to enhance the certainty of the evidentiary claims being made about it (Latour, 1990, p. 46).

In a comprehensive study of the various markings and stains on the shroud, readers are presented with an enhanced reflectance photographs of the side-wound areas of the shroud image. The descriptions direct the viewer to "observe" indisputable evidence of blood flow and wound marks:

> Menisci are clearly visible at the edges of the stains. Heavily colored portions of the stains have not diffused very far, which suggests high viscosity... In some areas there is evidence of some apparently colorless fluid bordering or diffusing farther out than the darker fluid (one could thus conclude that this fluid would be thinner than the darker fluid, c.f. below). (Jumper et al., 1984, p.459)

The authors then describe wound marks on the image as follows:

> These images appear to be located at distinct and clearly identifiable parts of the anatomy of the man on the Shroud, which would suggest they are wounds... these [scourge marks] appear to be somewhat different from the other two types of blood images. Under U V fluorescence, they appear to be darker than the image and, also, to be much more sharply defined than they appear in visible light (JO), as would be expected on the basis of the known spectral characteristics of iron porphyrin compounds... the geometric similarity of groups of these dumbbell-shaped marks is also quite striking *(21)*. Fine "scratches" from the distal ends of these dumbbell-shaped marks appear in the UV-stimulated fluorescence photographs. (p. 459-460)

They conclude that:

> ...there is nothing unaccounted for in the blood areas that would lead one to suspect that anything but blood formed the blood images *(12)*. We therefore do not agree that there has been an attempt to artistically enhance a "preexisting" blood image *(13-15);* nor do we feel that these are "painted" blood images. (p. 468)

Image Four: Photographic negative of the Turin Shroud[32]

[32] © 1978 Barrie M. Schwortz Collection, STERA, Inc. Reproduced with permission.

Image Four shows the shroud as a photographic negative. This served as the main reference for a range of the STURP team's evidentiary claims, and particularly claims that the cloth showed evidence of blood. Importantly, these claims had implications for precipitating a controversy within the shroud science community, which I account for in the chapters to follow.

In Heller and Adler's (1980) report on the evidence of blood of the cloth, the authors note that the above image:

> ...shows that the Shroud image contains areas from the wrists, arms, and feet that correspond to the blood stigmata of a classical crucifixion. In addition to these, there appear to be head and flank wounds that also bled. All these areas appear on the cloth as brownish red stains. (Heller & Adler, 1980, p. 2742)

In Jumper et.al. (1984), the authors describe the markings as follows:

> The blood images appear as three major types: areas that might be termed wounds, on the wrists, for example; areas of blood flow, along the small of the back, for example; and "scourge" marks that are ubiquitous over the torso and legs.... There is no evidence of the ubiquitous presence of any stains, dyes, or pigments in these body-only image areas. (Jumper et al., 1984, p. 458, 467)

Both text and imagery in science communicate a particular narrative of the object of interest, and in doing so, persuade the intended audience of its facticity or truth. Thus scientific representations, as Law and Wittaker suggest, are "the literary lever which helps to move the readership, to persuade it that this does indeed go with that rather than the other. And they are the representatives of endless awkward objects and processes that are left behind in the laboratory" (Law & Wittaker, 1988. p. 161).

The STURP team's visual products are indeed more than simply objective renderings of the artifact. I suggest that STURP's techniques of representation did not simply improve upon, or un-problematically disclose the shroud's obscured properties pursuant to the question of the shroud's composition and provenance. Rather, their scientifically rendered images, coupled with the strategic use of linguistic resources, suggest a capturing and crafting of a new artifact -one that acquired its legibility through these scientifically circumscribed representations. STURP's findings, as they are expressed through these representations, have implications for how viewers are encouraged to *see* and *comprehend* the scientifically-mediated shroud.

The discursive products of science are invariably bound up with science's "disciplinary practices and purposes" and the myriad constraints contained within science's places of production (Pauwels, 2006, p.5). As such, the outcome or "effect" of STURP scientists' interventions on the shroud must be understood as both intentional and unintentional. As I have shown, STURP scientists confronted several constraints of resources, time, and accessibility that had some bearing on the outcomes of their work. Simultaneously, the scientifically-mediated shroud is embedded with purposeful choices in terms of what to disclose, how to organize its features, how to delineate the evidence for its composition, and how to govern its comprehension.

Conclusion

The aim of this chapter has been to offer analytical insight into what was entailed by STURP scientists' attempts to *comprehend,* and *make comprehensible,* an object whose enigmatic attributes posed rather exceptional challenges. My basic position thus

far has been that STURP scientists did not simply clarify and disambiguate the Turin shroud, but captured and altered the artifact in ways that had implications for how it was represented, and how it was to be properly comprehended. By extension, I have sought to understand what *kind* of reconfigured artifact was evoked by STURP scientists' intercessions. Put another way, how did their mediations inscribe a new narrative on this artifact? To this end, I've situated my analysis within intersecting conceptual frameworks in science and technology studies that are located under the constructivist agenda, broadly comprehended. Three analytical themes - translation, capturing and construction – have guided my interpretation of the STURP inquiry and the knowledge claims derived by it.

The notion of translation underlies my approach to understanding how the Turin shroud came to be defined at the outset as a scientific problem. Such a conversion of the artifact was indeed essential, if any meaningful knowledge could be derived from STURP scientists' work. Drawing on Callon's (1999) idea of displacements and transformations, I have attempted to show that translating the shroud into an object of scientific consequence (and thus shifting the artifact into the purview of STURP scientists' body of expertise) entailed both practical and purposive actions on the part of STURP scientists. To translate, as Callon notes, "is to displace…to express in one's own language what others say and want" and to "establish oneself as a spokesman" (Callon, 1999, p. 81). The STURP inquiry would not have materialized without the concerted efforts of STURP scientists to re-cast the problem (i.e., what is the source of the shroud's ambiguities?), identify the apposite means of resolution (how will science inform the problem?), secure consensus among several social actors, and establish a "discourse of certainty" (i.e., how

might the actors be strategically enrolled and invested in the problem?) (Callon, 1999, p. 81).[33]

I have drawn as well on the theoretical importance of the *place of production*, the makeshift STURP lab, as significant to how the shroud was made amenable (or perhaps more precisely, *made docile*) to STURP scientists' rendering practices, and was ultimately changed by them.[34] Here I have suggested that the relations of production in the STURP lab, the contingencies and constraints that unfolded therein, the instrumental and social practices engaged by scientists, and the "disciplining" of the lab space itself were critical elements in re-shaping the artifact. The STURP lab, moreover, was a site of symbolic consequence insofar as it has come to represent the place of authority, or "truth spot" in the broader sphere of shroud science.

I have attempted to highlight the significance of scientific representation for understanding how objects are "mediated by the very instruments and methods used to depict them" (Pasveer, 2006, p.43). I drew upon the STURP team's discursive practices (in particular their literary, and to a lesser extent, visual representations of the shroud) to illustrate the means by which a scientifically-mediated artifact is constituted, or *brought*

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[33] Translation is a discursive process as well. The work of converting the shroud into an object of the scientific imagination meant *imagining* the artifact differently and applying a different repertoire of descriptors to it in order to properly comprehend it. Recall, for example, that STURP members confronted the shroud by separating the *materiality* of the object from its religious and cultural significations.

[34] I have not drawn in any theoretical depth on the material agency of the shroud except to suggest, perhaps, that the practices of "capturing" this artifact implies a "struggle" of sorts between the material and human actors. No object of science is wholly "docile", particularly if we take into account Andrew Pickering's (1995) concept of the "dance of agency" that transpires between scientists and the material objects they construct. Pickering argues that scientists create their objects and machines and then adopt a passive role in monitoring their performance. In this moment of "human passivity" the object "performs" its material agency (Pickering, 1995, p. 22-23). Michael Lynch has similarly noted that rendering objects "docile" does not imply that they "fail to resist." Rather, objects become "civilized" through the disciplinary organization of scientific practice (Lynch, 1985, p.44).

into being. The shroud that the viewer is asked to comprehend is a shroud *captured* by scientists' epistemic interventions, inscribed with a new narrative of socio-material relations and effectively re-constructed through their techniques, practices and choices. The importance of STURP scientists' discursive representations of the shroud is that it has implications for how the viewer is to now see and comprehend the ontological status of the artifact. As I will demonstrate in Chapter 3, the STURP team's scientifically mediated representations of the Turin shroud were also importantly implicated in their authority to make and defend their knowledge claims about the shroud's provenance.

Finally, STURP scientists did not simply construct a new cultural object, but more importantly (and perhaps unintentionally) produced a new object of contestation. As I will demonstrate in the following chapters, the importance of the scientific construction of the shroud rests in the implications it had for igniting contentions among scientists and forcing the artifact into the center of two heated controversies concerning its material properties and provenance.

Chapter Three: Dissent, Controversy and the Contested Truth of the Artifact: The Great Blood Dispute

> The relationship between religion and science has always been somewhat uneasy, at times hostile, and one frequently marked by misunderstandings – both minor and major. The potential for misunderstandings and hostility increases dramatically when fundamental science and technology are applied to one of the most famous and revered relics in all of Christendom – The Shroud of Turin.[35]

Introduction

The quote above leaves the impression that the contemporary antagonisms provoked by the scientific study of the Turin shroud are reminiscent of the putative discord between science and religion, writ large. Indeed, the shroud controversy has often been perceived in these terms, and perhaps such judgments are to be expected. The domain of science is not only ascribed a singular epistemic preeminence in Western society, but is often presumed to be incommensurable with all other forms of cultural rationality, most notably religion (Coleman & Dysart, 2005). But while the broader relations between science and religion certainly form the backdrop of the contemporary Turin shroud conflicts, rarely have these disputes been symptomatic of any rigid "polarities" between these domains. Rather, the contemporary shroud conflicts embody a complex arrangement of epistemic and political struggles among scientists that intersect and compete, at times quite unpredictably, with the interests and ideologies of other

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[35] Gove, H.E. (1996). *Relic, icon or hoax? Carbon dating the Turin shroud.* London: Institute of Physics Publishing.

social groups who bear a stake in the dispute. If we imagine the Turin shroud controversy in cartographical terms, we encounter several "regions" of conflict where entanglements and alliances are often unanticipated, and where boundaries are, more often than not, tenuously cast. The notion that the scientific study of the shroud has bred intensive "hostility and misunderstanding" is unmistakable. The task of this chapter is to lend empirical and theoretical clarity into the character, effects, motivations and implications of these complex tensions among social actors.

In the previous chapter I argued that the STURP team's evidence of the shroud's material composition, as articulated by their discursive representations of the artifact, effectively brought into being a newly visible and comprehensible object. The new scientific narratives of meaning inscribed on the artifact marked an important shift, therefore, in how the shroud would be seen and comprehended by multiple audiences.

In this chapter, I draw upon a related effect of the STURP inquiry. My starting point is that STURP scientists' intercessions on the shroud, and the knowledge claims derived by them, also evoked a new object of *contestation*. Here I explore what became known in shroud science culture as "The Great Blood Dispute," a phase of conflict that erupted among shroud scientists following the 1978 STURP inquiry.[36] The blood dispute was precipitated by diametrically opposing evidence claims concerning the organic and chemical composition of the shroud image. At issue among scientists was a simple but seemingly critical question: did the shroud image contain human blood, or was it composed only of paint? At stake in the dispute was the question of *whose* evidence

[36] The term was originally coined by professor of philosophy Dennis Dutton in a 1984 book review titled "Requiem for the Turin Shroud", published in the *Michigan Quarterly Review*.

would resolve the "human artifice theory" (i.e., the longstanding contention that the Turin shroud was a medieval painting). As with most socio-scientific controversies, this single point of contention among shroud scientists –blood or paint–escalated into a protracted, passionate and at times politicized conflict. The blood dispute thus highlights the Turin shroud's significance not only as an object of scientific interest but one of epistemic, ontological and political consequence. As such, the dispute signifies a critical and sociologically compelling turning point in the larger historical antagonisms over questions of the shroud's authenticity.

My analysis explores how the blood dispute became an intellectual and political struggle among scientists to assert their claims to credible and authoritative evidence concerning the material composition of the shroud, and by implication, a reliable determination of its possible historical origins. I will show that the tactics of the disputants entailed discursive constructions of "dissenters" and "pseudoscientists" who "imperiled" a truthful representation of the artifact. The terms of the conflict, often rhetorically expressed, were concerned with the implications of specious scientific claims not only for the shroud's scientific misinterpretation, but also as a violation of scientific integrity itself.

Beyond the question of *how* scientists waged their epistemic battles over the question of evidence, however, I also address *why* the issue invited such a protracted and vitriolic struggle. What rested beneath the manifest intellectual and political tensions among disputants? What values were expressed by this controversy, and what was seemingly at stake in its resolution? Here I argue that the blood dispute reveals a

commitment on the part of disputants to the *intrinsic value* of the struggle itself. That is, the blood conflict was underlined and driven by a commitment on the part of disputants to deliver a truthful interpretation of the shroud to the broader social milieu. Indeed, I suggest that disputants' commitment to delivering a truthful account of the shroud was bound up in their conviction that science possessed both the epistemic authority and the *cultural obligation* to do so.

Finally, I will show that while the blood conflict was waged in the empiricist language of science, the dispute itself signifies, in some ways, an extension of, rather than a radical break from, the historical concerns regarding the legitimate interpretation of the Turin shroud, and the implications of this interpretation for the public understanding of the artifact.

My data is comprised of a number of sources. I examined a multi-disciplinary selection of professional journals in which the conflicting evidence claims about the shroud's material properties were disseminated and debated among contending shroud scientists and their adherents. I also examined a series of correspondences exchanged among disputants that have been made publicly accessible. I placed specific analytical focus on the rhetorical devices, literary techniques, enrollment tactics, and other strategies deployed by disputants to dismantle the credibility of opponents' knowledge claims and articulate the intellectual primacy of their own evidence.

My data is also comprised of first hand narrative accounts of the dispute written by shroud scientists in popular books intended for mainstream consumption. Personal narrative, as it is expressed in popular scientific literature, tends to be underexplored as

an element of socio-scientific controversies, yet it offers a singular lens into the relations of conflict and actors' own interpretations of their place in it. In the context of the shroud controversy, therefore, disputants' personal narratives represent a distinctive and indeed a sociologically compelling dimension of this controversy. My specific interest in these narrative accounts centered on the heuristic value of narrative expression as a social act and a critical component of the blood dispute.[37]

My analysis of disputants' popular narratives was guided in part by the insights of narrative sociology, a methodology that takes seriously the "intrinsically social nature" of story telling as a collective, social and often political process (Maines, 1993, p. 21). The act of telling a story is both "an invitation to bond" with the story teller and an important means by which to consign meaning and shared understanding of the version of events being communicated (p. 21). As David Maines observes, "since all socialized humans are storytellers, they are always in a potential storytelling situation when interacting with or encountering others...narratives and narrative occasions are always potential sites of conflict and competition as well as cooperation and consensus" (p. 21).

"All this is fiction": Enter the Dissenter

> This is a story of the irresistible force and the immovable object. I regard me as the former and my (dis)-loyal opposition as the latter. Anyone is welcome to reverse the assignment if they wish. It seems the crux of the matter is-what is the image (blood or

[37] Keeping in mind that books published for mainstream readership are conditioned by various editorial circumscriptions that bear on how the story is "told and sold," narrative accounts of controversy nonetheless offer a rich understanding of how these disputants placed themselves more intimately as protagonists in the conflict, how they construed the complexities and stakes of conflict itself, how they effectively constructed the "antagonists," how other actors are drafted into the plot, and perhaps most importantly, how audiences are "enrolled" in the debate itself.

> paint)? If we could answer that question to the satisfaction of the irresistible force and the immovable object, we could all be friends again. -*Walter McCrone, Former STURP member*[38]

From the earliest history of the Turin shroud, disputes have centered around whether and to what extent authenticity claims would corrupt the artifact. Indeed, proclaiming that the shroud was a genuine relic of the crucifixion (and more profoundly, material evidence of the resurrection), had great potential for its exploitation.[39] Such was the prevailing concern among prelates and theologians, whose anxieties were rooted not only in the "terrible risk" of making a single relic an "article of supreme religious faith," but in the consequences of authenticity claims for the grievous misapprehension of Christian doctrines (Tribbe, 2006, p.185). In his *Treatise on Relics*, John Calvin (1543) observed that there is no biblical record of a funeral shroud imprinted with Christ's image. He thus asked how it is conceivable that "those sacred historians, who carefully related all the miracles that took place at Christ's death, should have omitted to mention one so remarkable as the likeness of the body of our Lord remaining on its wrapping sheet?" (p. 97).

Such misgivings were also made explicit in a 1389 memorandum issued to anti-Pope VII by Bishop Pierre d'Arcis, who, like his predecessor Bishop Henri de Poitiers of Troyes, denounced the shroud as "a work of human skill and not miraculously wrought or bestowed"(McCrone, 1999, p. 1-2). Centuries later, in 1902, French cleric and religious historian Ulysse Chevalier, throwing "the full weight of his reputation against it,"

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[38] The McCrone Research Institute. Retrieved from: http://mcri.org/home

[39] There was good reason for such unease, as several "shrouds" of Christ have appeared in the relic peddling trade throughout the centuries (Wilson, 1998).

published several historical documents testifying to an account of the shroud's early history that cast considerable doubt on its authenticity (Meacham, 1983; Weaver, 1980, p.743).

But despite these historical efforts to resolve the ambiguities of the shroud, closure on the matter was always short-lived. The human artifice theory emerged again in the early 1980s, though this time the question of whether or not the image was an artistic rendering was translated into a scientific problem subject to the evidentiary battles of shroud scientists. What follows is an account of how the question of the shroud's material constitution was adjudicated within both the specialized forums of science and beyond its boundaries.

Contests of Evidence

As discussed in chapter two, one of the primary objectives of the STURP team's Turin inquiry was to determine the physics and chemistry of the shroud image, including the alleged blood stained areas on the cloth. In a collection of peer-reviewed scientific and technical journals, STURP scientists concluded, unequivocally, that there was no evidence of any foreign material on the cloth that might suggest that the shroud image was painted. STURP scientists found no pigment particles, dyes, stains, inorganic elements, or other forms of painting media on any part of the shroud. They further claimed to have found no evidence of brush strokes, and no reasonable evidence of any mechanisms by which the image could have been artistically rendered (Schwalbe & Rogers, 1982). The bloodstains, they argued, were "likely" composed of primate blood, an assertion later strengthened by STURP chemist John Heller's "positive presumptive

evidence" of human blood (Heller & Adler, 1980, p. 2744).

These findings proved nothing, of course, about the provenance of the shroud or the identity of the man in the image, but they were consequential nonetheless. Taken as a whole, the STURP team's material evidence appeared to settle, quite persuasively, the long-standing human artifice theory. By implication, the team's evidence opened the way for renewed contemplation about the shroud's possible origins, and thus shaped new paths for scientific, historical, archeological and even theological inquiry.

As I argued in chapter two, STURP scientists' intercessions on the shroud effectively positioned them as the authoritative custodians of the prevailing "shroud science" paradigm. I apply Thomas Kuhn's concept quite literally here. The STURP team's collective body of experimental methods, instruments, techniques, and empirical outcomes established a "shroud science paradigm" by necessity, insofar as no comprehensive methodological or theoretical models preceded the STURP inquiry. That the STURP team's work was paradigmatic (and by implication *authoritative)* is implied in much of their published work. In the conclusion of a major report published in *Archeology Chemistry*, for example, STURP scientists stated:

> In the end, the question of the authenticity of the Shroud as the burial cloth of Jesus of Nazareth remains open-ended. We should all keep in mind that science is really not in a position to ever prove categorically that the Shroud is authentic (i.e., Jesus' burial cloth). We have, however, examined the probability that the Shroud was artistically produced and find it improbable. The question is left so that those who wish to believe it authentic are not hindered with scientific objection to doing so. *However, without proof of authenticity, those who choose to believe the Shroud is not authentic are also free to do so without scientific objection, provided they do not assert a production mechanism that is excluded by the information now available.* (Jumper et al., 1984, p. 474, emphasis mine)

But while the STURP team's findings certainly appeared to resolve the human artifice theory, their claims simultaneously had implications for inviting scrutiny and criticism. The "blood dispute" was ignited when Dr. Walter McCrone, a particles expert and forensic microbiologist, produced evidence that directly contradicted the STURP team's findings. McCrone was a renowned forensics expert who had taken part in the nascent stages of the STURP team's planning for the Turin inquiry. Having gained considerable recognition in the scientific community for his controversial debunking of the Vineland Map of America, McCrone's expertise was held in high esteem by STURP members. [40]

While McCrone did not take part in the on-site Turin inquiry itself, he was asked by the STURP team to conduct a post-Turin forensic analysis of a large sample of micro-fibers extracted from the surface of the cloth. Following the Turin expedition, McCrone was given tape samples of thousands of linen micro-fibers corresponding to 32 different test point areas of the shroud (fourteen non-image areas, twelve body image areas, and six "blood" image areas). His task was to determine the source and substance of the various colorations on the fibers. Using a simple technique called polarized light microscopy, McCrone found that all of the image components of the shroud were largely composed of iron-oxide paint (specifically red ochre and vermilion tempera). The shroud image appeared to have been painted with red ochre, and the blood areas were enhanced with vermilion (a synthetic form of mercuric sulfide). McCrone attributed the sepia coloring of the shroud to the gradual yellowing of these paint media (McCrone & Skirius,

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[40] The Vineland Map is a 15th century artifact once believed to be a pre-Columbian map of the Norse exploration of America. In 1972, Dr. McCrone conducted a forensic analysis of the map's ink and determined it to be a type of ink used only after the 1920's. His finding caused a considerable upheaval among the Yale researchers who had discovered the map (Tribbe 2006).

1980; 1999). Noting that these paint media were common in Europe in the 14th century, McCrone concluded that the image was created by an artist well versed in crucifixion art. The artist, he theorized, had used an Anglo-German painting technique popular in the 14th century to create a "grisaille," or a faint monochrome image (McCrone, 1999).

McCrone also employed multiple forensic tests to determine the presence of blood. Contrary to the STURP team's findings, he found no trace of blood on any area of the shroud (McCrone & Skirius, 1980; McCrone 1990; 1999). Most compelling, of course, was that McCrone's proposed 14th century date for the cloth correlated directly to the time of the shroud's first documented appearance in history (see Appendix A). His evidence suggested, therefore, that any outstanding questions about the shroud's origins were now categorically resolved, as were any questions concerning the technologies and techniques by which the image was rendered. The image was simply a painting, albeit a clever and enchanting one.

Much like the STURP team's visual representations of the shroud, McCrone's rival claims were encoded in, and saliently expressed, by several photographic images of the shroud fibers he'd examined. Just as STURP scientists' visual images brought the artifact "into being" so too did McCrone "evoke" an artifact, though one that disclosed a decidedly different truth. His images included intensively magnified deposits of particles on the cloth that revealed nothing resembling blood but only traces of red ochre and vermillion pigment. To reinforce this visual evidence, McCrone included comparative images of modern red ochre pigment which, when viewed alongside the particle deposits on the shroud, revealed no discernable difference between the two.

McCrone also provided comparative magnified images of linen fibers from the shroud and two "real-blood-coated" fibers to draw attention to a distinct contrast between the images. Accordingly, McCrone challenged the potency of the STURP team's visual representations by laying claim to an improved (and by implication *truthful*) version of the artifact. He was decidedly unimpressed by the fact that the STURP team's findings of blood deposits were procured by two members who specialized in blood chemistry. In a personal discussion with shroud historian Ian Wilson, McCrone made an unequivocal judgment of the STURP team's collective evidence: "all this is fiction – there is no blood on the shroud image" (Wilson, 1998, p. 89).

In 1980, McCrone and a colleague published their findings in a journal called *The Microscope:*

> Our work now supports the two Bishops and it seems reasonable that the image, now visible, was painted on the cloth shortly before the first exhibition, or about 1355. Only a carbon dating test can now resolve the question of authenticity of the "Shroud" of Turin. A date significantly later than the first century would be conclusive evidence that the "Shroud" is not genuine. A date placing the linen cloth in the first century, though not conclusive in proving the cloth to be the Shroud of Christ, would, no doubt, be so accepted by nearly everyone. Our work would then indicate later embellishment of an earlier image or, much less likely, that an artist was able to obtain a 14x3+ foot linen cloth dating from the first century. That an artist either enhanced an earlier image or created the entire image is inescapable. (McCrone & Skirius,1980 in McCrone, 1999, p. 138)

It should not escape our attention that McCrone's reference to the shroud in *The Microscope* is enclosed in quotations. If we take seriously the mechanisms of persuasion in scientific communication (however understated), McCrone's qualifier implies two things. First, it underscored his rival position- the artifact was simply an archeological object that could not be attributed, with any seriousness, to a more exalted provenance.

Second, McCrone's reference to the shroud as the "shroud" leaves us, interestingly, with two contending objects inscribed with two decidedly contrasting scientific narratives. Thus the blood conflict sprang in part from an implicit "politics" of representation.

Before turning to the controversy that followed, it is worth taking brief account of how the rival claims about the shroud were framed and disseminated in the media discourse. In a 1980 article titled "New Data Suggest the Image on the Shroud is Genuine," *The New York Times* reported that STURP scientists "unanimously reject the recently publicized allegation that the image was somehow painted on" because the image was "too thoroughly inexplicable to be a deliberate fake" (Monagan, 1980, p. C3). A 1980 *Globe and Mail* story reported that although the Church "has avoided taking a formal stand on authenticity of the shroud" (this was "the task of historians and scientists"), the Vatican was "quick to challenge Mr. McCrone's contention," denouncing it as "oddly curious, unreliable and far- fetched" (A U.S. Scientists Says,1980, np). The article further noted that "...other members of a committee on which Mr. McCrone had served denied the scientist's claim....the shroud has been the subject of thorough scientific study recently, and a team of U.S. scientists said their preliminary findings tended to validate it" (np).

It is quite reasonable, of course, to assume that the media's implicit pro-authenticity slant was intended to captivate the public's imagination, particularly in light of the unorthodox subject matter. In his account of media framing of maverick science, James Dearing (1995) argues that the general public "often combine[s] beliefs in science and in superstition, with little apparent contradiction. Thus for members of the public to

believe in a maverick and highly improbable theory is a relatively simple decision as long as the maverick is perceived to be credible" (Dearing, 1995, p. 344). To be sure, the public imagination would have certainly been piqued by a story that leaned, even implicitly, towards the possibility that scientists might have the capacity to endorse the divine provenance of a Christian relic. Indeed, even *Science* magazine tinkered with this idea. In an article entitled "The Mystery of the Shroud Challenges 20th Century Science," STURP member Ray Rogers, apparently caught in a "philosophical frame of mind," was quoted as saying, "'What better way, if you were a deity, of regenerating faith in a skeptical age, than to leave evidence 2000 years ago that could be defined only by the technology available in that technical age?'" (Culliton, 1978, p. 239).

It is important to take into consideration the extent to which media accounts were bound up, at that stage, in the efficacy of STURP scientists' authority as purveyors of compelling evidence about the artifact. This is not only because science is generally granted considerable cultural authority, but in large part because the STURP inquiry itself received significant public exposure. As noted in chapter two, the Turin inquiry was granted substantial coverage in two prominent magazines - *Harper*'s and *National Geographic* - and was the topic of two BBC documentaries between 1978 and 1982.[41]

These media, I suggest, afforded a scrutiny and understanding of the STURP inquiry that calls to mind, in an interesting way, the practice of "virtual witnessing," Shapin's conceptual notion of how literary and visual technologies offer the viewer (in

[41] In 1978 the BBC released *The Silent Witness,* which featured many of the graphic images produced by the STURP inquiry. In 1982 the BBC released "*The Shroud of Jesus: Fact or Fake*" which depicted re-enactments of STURP scientists' lab experiments.

this case, the public) the "vicarious experience" of being present at the site of discovery or experimentation, and therefore witness to the evocation of a scientific fact (Cunningham, 2001, Shapin, 1984; Shapin & Schaffer, 1985).[42] Media depictions function as discursive mechanisms or "visual sources" by which such an effect is provided. Pictorial representations of shroud science in action, accompanied by narrative embellishments, provide a rather vivid and lively experience of both seeing shroud science unfold and granting a certainty and authority to the experimental practices being carried out before the viewer's eye.

In its original usage, virtual witnessing refers to 17th century literary technologies, specifically scientific text and images, that were crafted in order to confer agreement among a multiplicity of readers (Shapin & Schaffer, 1985). I suggest that the concept finds some relevance to the contemporary resources deployed for making visible and credible to laypersons the "experimental scene" of shroud science. As noted in chapter two, the lengthy pictorial account of the STURP inquiry featured in *National Geographic* contained important elements of the witnessing practice: science is drawn out of private space of the laboratory and brought into "public view," where experiments are performed in the "social space." Accordingly, two things are accomplished: the distant witness can observe "experimentally produced phenomena," and the testimony of credible witnesses present at the scene are able to attest to the fidelity of the experimental work being

[42] In his analysis of the BBC documentary "The Shroud of Jesus," Harry Collins (1987) makes a somewhat similar observation, arguing that the re-enactments carried out by shroud scientists served to inspire a public understanding of the scientific apparatus as "a certainty-rendering machine." The myriad "contingencies and failures" of experimental work are well concealed by the performance of STURP scientists' experiments (Collins,1987, p.701).

carried out before their eyes (Shapin, 1984, p. 488).[43] Virtual witnessing is therefore a technique both deployed by media and engaged in by the public. As such, we can't overlook the possibility that such techniques played an important role not only in how conflicting evidence was publicly framed and consumed, but whose evidence appeared to bear a greater authority and credibility.

Let's return, however, to the issue at hand. Walter McCrone's dissenting position (and *disposition*) was unquestionably consequential to the flow of the scientific narrative of the shroud that was mounting up to that point. In other words, the STURP team's collective evidence had the effect of bringing an "irrefutable" comprehension of the shroud to the broader social world (i.e., it is not necessarily an authentic relic of the Crucifixion, but it is undeniably something *other* than a painting). STURP scientists resolved some of the shroud's enigmas, but concurrently introduced new "unknowns". As such, STURP scientists opened the way for new speculation concerning the artifact's provenance, and a scientific "narrative" of the artifact was unfolding. McCrone's conflicting evidentiary claims, much like the cynical objections raised by his historical predecessor Bishop D'Arcis, disrupted the narrative of the shroud entirely. It is rather interesting, in fact, that in his statement of findings McCrone invites "the two Bishops" (Poitiers and D'Arcis) as "allies." Despite the "modern" concerns of the blood dispute,

[43] In the case of the National Geographic article, the key "credible witness" is undoubtedly the writer, whose somewhat affected narration of the STURP team's examination of the shroud reveals to the distant witness a breathtaking glimpse of science in action: "Perhaps never before had an object of art or archaeology been subjected to such exhaustive examination. The scientists bombarded the relic with ultraviolet radiation and X rays and watched for fluorescence. They measured variations in the way the image, the 'blood', and the background emitted or reflected energy across a wide range of the electromagnetic spectrum...elated but exhausted, the team finished its work on Friday night. The shroud was rolled in red silk, replaced in its silver bound reliquary, and returned to the ornate chapel where it has rested for centuries"(Weaver, 1980: p. 749).

historical actors are seemingly strategically mobilized to weigh in on the issue. Rarely do we see disputants in contemporary socio-scientific controversies invoking the "ghosts of disputants past" as reliable spokespersons and collaborators in the debates.

When the veracity of a scientific fact becomes the source of skepticism and controversy among scientists - when the dissenter dares to doubt the authority implied by the claim – disputants mobilize an array of resources from which a purified claim emerges (Latour, 1987, p.79). Indeed, Latour has argued that the construction and stabilization of a scientific claim is often best understood through the *conflict* engendered by it. We are urged, therefore, to look to the circuitous and laborious exertions of both defenders and dissenters to either defend or dismantle a claim.

The ensuing conflict, as it was expressed in the specialized discourse among shroud scientists, began with the STURP team critiquing McCrone's experimental methods and emphasizing the faulty reasoning derived by them. To make their objections more plausible, STURP members compared McCrone's findings to the integrity and soundness of their own diverse methodologies, the efficacy of their instrumentation, and the strength of their collective multi-disciplinary expertise in providing a decisive judgment of the shroud's material composition. In a 1982 article published in *Analytical Chimica Acta,* Schwalbe and Rogers refuted McCrone's conclusions on the grounds that they proceeded from an inherently flawed methodology:

> McCrone and Skirius postulated in their work that iron oxide contributes to the coloration of the image, *but they gave no quantitative estimates* either for the number densities of particles adhering to the fibrils or for macroscopic areal weight concentrations of the material... McCrone's opposite conclusion...is not well supported because his positive amido-black tests were observed in "blood" areas only and *cannot be taken as evidence for the presence of a protein-based*

> *tempera vehicle for the image areas generally.* In our view, McCrone's inference has two problems: first, no protein has been observed by any method in pure-image areas as distinguished from "blood" areas; second, *a more selective test must be employed, because amido black stains cellulose easily, making false positive results probable.* (Schwalbe and Rogers, 1982, p. 13, 17-18, emphasis mine)

STURP members also called attention to the judiciousness of their interpretations of the shroud image in contrast to McCrone's apparent lapse in analytical circumspection. In a comprehensive article published in *Archeological Chemistry*, STURP scientists noted that their findings and proposed theories of image formation were presented with "full recognition of [their] drawbacks," while McCrone's theories were, "not so cautious" (Jumper et al., 1984, p. 448). They argued that McCrone's conclusion that the shroud was simply a "clever painting" was premised on a decidedly sparse and unreliable set of data, and was therefore implausible and rather impetuous. They further charged that McCrone's theories concerning the artistic mechanism by which the shroud image was created were purely speculative, ahistorical, and by implication insufficient as a scientific explanation:

> McCrone argues that shading an image with distance is the natural way for an artist to place a body image on cloth, since locations where the body comes closer to the cloth would be locations where one would expect the shading to be more intense. We do not question whether a medieval artist has the intelligence to think in such terms, but we do question whether this might be *an unwarranted extrapolation from a twentieth century mode of thought to a medieval way of thinking*... Based on the above discussions, we think that there are significant technical and historical difficulties with the hypothesis that the 3-D Shroud image was the work of an artist. *For these reasons, we are skeptical that the artist hypothesis is an adequate explanation for the shading structure of the Shroud image.* (Jackson et., al ,1984: 2253, emphasis mine)

STURP scientists also argued that their methodological exactitude, the visual evidence

provided by their experiments and instruments, and the chemical results of their experiments were more than sufficient to render McCrone's position indefensible. They reiterated their position in the conclusion of their article in *Archeological Chemistry* by reasserting the strength of their assembly of evidence to defeat McCrone's findings:

> [The blood images] are clearly made of blood, likely human; however, we cannot state absolutely that the blood is of human origin. It is in these blood areas that we... find proteins present of the types associated with blood serum. Finally, there is nothing unaccounted for in the blood areas that would lead one to suspect that anything but blood formed the blood images *(12)*. We therefore do not agree that there has been an attempt to artistically enhance a "preexisting" blood image *(13-15);* nor do we feel that these are "painted" blood images...let us take one last look at the most widely publicized explanation for the Shroud images, the painting hypothesis. This hypothesis, detailed in [McCrone & Skirius] proposes that both the body images and blood images are composed of the same substance, a gelatin-based paint whose pigment consists of iron oxides and mercuric sulfide (iron earths and vermilion). It is obvious that the chemistry presented in this chapter is inconsistent with this explanation. (Jumper et al., 1984, p. 467)

McCrone responded by arguing that STURP scientists lacked the appropriate forensic expertise to even *comprehend* the problem of the shroud, let alone extrapolate on questions of its origins. In McCrone's view, the STURP team's multi-disciplinary expertise and the collection of evidence procured by it produced nothing other than non-equivalent data and "grossly disparate conclusions" (McCrone, 1990, p.83). As he noted, "I can accept that I am outnumbered, but I refuse to believe I am outgunned" (Meacham et al., 1983, p. 298). McCrone charged, therefore, that whatever the STURP team's visual evidence purported to reveal about the shroud's true properties was largely immaterial. STURP scientists were not qualified to see what he himself, as a microscopic and forensic expert, was trained to see. McCrone suggested, therefore, that STURP scientists'

representations were circumscribed by a distorted gaze and consequently disclosed a spurious visual depiction of the shroud. As he stated:

> Others who have looked at the fibers from the sticky tapes *do not see the paint pigment obviously present there. They prefer to believe the image is blood, yet my work shows no blood - only red inorganic pigments, iron earths and vermilion....* no one has spent anything like the time and effort I have put into study of these 32 tapes with their thousands of "Shroud" linen fibers and millions of pigment particles. *No one in STURP has this specialized background in small-particle identification, trace analysis, or the study of paintings by microanalytical methods...* I would very much like to see my work evaluated and repeated by one of my peers in this highly specialized field... I have confidence that the eventual carbon-dating will clear the record and show the "Shroud" to be of medieval origin. (Meacham, et al., 1983, p. 298)

In many ways the blood dispute adhered, at least in its initial stages, to the conventions of scientific debate and disputation as it typically transpires in "internal" forums. That is, the tone of the initial disagreement between STURP members and McCrone reflected fairly conventional modes of agonistic disputation, where contenders engage in a moderately civil exchange and wage their epistemic differences in what Pinch and Collins (1984) call "cool blood" (p. 522).

The discursive techniques deployed by STURP scientists were clearly organized around emphasizing the extent to which their collective expertise evoked a reliable body of knowledge, while McCrone's methods simply elicited flawed evidence. When scientific dissent involves diametrically contrasting evidence claims, the conventional line of attack among disputants often entails undermining the premise of disputants' fact claims and laying bare the relative deficiencies in methods, techniques, instrumentation and interpretations. Fujimura's and Chou's (1994) concept of "styles of practice" is helpful here. Drawn from Ian Hacking's concept of "styles of scientific reasoning" (i.e.,

diverse ways of "thinking, talking, arguing and showing"), styles of practice refer to "collectively produced work processes, methods and rules for constructing data and theories and for verifying theories" (p.1017.) According to Hacking, statements are true or factual in reference to the particular set of "self stabilizing techniques within a particular style of scientific reasoning" (p. 1021). In controversy, styles of practice are employed by disputants as the frame of reference for "adjudicating truth." In Fujimura's and Chou's case study of the controversy over the cause of AIDs, for example, disputants evoked contrasting styles of practice - epidemiology versus laboratory styles – to verify their causal theories of AIDs. To a lesser extent, the conflicting styles of practice – McCrone's microscopical work versus the STURP team's multiple methods - were deployed in the dispute as what Fujimura and Chou call "self-authenticating systems" for verifying the truth of the shroud (p.1017).

Constructing Antagonists: Dissent in Two Forums

The question of the shroud's material composition was not resolved, of course, by the contests of evidence waged in the professional journals. Indeed, the issue itself lost momentum in the specialized debates, and Walter McCrone and the STURP team remained in a deadlock. [44] Delborne (2005) notes that "agonistic engagement may be a sufficient account of how many scientific controversies unfold, but it only describes a

[44] In making the distinction between internal and external forums of conflict, I do not mean to imply any fixed demarcation between them. Collins and Pinch (1979) remind us that the separation between contingent and constitutive forums of scientific debate is not based on any rational, universally accepted demarcation criteria. Rather, demarcations are tenuous and socially contingent. Boundaries are, as Caroline Picart (1994) observes, "something more fluid, and even porous/partially differential, such that what appears to be in the constitutive forum could be a disguised diffusion or extension of the contingent forum" (Picart, 1994, p.9).

portion of the possible responses scientists can use when engaged in defending against impedance" (p. 522). Indeed, like many scientific conflicts, and particularly those in which non-scientific actors have a stake, the contested issue shifted to new discursive battlefields. Accordingly, new contenders (scientists and non-scientists alike) were enrolled in the debate, and new strategies and terms of the dispute were mobilized.

As I will demonstrate here, when the blood conflict was waged in other disciplinary forums, as well as in what I call the "narrative spaces," the tactics of dispute entailed the discursive construction of dissenters and pseudoscientists who imperiled a truthful account of the shroud. By implication, the terms of the conflict shifted as well. The blood dispute was no longer simply an intellectual disagreement about whose evidence attested to the shroud's material composition. It increasingly became a battle concerning the *implications* of spurious evidence in two respects. First, that fallacious scientific claims would have consequences for the shroud's scientific misinterpretation. Second, that fallacious claims signified a breach of scientific integrity itself. It is here that disputants rhetorically expressed the "stakes" of the dispute.

One of the most vitriolic assaults on McCrone took place in a 1983 article published in *Current Anthropology*. Written by archeologist William Meacham, the article purported to be an objective anthology of the scientific, archeological and historical evidence which, when taken as a whole, disproved McCrone's claim to the human artifice theory. It is important to note at the outset that despite Meacham's allegedly "disinterested" review of archeological and scientific data, his pro-authenticity predilections were unmistakable. Drawing on correlations among historical crucifixion

techniques, art history, biblical accounts of the crucifixion, the STURP team's scientific data, and an archeological analysis of the pathology of the wounds on the shroud image, Meacham argued that:

> ...applying standards of proof no more stringent than those employed in other archaeological/historical identifications, one is led, I submit, to an almost inescapable conclusion about the Shroud of Turin: it *is* the very piece of linen described in the biblical accounts as being used to enfold the body of Christ. The pattern of data revealed by the Shroud is unquestionably unique, it concurs in every detail with the record of Christ's death and burial, and it is unfakeable. The combination of premortem, postmortem, and postentombment information cannot be matched with any other known or hypothetical series of events. (Meacham et al.,1983, p. 294).

I call attention to Meacham's partisan position here because it had implications for how McCrone's evidence was interpreted and categorically dismissed. Consider two separate examples of Meacham's rhetorical endorsement of the STURP teams' data, and his devaluation of McCrone's methods:

> ...the evidence from anthropology, archaeology, and art history corroborates in a compelling manner that of medical and scientific analyses. It should now be considered well-established that the Shroud is indeed an archaeological document of crucifixion - a conclusion reached by STURP and most serious students of the Shroud since the 1930s. *Attempts to interpret it as a painting ... are untenable, derive from consideration of only a small portion of the evidence, ignore the vast array of data to the contrary, and need not be discussed further... my dismissal of McCrone's claims is more than amply justified by the battery of Commission and STURP tests*...(Meacham et al.,1983, p. 291, 308).[45]

[45] Meacham takes considerable liberties here in his interpretation of the STURP team's evidence. While STURP scientists held fast to their assertion that the shroud image could not have been painted, they did not make any official claims that the shroud was an authentic artifact of the crucifixion, as Meacham implies. In response to Meacham's article, STURP physicist John Jackson cautioned: "My general impression after reading this paper is that scientific research on the Shroud is sufficiently complete that the authenticity question can reasonably be settled in favor of the Shroud as the true burial cloth of Jesus. If others have also received that impression from this article, then it is important to point out that, although the case for the Shroud's authenticity is rather good, in my opinion, the authentication question is still open

> The evidence for blood is a point of empirical data on which the skeptics *reveal the weakness of their position and methods*...McCrone claims that his work (published in his own magazine) shows no blood. But according to the work of Heller (Professor of Life Sciences at the New England Institute), Adler (Professor of Chemistry at Western Connecticut) and Bollone (Professor of Legal Medicine at Turin University) - all published in peer-reviewed scientific journals - "there is *nothing* else on earth which could give this battery of positive criteria other than blood" (Heller, personal communication, 1982). Claims that false positives could be obtained from tempera paint are undemonstrated and incorrect. (p. 307)

We see that Meacham's linguistic techniques stressed the superior strength of evidence proffered by STURP's "battery" of tests, as well as the comparative level of expertise between STURP members and McCrone. As well, Meacham laid bare the relative legitimacy of STURP's peer-reviewed work, which he suggested sufficed to both refute McCrone's theory and apparently resolve the problem of the shroud's authenticity entirely. Implicit in Meacham's discourse was the effort to construct and expose the dissenter whose motives and claims are dubious. Meacham effectively dismissed McCrone as a "charlatan camoufle," the "disguised charlatan" who lacks the qualifications or resources to judge or interpret an opponent's work (Fujimura & Chou, 1994, p.1019).

Meacham also appealed to his audience to consider the serious negligence in McCrone's methods and the implications of this for his untenable conclusions. In a further rhetorical strategy Meacham seems to marvel at McCrone's obstinacy in the face of the STURP team's forceful evidence, implying that McCrone's scientific evidence is not only a threat to the reliable depiction of the shroud, but ultimately destructive to McCrone's own standing as a scientist:

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and I therefore would not want to discourage further research on the Shroud (Jackson, in Meacham, 1984, p. 297).

> McCrone's claims have been convincingly refuted in several STURP technical reports (Pellicori and Evans 1980:42; Pellicori 1980:1918; Heller and Adler 1981:91-94; Schwalbe and Rogers 1982:11-24). The results of previous work by the Italian commission also run totally counter to those claims (Filogamo and Zina 1976:35-37; Brandone and Borroni 1978:205-14; Frei 1982:5). *Undaunted, McCrone (personal communication, 1982) continues to stake his reputation on the interpretation of the Shroud image as "an easel painting . . .* as a very dilute water color in a tempera medium." (Meacham et al.,1983, p. 289)

Meacham's characterization of McCrone as the doubter and dissenter in the midst was expressed in terms of an implicit language of *risk* to the normative conventions, prescriptions and ethics of science. That is, the subtext of Meacham's attacks suggested that to refute STURP's judiciously assembled paradigm was not only untenable, but a breach of scientific integrity. Moreover, McCrone was depicted as a threat to the principled scientific interpretation of the shroud. In a response piece that followed Meacham's article both Meacham and STURP member John Jackson expressed this position in a decidedly moralizing tone. Meacham observes:

> [McCrone] declined at least two invitations to discuss his findings in the multidisciplinary framework of STURP; he has declined invitations to present his work at scientific congresses. He did not follow the STURP "covenant," which he signed, to publish in peer-reviewed scientific literature. And, as he admits, he has not responded in print to the arguments of Heller and Adler, Pellicori, Riggi, and Schwalbe and Rogers on the physics and chemistry of the image. (Meacham et al., 1983, p. 308)

John Jackson notes:

> Science is a well-defined pattern of human thought, and, especially in the case of the Shroud, where bias can so easily enter in, adherence to sound reasoning and analysis is essential. In essence, science is the process of discarding hypotheses when there are observational data with which they are inconsistent. *All too often, however, investigators unknowingly do just the opposite; they start with an observation (i.e., iron oxide on the Shroud) and "deduce" the hypothesis (i.e., the image is due to an iron-oxide pigment) and call this science. I have become*

> *sensitive to such abuses of the scientific method.* The careful scientist will rather start with various hypotheses to explain some observation (e.g., the image is an iron-oxide pigment or iron oxide is due to translocated aged blood fragments, etc.) and look for independent observations (e.g., body image not the color of iron oxide, density of iron oxide not sufficient to account for body image, etc.) that will discriminate between hypotheses. *Thus, the scientific method whittles away unacceptable hypotheses, leaving in the end, ideally, the correct explanation for the Shroud image.* (Meacham et al.,1983, p. 297)

Walter McCrone, indeed "undaunted" by his detractors, responded to Meacham's charges as follows:

> I appreciate Meacham's attempt to provide a balanced approach to the discussion of the Turin "Shroud." He succeeds well in all areas except one - inclusion of the only direct physical data on the image particles from the "Shroud." Nowhere does he present or discuss my work: what I did, how I did it, or why he considers my conclusions invalid. He does not even reference one of my detailed papers ...I feel like Hughes Mearns's "little man who wasn't there": "He wasn't there again today/Oh! How I wish he'd go away." I will not try to explain why my work is ignored, but I will say the painting hypothesis is not untenable. It is based on sound microscopical examinations carefully done by an experienced microscopist who has studied many dozens of paintings over many years by the same procedures. Whatever reputation I may have rests confidently on the conclusion that the "Shroud" is entirely an artist's work...(Meacham et al.,1983, p.298)

McCrone also made several retaliatory statements against the STURP team in later publications, noting that his findings would serve, if nothing else, than to "reemphasize the importance of scientific objectivity" against the "disparity of opinions and the one-sidedness of those opinions" among his detractors (McCrone, 1990, p.77).

A heated debate among several allies for both STURP and McCrone followed Meacham's article. The debate centered far less on the question of authenticity than on the tenability and implications of Meacham's conclusions. The skeptics, who largely supported McCrone, denounced Meacham as a "man on a crusade," and dismissed his anthology as little more than religious apologia replete with "subjective" "puzzling,"

"gullible," "whimsical," and "one-sided" inferences. As one contributor argued:

> Meacham attempts to prove a religious claim scientifically. He fails to do so. High-tech shroudology, like "scientific creationism," ultimately fails to resolve an intrinsically religious question via science. A massive investigation could be mounted with the goal of proving the Shroud fraudulent, but who would finance it? And what difference would it make? Only confirmation of the Shroud is newsworthy, yet only disproof is scientifically possible. Religion and science can be in conflict, but they need not be so on the practical level. Meacham violates the "sleeping-dog" rule - stirring up an issue which redounds to the detriment of his religious viewpoint when examined in detail. A religious person should ask, Why denigrate religion by subjecting it to materialistic tests? A scientist should ask, Why should a religious claim be granted the boon of hopeful suspension of disbelief rather than skepticism? (Meacham et al., 1983, p. 296)

Supporters for McCrone further argued that his findings represented the only truly convincing body of material evidence in the midst of what appeared to be a mounting pro-authenticity fervor in the shroud science culture. [46] As one contributor wrote:

> Walter McCrone, probably the best-known forensic microbiologist in the world, identified vermilion and hematite pigments in the "bloodstains" on the cloth - pigments which would seem to prove the image fraudulent (McCrone 1982, Mueller 1982). Meacham dismisses McCrone. STURP also dismissed him when he came to conclusions contrary to their hopes ("I was completely ignored" [Angier 1982:60]). Contrary to Meacham, STURP publications *do not* prove McCrone's arguments wrong. (Meacham et al.,1983, p.296)

The STURP team was accused of lacking scientific objectivity at the outset, and detractors further charged that STURP's alliance with "religious devotees" of the shroud (in particular the Holy Shroud Guild) rendered their claims scientifically meaningless and decidedly negligent.

Meacham did, of course, have his share of supporters. Many contributors commended Meacham's overview of the evidence, calling it "balanced and well

[46] McCrone's defenders in this debate included scientists, anthropologists, and journalists.

rounded," "thoughtful," "scientific" and "rational." Unfazed by his detractors, Meacham dismissed their objections as little more than rhetorical mud-slinging:

> Beyond misunderstanding lies invective, and the comments of Cole, Nickell, Schafersman, and Mueller are phrased in an emotive tone not conducive to reasoned discussion... I leave it to the reader to decide whether this is the type of rhetoric usually associated with a carefully reasoned argument, not to mention a "powerful case." (Meacham et al.,1983, p. 306)

Narrative Forums

In addition to migrating to other disciplinary forums, the blood dispute found its way into several books published by both STURP advocates and their adversaries. For my purposes I have limited my analytical focus to three books authored by shroud scientists themselves. Here I consider the implications of the dispute's shift from an internal, localized disagreement to one waged in the public arena, expressed in what I call the "narrative forum", where disputants drew on literary conventions to shape the story of the conflict and their place in it. It was here that disputants articulated for readers the *role* and *responsibility* of science in matters of religious consequence. To this end, disputants engaged explicitly in strategies of "enrollment," such that readers were encouraged to understand a particular "truth" of the blood conflict and its stakes for science. It is here as well where strategic "constructions" of antagonists served as a means of persuading the public of the implications of the blood dispute for the integrity of shroud science.

Heller's Voyage

In 1983 STURP biophysicist and medical doctor John Heller published *Report on the Shroud of Turin*, a book in which he chronicled his experiences with the STURP

inquiry and the post-Turin conflicts that ensued between the STURP team and Walter McCrone. Heller's is a story of a group of skeptical yet deeply inquisitive scientists who embarked on an unorthodox "journey" - one that repeatedly confounded their rationalist sensibilities. In a chapter entitled "The Physics of Miracles?" Heller began the story by reflecting:

> By profession I am a scientist; specifically a biophysicist. By genesis, I am a New Englander, with all the skepticism and conservatism of the breed. All this being the case, I have always felt that relics are nothing but flummery from the Dark Ages...[the shroud] was different... many of us were quite confident of our technical adequacy. Some may have even been cocky. But none of us survived this extraordinary voyage into the unknown without becoming more humble and more aware of the dimensions of our ignorance. Scientific hubris may have been our mutual sin at the outset. Now we have learned better. (Heller, 1983, p.1, 38)

A later passage reads:

> Science undertook its specialty, which is measurement. We were supremely confident that the answers would – indeed must – be forthcoming. Many team members were ordered or threatened to desist from the project, yet they persevered. Though it was believed that there would be a confrontation between science and religion, none occurred. Rather, the relationship was harmonious and synergistic. All of us have been changed by the project. I believe we have grown. (Heller, 1983, p. 221)

We might construe Heller's reflection as inflated rhetoric employed to intrigue his readership, but this would perhaps elide a deeper understanding of Heller's communicative choices here, as well as what is implied by them. Dorothy Nelkin (2004) has argued that religious imagery –what she has termed "God talk"–is common in scientists' descriptions of their experiments, practices and discoveries. Importantly, scientific discourse couched in religious and mystical metaphors function to amplify the importance of scientific work by elevating it to the level of spiritual consequence. While

certainly hyperbole in scientific communication reflects, in part, an enthusiasm about the capacities of science, it is not mere rhetoric. Instead, such linguistic techniques evoke a sense of the sacrosanct nature of scientific work. As Nelkin writes:

> Many scientists believe they are engaged in a profoundly religious pursuit, investing their research with spiritual meaning...Physicists have been especially inclined to associate their work with cosmic principles. Albert Einstein called science a 'cosmic religious feeling'... Biologists, until recently, were less inclined to deal with cosmic questions and, concerned about the persistent sensitivity to Darwinism, even defensive about the spiritual implications of their work (Brown 1947). Today, however, many geneticists in the United States are attracted to religious metaphors. They call the genome the Bible, the book of life, or the Holy Grail. DNA is not just a biological entity in the rhetoric of science; it is a so-called sacred text, the core of essential humanity, or the master code... Science to some observers is increasingly treading on theological turf. The theories of genetics and evolutionary biology are touching on cosmological and essentialist questions—about the origin of life, the nature of destiny, the prediction of good and evil, the perfectibility of human beings. (Nelkin, 2004, p. 139-140)

David Noble (1999) has similarly argued that spiritual yearning and the pursuit of transcendence underlie the manifestly rational, utilitarian goals and language of modern technologists and scientists. Thus the domains of science and religion, long presumed to be divided by the secularist shifts of the Enlightenment, have always been, and continue to be intimately intersected and driven towards "divine" aspirations. Consequently, the language and practices of modern science and technology are suffused with religious preoccupations and themes (Noble, 1999).

Heller's use of religious metaphors implies, to some extent, a similar spiritual profundity underlying the STURP team's encounters with the shroud. Certainly his language evokes some compelling imagery: science confronts the enigmatic religious object, science persists in its pursuit of its latent truths, scientists are "chastened" by the

challenges of the experience itself. But perhaps more importantly, Heller set a tone in his narrative that bears on the way the blood conflict was to be understood by the audience. The reader was invited to consider the profound and difficult responsibility of shroud science. In this backdrop, the conflict over the shroud's material composition took on a deeper importance, and the need to expose and defend against detractors was critical.

Heller employed a number of strategies to enroll the reader in the blood dispute, and to provide the reader with an understanding of how Walter McCrone represented such a profound risk to the enterprise of shroud science. In a chapter entitled "Necessary and Sufficient Proof," Heller emphasized McCrone's recurrent abuse of the fundamental epistemic and ethical values that inhere in scientific practice, and the implications of this for his misinterpretation of the shroud's material composition. To make his characterization of McCrone plausible, he presented the reader with an explanation of the apposite procedures and ethos of science and its experimental practices, and thus laid bare the fundamental difference between McCrone's conjecture and the STURP team's sound evidence:

> ...science demands particular criteria for proof of anything. One criterion is embodied in the words "necessary and sufficient". In order to establish that something is a painted object, it is necessary to show colorants. However, this is not sufficient...it was McCrone's claim that the images were made of red ochre – also an artists' pigment – and gelatin. We could find neither. It was also his contention that the blood was not blood, but red iron oxide. We found blood – lots of it – by many different methods in the blood areas. ..it was now 1981, three years after the team's trip to Turin. McCrone sent me a xerograph of his third paper. In it he stated that the blood was paint, a mixture of iron oxide and mercury sulfide. He claimed that he had found nine microscopic particles of vermillion. Necessary? Yes. Sufficient? No. There was not enough iron oxide or vermillion to account for one painted drop of blood, let alone all the gore on the Shroud...McCrone had also mentioned that he had seen "orpiment, ultramarine,

> azurite, wood, charcoal, and madder rose." He did not cite any experimental evidence other than that he saw them. We examined every particle type we could find and tested it chemically, and could not corroborate any of his observations. (Heller, 1983, p. 194)

The appeal to the logical criteria for establishing scientific proof is clearly an effective means of persuasion for lay readers. Indeed, science finds its authority in articulating for the lay public the "canons" of scientific method and its rigid empirical "commandments". Heller's delineation of "what science demands" of the scientist also set the stage for assaulting Walter McCrone's professional legitimacy and his commitment to the ethos of science.

Just as McCrone's competence, credibility and ethics fell under scrutiny in debates in the professional journals, so too were they held up to reproach in Heller's account.[47] Throughout the book, Heller constructed an image of McCrone as an inherently negligent practitioner driven by duplicitous motives. He described, for example, McCrone's extreme carelessness in his handling of the tape samples of the shroud fibers, claiming that McCrone destroyed some of the valuable fibers and ultimately contaminated the physical evidence. McCrone's alleged mishandling of the tape samples would be the enduring sticking point in the blood dispute. In a posthumously published monograph STURP member Ray Rogers (2008) framed McCrone's handling of samples as an egregious assault against the STURP team's

[47] Indeed, at the very outset Heller held McCrone responsible for nearly dismantling the STURP inquiry before it even began. When the STURP team was developing its research protocols, McCrone had apparently proposed to apply radiocarbon dating to the shroud, insisting that this was the only reliable test for determining its provenance. When Shroud custodians denied the use of any experiments that could potentially damage the cloth, McCrone usurped their authority by appealing directly to King Umberto, the last surviving owner of the shroud. McCrone's politically imprudent gesture apparently incensed the shroud custodians and, according to Heller and other STURP scientists, nearly pulled down the entire expedition.

meticulous preservation of their data. Rogers notes that by destroying the carefully preserved fibers and introducing contaminants to the adhesive surface of the sample tapes McCrone committed an "unconscionable" and "abusive" breach in protocol (Rogers, 2008, p.24).

McCrone was thus typified as a risk to the principled interests of shroud scientists. In a passage recounting a post-Turin meeting where McCrone first presented his opposing findings, Heller described an exchange between McCrone and several STURP members. Here the reader is drawn in as a witness to an interrogation-like exchange where STURP scientists' rigor is contrasted with McCrone's apparent apathy:

> "Dr. McCrone, how do you know those red dots are iron oxide? "Experience." Did you test them chemically? "I don't have to. Experience. Besides, it's birefringent." "How do you explain the X-ray fluorescence studies and Gilbert's curves." "They must be wrong." "How does your iron-oxide paint jibe with the negative image and 3-D formation?" "Oh any competent artist could have done that." "Do you mean that you just looked through your microscope and without doing specific tests for iron oxide, can proclaim it a painting?" "Yes." And with that he left the meeting. (Heller, 1983, p. 141-142)

Perhaps the most persistent rhetorical device Heller employed in his account is to underscore his complete mystification concerning McCrone's obduracy in the face of such persuasive evidence of "blood" on the shroud. Indeed, he employed decidedly affective language to describe his response to McCrone's "curious" conjectures:

> *I was bewildered.* Here was a particle expert claiming that the images were the result of iron oxide red paint and the "blood" was iron too...I could not discount the team members' findings. As scientists, they were too good and too meticulous for me to doubt them. Furthermore – and this was the most important – their results were consistent with one another in that they all seemed to rule out an artist. (Heller, 1983, p. 140, 141-142)

> McCrone had gone further with his iron oxide claims. He stated that the iron oxide was extremely finely ground, most of the particles being less than one micron in size. He then made the *curious statement* that submicron particles of iron oxide had not existed before 1800. It was at that time, he explained that the process of grinding iron oxide into submicron particles was perfected...McCrone stated that there may possibly have been a very faint preexisting images that someone touched up after 1800 with iron oxide paint to make them more visible... *I was astonished*...to suggest that someone sneaked [the shroud] out and repainted it after 1800 is to claim a gigantic collusion on the part of the royal House of savoy and the Catholic church. (Heller, 1983, p. 148-149)

Finally, Heller considered the broader implications of McCrone's refusal to see the errors in his own rival evidence and his intransigence in the debate. Heller suggested, for example, that the rivalry could have implications for distorting the public's understanding of shroud science. He noted that,"McCrone, in spite of his reputation was sticking his neck out, way out. What would the public think? They would have no way to judge a dispute among scientists. That would be a terrible devil's brew..." (p. 141). While it is not clear how the public might have been affected by the lack of consensus among shroud scientists, Heller's point was to underscore the disruption created by McCrone's contrarian position.

Skeptical Antagonism

In 1990 STURP engineer Ken Stevenson and professor of philosophy Gary Habermas published *The Shroud and the Controversy: Science, Skepticism and the Search for Authenticity.* Here the blood dispute was framed in terms of a bitter personal and intellectual battle waged in an atmosphere of what the authors called "skeptical antagonism", whereby opponents (chief among them Walter McCrone) do nothing more

than "bristle with intemperate rhetoric" and "caustic verbiage" (Stevenson and Habermas, 1990, p. 18). In a chapter titled "Science, Skepticism and the Shroud", the authors explained to the reader that the unfounded dissenting positions of McCrone and his adherents posed serious consequences for shroud science:

> ...articles by Shroud opponents...have led to widespread confusion at best and downright misinformation at worst. Some of these articles contain some pseudo-scientific statements, poorly researched (if researched at all) conclusions, and much ad hominem argument. Most articles purporting to disprove the authenticity of the Shroud are partly based on Dr. Walter McCrone's work...(Stevenson & Habermas, 1990, p.26)

Skepticism, the authors wished to make clear, is distinct from rational opposition grounded in sound empirical evidence. They argued that most shroud opponents who claimed to have scientific expertise typically had only "a special ax to grind" in the authenticity debates (p.194). Stevenson and Habermas's objections to McCrone were primarily based in their distrust of his motives. McCrone's dissenting evidence, they argued, was never based in expert scientific opinion but was driven by his personal ambition to demystify the shroud, just as he had debunked the Vineland Maps. In doing so, McCrone misrepresented the facts. They noted:

> ...McCrone was a dissenter who insisted that he alone knew the truth about the cloth. STURP presented no hoopla, no mysterious miracle statements, no confusion – merely clearly demonstrable statements of facts reviewed by peers and experts...the fact that McCrone, like the other major Shroud opponents, spends much of his energy attacking the qualifications of STURP should be a dead giveaway that his case cannot stand on its own merits. (Stevenson & Habermas, 1990, p. 27)

The authors further suggested that McCrone's agenda in the blood dispute was simply to undermine the STURP team's evidence by exposing their alleged religious predilections.

> McCrone [has] an ax to grind. [He] seem[s] bent on demonstrating STURP to be a clique of religious zealots out to prove a point...warning bells should be sounding at this point. The issue at hand is not Christianity nor the historical existence of Jesus Christ. The issue is not the particular religious bent of the individual scientists who happened to be among the privileged few selected to study this enigmatic cloth ...nor is the issue some supposed agenda to change the prevailing scientific view of the cosmos. The issue is what the scientific evidence does say concerning the possible authenticity of the cloth...certainly the need to resort to a denigration of the scientists on the basis of their religious preferences shows a decided bias on their part. (Stevenson & Habermas, 1990, p. 27, 196)

Thus the authors constructed McCrone as a self-serving adversary committed not to rigorous scientific research, but to asserting his contrarian findings solely for the sake of positioning himself in the conflict as the iconoclast.[48] Like Heller's account of McCrone, Stevenson and Habermas accused McCrone of basing his rival theories on inadequate methodology and questionable evidence. They noted:

> Perhaps most devastating to McCrone's case is McCrone himself...his identification of the chemicals he claimed were painted on the shroud was based strictly on viewing characteristics, which he believed only he had the expertise to determine. In other words, he thought the image "looked like" a painting. Certainly it can be argued that to make a major scientific statement on the basis of what the image "looked like" in the face of all other scientific data seems highly subjective. (Stevenson & Habermas, 1990, p. 28)

But Stevenson went further by charging that McCrone deliberately misrepresented facts to corroborate his interpretations:

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[48] A similar claim was made years later by chemist Ray Rogers who charged that McCrone had "not followed the simplest procedures of rigorous analytical chemistry. He had not run 'blanks'. He did not test his method for false positives. All he wanted to do was debunk the Shroud. A rigorous scientific study requires as many independent observational methods as possible. It is unconscionable to allow antireligious sentiments to direct science as it is to demand a specific theological answer. I was disappointed to find that Walter could not be objective when he wanted publicity" (Rogers, 2008, p.37).

> STURP has never claimed to have the only answer to the cause of the image or blood stains, choosing instead to leave the issue open for further study. This represents an excellent example of how STURP's unflinching attention to scientific credibility paid off. Nowhere does STURP succeed better than in laying to rest the theory that the shroud was the work of a human hand. Unfortunately [those who have] jumped on the McCrone bandwagon seem blissfully unaware that for purely technical reasons the painting theory, regardless of the methodology, is a dead issue. Amazingly enough they continue to flog away at the now rotting carcass of this long dead issue...McCrone implied in *Discover* that STURP declares the Shroud to be a miracle. Once again, McCrone misrepresents the facts... (Stevenson & Habermas, 1990, p. 29-31)

McCrone's Judgment Day

Walter McCrone responded to his adversaries with *Judgment Day for the Shroud of Turin*, a lengthy retrospective account of the blood dispute in which he detailed his experimental work and provided a decidedly revised history of shroud science and its myriad discontents. The opening statement of the book makes explicit the intentions behind his account of the blood conflict and its implications: "[This book] serves as an excellent example of the scientific, personal and social issues that come into play when emotions, prejudices and perceptions of science interact with classical scientific methods and ethics" (McCrone, 1999, p. xviii).

At the outset, McCrone embraced his role as the dissenter among the "dramatis personae" of the blood conflict (p. 177). He began his narrative by recounting a correspondence with Father Peter Rinaldi, the vice-president of the Holy Shroud Guild who had become a self-appointed conciliator between McCrone and the STURP team, and who warned McCrone that his opponents in the shroud science community had come to refer to him as "The Adversary". This was, McCrone tells the reader, a welcomed designation: "I've had several titles in my time, but none so distinguished as this one" (p.

198). McCrone's narrative unfolds, therefore, as the story of a lone dissident in the shroud science culture who was determined to amend the misguided and ultimately negligent claims about the shroud that were being disseminated by STURP scientists and their supporters. McCrone's narrative reveals a struggle to defend the integrity of science against the contaminations of inferior scientific practices and religious politics. Indeed, McCrone compared his struggle in the blood dispute to the famously maligned astronomer Galileo, who battled against the Church's condemnation of his heliocentric theories:

> Both situations find a scientist pitted in a serious confrontation with the Church where I am known as the "adversary". One difference, for which I am thankful, is absence of a threat of physical harm in my case...Galileo was so pressured and harassed that he was forced to recant. I am free to ignore the pressures from STURP or the Church and I certainly have not recanted...civilization and science have progressed significantly during the intervening years. The Church has progressed during those years – let us say less significantly. (McCrone, 1999, p. 173)

McCrone thus invited the reader to contemplate the blood conflict in terms of the broader political and ideological tensions between science and religion, relying at times on the dramatic potency of the conventional "warfare thesis" to buttress his position.[49] The Church, he explained, has never been known for "being a friend of science and scientists", and its influence on science has been historically oppressive (pp. 22-23). McCrone drew upon the church's suppression of scientific progress throughout history, noting that there has always been "a truth by science and a truth by religious faith" (p. 25). Truth by faith he noted, "was just as true as scientific fact and took precedence in

[49] The warfare thesis (or "warfare narrative") refers broadly to the assumption that there is an inevitable and irreconcilable antagonism between science and religion (Evans & Evans, 2008).

case of disagreement" (p. 25). The contemporary blood conflict, he suggested, is premised on the same truth wars.

McCrone's aim was clearly to construct STURP members as both scientific backsliders and pro-authenticity converts whose attacks on his rival evidence reflect only their shroud "fundamentalism". Like his adversaries, McCrone provided the reader with a detailed account of how the scientist *should* properly proceed if a truly reliable understanding of the provenance of any archeological relic was to be rendered. Invoking a distilled version of Karl Popper's falsification principle, McCrone advised that a scientific study of a disputed artifact proceeds on the assumption that the artifact is forged. Working backwards, the scientist looks for the mistakes the forger made in the artifact's construction. STURP scientists, McCrone argued, failed to undertake this conventional code of practice at the very outset, preferring instead to "[try] to find substances they think a real shroud would have – blood, sweat, aloes...they also devote a good share of their time trying to explain away the observations anyone has that do not support an authentic shroud" (p. 150).

McCrone thus characterized STURP members as pseudoscientists engaging in "mental distortions", "grasping at straws", producing evidence completely devoid of scientific reasoning, and ultimately being "lost in the trees" (p., 154, 160).[50] He thus

[50] As noted in chapter one, in her analysis of the sociobiology debates among scientists, Ullica Segerstråle (2000) calls these assumptions "politics by scientific means." That is, when either side in a dispute believes they are pursuing "good science," opponents find recourse in accusing the other side of rooting their claims in political and ideological interests (p. 10). Mulkay and Gilbert (1982) similarly argue that scientists "account for error" by attributing error to subjective influences. As such, "correct belief is treated as the normal state of affairs. It is regarded as relatively unproblematic and, on the whole, requiring no special explanation. In contrast, error is almost without exception portrayed as due to the intrusion into research of non scientific influences which have distorted scientists' understanding of natural phenomena" (p. 166).

enrolls readers to take seriously what stands at the heart of the dispute, and why, ultimately, the dispute is irreconcilable:

> The STURP scientists have one overriding disadvantage – they have absolute "faith" in an authentic Shroud. It has to be first century and it has to be the Shroud of Christ. This causes some of them, in particular, the leaders of the group, to ignore or misinterpret the data they and others determined...they bolstered their side of the argument with inappropriate data, bad data, or intentional mis-interpretations...they are obviously hampered by a lack of background and a surfeit of "faith". The variation between their conclusions and the truth concerning the Shroud image is due to incompetence, deceit, or a combination of the two. (McCrone, 1999, p.155, 169)

Faith was, to McCrone, utterly fatal to the ethical and reliable scientific interpretation of the Turin shroud, and was the key divisive force in the conflicts.[51] This was the crux of the matter for McCrone. To support his position, McCrone provided readers with definitions of faith proffered by American writer Ambrose Bierce: "[faith is] belief without evidence in what is told by one who speaks without knowledge" (p. 171). He also quoted atheist H.L. Mencken, who once declared that "faith may be defined briefly as an illogical belief in the occurrence of the improbable...the most common of all follies is to believe passionately in the palpably not true" (p. 171).[52]

It is in McCrone's version of the dispute that the reader was invited to consider his struggle in the midst of shroud politics. Here he included a series of letters exchanged with Father Peter Rinaldi of the Holy Shroud Guild, wherein McCrone attempted to defend his contrary claims against Rinaldi's objections. McCrone showed the reader that

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[51] McCrone and STURP member Ken Stevenson differed on the matter of faith and its rightful place the scientific domain. In his book, Stevenson entreated the reader to understand that for the scientists, "any unproven theory requires faith..." (Stevenson & Habermas, 1990 p. 203).

[52] Henry Louis Mencken was the caustic American journalist best known for his satirical depiction of the Scopes Trial.

while Rinaldi was attempting to act as a mediator in the blood dispute, Rinaldi's interests rested ultimately in promoting the STURP team's findings and convincing McCrone to recant his position regarding the material properties of the shroud. In one letter Rinaldi asked, "are you, Doctor McCrone, quite ready to discount the opinion of renowned art experts and respected medical scientists, and attribute this negative image to the painting technique of a medieval forger? Why are you so keen to endow this forger with miraculous powers...?" (p. 181-182). In several letters that follow, Rinaldi attempted to convince McCrone of the risks of "public confusion" over the divergent opinions of the shroud if he fails to find common ground with STURP scientists. McCrone noted, "Father Rinaldi did not know what he was asking me to do. He was asking me to fight a duel but wait for my antagonist to fire first with an automatic weapon" (p. 185).

The sense that McCrone's doubt would have grave implications for STURP's findings was indeed a persistent source of concern among STURP supporters, and particularly members of the Holy Shroud Guild. In a letter to the STURP team, Guild president Father Adam J. Otterbein warned, "it would do harm to the credibility of the Shroud, harm that would never be repaired, harm which should not be done if there is no real evidence to support the doubt that would be created".[53] In another letter to Guild vice-president Rinaldi, Otterbein composed a rather overwrought speculation of the potential consequences of McCrone's dissenting evidence:

> I can only surmise the 'agony' that you have suffered as a result of this matter, and you should be a nervous wrech [sic] after sitting on a "time bomb"... I think we all had the world-wide picture of a potential disaster. The matter is far from

[53] Source: The Holy Shroud Guild Archives, retrieved from: http://holyshroudguild.org/hsg-archives.html

> settled yet, and I do not know the ultimate resolution but STURP is trying to plan for any possible disater [sic]. At the same time they feel that they have "wasted" too much time – valuable time in "defensive" planning.[54]

Brian Martin (1999) reminds us that dissident scientists are often likened to "heretics" whose defiance of the positions and interests of the dominant group falls prey not only to attack but outright suppression in a number of forms. Certainly there were concerted efforts made by STURP members and their supporters to exert some form of *control* over the dissemination of McCrone's rival evidence. This too was revealed in a series of letters between Rinaldi and Otterbein, in which Otterbein explained that STURP members considered McCrone a "scientific problem" who needed to be carefully "handled" if any sort of compromise could be reached between them. Otterbein wrote in another letter that "it would make good Press later on, to be able to say that McCrone presented his data to STURP, and they sat down to discuss it, even though STURP later rejects his data or theory".[55]

One of the guiding questions of this chapter asks how and why contemporary conflicts were waged when the question of the shroud's authenticity became one of scientific consequence and deliberation. My starting point has been that STURP scientists' evidence claims about the shroud's physical properties, and what these findings implied for the shroud's possible origins, effectively (though perhaps unintentionally) precipitated a new era of conflict that was both distinct from, and similar to historical antagonisms over the shroud's authenticity. When the shroud became the subject of competing scientific claims, disputants drew upon the rhetorical and political

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[54] Ibid
[55] Ibid

tactics that inhere in episodes of scientific dissent. The blood dispute thus involved fierce contests of evidence, a "politics" of competing discursive representations, and contending notions of sound and ethical science.

The struggle among disputants was largely premised on competing assertions about whose science constituted the *trustworthy* envoy of truth. That is, much of the energy of the dispute centered around who was better armed, allied, positioned and ethically conscientious to lay claim to a definitive interpretation and representation of the artifact. The blood dispute was often underlined by a moralizing tone, where dissenters and "pro-shroud" scientists were characterized as risks to the normative demands of science, and a risk to the integrity of shroud science more specifically. Disputants thus constructed each other as antagonists (i.e., shroud zealots versus iconoclasts) who possessed a vested interest in defending their evidentiary claims. STURP scientists and their adherents emphasized the critical necessity of exposing the dissenter, in a manner similar perhaps, to "calling out the sinner." From their perspectives, McCrone, as the "doubter" in this conflict, posed a serious risk to the scientifically responsible account of the shroud.

For McCrone, the task was largely to delineate the dangers of pseudoscientific claims for promoting a misguided understanding of the artifact. At the end of *Judgment Day*, for example, he contemplated the consequences of a scientific study compromised by political interests and the preconceptions of scientists apparently seduced by the pro-authenticity fervor. He suggested that the conflict over the matter of blood or pigment translated into concrete implications for the public understanding of the shroud.

> The general public, faced with STURP's negative interpretation of my results and the numbers (30 STURP "scientists" against one lone microscopist), vote for an authentic Shroud and dismiss and/or denounce my conclusions. The Church...continued to defend the authenticity of the Shroud...[STURP] lacked the guts necessary to face up to leaders of the group. Any evidence found by them suggesting a painting was ignored or rejected. Some evidence supporting authenticity was manufactured because they were so certain the Shroud is authentic....I find all of this very sad and very disturbing. (McCrone, 1999, p. 321)

The underlying principles being defended in this conflict suggest not a radical break from the historical shroud conflicts, but a continuation of concerns regarding the legitimate interpretation of the artifact, the legitimate custodianship of the object, and the implications of its misrepresentation.

Truths of, and Truthfulness to, the Artifact: The Intrinsic Value of Conflict

In the debates that followed William Meacham's anthology of shroud science, one skeptic posed a simple question: "If [the shroud] is genuine, so much the better; if not, who's bothered?" (Meacham et al., 1983, p.296). A similar question might be raised here. Why was it important for scientists to establish reliable knowledge about the material attributes of a religious relic, and in doing so, compete over the legitimacy of their evidence claims? Why did the conflict over an object of relatively little scientific value endure for so long and with such vehemence? Certainly the conflict was provoked, sustained and intensified by the force of disputants' attacks on their respective ethics, integrity, motives, credibility and vested interests. The blood dispute, however, was not driven solely by these factors. Indeed, the accusations leveled by actors in the dispute were not well reconciled at times to the realities of the situation. That is, while Walter

McCrone did hold fast to his contrarian views, it is doubtful that his motives were as duplicitous or destructive as some STURP scientists and their supporters suggested. In a letter to Father Rinaldi, McCrone indicated, in fact, that he was deeply disappointed by his findings, and noted that he "would have loved to find incontrovertible evidence in favour of authenticity" (McCrone, 1999, p. 176). Similarly, while a few members of STURP did indeed adopt a "pro-authenticity" belief, few could truly be described as shroud "zealots", and certainly none are accurately described as pseudoscientists.

I propose that deeper values that are equally relevant to the question of why the dispute unfolded and endured are expressed by the blood conflict. Specifically, the conflict reveals a commitment on the part of disputants to a certain *intrinsic value* of the struggle itself.[56] That is, the blood conflict is underlined, to some extent, by a faithfulness on the part of disputants towards bringing about a truthful interpretation of the shroud to the broader cultural understanding of the artifact. This was not only a matter of science's authority to evoke this knowledge, but ultimately its *obligation* to do so. Thus, the blood conflict reflects a struggle to be *truthful about* the artifact. In other words, if we look beyond the explicit tensions around questions of evidence and political motives, disputants also betray an ardent commitment to the "thing itself." These

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[56] Consider, for example, both the explicit and implicit factors underlying the long history of epistemic and ideological disputes between scientists and creationists. Science studies scholars have shown that scientific authority is deployed against the irrational, pseudoscientific claims of creationist and their consequent "perversion" of scientific theory. In these disputes "boundary work" is enacted by scientists in the pursuit of professional and intellectual primacy over other forms of cultural rationality (Gieryn et., al, 1985; Nelkin, 1982, p. 192). But the disputes are also implicitly ignited by scientists' claims to a moral authority and *responsibility* with respect to proffering and defending a rational understanding of fundamental questions of human life. This is science's ascribed role in the social world. A similar principle is reflected in the blood conflict: science charged with delivering to the public consciousness an empirically reliable understanding of this artifact.

underlying elements of the dispute are important to understanding its deeper intricacies. To illustrate these points, I return briefly to the actors in this conflict.

Walter McCrone's struggle to defend his claims was not only a critical scientific undertaking but was, at times, a near moral enterprise. This was made rather explicit in the premise of McCrone's own narrative account of the blood dispute. In the introduction of *Judgment Day* McCrone described his story as an account of "the scientific assault on the Shroud" (p. xx). I construe this opening statement as more than simply dramatic effect. McCrone brought to the reader's attention the consequences of biased scientific interventions for misrepresenting the shroud to the broader cultural imagination. For McCrone, the responsibility of disclosing the true properties of the shroud was bound up with science's fundamental role in being accountable to a proper cultural understanding of the artifact. In the face of potentially spurious claims and misguided interpretations, science is obligated to reveal and defend the material artifact with integrity, and to do so at considerable expense. McCrone thus suggested that the endeavor to procure a truthful and scientifically reliable interpretation of the religious artifact invites the risk of vilification but simultaneously provides the inspiration to defend one's convictions. This notion is well articulated in the following passages from *Judgment Day*:

> Any scientist working in the glare of such attention feels immensely *honored but under great pressure to reach a correct final conclusion*...he should also expect (this I say more in retrospect) that any suggestion that the Shroud is not authentic, will result in bitter denunciation from millions of believers in the Shroud. He soon realizes that not too many generations ago, he might have faced "burning at the stake". Today, he is only disbelieved, dishonored, vilified or, if lucky, ignored. (McCrone, 1999, p. xxi, emphasis mine)

> I'm still trying trying trying to convince...the Turin group, the Church and the World that the "Shroud" is a 1356 fake. I suspect it should be termed a "fake"

> however, only because the world, so long, believed it to be truly Christ's shroud. (McCrone, 1999, p. 210)

The passage below is perhaps the most striking illustration of McCrone's commitment to the integrity of shroud science. We come to understand not only what McCrone felt was at stake in the shroud science enterprise, but also that the "beautiful scientific challenge" of the shroud is a problem worth resolving and defending:

> I should have "dropped out of the race." They [the church] didn't need me to give them scientific evidence of the Shroud's authenticity. Only the fact that I felt I would be able to prove scientifically what others "knew in their hearts" pushed me into the project. It was, I believed, a beautiful microanalytical problem as well as a beautiful Shroud. In short – a visible image must have atoms and molecules – they must be identifiable – this should tell what constitutes the image...the beauty of the scientific challenge impelled me. The Shroud itself was then incidental because I thought I knew the final answer...(McCrone, 1999, p.67)

Finally, McCrone concluded his account of the blood conflict with a significant cautionary pronouncement. He asked the reader to comprehend the consequences (indeed, the near moral consequences) of biased scientific fact claims about the Turin shroud. In the first passage McCrone stated that STURP's conclusions exemplify a grievous form of scientific misconduct - STURP scientists effectively contravened their moral and ethical responsibilities as scientists by disseminating pseudoscientific knowledge under the pretense of scientific rigor. In the second passage, McCrone extended the implications of STURP's flawed techniques and conclusions to the broader social world. He noted that STURP not only deceived "millions," but that their erroneous techniques *might have implications for how other scientific problems are approached and resolved*:

> ...I find all of this very sad and disturbing. That a group of individuals, some of them scientists and all hiding under that disguise used entirely inappropriate instrumentation for a such a study is "sad and disturbing." Their publication of more than 30 pseudoscientific papers in a variety of learned scientific journals is also "sad and disturbing." The basis for sadness is obvious – the use of deceit to prove a falsehood. (McCrone, 1999, p. 321)
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> Using STURP techniques and instruments, no one could be sure whether the Turin Shroud is genuine or an imitation. Yet their publications sound scientific and the results have convinced millions of people that they can believe STURP's conclusions. The disturbance I feel is not only that STURP is guilty of bad research, and in several cases deceit, but that those non-imaging techniques are used in most laboratories in the world. If such technique do such a bad job on highly visible problems like the Vineland Map, and Turin Shroud, how many important industrial and medical etc., problems are also mis-solved every day? (McCrone, 1999, p. 322)

How did STURP scientists express their obligation to render a truthful account of the shroud? What, in fact, was their scientific "truth" about the artifact? Certainly not, as McCrone suggested, that it was a genuine article of the crucifixion. Although STURP promoted and defended the veracity of their findings, nowhere in the professional literature did STURP scientists, as a collective, make any *definitive* authenticity claims. Indeed, Ken Stevenson, STURP's most ardent believer in the shroud's authenticity, cautioned in his book that STURP members had no desire for the scientific interpretation of the shroud to encourage its religious veneration. This would, he argued, ultimately misinterpret the purpose of shroud science (Stevenson & Habermas, 1990). Neither did STURP chemist John Heller ever proclaim that STURP scientists arrived at a definitive conclusion about the shroud's provenance. In *Report of the Shroud of Turin*, he noted that despite the STURP team's supreme confidence that the shroud's origins could be ascertained, they ultimately "failed" in this endeavor.

I suggest that the matter of concern for STURP, at least in this particular phase of dispute over the shroud, was not to "authenticate" the shroud (an obviously inconceivable undertaking) but to defend against the liabilities of unfounded skepticism. Claims that the shroud was simply a human rendering (and by implication a spurious relic) subverted the correct understanding of the artifact. The struggle for STURP scientists, therefore, was to ensure that the "black box" of shroud science remained open, and thus defend against the detractors who threatened this endeavor. STURP's responsibility, as scientists, to bring a truthful account of the shroud to the broader social milieu, and to defend this undertaking, is well articulated by Heller and Stevenson:

> We had – as have all scientists – an obligation to the public…in the case of Shroud research, there was suddenly a new and large constituency to whom we were responsible. It was a global one, not just a national one, and it included the devout, atheists, agnostics and those who were simply curious. It was therefore essential in my view that we do the most careful job of which we were capable, verifying everything again and again, going through the peer review process, and publishing. (Heller, 1983, p.185)

> Despite the fact that science alone can't address this major issue [of authenticity], science can and does address many areas that contribute dramatically to our ability to answer the question of identity. Unfortunately in the shroud story, personalities, politics, and other nonscientific issues have severely muddied the waters of objectivity…(Stevenson & Habermas, 1990, p. 24)

A related insight I wish to address concerns the value ascribed by shroud scientists to the artifact. Specifically, I suggest that the blood dispute was animated and sustained not only by disputants' commitment to delivering and defending a truthful account of the shroud, but also in their desire to be truthful *to it.* I argue that shroud scientists, regardless of their perception of the shroud's provenance, developed (though perhaps unintentionally) an affinity to the object itself. Importantly, this had some bearing on the intensity and

duration of the blood dispute. In quite a different context, Wendy Faulkner (2000) has drawn our attention to the sensual, aesthetic, and even spiritual sensibilities that are often evoked by engineers' engagement with the technical objects in their world. From Faulkner's perspective, the pleasure derived by engineers' interactions with technical objects – tinkering, creating, experimenting and effectively "making things work" - is bound up in a desire to conquer nature and compensate for their own "limited sense of strength or potency" in the social world (p. 106). More recently, Myers and Dumit (2011) have examined scientists' performative engagement with technologies that visualize their experimental work and allow them to become "entangled" within the inquiry and the narratives they are conveying to audiences. The authors argue that scientists engage in "*affective entanglements* with their objects and instruments and the ongoing transformation of their modes of embodiment inside of their experiments" (p. 249).

I suggest that a similar emotional fidelity to the shroud has underlined disputants' engagement with it, though perhaps this affinity is less akin to Faulkner's notion of "pleasure" and more in keeping with Michael Polanyi's notion of "passion." In a brief discussion piece called "Passion and Controversy in Science," Polanyi argues that in science:

> Passions charge objects with emotions, making them repulsive or attractive; positive passions affirm that something is precious. The excitement of the scientist making a discovery is an intellectual passion, telling that something is intellectually precious and, more particularly, that it is precious to science. And this affirmation forms part of science...this appreciation depends ultimately on a sense of intellectual beauty; that it is an emotional response which can never be dispassionately defined, any more than we can dispassionately define the beauty of a work of art or the excellence of a noble action. (Polanyi, 1956, p. 921)

The notion that scientists develop passionate commitments to the objects of their inquiry – that in some ways they can come to *consecrate* them - is perhaps not a revelation in and of itself. In the context of the blood dispute, however, this idea emerged as a finding I did not quite anticipate, but which nonetheless provides some deeper insight into some of the overarching questions guiding my exploration of this peculiar controversy (i.e., what transpires when scientists collide with an object that bears no real scientific value, from which no professional acclaim can be derived, which demands unusual sacrifices and risks, and which provokes intensive conflict?).

In recognizing disputants' commitment to be truthful to, and about, the object of contention, we perhaps begin to see a dimension of the blood conflict that could easily go unheeded if the analysis confines itself only to the clamoring and clashing of the victorious and vanquished. That is, if we attend only to the explicit dimensions of the blood dispute –politics, polemics, power, vested interests, professional antagonisms, and epistemic contests–we lose sight of the deeper motivations and values that rested beneath it, which were expressed by it, and indeed which perhaps fuelled the antipathies even more profoundly.

Convergence, Divergence and the Relations of Conflict

While my analysis has thus far sought to offer insight into scientific disputes waged over non-scientific objects, I propose to explore what this controversy might reveal about the relations of conflict more generally. As noted in chapter one, Simmel (1955) offers some theoretical guidance for understanding the complicated texture of conflict among social actors, and for looking past (or perhaps better stated, into the *heart*

of) its manifest antipathies, divisions and impulses. Specifically, Simmel asks us to consider the phenomenon of conflict not in terms of dualistic and ultimately divisive energies. Instead, conflict contains "convergent" and "divergent" currents, or unitary and dualistic relations that flow simultaneously within a social unit. This is both the substance of conflict and its sociologically "positive character" (or a "constructive" function). Unity does not consist only of "consensus" and "total group synthesis." Rather, unity is comprised of both consensus and discord. Accordingly conflict often constitutes the relation of the group. Conflict thus contains elements of unity, and strives to achieve unity, irrespective of whether the conflict ends in resolution or with the annihilation of a disputant. Conflict is moreover, a fundamentally necessary and positive form of sociation. As Simmel argues, factors of "dissociation" (hate, envy, desire, need), which are arguably the causes of conflict, ultimately produce, in myriad forms, a sociating effect. There is, therefore, a positive value and function inherent in conflict itself, insofar as its absence "could show no real life process" in social relations among actors (p.15). Thus Simmel asks us to perceive conflict as a set of relations among social actors that are simultaneously integrative and divisive, and which bear out the "vitality" and force of those relations (p. 15). As he writes:

> Just as the universe needs "love and hate", that is, attractive and repulsive forces, in order to have any form at all, so society, too, in order to attain a determinate shape, needs some quantitative ratio of harmony and disharmony, of association and competition, of favorable and unfavorable tendencies. But these discords are by no means mere sociological liabilities or negative instances....society as we know it is the result of both categories of interaction, which thus manifest themselves as wholly positive. (Simmel, 1955, p. 15-16)

Competition is similarly marked by a unifying element that organizes the nature and objectives of a struggle. Competition, Simmel argues, often entails intensity and passion among competitors, but proceeds in pursuit of a beneficial outcome itself rather than the annihilation of an adversary. Antagonism among competitors (the subjective element in conflict) is thus both "purely formal" and importantly practical - that is, antagonism between competitors serves a "common purpose of both parties" and simultaneously allows the loser to "benefit from the victory of the victor" (p. 59). Simmel notes, for example, that "ambitious competition in the field of science aims not only at the adversary but at the *common* aim, on the assumption that the knowledge gained by the victor is the gain and the advantage of the loser" (p.59). Competition among social actors, then, produces a "supra-individual" objective. As Simmel explains:

> ...the subjectivity of the final goal and the objectivity of the final result interweave in the most fascinating manner. A supra-individual objective or social unit supersedes both parties and their fight. Each party fights its adversary without turning against him, without touching him, so to speak. The subjective, antagonistic mainspring thus leads to the realization of objective values, and victory in the fights is not really the success of the fight itself but, precisely, of the realization of the values outside of it. Here lies the immense value of competition for the social circle of which the competitors are members...[competition] offers subjective motives as the means of producing objective social values; and from the standpoint of the competing parties, it uses the production of objective values as a means for attaining subjective satisfaction. (Simmel, 1955, p. 59-60)

Competition, like other forms of conflict, produces a sociating effect, which illuminates its sociologically positive function. Simmel suggests, for example, that competition is a "synthetic force" through which competitors contend "for the other," for "the many" and for "the few". It is a "web of a thousand sociological threads by means of conscious

concentration on the will and feeling and thinking of fellowmen...of the delicately multiplied possibilities of gaining favor and connection" (p. 62).

I want to briefly consider the complexities of the blood dispute in light of Simmel's idea of conflict as containing an implicit unifying element. In its manifest form the conflicts between actors in the blood dispute comprised a tangle of competing epistemic claims and spiteful assaults on disputants' character, integrity, ethics, and motives. The question of who "prevailed" in the end is uncertain. That is, the issue under dispute seems to have found no resolution, and relations among the disputants are eternally divided. Certainly Walter McCrone, in describing the blood dispute as the clashing of an "irresistible force" and an "immovable object" perceived the tensions to be hopelessly irreconcilable.

But Simmel argues that when social actors clash over a common cause or a "supraindividual aim" (in this case the deployment of a *truthful representation* of the Turin shroud) the tensions among disputants are perhaps the most "merciless," precisely because disputants are collectively committed to an overriding goal or cause that justifies the intensity and duration of the fight. Indeed, the battle over a cause as the common basis of a conflict often renders the dispute irreconcilable. Yet despite its apparently divisive character, the conflict is underlined by *unity* in virtue of the fact that actors are invested in a common objective. Indeed, unity is often expressed through disharmony. As Simmel writes, "...[disputants'] consciousness of being mere representatives of supra-individual claims, of fighting not for themselves but only for a cause, can give conflict a radicalism and mercilessness..." (Simmel, 1955, p.39). On its surface the blood dispute

demonstrates the clash of competing interests, but it is simultaneously a set of social relations marked by converging interests. The dispute thus contains an intersection of unity and disharmony, inasmuch as the disputants expressed, through their discord, a common unity around the dissemination of a reliable interpretation of the shroud.

Viewed in light of Simmel's perspective, we can perhaps understand some of the implicit reasons for both the bitterness and persistence of this particular controversy, and for disputants' investment in this struggle. Simmel notes that inherent in some forms of competition among actors is an "objective overall value" that supersedes the interests of disputants and the struggle itself. The struggle is often waged antagonistically but "the victory in the fight is not really the success of the fight itself but, precisely, of the realization of values outside of it. Here lies the immense value of competition for the social circle of which competitors are members"(Simmel, 1955, p. 59).

Conclusion

As noted earlier, it was professor of philosophy Denis Dutton who first referred to this conflict as "The Great Blood Dispute." His hyperbolic term of reference is, however, a sarcastic one. Asking "what, after all, hangs on the question of blood vs. paint?" Dutton dismissed shroud science and its discontents as little more than an episode of scientific inquisitiveness gone awry (Dutton, 1984, par. 13). There was, to Dutton, no problem of any consequence worth battling over, and the conflict itself was hardly a compelling one in science's recent history. Such was the attitude of many skeptics who believed that the question of the shroud's provenance should never have been waged on science's battlefields. As a focus of sociological interest however, the blood conflict represents a

compelling and complex episode of relations and tensions among social actors that ultimately failed to find resolution. While much of this dispute reflected elements common to eruptions of scientific dissent, the dispute also embodied unexpected alliances and interests that shed some light on why the problem of the shroud's material properties became so consequential to the actors involved.

It is not clear whose evidence prevailed in the end, and the human artifice theory seemed to be left open to infinite debate. Walter McCrone stated in his last public interview before his death in 2002: "I'm willing to say categorically and to my dying day that there is no blood on the shroud."[57] STURP members were equally resolute in their position that the shroud was not a humanly-rendered artifact. In the next chapter I examine how the outstanding question of the shroud's historical status found its way into another episode of conflict, this time waged as an immense public and political battle among a new group of contenders, and on the basis of new epistemic concerns.

[57] Source: http://shrouduniversity.com/librarypodcasts.php

Chapter Four: Controversy and the Contested Path to Certainty: The Radiocarbon Dating Disputes

> Attempts to date the Turin Shroud are a great game...but don't imagine that they will convince anyone . . . the scientific study of the Turin shroud is like a microcosm of the scientific search for God: it does more to inflame any debate than settle it (Ball, 2008, p. 349).

Introduction

In the previous chapter I traced the complexities of the epistemic and political antagonisms surrounding the "human artifice theory," and examined how shroud scientists engaged in contests of evidence to define the material properties of the shroud and thereby impart a plausible theory of the artifact's historical origins. In this chapter I enter into another conflict that was waged alongside the "great blood dispute." Here the contentions among scientific and non-scientific actors were focused around the use of radiocarbon dating as the appropriate technique for settling the provenance question.

I explore the radiocarbon dating conflicts with an analytical focus on how and why scientists and other social actors sought to call forth definitive evidence of the Turin shroud's provenance, and more importantly, why this pursuit precipitated such antipathy and struggle. I will demonstrate that the question of the shroud's provenance became what I call a "priority of narrative" dispute. That is, the truth of the shroud's historical status was contingent upon who would shape and govern the correct scientific narrative of the artifact. My analysis also concentrates on the actions and strategies of one key dissenter in the shroud science community whose pursuit of the radiocarbon dating venture reflected an effort to "purify" the science of the shroud and purportedly safeguard the integrity of the radiocarbon dating enterprise.

To lend conceptual insight into the dynamics of this controversy, my analysis draws in part from Victor Turner's social drama framework. As I will demonstrate, Turner's paradigm provides analytical clarity with respect to the symbolic actions of contenders in each dramatic phase of this controversy as it was waged as a public dispute.

New Contentions, New Dissenters: Radiocarbon Dating Under Dispute

A common method by which experts determine the provenance of archeological relics and ancient textiles is through C14 radiocarbon dating. This technique proceeds from the principle that living organisms absorb carbon dioxide and minute levels of the radioactive isotope carbon 14, which decays measurably at the moment of an organism's death. The rate of decay can be measured in proportion to carbon 12, thus yielding a theoretical date of the organism's death (Wilson, 1998).

During the initial planning phase of the 1978 Turin inquiry, the STURP team had included radiocarbon dating in their proposed research protocols. Shroud custodians denied the team's request, however, on the grounds that radiocarbon dating would cause too much extraction damage to the cloth. At the time, the team proposed to use a traditional technique developed by Walter F. Libby called the "proportional counting technique" or the "Libby Method," which would have required extracting a pocket-sized sample from the shroud (Wilson, 1998).[58]

In the late 1970s, however, there were significant developments being made to radiocarbon dating techniques that imposed considerably less damage to artifacts. In

[58] Libby was awarded a Nobel Prize in 1961 for his achievements in radiocarbon dating techniques. Interestingly, shortly following the award, Libby asked Turin authorities for permission to date the shroud, but his request was denied (Wilson, 1998).

1977, a new method called Accelerator Mass Spectrometry (AMS) was invented at the Nuclear Structure Research Laboratory in Rochester, New York. Several institutions and laboratories collaborated in developing this technique, including the University of Toronto, the Lawrence Berkley National Laboratory, and an American corporation called General Ionex (Gove, 1996). According to Harry Gove, a professor of physics at the University of Rochester in New York and one of the key developers of AMS, this new technique was becoming "the method of choice for radiocarbon dating of organic matter, especially when the amount of material was limited" (Gove, 1996, p. 7).

In 1977, Harry Gove received a letter from Reverend David Sox, an Anglican priest and general secretary for the British Society for the Turin Shroud, asking if AMS could be applied to determine the provenance of the shroud. When Gove asked Sox if dating the shroud might "profoundly disappoint many people whose religious faith was buttressed by their belief that this was the burial cloth of Christ," Sox replied that it was "important to discover the truth" (Gove, 1996, p.20).

The naïve or common sense view of science generally holds that science is positioned and well armed to impart certainty in the midst of doubt, and to impose its epistemic command over equivocality. In his book *The Blind Spot: Science and the Crisis of Uncertainty*, William Beyers (2011) notes that science is thought to be "intimately connected with the human yearning for certainty" and with providing "a vision of an existence in which doubt has been vanquished and certainty reigns supreme" (p. 49-50). Such an assumption is implied by Sox's belief that the seemingly critical task of discovering the truth of the shroud should be delegated to the techniques of science

(Sox does not clarify, however, why or for whom this "truth" might be important). But this exchange is noteworthy in that it exemplifies the complexity of motives and investments in the social relations of the shroud science controversy. Here a religious actor proposes a technical intervention on the artifact, irrespective of the outcome. Conversely, it is a scientific actor who speculates about the implications of science's intrusion on a sacred object. As I have argued previously, there is rarely a point in the shroud controversy at which presumed boundaries between scientific and non-scientific actors are static. Indeed, such an oppositional dynamic elides the complexity and shifting positionality of the actors.

At the time of Sox's inquiry, AMS was still in its developmental stages. Gove assured Sox, however, that once the necessary refinements were completed, AMS could certainly be applied to determine the provenance of the shroud. Provided the shroud custodians granted permission, his lab would be well positioned to carry out the testing. Thus while Gove was initially concerned about the implications of radiocarbon dating the shroud, he was confident in the command of AMS to do so. He proclaimed it:

> ...the only measurement that could provide definitive information on a fundamental property of the Turin Shroud, namely the time when the flax used to make the shroud's linen was harvested...wide public interest in the shroud and consequently in a new scientific technique that could unambiguously establish its age made it a legitimate object to be tackled by AMS. (Gove, 1996, p. 7)

Thus a practical resource for resolving the shroud's ambiguities and imparting epistemic certainty was available. The radiocarbon enterprise was premised at the outset, therefore, on the belief in the superior capacities of technique and instrumentation to provide what Knorr-Cetina (1999) calls an "end-of-the-line" verdict in the enduring

debates about the shroud's ontological status (p. 53). Indeed, as Harry Gove indicates, *measurement* signified the only authoritative means by which to faithfully and objectively disclose the shroud's true historical origins and bring a long history of authenticity debates to a close. Within the practices of science, measurement constitutes a powerfully persuasive device for defining and verifying the attributes of phenomena. As Knorr-Cetina (1999) notes, measurements "count as evidence" (p. 53). They are:

> ...considered capable of proving or disproving theories, of suggesting new phenomena, of representing more or less interesting – and more or less publishable – "results". This view holds irrespective of the fact that measurements are theory-laden, prone to raise arguments in crucial cases, and sometimes subject to reinterpretation. Within the framework of their dependence on a certain paradigms and tradition, measurements count as self-sufficient quantities; they are granted a powerful role in validating knowledge, and they are considered irreplaceable witnesses and arbiters in scientific disputes. (Knorr-Cetina, 1999, p. 53)

Yet despite the fact that new techniques in radiocarbon dating offered a straightforward route to establishing definitive evidence of the shroud's historical origins, the question of dating the cloth quickly became a contested issue. In 1978, at Sox's urging, Gove travelled to Turin to attend a two-day international congress on the shroud that preceded STURP's inquiry. His purpose was to propose the new AMS method of carbon dating to shroud custodians. Prior to the meeting, Gove and a member of General Ionex submitted a paper to the Congress detailing the AMS method and the required sample size. Gove indicated that in order to produce the ten milligrams of carbon, they would need to extract a one square centimeter sample from the cloth. Gove further indicated that he could produce an accurate age of the cloth within approximately 150 years.

Gove's proposal and indeed his very presence at the congress was met with considerable resistance by shroud custodians. Prior to the meeting, the formal representative for the pontifical custodian of the shroud announced to the media that no carbon dating on the shroud would be permitted on the grounds that there were no available methods to establish a reliable date. When Gove was preparing to present his proposal to the delegates, a Turin cleric once again read a statement objecting to the use of any carbon dating on the shroud. Gove, in turn, objected to the position of the shroud custodians and clarified to congress members that it was indeed possible to carry out a non-destructive radiocarbon test on the shroud. He further stated that he was willing to make a small extraction from the cloth while in Turin and carry out the testing in his Rochester laboratory. As he notes:

> I merely wanted to ensure that the Turin authorities knew that it could be done in an accurate and credible way using an amount of cloth that would in no way affect the shroud's appearance. My further concern was that if [radiocarbon dating] were ever done it should be done by the people who knew what they were doing and were dispassionate and not under the control of true believers. (Gove, 1996, p. 29)

Gove's proposal was ignored. Likening this resistance by Turin authorities to "throwing holy water on the devil," Gove recalls, "it was clear that [shroud custodians] had orchestrated a campaign to deal with the carbon dating question" (p. 31).

The resistance to carbon dating that unfolded in Turin did not, however, close the matter entirely. In 1979, Sox encouraged Gove to send another proposal on behalf of his own Rochester lab and four others (the University of Toronto, General Ionex, the US Geological Survey group and the Brookhaven National Laboratory group) directly to Cardinal Ballestrero, the archbishop of Turin and the Pontifical custodian of the shroud.

Gove sent a letter to the Cardinal proposing both the AMS method and a refined version of the Libby decay counting method, and included a brief description of the instruments and testing protocols that would be applied.

The letter was sent via an emissary to the cardinal, Don Pierro Coero-Borga, but was never delivered. At a meeting in 1979, Gove was informed by the scientific advisor to the archbishop of Turin, Luigi Gonella, that Coero-Borga had deliberately withheld the letter. Gove ventured two possibilities for why his proposal was ignored. First, he presumed that he'd incensed the Turin authorities during the International Congress by challenging their resistance to carbon dating the shroud. Second, Gove believed that the authorities in Turin, accustomed to governing any direct interventions on the shroud, were wary of the results that might emerge from any radiocarbon tests, and were, therefore, unilaterally rejecting all radiocarbon dating proposals (Gove, 1996).

That same year, Gove was invited by STURP member John Jackson to attend the STURP team's post-Turin data analysis meeting in Santa Barbara. In his book *Relic, Icon or Hoax?: Carbon Dating the Turin Shroud*, Gove admits to somewhat grudgingly entering into the shroud science community and confronting its "exasperating" complexities. At the same time, Gove believed that AMS could categorically resolve the shroud's provenance and bypass the "ridiculous complications" that he felt had come to characterize shroud science up to that point (Gove, 1996, p. 54). Gove also saw an opportunity to publicly demonstrate the "power of carbon dating by AMS" (p. 14). As he notes:

> As a scientist can one justify spending any of one's professional time on a religious artifact? My main justification was that dating the shroud captured the

> public imagination and it would be a tremendous boost for this new and publicly unknown technique. Other than this, it would be difficult to argue that dating the shroud served any scientific purpose. (Gove, 1996, p. 2)

But Gove did, in fact, have another justification for participating in the Santa Barbara meeting. Much like Walter McCrone, the dissenter in the "great blood dispute," Gove had little confidence in the STURP team's capacity to offer any scientifically meaningful conclusions about the shroud's provenance. Importantly, Gove was convinced that many members the STURP team were predisposed towards establishing the shroud's correspondence to the historical Jesus. [59] As Gove notes:

> It is well known to scientists that one can sometimes obtain a desired result by subconscious manipulation of the technique or the data. It is a human flaw that must be carefully guarded against. It is easily circumvented by not having preconceived notions of what the answer should be. A belief that the shroud was the genuine article was the stuff of which STURP was made...(Gove, 1996, p. 8).

Thus Gove entered the shroud science community as a new dissenter, whose underlying objective was to shift the science of the shroud away from the prepossessions of scientific believers. Gove's misgivings about the STURP team's purported lack of objectivity is clear in his interpretation of the Turin inquiry:

> The [STURP team's] aim was to establish that [the shroud] was, indeed, the burial cloth of Jesus Christ....[the STURP team] comprised mainly true believers in the shroud's authenticity. They overwhelmed the Turin ecclesiastical authorities and their scientific advisors with their aerospace technology and their insistence on military-like secrecy and discipline. Like all the scientific investigations that had gone on before, their results were inconclusive and generally of negligible importance despite the time and money expended. (Gove, 1996, p.6)

[59] This assumption was quite inaccurate. The purpose of the STURP inquiry was to determine the organic properties of the cloth and investigate how the image might have been formed.

Gove's concerns about the STURP team's predilections are clear in his initial impressions of the Santa Barbara meeting:

> During the whole meeting, I had been taking notes as if what this motley mixture of scientists, priests, ministers, and peacetime warriors were reporting provided significant information regarding the real question of authenticity of the shroud. They seemed to me to be a group of kids playing with expensive toys, hoping they would reveal some ultimate truth – a truth of which most of them were already convinced...Throughout the whole of the day's discussions I kept wondering to myself why Jackson, Jumper and another member of the STURP team were all wearing crosses around their necks. Hardly evidence of the dispassionate scientists they professed to be. So far as I knew, they were neither priests nor ministers. One of course should never knock piety, but its ostentatious display by these shroud scientists did nothing to recommend their scientific detachment. I would have baldly asked them for the reason for this Christian ornamentation but I refrained. (Gove, 1996, p. 51-52)

Gove's disposition towards the STURP team brings to mind an archetypical image of science as inherently neutral and agnostic. Here Gove implicitly evokes the putative "norms" of scientific inquiry, particularly as they pertain to values of rationality and impartiality. It is clear that Gove entered the shroud science forum with a firm conviction that "legitimate" science is not encumbered by any a priori belief in an "ultimate truth." Good scientists, Gove further suggests, do not abdicate their prescribed role as dispassionate inquirers by embracing piety, nor do they confuse the boundaries between belief and objectivity.

Gove's dissenting position and his conviction that carbon dating represented the only authoritative mechanism by which to settle the status of the shroud reveals an effort to purify the science of the shroud, and in doing so, preclude any distorted evidentiary claims about it. In his presentation to team members he recalls:

> I probably indicated fairly directly in my talk that I believed that the carbon dating was the *only scientific test that was worth carrying out on the shroud at the*

> *present time.* Such a measurement, that could now be carried out using a negligibly small sample, would settle once and for all whether the shroud could have been Christ's burial cloth or whether it was of more recent vintage and thus either a relic or an icon (or a deliberate hoax). The nature of any further tests should be predicated on the shroud's age. (Gove, 1996, p. 54, emphasis mine)

The idea of purification is a familiar one in science studies. It is implied in Latour's (1987) study of the construction of scientific knowledge claims. Specifically, within the practices of science, purification entails the separation of nature and the social in producing and stabilizing scientific facts (Latour, 1987). Although inchoate fact statements are inevitably and "promiscuously" mixed with the social and the natural, they are "transmuted" into harder statements that bear no trace of the social relations of their production. The scientific fact is thus "purified," and as John Law (2007) explains:

> With most of the messy relations gone we are left with nature, a textual account of nature, and a set of more or less formulaic statements about method that purport to explain why the latter reflects the former. The intermediate and heterogeneous relations of production are deleted to generate two quite distinct and separate domains: reality on the one hand and knowledge of reality on the other. (Law, 2007, p.5)

Purification is similarly intrinsic to the boundary work practices of science. Gieryn (1999) argues that when scientists seek to claim autonomy over how to investigate problems, science becomes "purified," or selectively "demarcated from all political and market concerns" that might contaminate the truth (p. 23).[60] In both senses, purification

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[60] Susan Leigh Star (1983) describes a similar process in the work of science. Simplification refers to the pragmatic response to transforming "ill-structured" problems in science to "well-structured" resolvable problems. As is implied by its name, simplification is a practice of removing and reducing constraints in the fact-making process and effectively "stripping it of its complications," often in the face of technical constraints or temporal exigencies (Star, 1983, p. 224). This entails, for example, removing contaminants, discounting data, choosing between results, streamlining findings, and settling chaos in order (Star, 1983).

refers to a filtering out of contaminations (social, political, ideological) as part of the legitimate practice of science. I suggest that "purifying shroud science" describes Gove's practical actions, rhetoric, and disposition towards the STURP team.

Shortly following his presentation to the STURP team, Gove was informed by STURP member Robert Dinegar, a research chemist at the Los Alamos National Laboratory, that the STURP team had formed a radiocarbon dating committee and was planning to make recommendations to the archbishop of Turin regarding proposals to carbon date the cloth. Dinegar invited Gove's Rochester radiocarbon lab and another lab from Brookhaven, Connecticut to participate in the proposal. Gove was ambivalent about the collaboration. It was clear to him, on the one hand, that the STURP team had established valuable connections with authorities in Turin and were positioned to obtain a sample of the cloth. Simultaneously, Gove was apprehensive about the STURP team's predilections, noting that "they were too convinced in their hearts that the shroud was Christ's burial cloth" (Gove, 1996, p. 57). Consequently, Gove indicated to Dinegar that Rochester and Brookhaven would work and publish independently of the STURP team.

It is worth noting that the STURP team's participation in radiocarbon dating the shroud was not the only source of concern for Gove. Walter McCrone, the lone dissenter in the blood dispute, was also vying to radiocarbon date the shroud, and saw Gove as a potential collaborator. Despite the fact that both Gove and McCrone shared similar misgivings about the STURP team's expertise and objectivity, Gove was suspicious of McCrone's rather fervent campaign to debunk the shroud. As he notes, "I sometimes think that McCrone dreamed of becoming history's greatest iconoclast. Having, in his

view, demolished the authenticity of the Vinland Map, he saw the chance to do the same to the Turin Shroud!" (p. 19).

There were other contenders vying to take the lead in the carbon dating quest as well. The Sindonology Center of Turin, who accused shroud custodians (namely Cardinal Ballestrero) of "selling out to Americans" argued that if radiocarbon tests were to be carried out, the Center should have absolute control over the procedures, results and their dissemination. In 1979, Cardinal Ballestrero, responding to these manifold pressures, turned the issue over to a third party - the Pontifical Academy of Sciences. Founded in 1603, the Academy is a multi-denominational organization of scientists appointed by the Holy See to promote innovation in the physical and natural sciences. Its members are international, non-sectarian, and nominated by the Supreme Pontiff.[61] The Academy was asked to advise on how a radiocarbon dating project should best proceed, and who should be involved in carrying out the tests.

The Academy did not reach a decision and the issue remained dormant until 1982, when a representative of a radiocarbon dating laboratory in Harwell, England approached the British Museum's Research Laboratory about conducting blind tests on samples of ancient textiles as a prelude to possibly applying radiocarbon dating tests to the shroud. In 1983, the Museum's director, Michael Tite, invited seven radiocarbon laboratories, including Harry Gove's Rochester lab, to conduct an inter-comparison laboratory test of two ancient textiles (a sample of an Egyptian linen and a cotton textile from Peru). The

[61] Source: The Pontifical Academy of Science:
http://www.vatican.va/roman_curia/pontifical_academies/acdscien

role of the British Museum would be to collate the results of the tests and determine if an offer to date the shroud could be conveyed to the archbishop of Turin.

The results of the inter-comparison laboratory tests were presented in 1985 at the 12th International Radiocarbon Conference in Trondheim, Norway. Based on these results, Harry Gove was asked to organize a meeting with the participating laboratories, prepare a detailed protocol for dating the shroud, and submit it to Turin authorities. Gove indicated that he wanted to submit the proposal to the Pontifical Academy of Science along with the Turin authorities. Given what he felt was a pervasive risk for religious bias to interfere with the epistemic interests of carbon dating the shroud, Gove believed that the Academy's involvement in the process "provided a bridge between science and religion" (Gove, 1996, p.152).

Upon reviewing the Trondheim protocols, the president of the Pontifical Academy requested permission from Cardinal Ballestrero to hold a meeting in Turin to discuss procedures for dating the shroud. It is important to note that the STURP team had also submitted a radiocarbon dating proposal to the Academy, along with a list of additional tests they wished to carry out on the shroud. The Academy approved the Trondheim protocols, an endorsement that signified a critical triumph for Gove, who, as one of the main skeptics of STURP's work, was determined to eliminate their participation in any radiocarbon dating ventures. Indeed, Gove indicated in a subsequent letter to STURP team member Robert Dinegar that while STURP representatives could attend the forthcoming Turin meeting as "bystanders," there was no further need for their

involvement in the carbon dating process, "particularly since some people already had reservations about STURP's impartiality concerning the nature of the shroud" (p. 88).

The Turin Protocols

In 1986, preparations were under way for the June radiocarbon workshop in Turin. The purpose of the workshop was to map out the protocols and procedures for dating the shroud, to establish the roles of the laboratories involved, and to determine the coordinating role of the British Museum. The invited delegates comprised members of the Pontifical Academy of Science, several scientists, and representatives from the Vatican and Turin.

There were, however, additional tensions emerging during the 1986 workshop planning phase. Professor of physics and scientific advisor to Cardinal Ballestrero, Luigi Gonella, insisted that the workshop should be held in Turin at the Turin Polytechnic, where he held a faculty position. Gonella's reasons (conveyed in a personal meeting with Gove) were that the shroud was ultimately "Turin's responsibility," and as such the meeting should be held under the auspices of his institution. The Pontifical Academy of Sciences responded to Gonella's demands by stating that holding the meeting anywhere other than the Vatican headquarters would violate Vatican tradition (Gove, 1996).

It is important to note that the shroud has been embedded in a long history of institutional territorialism and considerable tension over who can claim custodial entitlements to it. When deposed king of Italy Umberto II bequeathed the shroud to the Vatican, it remained in Turin but was legally owned by the Holy See. There is a

persistent fear among Turinese citizens and local church authorities that the shroud will eventually be returned to the Vatican, as Turin is only its provisional place of residence. Indeed, Gove has noted that "all hell breaks loose" in Turin when any proposals are made to transfer the shroud to the Vatican's custody.

Gonella's concerns were also rooted in his alliance with the STURP team and his strong endorsement of their involvement in the carbon dating project (the STURP team had strong institutional ties with the Turin Polytechnic). As such, it is possible that Gonella did not want to confront the risk of Vatican representatives usurping the carbon dating project. Although Gonella eventually prevailed in his efforts to keep the meeting on Turin soil, the decision was made to hold the meeting at the Seminario Metropolitato of the Turin Archdiocese and not the Turin Polytechnic.

The three-day Turin workshop began on September 29, 1986, at the Seminario Metropolitato, where the twenty-two delegates debated the protocols and procedures for the radiocarbon dating project. Participants were divided on a number of issues. Delegates debated over whether or not the testing should be carried out as a "blind" procedure, where none of the labs would know if they were testing a genuine piece of the shroud or a dummy sample. Harry Gove insisted that the labs should all test a genuine sample of the shroud and that, in fact, the motivation of his staff at the Rochester lab would be seriously diminished if they did not receive a sample of the shroud. Gove proposed that another reason for ensuring his lab received a shroud sample had to do with its public appeal. He noted:

> On the question of whether each lab received a shroud sample, I pointed out that physics was fun, especially when occasionally one got to make a measurement

> that would have enormous appeal to the public. Normally in our lab we measured such things as the shapes of nuclei...such research generally caused the public's eyes to glaze over. *When it came to the shroud, there was enormous public interest. One of the rationales I had used for our lab being involved in the shroud was that occasionally we should do something of interest to the public. After all, it was their tax money ultimately that funded our laboratory...whether one called such sentiments political and not scientific, they were very real.* (Gove, 1996, p. 168, emphasis mine)

The second issue under debate concerned how many radiocarbon laboratories could justifiably be involved in the dating project. William Meacham, (the archeologist who had participated in discrediting Walter McCrone in the "Great Blood Dispute") insisted that, in his experience, seven laboratories was "unusual" and excessive. He suggested that at most the project would only require three or four labs. One lab representative pointed out, however, that if fewer than seven labs were involved, "a hundred things could go wrong," and that involving only four labs was an "impossibly small number" (Gove, 1996, p. 157). The number of labs was also a matter of ensuring both public interest and public trust in the final results. Teddy Hall, a representative of the Oxford lab, noted that the scientists involved in the radiocarbon project were dealing with people "outside of the scientific community" and as such:

> ...[employing seven labs] is the approach we ought to take because we are going to be under fire and if we can say that six out of seven agree with each other we are in a much better position than if we say that two out of three agree. I am quite sure that the more we have the more convincing we will be. (Gove, 1996, p. 160)

It is interesting to note that the decisions concerning how to proceed were being shaped in part by scientists' heightened awareness of the public's interest and possible scrutiny of the radiocarbon dating procedures. We see here a reverse of Latour's idea that

the scientific lab "circulates" in the world, or renders the world a "vast laboratory" (Latour, 1999, p. 272). Here the non-scientific world "enters" into science's forum and circumscribes the deliberations.

The most significant outcome of the workshop was that it led to the STURP team's formal elimination from the radiocarbon dating venture. As noted earlier, one of Gove's agendas in pursuing radiocarbon dating was to ensure that the procedures were carried out as objectively as possible, a goal that, in Gove's view, demanded that the STURP team be denied any involvement in the project. STURP members were interested in playing a role in the carbon dating enterprise, and were also pressing to conduct further tests on the shroud (allegedly for the purpose of determining the shroud's conservation needs). Gove's objections to the STURP team's involvement in the testing was premised, as noted, on their relative lack of expertise in matters of carbon dating, coupled with what he perceived to be their considerable bias towards the cloth's authenticity. Gove was also deeply concerned about the destructive way in which the STURP team had handled the shroud in their battery of tests in 1978. In his description of a slide show presented by a STURP team representative at the Turin workshop, Gove writes:

> [The presentation] concerned the tests on the shroud carried out by STURP in 1978. It showed the shroud racked up on a frame devised by STURP and bathed in bright lights including ultraviolet for photography, for X-ray analysis and for other measurements...[the presentation] had a profound effect on several people present...because it vividly demonstrated how very intrusive the STURP tests were. (Gove, 1996, p.158)

Indeed, STURP's bid to carry out further tests on the cloth to determine image formation and issues around its conservation was further impeded by the Brookhaven lab

representative, Garman Harbottle. Following STURP's presentation, Harbottle leveled severe criticism against STURP's previous round of tests. As Gove recalls:

> ...Harbottle dropped what turned out to be a real bombshell...he said he agreed that conservation was very important. As far as he knew, no adequate conservation of the shroud had yet been carried out. The STURP tests conducted so far involved exposing the shroud to powerful visible light, ultraviolet light and X-rays. They could have been very harmful to the shroud...he also could hardly imagine that the collection of surface detritus with sticky tape could be beneficial to the shroud...the STURP tests constituted a stress no matter how careful they think they were being. As far as he was concerned, many of the STURP tests were very intrusive indeed...(Gove, 1996, p. 166)

Gove's efforts were successful. At the end of workshop, there was no formal role assigned to STURP team members in the final protocols. The workshop concluded with a consensus around a list of the following protocols:

> (1) The participating labs would extract a minimum amount of cloth to "ensure rigorous scientific results and to ensure public credibility."
>
> (2) Seven labs would be involved to ensure the correct statistical results. Five labs would use the AMS method and two would use a method called "small counter". The candidate institutions included Brookhaven National Laboratory in New York, The University of Arizona, Oxford University, Rochester University in New York, the Federal Institute of Technology in Zurich, the Centre for Faibles Radioactivities in France, and Britain's Atomic Energy research Establishment at Harwell, England.
>
> (3) Samples would be taken "from areas devoid of other possible information content and outside the image itself." [62]
>
> (4) Seven samples would be removed from the shroud and distributed to each lab, along with dummy samples.

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[62] Information content refers to areas of the shroud that contain scorch marks and damage from a 1532 fire that broke out in the Sainte Chapelle, in Chambery, where the shroud was stored. Molten silver from the reliquary in which the shroud was kept damaged one corner of the cloth. It was believed that samples from the scorched areas would yield erroneous results.

(5) The conservator of the British Museum would supervise the distribution of samples and act as guarantor of the result.

(6) The sample extraction would be recorded and witnessed on closed circuit TV by lab representatives.

(7) The final results would be published as a collaboration of all participating labs.

(8) Results were to be submitted to the Pontifical Academy of Science, the British Museum, and the Metrological Institute of Turin. (Tribbe, 2006, Gove, 1996, p. 174-75)

The Turin Protocols required approval from the Vatican Academy of Sciences and the Archbishop of Turin and Pontifical custodian of the Shroud, Cardinal Anastasio Ballestrero. In 1987, Gove published the Turin protocols in *Nuclear Instruments and Methods in Physics Research,* where he justified to the broader scientific community the rationale and utility of the carbon dating project:

> There are several reasons why the Turin Shroud is an object of legitimate interest for scientists involved in radiocarbon measurements by AMS and by the use of small proportional counters....It is widely believed by some fraction of the general public to be Christ's shroud. However unlikely this may seem to skeptics and/or agnostics it does engender a very substantial degree of interest by a broad spectrum of people throughout the world. A scientific technique which can provide answers to its origin will have high public visibility...the procedures for carrying out a dating of the shroud cloth must produce a result which is credible to the public in general and to knowledgeable scientists in particular. It is generally accepted that there will probably be only one opportunity to make the measurement and so it had better be done properly (Gove, 1987, p. 193).

I want to consider in more depth Harry Gove's role in the STURP team's expulsion from the carbon dating project. Thus far I have shown that Gove's concerted attempts to preserve the credibility of the radiocarbon dating project by denying any

legitimacy to the STURP team's work, and further denying STURP members any entitlements in the carbon dating enterprise, was rooted largely in three concerns. First, Gove believed that some STURP members' religious predilections would discredit an otherwise credible and objectively sound undertaking. Second, Gove had misgivings about the relative scientific value of the STURP team's 1978 work. Third, he believed that their experimental work on the shroud might have actually caused serious damage to the cloth. As he stated:

> I believed STURP's members to be so convinced it was Christ's shroud that I was determined to prevent their involvement in its carbon dating...*I feared the most important measurement that could be made on the shroud would be rendered less credible by their participation.* Fortunately in this I was successful...I am happy to say that, in the end, they played no role in its carbon dating. (Gove, 1996, p. 6-7, 8, emphasis mine).

Gieryn (1999) offers a fitting conceptual framework in which to situate Gove's objections to the STURP team's involvement in the carbon dating enterprise. Boundary work - the practical, rhetorical and often instrumentalist efforts carried out by scientists to forge demarcations, protect its autonomy, and enhance its authority - is well represented in Gove's dissenting actions. Two genres of boundary work in particular – expulsion and expansion - find relevance here. Expulsion constitutes the mechanism of social control deployed by scientists when rival actors "seek to legitimate their claims" inside the "authoritative capital space" of science (p.15-16). As implied by its name, boundary work carried out by expulsion constitutes a rigid delimitation of what science *is* and what it *is not*. As Gieryn explains:

> Real science is demarcated from several categories of posers: pseudoscience, junk science, popular science. Boundary work becomes a means of social control: as

> the borders get placed and policed, "scientists" learn where they may not roam without transgressing the boundaries of legitimacy, and "science" displays its ability to maintain monopoly over preferred norms of conduct. Expulsion often pits orthodox science against heterodox, mainstream against fringe, established against revolutionary – but of course the issues in dispute is who and what belongs to each side. Neither side wishes to challenge or attenuate the epistemic authority of science itself, but rather to deny privileges of the space to others who – in their pragmatic and contingent judgment – do not belong there. (Gieryn, 1999, p. 16).

"Expansion" refers to the boundary work enacted when two or more rival epistemic authorities "square off for jurisdictional control over a contested ontological domain" (p. 16). Here the demarcations are cast between science and "less reliable, less truthful, less relevant sources of knowledge" (p. 17). Both genres of boundary work were in operation here as the carbon dating project was being shaped. Gove's boundary work in particular was expressed by his efforts to ensure an uncorrupted path towards closure, and his conviction that the elimination of the STURP team was the appropriate, objective and scientifically ethical way forward in determining the provenance of the shroud.

As I have noted, Gove's single-minded struggle to prevent the STURP team's participation in the carbon dating venture also reveals a concerted effort to *purify* the science of the shroud. My use of the idea of purification refers to the deliberate actions on Gove's part to shift shroud science away from any distorted interventions and reclaim it as a pure, systematic and methodical set of practices and techniques. To impart a scientifically pure, factual narrative of the artifact demanded that its "fictions" were dissolved, and that the perpetrators of these fictions were displaced. To Gove, purifying shroud science was the only way, as he indicated, to "ensure rigorous scientific results"

and procure "a result which is credible to the public in general and to knowledgeable scientists in particular" (Gove, 1987, p. 193).

Gove's disdain for the STURP team echoes that of Walter McCrone and other skeptics who seem convinced that "piety" precludes scientific impartiality. Indeed, the archetype of the "dispassionate" scientist is a familiar one in shroud scientists' narratives and is especially conspicuous in Gove's assessment of the STURP team. It must be noted, however, that there is nothing about an agnostic scientist to recommend his or her scientific detachment either. Anti-religious views can no doubt prejudice one's perspective of a devotional object in equal measure, but this point tends to be obscured in dissenters' accounts of shroud science. It is important consider, therefore, how Gove's assessments of the threats to shroud science, his performance of boundary work, and his attempts to purify the carbon dating venture illuminates how scientists "account" for erroneous beliefs in science.

At the heart of Gove's critique of the STURP team has been the claim that their science is epistemologically inadequate, distorted by prejudice, and indeed extremely detrimental to the shroud. STURP is always, and in every way, "in error." Mulkay and Gilbert (1982) have provided insight into the linguistic devices used by scientists to "account" for correct belief versus error in determining the validity of experimental evidence. They contend that the practice of accounting for error is an asymmetrical one, where error is attributed to non-cognitive factors that "[distort] scientists' understanding

of natural phenomena" (p. 166).[63] Such an asymmetry is a consequence of the belief that correct knowledge proceeds from the normal, rational routines of science, while erroneous claims are anomalies that must be explained away using "elaborate repertoires of interpretive resources" (p. 166). This entails interpreting errors in terms of incompetency, lack of understanding, "subjective bias," "narrow disciplinary perspective," rivalry, emotional investment, etc. (p. 176).

The aforementioned accusations are familiar ones in the rival discourses of shroud science. Gove's accounts of the STURP team's scientific interventions are especially asymmetrical, and organized to foreground the correct way forward with respect to determining the shroud's historical status. But accounting for error is an interpretive mechanism – a constructed social device that is "linked to a particular conception of scientific knowledge and rationality" and a way of preserving the "underlying assumptions that genuine knowledge is immune from non-cognitive influences" (Mulkay & Gilbert,1982, p. 181-182).

I have noted earlier that Gove's dissenting conduct and rhetoric suggests a kind of "performance" of the putative normative values of science, particularly as they relate to principles of rationality and impartiality. Mulkay (1976) has argued elsewhere that while norms and counter-norms in science are neither functional nor inherent in any "dominant ethos," they are nonetheless often evoked by scientists to "categorize professional actions differently in various social contexts and, presumably, in accordance with varying social

[63] Pinch and Collins (1993) similarly note "when something goes wrong with science, the scientific community reacts like a nest of ants with an intruder in their midst. Ants swarm over an intruder giving their lives for the nest; in the case of science it is human bodies that are sacrificed: the bodies of those responsible for the "human error" that allowed the problem to arise" (Pinch and Collins, 1993, p. 142).

interests" (p. 645). Scientists thus evoke norms selectively in order to serve particular interests or objectives, and do so by recourse to what Mulkay calls a flexible repertoire of "justification and evaluation" (p. 645). Gove's dissenting actions and motives (i.e., purifying shroud science, expelling the STURP team) and his justifications for these actions (i.e. only pure science will allow for the correct evidence) need to be understood in light of the flexible and subjective "verbal formulations" that comprise the "complex moral language" of science (p. 645).

But to what extent was Gove successful in his efforts? If we have learned anything about the complexities of socio-scientific conflicts, we know that practical and technical solutions to a scientific problem, no matter how refined, are rarely removed from the volatile social relations from which they are wrought. The conflicts associated with the shroud of Turin are no exception. Once the radiocarbon dating protocols were in place, one might have expected that the resolution of the shroud's origins was close at hand. But the shroud had become entangled in too many competing political, epistemic and institutional interests to permit its graceful transition into this final phase of scientific intervention. In the next section, I take account of an eruption of a new conflict —what I call the "chosen three" controversy— that once again shifted the direction of the carbon dating enterprise. This episode erupted along a fault line already well established in the carbon dating disputes that preceded it. Specifically, the controversy largely involved Gove and his supporters in contention with church authorities.

The Chosen Three: Conflict, Social Drama and the Politics of the Artifact

To lend conceptual interpretation to this phase of conflict as it was waged in the public arena, I draw on the insights of Victor Turner's social drama framework. As I will demonstrate, Turner's paradigm, and particularly the concepts of "breach" and "crisis," illuminate how the fight to carbon date the shroud was translated into a critical political and ethical issue for science and for the public. Turner's paradigm is useful in comprehending controversies in terms of their "underlying dramatic structure" (Gross, 2005, p. 44). In many conventional accounts of scientific controversies, analysts map out the phases of the conflict from its eruption through to its closure. The value of Turner's framework is that each phase of public conflict can be assigned a specific analytical significance. Understood in terms of a social drama, controversy, in Turner's conception, is a sequence of events that embodies particular forms of rhetorical expression and symbolic action on the part of contenders. The utility of this type of analysis is that we may comprehend the role and function of symbolic language and actions as critical to how a controversy unfolds, develops and terminates.

While the seven radiocarbon labs were now poised to proceed with the carbon dating venture, nearly a year passed before they received any communication from Vatican authorities. No reason was given for this lapse in communication. Then in October, 1987, each participant of the Turin workshop received a letter from Archbishop Ballestrero of Turin, the Pontifical custodian of the shroud, announcing considerable and quite unanticipated revisions to the Turin protocols. Ballestrero advised that while he accepted, in principle, the "main line" of the protocols, he was compelled to reject several

critical items. Acting on the "positive instructions from the Holy See," Ballestrero indicated that four laboratories, including Harry Gove's Rochester lab, were now eliminated from the carbon dating enterprise, and that only the Arizona, Oxford and Zurich laboratories would be permitted to go forward with the project (Gove, 1996, p.213). This decision, emerging from "long deliberation and careful consultation," was based on what Ballestrero called a "criterion of internationality" and a "consideration for the specific experience [of the chosen three laboratories] in the field of archeological carbon dating" (p. 214).

Ballestrero further indicated that only three samples would be removed from the shroud, and that the carbon dating procedures would include blind testing with the use of control samples. The sample extraction was to be presided over by Ballestrero himself, with no representatives of the chosen labs in attendance. Professor Chagas (of the Pontifical Academy of Sciences) would be permitted to attend the sample removal as a "guest" of the Cardinal, but would play no role in the testing, evaluation or dissemination of the results. Ballestrero concluded the letter with the following statement:

> The decisions took more time to be worked out than originally wished, owing to the situation without precedents created by a number of competing offers tied into a rather rigid proposal, *and also by the initiative of some participants of the workshop who stepped out of the radiocarbon field to oppose research proposals in other fields, with implications on the freedom of research of other scientists and on our own research programmes for the Shroud conservation that asked for thorough deliberations.* (Gove, 1996, p. 214, emphasis mine)

Ballestrero's closing statement signifies a not so subtle admonishment of Gove and his supporters for the STURP's team's exclusion from the carbon dating project. In Gove's account of receiving Ballestrero's letter he notes:

> [Ballestrero's] thinly veiled accusation that we were attempting to prevent STURP from carrying out its scientific investigations was quite accurate. It was not a question of interfering with anyone's scientific freedom. It was just that several of us felt very strongly that the dating of the shroud must not involve STURP and had to precede any sort of measurements that were done by STURP. I certainly did. There was also the concern that some of STURP's tests might damage the shroud...I had never before been directly or indirectly chastised by a cardinal and I resented the falseness of his charges. (Gove, 1996, p.114)

Gove was unwilling to capitulate to Ballestrero's directive on several grounds. At the outset he believed that the Cardinal's decision was influenced by the interests of Luigi Gonella, who, like William Meacham, was opposed to the participation of seven laboratories. Gove also believed that Gonella greatly resented the fact that the STURP team had been summarily eliminated from the carbon dating enterprise and had therefore conspired with Ballestrero to eliminate the Rochester laboratory from the project. Indeed, there had always been considerable tension and mistrust between Gonella and Gove. Gonella accused Gove, for example, of having political motives in the carbon dating pursuit, and suggested that Gove's intentions were to exploit the Turin shroud as a "lever" for extracting support funds from the National Science Foundation (Gove, 1996). Thus the decision, in Gove's view constituted an act of retribution for his dissenting actions.

Gove's major concern, however, was that three laboratories were simply too few to produce a statistically reliable measurement. As he notes, if one of the three labs obtained an outlier result, "it would be impossible statistically to identify it and the three measurements would all have to be included in the average thereby producing an incorrect result" (Gove, 1989, p. 237). Gove further rejected what he called the

"absolutely outrageous" decision that no laboratory representative would participate in the sample removal, given that the labs could never be certain if they'd received genuine samples from the shroud (p. 215).

Cardinal Ballestrero's unilateral decision to eliminate four labs from the enterprise without reasonable justification ignited a brief but dramatic episode of conflict between the rejected laboratories and the church. On a fundamental level, the cardinal's decision speaks unmistakably to the role of institutional power in circumscribing the direction of the carbon dating project. By extension, the cardinal's decision ostensibly reflects the importance of alliances in shaping the direction of the carbon dating venture. While Harry Gove was able to secure endorsement for his vision for the project, it is clear that he made a critical mistake in failing to recognize the value of alliance building in the shroud community. We cannot say for certain, but a more successful tack for Gove might have been to exercise greater tolerance of the pre-dispositions of STURP scientists, and to be more diplomatically mindful of their close ties with church authorities as well as their success in the delicate relations of institutional power.

Importantly, the Cardinal's decision constituted a renewal of the conflict over the carbon-dating project. Victor Turner's paradigm of social drama provides useful conceptual tools for elucidating how the decision can be seen in this way. The Cardinal's amendments to the Turin protocols exemplify a "breach," or the "symbolic trigger of confrontation or encounter" that signals the first act in the social drama of conflict (Turner, 1974, p. 38). The breach deliberately disrupts the "regular, norm-governed social relations" of a social group; in this case, it disrupts the norm-governed consensus

concerning the Turin protocols and their implementation (p. 38). The breach is thus an incendiary gesture that brings "an underlying social conflict vividly to public attention" (Gross, 2005, p.44).

The breach is often expressed rhetorically in "altruistic" terms, whereby the individual who instigates the conflict purports to act in the name of a nobler purpose, or on the behalf of all interested parties (Turner, 1974; Gross, 1996). This idea is well articulated by the Cardinal's stated justifications for the revised protocols, and the elimination of the four laboratories. In his letter Ballestrero stated that the decision was premised on the expertise of the chosen labs, which would yield "important objective datum to the scientific quest that has long been growing on the illustrious image entrusted to my stewardship" (Gove, 1996, p. 215). The cardinal similarly implied that the revised protocols acted in the interest of ensuring public trust in the credibility of the project. He advised that participants must consider the "great attention from the public and the press that all of us know this measurement is attracting" (Ballestrero, in Gove, 1996, p. 215).

If a breach cannot be swiftly contained (in this case, containment would have required that the eliminated labs concede to the amendment) the conflict escalates into a phase of crisis that assumes various forms. Social actors engage in resistant actions, "latent tensions" begin to manifest, allies are mobilized, and "old wounds are opened" (McFarlane, 2004, p. 1253). Moreover, the language of opponents assumes a specific rhetorical style. Harry Gove turned immediately to the task of amassing his allies and securing solidarity in the face of what he and Garmon Harbottle (of the Brookhaven lab) considered a potential "disaster" for the carbon dating enterprise. His first act of

resistance to the Cardinal's missive was to appeal to all seven labs to formally voice their collective opposition to the amended protocols. Gove also composed a letter to Pope John Paul II urging him to reconsider the amendments. He did so based on the advice of Carlos Chagas, who believed that the labs should take "one more shot at the pope to persuade him to tell Ballestrero that he is getting bad scientific advice" (Gove, 1996, p. 216). Gove then made an appeal for collective action by issuing a memo to the seven laboratories requesting that they sign the letter to the Pope:

> Some of us have been persuaded, as a result of the 10 October letter from the Archdiocese of Turin, that a direct appeal to the pope, preferably through Professor Chagas, is now in order. Such a letter would only have a chance of being effective if it were signed by at least six of the seven laboratories and I would only send it in that circumstance. If, for whatever reason, this proposed appeal fails it is hard to see how this unilateral rejection of the Turin Workshop Protocol by the Cardinal of Turin can be concealed from the world press...(Gove, 1996, p. 217)

In his letter to the Pope, Gove warned of the implications of the Cardinal's decision:

> It is our collective impression that Cardinal Ballestrero has received very unwise scientific advice. The proposed modifications *will confirm the suspicion of many people around the world that the Church either does not want the Shroud dated or it wants it done in an ambiguous way*. The procedure that the Cardinal of Turin is suggesting is *bound to produce a result that will be questioned in strictly scientific terms by many scientists around the world who will be very skeptical of the arbitrarily small statistical basis when it is well known that a better procedure was recommended.* Since there is great world expectation for the date of the Shroud, the publicity resulting from a scientifically dubious result *will do great harm to the Church*. We respectfully urge your Holiness to persuade the Cardinal of Turin that the scientific advice being given to him is not shared by the world experts in this field. He should be urged to seek the advice of the eminent scientific organization expressly created to advise you, namely the Pontifical Academy of Sciences that enjoys the respect of the scientific world at large. *Rather than following an ill-advised procedure that will not generate a reliable date but will rather give rise to world controversy, we suggest that it would be better not to date the Shroud at all.* (Gove, 1996, p.219, emphasis mine).

It is within the crisis stage of social dramas that social actors operate in what Gross (1996) called an "ideological-rhetorical world," whereby disputants mobilize their respective ideological positions and articulate them through both symbolic action and figurative language (Turner, 1974; Gross, 1996, p. 186). Gove's appeal to the Pope clearly reveals an effort to assert science's epistemic and ethical obligation to proceed with the carbon dating by credible and legitimate means. Gove articulates the need to safeguard the artifact from a potentially ambiguous narrative rendered by "ill-advised" and questionable procedures that could, in the end, engender a "world controversy." Implied here as well is that the public has now a stake in how the truth of the shroud is procured and disclosed, a truth that can only be delivered by credible experts. Gove's symbolic language is replete with the rhetoric of "risk" in choosing ambiguity over certainty and its implications for the shroud, the church's credibility, the integrity of science and the public's understanding of how science should proceed in what had now become a critical undertaking.

Despite Gove's concerted efforts to mobilize the collective support of the seven labs, he received mixed responses from his colleagues. Teddy Hall, who headed one of the chosen labs at Oxford, was reluctant to sign the letter, fearing that the Cardinal would construe it as "blackmail" and consequently dismantle the project entirely (Gove, 1996). In Gove's view, Hall's reluctance to endorse the letter betrayed Hall's vested interest in participating in what was becoming an increasingly publicized enterprise. Indeed, Hall had a professional stake in the carbon dating ventures. He had allegedly

offered the BBC an "exclusive" on the Oxford's role in the shroud dating for a "fancy price" (Gove, 1996, p. 226).

The Arizona lab was similarly hesitant to sign Gove's letter, but the head of the lab, Doug Donahue, proposed instead to send a message to the archbishop on behalf of all three chosen laboratories. The letter indicated that the three designated labs, while "honoured" by being chosen, were nonetheless "hesitant to proceed" if Ballestrero would not reconsider his decision to exclude the four labs and revert to the original protocols. The letter further suggested that the "credibility of the enterprise to the public" might be seriously compromised if the original protocols were abandoned, and urged that only the original Turin protocols would "ensure a result that is scientifically rigorous" (Gove, 1996, p. 222).

Gove received no reply from the Pope, and Ballestrero stood firm on the amended protocols, despite the appeal from the chosen laboratories. Gove also received word from Father Peter Rinaldi (who had helped organize the 1978 Turin inquiry) that Gonella had a "contingency plan" in the event that the chosen three labs refused to carry out the project. Specifically, Gonella intended to invite a number of other international laboratories to take over the project if the current arrangements threatened to fall through.

Gove then attempted to enlist allies outside of the scientific community whose symbolic authority he believed might have some influence on the matter. Gove contacted two New York state senators, Al D'Amato and Daniel Patrick Moynihan, and asked them to compel the Ambassador to the Holy See, Joseph Kennedy, to inquire on behalf of the

Rochester labs as to why "such distinguished laboratories" were excluded from the new protocols. In his letter to Senator Moynihan, Gove wrote:

> I am sorry to trouble you with such an apparently inconsequential request but I feel that the Archbishop of Turin Cardinal Ballestrero is receiving incredibly bad advice from his science advisor Professor Gonella on the most credible way to date the Turin Shroud. Furthermore, it is unbelievable that the laboratories which invented this new technique not be permitted to be amongst those applying it to such an important artifact. The fact that they are both located in New York State emboldens me to bring the matter to your attention. (Gove, 1996, p. 250)

Senator D'Amato never responded to the request. Senator Moynihan wrote to the Ambassador requesting that "some sort of explanation for this disqualification" which, while "surely not a matter of life or death" would be of interest to the concerned parties, and particularly the "esteemed research laboratories in New York State" (p. 368-269).

Gove and his supporters then shifted the problem into public view. Garmon Harbottle, who represented the rejected Brookhaven lab, contacted National Public Radio and *The Skeptical Inquirer* to "rail against" the Cardinal's decision (p. 216).[64] Harbottle and Gove followed with a press conference at Columbia University. In their public statement, they again employed risk language to underscore the implications of proceeding with an inadequate protocol that defied the rigorous demands of the scientific community:

> The new procedures suggested by the Cardinal of Turin and that he has now embraced, will, if implemented, yield a result for the date of the Shroud *that will certainly be vigorously challenged by the world scientific community for their flimsy statistical basis*. We urge the Cardinal of Turin to seek scientific advice from an unimpeachable source that was available to him from the very beginning, but that he chose to ignore, namely the Pontifical Academy of Sciences, which

[64] *The Skeptical Inquirer* is a popular journal that investigates controversial, fringe science and paranormal claims. Contributors to the journal have often discredited shroud science.

> enjoys worldwide respect in the world scientific community. *Only with the best advice of world experts on carbon-14 dating can a scientifically credible date for the Shroud of Turin be arrived at.* (Gove, 1996, p. 219, emphasis mine)

They concluded by stating that "the Archbishop's plan, disregarding the protocol, does not seem capable of producing a result that will meet the test of credibility and scientific rigor...it is probably better to do nothing than to proceed with a scaled-down experiment."[65]

The press conference succeeded in shifting the carbon dating controversy into public view. Within this phase of a social drama, the crisis often extends beyond the "limited area of social interaction" to wider sets of "relevant social relations" (Turner, 1974, p.38). Often the controversy will become "coextensive with some dominant cleavage" in wider sets of dominant relations in society (p. 38). Indeed, media representations of the chosen three conflict articulated a "dominant cleavage" between the church and science. In particular, the crisis was publicly framed as a confrontation between scientific rationality and ecclesiastical obstructionism.[66]

In an article entitled "Shroud of Turin Controversy Resumes," *The Chicago Tribune* framed the controversy as a dissolution of the brief but amiable relations between science and religious faith, a "harmony" that that had been forged by the mutual commitments of the church and science towards resolving the shroud's provenance:

> Ten years ago, at the invitation of the Roman Catholic Church, science probed the Shroud of Turin and, failing to prove it a fake, *ushered in an era of harmony between the physical and the metaphysical.* Friday, in New York, with a new

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[65] Source: http://www.shroud.com/

[66] By "framing" I am referring to the ways in which media will deploy particular "sources, language and metaphors" to shape the narrative in particular ways or favor one perspective over another (Coleman and Dysart, 2005, p. 7).

> battery of tests pending, *the union suffered its first public breach.* Two prominent New York physicists called a press conference to charge that the church's decision to submit the shroud to a carbon-14-age-dating process at only three laboratories, rather than at seven as initially agreed, will render the test virtually meaningless and open the door to "endless, endless, bickering.'... The broadside fired by Drs Harry Gove... and Garmon Harbottle...*opened a scene charged with implications of intrigue, political maneuvering, broken promises, questioned motives and bruised egos that threatened to renew rather than settle the controversy over the authenticity of the shroud, called by the late Paul VI 'the most important relic in history*'...the battle is not the first over the mysterious shroud. Through the centuries, men have killed for it, died for it, and spent fortunes to possess it. (Clark, 1988, n.p, emphasis mine)

A similar theme appears in the following excerpts. Here the dispute was characterized in terms of the church's subversion of science:

> ...the Vatican announced in January that it would allow only three laboratories to take part in the tests, instead of the seven original planned...*New Scientist, a British journal, accused the Vatican of revising the testing protocol to produce an ambiguous result that would allow people to continue believing in the shroud is [sic] a genuine relic.* (Glass, 1988, p. 24A, emphasis mine)

> The Roman Catholic Church *stands accused today of altering an elaborate scientific protocol to test the age of the Shroud of Turin so that the results will be ambiguous*....with fewer laboratories involved, the results will be less reliable. (Anderson, 1988, p. 22, emphasis mine)

> The Vatican's decision to limit to three the number of labs that will receive fingernail-sized shreds of the shroud *has prompted some scientists and skeptics to accuse the Church of not wanting to see an unequivocal result. Scientists had originally proposed the test be done at seven labs to minimize the chance for conflicting, or inconclusive, results.* (Saltus, 1988, n.p, emphasis mine)

In the above passages, the public is presented with an antediluvian perception of the church's antagonistic relationship to science. Specifically, the message conveyed here is that that the church's decision to reject the original Turin protocols is rooted in its wariness of science and its vigilance over science's intrusion on its relics.

The controversy was similarly framed in terms of the church's obstructionism and its implications for stonewalling the rational (and now *critical*) scientific interpretation of the shroud's provenance. Three newspapers reported that:

> The Holy Shroud of Turin, believed to be the cloth in which Christ was wrapped in his tomb, *has again become the subject of a dispute between scientists and the Roman Catholic Church*...The controversy has erupted because of changes in plans to subject a fragment of the linen to tests by seven research laboratories that specialize in carbon dating methods of analyzing archaeological and ancient remains. *The changes were described by one of the experts yesterday as 'someone in Italy obstructing the true path of science.'* The comment was from Dr Robert Otlet, of the Atomic Energy Research Establishment, Harwell, Oxfordshire, and a specialist in one of the two methods of carbon dating that were to be applied to the shroud. (Wright, 1988, n.p, emphasis mine)

> Garman Harbottle, a senior scientist at the Brookhaven National Laboratory in New York, said *that because Catholic Church officials have ignored the recommendations of an international panel of scientists, researchers may not produce enough solid data to solve a puzzle at least 500 years old*: Whether the imprint of a crucified man on this linen can possibly be that of Jesus Christ. Harbottle developed one of the carbon-14 dating techniques that enabled scientists to study the relic without destroying it. The problem is that although a scientific panel recommended that at least seven laboratories be given samples of the shroud to date accurately, the church has decided to send out only three samples. (Kava, 1988, np, emphasis mine)

> Hopes of scientifically dating the shroud of Turin, which legend holds to have been Jesus' burial shroud, have taken a turn for the worse. The plans of seven laboratories to test the shroud by the radiocarbon, or carbon-14, method have been rejected by a representative of the archbishop of Turin. *His substitute plan has shocked the scientists and cast doubt on the enterprise... Scientists in the seven labs said the move undermines the confidence that could have been placed in the effort.* (Hilts & Rensberger, 1988, p. A04, emphasis mine)

Scientists made several public statements deploring the church's decision. In this crisis phase of the controversy, scientists used the public forum as a space in which they could underscore the perils of the church's decision to amend the original protocols. It is clear by the blistering rhetoric in the following excerpts that the public is asked to consider

how the church's decision imposes serious harm to the scientific credibility of the radiocarbon dating venture, and ultimately impedes a truthful narrative of the artifact:

> The decision outraged, among others, Dr. Robert Otlet, director of one of the rejected labs, the British Atomic Energy Authority's Isotope Measurements Laboratory at Harwell. The lab is a world leader in dating archeological finds. "*I think it's as much a catastrophe as it would be if you allowed bulldozers to go over an archeological site before you've examined it,*" he said in an interview...(Glass, 1988, p. 24A, emphasis mine)

> Harbottle, from Brookhaven National Laboratory in New York, and Harry Gove, director of the nuclear structure research laboratory at the University of Rochester in New York, held a press conference last Friday to say that the project should be stopped unless all seven laboratories participated and the protocol was restored. "*I hope the three laboratories stand firm [against the decision] and say to hell with you, let's get a result we believe in or leave it undated*" Gove told the New Scientist. (Anderson, 1988, p. 22, emphasis mine)

> ...Dr Otlet said *the decision to change the tests would lead to a scientific catastrophe.* It would mean that the results on the issue of the shroud's historical provenance *would be wide open to criticism.* (Wright, 1988, np, emphasis mine)

> Professor Harry Gove, the lab director who spearheaded the campaign to carbon-date the shroud, said in a telephone interview that restricting the tests to three labs and thereby eliminating a second carbon-dating technique would make the results "a lot less believable." "*There are a lot of people who think the shroud is a phony, so if the dating is a number that comes around 2,000 years, they're going to say there's been some kind of conspiracy,*" he said. (Glass, 1988, p. 24A, emphasis mine)

It is apparent here that once the chosen three controversy found its way into the public arena, scientific actors played a considerable role in framing the dispute in terms of a faith/science polarity, thus moving the conflict to a qualitatively different plain. Importantly, the high stakes rhetoric expressed by scientists in the public forum exemplifies my position that scientists' motivation for pursuing the authenticity question was often rooted in their conviction that science possessed both the authority and

obligation to do so. In other words, science *must be permitted* to resolve the shroud's ambiguities, and the obstacles that preclude this aim (i.e., the church) must be exposed and censured. Indeed, we see this conviction reach fever pitch through the interactive processes of the social drama.

This publicly expressed censure of the revised protocols also embodies what Turner calls "redressive" action, the phase in a social drama whereby members of the "disturbed social system" attempt to restore order, often through ritualized public "performance." As Turner explains:

> It is in the redressive phase that both pragmatic techniques and symbolic action reach their fullest expression. For the society, group, community, association, or whatever may be the social unit, is here at its most "self conscious" and may attain the clarity of someone fighting in a corner for his life. (Turner, 1974, p. 41)

If opponents fail in their redressive actions, an "irreparable schism" between contending groups, what Turner calls dissolution in "communitas," will ensue (p. 39). This is precisely how the social drama of the "Chosen Three" controversy came to a close. Despite efforts to forestall what Harry Gove described as a "death sentence" to the shroud, the revised protocols remained intact, and the appointed players were firmly in place (Gove, 1996, p. 241).

In his account of this defeat, Harry Gove expressed regrets that bear a remarkable likeness to those shared by Walter McCrone in his own reflections of the blood dispute. Recall that McCrone, who was also defeated in his efforts to safeguard the integrity of shroud science, lamented the fact that his efforts were ultimately constrained by the

vested interests of his rivals, and that much had fallen away in the process. Gove makes a similar contemplation:

> Far and away the saddest and most deplorable aspects of the whole shroud dating enterprise are, firstly, the fact that a group of estimable people...most of whom were colleagues and some of whom were personal friends, now have a distinctly different and more suspicious attitude towards one another and secondly, a scientific investigation that could have been exciting and challenging and, above all, a great deal of fun, has turned sour. None of us has come out of it whole and pure although some more than others. It is extraordinary to me that twelve people including me, each one of whom has a vastly greater scientific standing than Professor Gonella, should have allowed such a mean spirited person to call the tune to which all of us danced in one way or another. (Gove, 1996, p. 245)

What can be made of the endeavor to establish definitive evidence of the Turin shroud's provenance, and what accounts for the struggles precipitated by this venture? As I have indicated, the initial quest to carbon date the shroud signifies an assertion of science's epistemological and technical authority to claim the path to empirical certainty. In this context, the problem of determining the shroud's provenance could be readily resolved through measurement, a technique that is believed to ensure governance over phenomena. Ian Hacking(1983) reminds us that measurement, "so obviously a part of scientific life," is the technique by which science creates a "stable numerical phenomenon" over which to impose "remarkable control" (p. 233, 237). Radiocarbon dating is reductive, precise, reliable and unambiguous, and was therefore the ideal mechanism by which to transform the "ill-structured problems" of the shroud into "well-structured" resolution (Star, 1983, p. 224).

Assigning the provenance question to the "authority of the machine" also speaks fundamentally to the hubris of technique to reduce, simplify and ultimately resolve complex phenomena (Pauwels, 2006). Indeed, Bruno Latour (1986) contends that the

surest route to resolving the myriad complex questions in science and settle the debates precipitated by them is to "look to the inscription devices," or the instruments through which phenomena are rendered intelligible (p. 161). As he observes:

> No matter if people talk about quasars, gross national products, statistics on anthrax epizootic microbes, DNA or particle physics; the only way they can talk and not be undermined by counter-arguments as plausible as their own statements is if, and only if, they can make the things they say they are talking about easily readable. No matter the size, cost, length, and width of the instruments they build, the final end product of all these inscription devices is always a written trace that makes the perceptive judgment of the others simpler. (Latour, 1986, p. 161)

But the road to empirical certainty is a meandering one, and it is paved with conflicting ambitions. How might we further construe this investment in the provenance question, the struggles provoked by it, and the discordant interests drawn into the fold?

Material objects are enlivened and defined by the narrative frameworks in which they are situated. They are, as Rom Harre (2002) argues "bound" into narratives. In his article "Material Objects in Social Worlds", Harre contends that:

> Nothing exists in the social world unless it has been introduced into that world by a human social and constructive act. By that I mean an act which fixes the category of the being in question...an object is transformed from a piece of stuff definable independently of any story-line into a social object by its embedment in a narrative. (Harre, 2002, p. 24-25)

According to Harre, things and discourses (or what he calls "social objects") are often bound in contradictory narratives. The narrative of disease, for example, can be simultaneously a "medical" and "lay" narrative (in most cases the technical grammar "displaces" lay grammar). Harre's point is that the meaning of social objects relies upon the "symbolic order" in which the object is defined. The relevance of Harre's insights is

that the shroud's ontological status has, much like any object, always been connected to the narrative of meaning constructed around it. Throughout its history the shroud has been a divine representation of the resurrection, an object of veneration, and an object of scientific interest. In other words, the artifact bears multiple "affordances", or different ways in which it can be used and interpreted (Harre, 2002). The carbon dating venture signifies an attempt to construct a coherent scientific narrative about the shroud, and by doing so, situate the artifact within a plausible historical trajectory. But *who* gets to weave the narrative?

Merton (1973) has argued that the "priority of discovery" in science – the claims of custodianship over inventions and ideas, has precipitated a long history of acrimonious conflict among practitioners seeking intellectual distinction (Merton, 1973). The radiocarbon tensions signify what I call a "priority of narrative" dispute, where the tensions were rooted in the question of who would claim and control the path to closure, and who could claim the authority and expertise to shape, govern and disclose the scientific narrative of this artifact. As I have noted, Gove's attempts to prevent STURP's involvement in the carbon dating venture signified an effort to dispel what he saw as a growing fiction about the shroud. The church's decision to reconfigure the carbon dating procedures and control the central players signifies another act of governing the shroud narrative and its authors. Gove's and Harbottle's charged public resistance represents a third move to reclaim the narrative of the shroud, and denounce the actors who threatened to create a specious story. In light of this, it is possible to understand why the shroud, an

object of seemingly negligible scientific value, emerged as an object of epistemic and political consequence.

Conclusion

In this chapter I have explored the impetus and the underlying conditions of the radiocarbon dating disputes. My purpose has been to shed interpretive light on the dynamics of this controversy, and the motives and strategies deployed by social actors entrenched in this conflict. What is compelling about the priority of narrative dispute is that the contenders were essentially competing to tell one story: the historical truth of the shroud. None of the social actors were attempting to make any radical claims about the artifact other than to determine its provenance. Interestingly, there was a unity of goals in this dispute that was fundamentally displaced by issues of control. As shroud historian Ian Wilson notes, "had the three chosen laboratories held their nerve and insisted that the original Protocol be maintained, history might have been very different. But...the prize was too great" (Wilson, 1998, p. 184).

Chapter Five: Demystifying the Shroud: Closure and Post-closure

> "...it is a shame that science gets involved in the testing of holy relics. It is like the loss of innocence in the garden of Eden. But once the question comes up, science has a responsibility to provide the answer." -Robert Hedges, Oxford Radiocarbon laboratory.[67]

Introduction

What, then, did the years of credibility contests, epistemic rivalries, and political wrangling over a sacred artifact amount to in the end? In this chapter I describe how the results of the radiocarbon dating of the shroud rendered a final verdict concerning its historical status, and purportedly brought centuries of debate to an abrupt end. Although the debunking of the shroud brings us now to the "moment of truth" in my narrative of the shroud controversy, the focus of interest here is not the brief closure wrought by this event. [68] Rather, I examine how and why the radiocarbon dating results precipitated a collective resistance on the part of members of the shroud science community, who denounced the validity and ethics of the radiocarbon dating venture on several grounds. I argue that this eruption of post-closure dissent cannot be simply interpreted as an adversarial resistance to the carbon dating results. Rather, the dissenting response of the

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[67] Rufford, N. (1988, August). Turin shroud: Vatican steels itself for `fake' result. *The Sunday Times*, n.p.

[68] Some clarification is useful here. I employ the terms "debunking" and "demystifying" largely because these are the term of reference most often used to describe the carbon dating results (particularly in media accounts). But it seems to me that to truly "debunk" the shroud would imply that a prevailing "truth" about it has been proven incorrect (in the way, for example, that Walter McCrone debunked the Vineland Maps, long claimed by Yale researchers to a pre-Columbian map of the Norse exploration of America). As I have shown in this case study, however, there has never been absolute or widespread consensus on the true status of this artifact. As such, it is more appropriate to suggest that the carbon dating result verified that the shroud was nothing other than a medieval relic.

shroud science community reveals the extent to which science embraces uncertainty as intrinsic to the creative repertoire of its practices and interests.

I conclude this chapter with a discussion of how the dynamics and character of the shroud controversy is in many ways consistent with the types of socio-scientific disputes that have comprised the analytic interests of science studies practitioners, but I also foreground the paradoxes and singularities of the shroud conflicts that contribute new insights with respect to how we might understand the nuances and complexities of controversy.

God is Not in the Details: The 1988 Radiocarbon Dating Results

Exactly ten years after STURP scientists gathered around the Turin shroud in the Palace of the House of Savoy, a new group of scientific and non-scientific actors convened to bear witness to a different kind of intervention on the artifact, and in pursuit of a different question. On April 21, 1988, the Turin shroud was removed from its reliquary and brought into the Sacristry of the Turin Cathedral, where Cardinal Ballestrero and Professor Luigi Gonella waited to supervise the sample extraction procedures. Also present were textile experts Franco Testore of Turin's Polytechnic's Department of Material Science, and Gabriel Vial from Centre International d'Étude des Textiles Anciens in Lyon. Michael Tite of the British Museum, and representatives from each participating laboratory (Professor Paul Damon, Professor Doug Donahue, Professor Ted Hall, Dr Robert Hedges and Professor William Woelfli) were also in attendance. Professor Giovanni Riggi was appointed to extract the sample.

The shroud was separated from its backing cloth and Riggi removed a sample 7cm long by 1cm wide from the bottom left edge of the cloth. Once removed, the shroud samples were divided into three equal pieces weighing fifty milligrams each and then taken into an adjacent room, wrapped in foil and sealed by Ballestrero and Tite in numbered steel containers. The lab representatives were given the samples along with three control samples. They were not told which cylinder contained the shroud samples. The control samples comprised linen from a tomb excavated at Qasr Ibrîm in Nubia, linen from the collection of the Department of Egyptian Antiquities at the British Museum, and threads from the cape of St Louis d'Anjou, which is stored chapel in the Basilica of Saint-Maximin, Var, France.[69] All of the operations, except for the wrapping of the samples, were documented on film (Damon et al., 1989).[70]

Once representatives returned to their respective labs, each group subdivided the samples and treated them with mechanical and chemical cleaning procedures to remove any contaminants, and then subjected all samples to radiocarbon testing. Approximately five months later, the results were forwarded to the British Museum Research Laboratory for statistical analysis.

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[69] Because the shroud bears a distinctive herringbone weave that could not be matched by the control samples, the laboratories were able to identify the shroud samples. As Damon et al. (1989) noted in the final published report, "If the samples had been unravelled or shredded rather than being given to the laboratories as whole pieces of cloth, then it would have been much more difficult, but not impossible, to distinguish the shroud sample from the controls. (With unravelled or shredded samples, pretreatment cleaning would have been more difficult and wasteful.) Because the shroud had been exposed to a wide range of potential sources of contamination and because of the uniqueness of the samples available, it was decided to abandon blind-test procedures in the interests of effective sample pretreatment" (Damon et al., 1989, p. 614).

[70] No explanation has ever been given for why representatives chose not to document this part of the operations on film. Harry Gove notes that this "flawed procedure" compelled conspiracy theorists to suggest that the samples had been substituted (Gove, 1996, p. 261).

On October 13, 1988, Dr. Michael Tite, Oxford's radiocarbon technician, Robert Hedges and Professor Edward Hall, held a press conference at the British Museum and announced to the world that the radiocarbon tests indicated with a 95% confidence level that the Turin shroud was only six to seven hundred years old. In what has become an iconic image in shroud science history, the three men stood before a blackboard on which was written "1260-1390!", the calibrated calendar age range yielded by the radiocarbon dating. Following the release of the results, Professor Hall gave a lecture at the British Museum entitled "The Turin Shroud: A Lesson in Self Persuasion." In it he stated that in view of the radiocarbon dating evidence, anyone of "scientific worth" who still believed the shroud to be authentic might well join the "Flat Earthers" (Wilson, 1998, p. 185).

Cardinal Ballestrero also held a press conference in Turin that same day and stated, "I see no reason for the Church to put these results in doubt" ("Shroud of Turin Only", 1988, np). He further indicated that, "by entrusting the evaluation of these results to science, the Church confirms its respect and veneration for this icon of Christ" (Boyes, et al, 1988, np).

The outcome of the radiocarbon dating tests generated considerable international media attention. Some newspaper reports emphasized the prowess of modern science, proclaiming that the shroud had been dealt a "serious blow" and that the "age of mystery" had effectively been brought to a close (Priestland, 1988, n.p; Steinfels, 1988, p. A10; , n.p, 1988). *Time* magazine reported that the carbon dating tests "ended the most intensive scientific study ever conducted on a religious relic" (Ostling et al., 1988). The theme of scientific conquest is also apparent in the following excerpts:

> ...*the collapse of the Turin fantasy* ought to be welcomed by all serious believers; not least by the Italian hierarchy who might otherwise have been stuck with the greatest object of superstitious idolatry since the true cross...a believer might say that it is the church, and not the shroud, which has been put to the test and has been delivered from temptation. (Priestland, 1988, n.p, emphasis mine)
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> ...it was the enthusiasm of certain scientists who first brought the shroud out of relative obscurity at the end of the 19th century and the set the level of interest on course towards its 1988 crescendo. *Now science knocks it down*...it can never be proved that a particular relic is genuine, though it may be provable that it is not. *That is the fate that has befallen the Shroud.* It is a warning to the Roman Catholic Church, if a warning is needed, never to make the mistake of investing relics with evidential power or supernatural properties. ("Spirit of the Shroud," 1988, n.p, emphasis mine)
>
> It had taken decades of convoluted and controversial effort, and the test, according to once scientist, represented, "the most significant interaction of the Church and science since Galileo. (Sox, 1988, n.p)

But while many newspapers declared that science had exposed the shroud as a "holy red herring," a "fraud," a "fake" and "a good forgery," some media discourse focused on the "enduring mystery" of the shroud image, notwithstanding its seemingly diminished status as an archeological relic. As one newspaper reported, "the Shroud of Turin doesn't seem likely to disappear into obscurity following the publication of the dating tests. It should survive indefinitely. However it came to be, it remains an enduring work of mystery and beauty" ("The Shroud Keeps," 1988, p. 16). Another article noted, "the results of the carbon dating tests conducted by three laboratories ended a debate over the origin of the cloth that has raged for hundreds of years. But another mystery remains unsolved: how the shroud came to bear the image of what scientists have called a real human form of a whipped and crucified man" ("Archbishop of Turin Agrees,"1988, p. A20).

As noted, while Cardinal Ballestrero stated to the media that the church accepted the carbon dating results, he simultaneously declared that "the tests do not close the book on the shroud," and that "the problems of the origins of the image and of how it was conserved remain in great part unsolved and require further research and studies" (Suro, 1988, p. A1; Gruber, 1988, p. A20). The Cardinal then emphasized that the scientific findings would have no bearing on the spiritual significance of the artifact to the faithful, inasmuch as "the value of the image is pre-eminent with respect to the eventual value of the historical evidence" (Gruber, 1988, n.p). Ballestrero's advisor, Luigi Gonella, publicly objected to reports that the shroud was a forgery, stating that, "a forgery means it was made for the specific purpose of deceiving people. This is possible, but there is no proof of that. It could be a medieval icon" ("Archbishop of Turin Agrees," 1988, p. A20).

We are by now familiar with the various terms and conditions by which controversies are thought to find closure. Although controversies rarely achieve "pure closure," provisional closure is typically rendered by the intervention of other authoritative bodies (e.g., legal or governmental), the usurpation of interests by powerful actors, the collapse of divisions among contenders, consensus within scientific communities, or simply the natural death of a controversy. Nelkin (1995) reminds us, however, that controversies concerning profoundly conflicting "visions of the world," or those deeply entrenched in rival belief systems are rarely terminated.

What is compelling about this moment of closure in shroud science history is the distribution of actors and their alliances, and how this distribution disrupts our expectations about the boundaries implied by closure. Closure doesn't inevitably seal the

anticipated boundaries among contenders nor does it foreground the conflicting world views among actors. Given that the carbon dating results purported to provide epistemic closure, the social actors we would most likely assume would resist the findings would be religious authorities who perhaps had the greatest stake in the outcome. Yet the church aligned quite readily with the scientific findings. There was here, both scientific and religious closure around the outstanding question of the shroud's historical status, and an alignment of two domains whom we might have expected to be in tension over the claims and implications of the results. Interestingly, this moment of scientific closure was functional - it provoked the church to quickly assign a new symbolic meaning to the artifact such that its status wasn't inexorably diminished by its scientific "unshrouding." The church, in reminding believers that the shroud remains a venerated object and a commanding representation of the Passion, re-elevated the artifact to a new symbolic level.

End of the Line Verdict?: Post-closure and Collective Dissent

What exactly was accomplished by demystifying the Turin shroud? Certainly the carbon dating results assigned a reliable historical narrative to the artifact, which, as I argued in the previous chapter, was the impetus of the carbon dating venture and the source of its divisiveness. For some, the results offered a measure of public acclaim. For others, they provided professional vindication. Walter McCrone, the dissenter in the Great Blood Dispute, stated publicly, "my name used to be mud in the church. I think now I have a little more credibility" (Ritter, 1988, n.p).

But the much sought after "end of the line verdict" did not evoke a sense of vindication for *all* of the actors who struggled in this controversy. Let us return for a moment to our central protagonist. Although Harry Gove did not dispute the veracity of the carbon dating results, and indeed considered the outcomes to be "a triumph for AMS," he also made a paradoxical and indeed quite compelling observation:

> Although I had known for five months that some date close to this was the answer, I still felt pangs of regret. *Regret that I had been largely responsible for this iconoclastic result and regret that there was now no possibility that the Turin Shroud was Christ's burial cloth.* I had really hoped in my heart for a first century date although my mind told me that was bordering on the impossible. (Gove, 1996, p. 287)

What can we make of this reflexive moment in Gove's contemplations of the carbon dating outcomes? Certainly we are compelled to question, at the outset, the extent to which hubris is truly the emblematic disposition of the scientist. Recall that John Heller, the STURP team's blood chemist, expressed a comparable moment of humility in his admission that the shroud science ventures brought into sharp relief the "dimensions of our ignorance" (Heller, 1983, p. 38). For Heller, the STURP team's "mutual sin" of "scientific hubris" was wholly disrupted by their failure to resolve the shroud's ambiguities, notwithstanding the command of their collective expertise (p.1, 38). Gove's remorse, caused not by failure but by *triumph*, yields a similar moment of humility and regret.

This is not to suggest that scientists do not weigh the merits and liabilities of their work, or contemplate and regret their wider implications. Certainly one of the most recognized admissions of remorse was made by J. Robert Oppenheimer, the "father" of

the atomic bomb, who evoked the Hindu verse, "Now, I am become Death, the destroyer of worlds" (Thorpe, 2006, p. 12). While Gove's role in demystifying a religious relic clearly did not impose consequences of any real comparison, his regret in becoming an "iconoclast" seems no less profound.

My point here is that Gove's reflections suggest that in the context of this controversy, closure summoned unintended effects, or at least unanticipated moments of doubt. In chapter three, I argued that the blood conflict was underlined by scientists' affective commitment to the artifact itself, a commitment that entailed disclosing its truths as well as being truthful to the artifact. Gove's ambivalence here reveals a similar kind of reverence toward the object, where the act of disclosing its historical truths compels him to question whether such knowledge needed to be made manifest. Did this knowledge entail a betrayal rather than an offering? I am reminded here as well of Akrich's et al., (2002) study of technical innovation, where she argues that the moment of innovative success often evokes a state of despondency among the social actors involved in the process. This feeling of ambivalence, analogous to the "depression which takes over after childbirth" is due in part to "this bizarre feeling...did we really want this? Who really wanted it?" (p.194).

But clarifying the provenance of the shroud with an unequivocal numerical measurement did not effectively seal the proverbial black box and dissolve the debates, even though initially it appeared to do so. For the scientists who assumed both direct and supportive roles in dating the shroud, there was little question that the results authoritatively settled the only critical problem worth pursuing. In establishing the

provenance of the shroud, this final scientific intervention seemed to foreclose any further debate on the issue of the shroud's origins, at least within serious scientific circles. The results also seemed to preclude any future scientific interventions on the shroud of any kind. Recall that Edward Hall of the British Museum declared that any scientist who doubted the results should join the "flat earthers." Science studies is familiar with the implications of this kind of polarizing disposition in moments of closure. As Simon (1999) notes, when some controversies are considered irrevocably closed by the scientific community, any further epistemic investment in the problem is "pathologized" by normal science, and the issue is relegated to the "crackpots, pseudo-scientists, frauds and a few sociologists of science" (p. 73). One might ask why it is necessary to pathologize opposing views, if, in fact, science is certain in its knowledge claims. Is it possible that pathologizing betrays traces of doubt, or a concern that a closed issue could be re-opened? In the context of this controversy, such pathologizing could also be construed as a scare tactic to other scientists – a threat that they will be condemned to the ranks of flat earthers.

In a 1989 article published in *Radiocarbon,* Harry Gove stressed that any future scientific testing could only be justified if "authorized by a high level scientific commission appointed preferably to the President of the Pontifical Academy of Sciences" (Gove, 1989, p. 968). Gove reinforced this point by cautioning that it was incumbent upon shroud custodians and serious scientists to remain vigilant in the face of any proposed scientific interventions of dubious legitimacy. Here he made a caustic reference to the STURP team's work, noting that "in particular, any further 'scientific'

measurements of the kind carried out in 1978 by self-appointed religious zealots should be studiously eschewed as, of course, they should have been all along" (p. 968). Interestingly, Gove's suggestion that the shroud should be guarded against the interventions of "zealots" implies, perhaps that scientists must now join the ranks of the "keepers" of the artifact, as "custodians" of its scientific truths. As with many religious relics, the shroud has always been strictly guarded by its owners and curators. Perhaps Gove believed that since scientists left their own mark on the shroud, the artifact has in some way come into their partial possession, leaving them entrusted with its careful guardianship.

But while the carbon dating outcomes appeared to achieve *epistemic* closure, the matter was far from settled, and the black box of shroud science sprung open soon after the results were announced. Borrowing a conceptual idea from Michal Lynch's (1998) work on legal disputes concerning forensic evidence, the post-closure resistance within the shroud science community was underlined by a "discursive production of uncertainty," where detractors deconstructed the evidence by exposing the myriad contingencies and interpretive flexibilities of the carbon dating enterprise (Lynch, 1998). Specifically, a discourse of collective dissent, embedded squarely in the public forum, called into question the legitimacy of the carbon dating evidence on several grounds, as well as the implications of specious scientific claims for misleading the public and the scientific community. Opponents also scrutinized the ethical credibility of the scientists themselves, and exposed the subversive politics of the radiocarbon venture.

Many dissenters within the shroud science community raised concerns about the viability of radiocarbon dating as a fail-safe method to establish accurate dates of archeological relics. In light of the fallibility of carbon dating, opponents believed that there were serious implications to reducing the provenance question to a single method of calculation. In his 1990 book *The Shroud and the Controversy,* STURP team member Ken Stevenson vehemently denounced the carbon dating results on these very grounds:

> The recent carbon 14 dating has raised more than a few concerns. Touted far and wide as proof that the Shroud is a hoax, this late [sic] addition to the Shroud data bank is not all what it is cracked up to be. In short the C14 data flies in the face of *all* the other data and yet is expected to stand by virtue of its name alone – in spite of the fact that most scientists will readily admit that C-14 is not infallible...the dating as it has been presented to the public (with limited and secondhand facts at best) is severely flawed and in fact proves nothing...if we are to arrive at a solid conclusion concerning the age of the Shroud, C14 is not nor should it be the acid test of the Shroud's possible authenticity... At the worst it has introduced inaccurate data that have been permitted to jeopardize the search for sound answers to the questions surrounding the Shroud's authenticity. In both instances, the 1988 C-14 tests did little to advance scientific study of the Shroud. (Stevenson & Habermas (1990, p. 44-51, 59)

Shroud scholar Ian Wilson bemoaned the "sheer hubris" of the radiocarbon labs for believing that "they alone possessed the key that would unlock the answer to the Shroud mystery" (Wilson, 1998, p. 194). In his book *The Blood and the Shroud,* he argued that the results did nothing more than to create a misguided trust (particularly on the part of the public) in the authority of carbon dating to resolve an intensely complex problem. Wilson further objected to the unilateral dismissal of all of the scientific evidence that preceded the carbon dating venture, and implied that the shroud was co-opted by the carbon dating labs:

> ...I most seriously question the near-infallibility that almost everyone, including

> scientists, journalists and lay people, has imparted to [the C-14] result. It is as if, because it had been produced by a nice, clean, high tech method, and by highly professional hard-nosed scientists claiming margin of error of little more than a hundred years, everyone must blind themselves to everything else that had been deduced regarding the Shroud and accept this single scientific test as the ultimate arbiter, overriding all else. (Wilson, 1998:188)

Archeologist William Meacham (the pro-authenticity advocate who, as discussed in chapter three, denounced Walter McCrone's work) also expressed serious concerns about the 1988 results. Indeed, Meacham had been a vocal critic of the carbon dating venture for years.[71] In a 1986 symposium on the shroud he argued that:

> There appears to be an unhealthy consensus approaching the level of dogma among both scientific and lay commentators, that C-14 dating will "settle the issue [of the shroud's provenance] once and for all time". This attitude sharply contradicts the general perspective of field archaeologists and geologists, who view possible contamination as a very serious problem in interpreting the results of radiocarbon measurement... even among social and physical scientists, there are numerous misconceptions about the radiocarbon method of dating; among journalists and the general public there are of course many more. But among specialists who frequently make use of the test, it is not considered as a method which produces an "absolute date" for every sample that can be measured... there is exhibited a lack of awareness of the pitfalls and uncertainties inherent in the C-14 method. (Meacham, 1986, p. 1, 4-5)

Meacham was also deeply critical of Harry Gove's original 1978 radiocarbon dating proposal, calling it "seriously flawed" by virtue of its "lack of consultation with archeologists and experts in the field" (p.2). He was equally critical of the STURP team's carbon dating proposal, charging that STURP scientists had failed to carry out adequate

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[71] Given these concerns, it is possible that Harry Gove' disappointment referred to earlier has to do with the fact that a first century date would have created a happier ending to the controversy, and perhaps advanced the credibility of carbon dating for those who doubted its effectiveness.

research "to the degree warranted by the object to be dated" and that the proposal lacked "significant input from a range of scholars" (p. 2).

Meacham's central objection to the dating results was that the shroud had been in circulation for centuries and was therefore likely to be contaminated by natural and human-made substances. Such contaminants, he argued, might have seriously corrupted the measurements. In a public debate with a carbon dating expert carried out in an online forum, he argued:

> When I attended the 1986 conference in Turin for planning the C14 dating of the Shroud, at the invitation of the Vatican Academy of Sciences, I argued strongly for an extensive testing program (including various staining and microscopic studies) that would have examined the Shroud samples in detail for contamination. This was met with arrogant dismissal by 5 of the 7 radiocarbon lab heads in attendance. They ridiculed the notion that contamination could account for more than 1 or 2% of the C14 after standard pretreatment.... the truth is that there are many possible sources of error which are not fully understood, and it simply behooves us to at least look for all the possibilities that we can.... *I always reiterate that C14 dating is a very useful technique and that most dates are reliable. But the fact remains that some are not, for reasons unknown.* (Sparks & Meacham, 1998)

Meacham would later go on to publish a scathing exposition of the carbon dating venture in a book provocatively titled *The Rape of the Turin Shroud: How Christianity's Most Precious Relic was Wrongly Condemned.*[72]

Another major objection raised by detractors concerned the carbon dating procedures themselves, and specifically the absence of blind testing of the samples. As noted earlier, the three labs were given a sample of the shroud and three control samples. As reported later in *Nature*, the labs representatives indicated that based on the distinctive

[72] See Meacham, W. (2005). *The rape of the Turin shroud: How Christianity's most precious relic was wrongly condemned.* Lulu.com.

herringbone weave of the shroud, they were able to immediately identify the shroud sample. Dissenters held this up as a serious breach in basic scientific testing protocol, and censured the labs for failing to adhere to the fundamental methodological conventions of science. Ken Stevenson noted, "to my horror I discovered that the labs openly admitted they knew when they were dating the Shroud...a true scientific blind study was never conducted..."(Stevenson & Habermas, 1990, p. 51, 57).

Several additional concerns were raised in the post-debunking discourse, the most prevalent of which can be summarized as follows:

Dating Anomalies: Critics suggested that the three labs failed to acknowledge a possible incongruity between C-14 dating and calendar years, a discrepancy known as "secular variation". Another anomaly cited by detractors was the "Devries effect," whereby radiocarbon testing can yield more than one date (Stevenson & Habermas, 1990). As Ian Wilson (1998) noted, "although radiocarbon-dating laboratory scientists are notoriously chary of admitting it, carbon-dating can produce results with errors considerably wider than their quoted margins" (p. 192).

Contaminations: Critics charged that the sample, extracted from the frontal bottom corner of the shroud, was too heavily contaminated by centuries of human handling to yield a reliable C14 date (Morton, 1988; Wilson, 1998). In 1996, two microbiologists forwarded a different contamination theory, arguing that the shroud might have been mis-dated due to a bacterial contamination found on a small remaining sample of the original fibers taken in 1988.Their microscopic examinations revealed that the fibers were coated with a "bio-plastic" veneer, a substance produced by bacteria and fungi,

which is often found on ancient textiles and artifacts (Gove, 1996).

Sampling errors: In 2002, STURP team chemist Ray Rogers published a critical review of the carbon dating procedures and argued that the labs had used an invalid sample. Calling this indiscretion "a major disaster in the history of Shroud studies," Rogers argued that the sample extracted in 1988 was not part of the original cloth but was extracted from a damaged section of the linen that had been repaired in the 16th century (Rogers, 2002, p. 13).

In 2003 Rogers obtained samples of warp and weft threads taken from the 1988 radiocarbon sample site by Professor Luigi Gonella. Drawing his own independent microscopic and microchemical analyses, Rogers found that the threads contained cotton and were heavily coated with dye. This finding was significant for casting doubt on the 1988 findings, because it confirmed that the samples taken were not from the original area.[73] In 2005, Rogers published his results in *Thermochimica Acta* challenging the 1988 carbon dating results and concluding that "the shroud is between 1300- and 3000-years old. Even allowing for errors in the measurements and assumptions about storage conditions, the cloth is unlikely to be as young as 840 years" (Rogers, 2005, p.192).

The "Chosen Three" re-visited: Opponents criticized the decision to reduce the testing labs from seven to three. Although Gove did not dispute the results of the radiocarbon tests, he nonetheless reiterated his concern about the exclusion of the four labs. In a letter to the editor in *Archaometry*, he noted that the revised protocols had left the project "fraught with peril" and that the inclusion of other laboratories would have "obviated this

[73] In 1532, a fire in the Sainte Chapelle, Chambéry damaged the shroud in several places. In 1534 the "Poor Clare Nuns" added a new backing and patches to the cloth. The backing is known as the "Holland Cloth."

potential risk" of an outlier result (Gove, 1989, p. 237). The elimination of the four labs also compelled critics to question the ethics and motives of the chosen labs. Shroud scholar Ian Wilson suggested, for example, that Oxford laboratory's Edward Hall's agenda was to "drum up maximum publicity" in order to secure future funding for his lab (Wilson, 1998).[74]

I opened the previous chapter with a quote that appeared in *Nature* in 1989, where scientist Phillip Ball calls the attempts to date the shroud a "great game." Certainly the struggles and antagonisms of the carbon dating enterprise can be likened to a rather brutal contest, inasmuch as the social actors in this controversy found themselves ensnared in a bitter competition over how to carry out the task of settling the shroud's provenance. Indeed, much like the controversies reviewed in chapter one, the carbon dating controversy evolved into a decidedly elaborate array of disputing interests and ambitions, relations of power, and epistemic, political and territorial clashes. Ball further suggests that the scientific study of the shroud, in being analogous to the scientific search for God, "does more to inflame debate than settle it." The carbon dating controversy was not, of course, a matter of scientists seeking to settle the fundamentally irresolvable question of the shroud's correspondence to the historical Jesus. Had the carbon dating results arrived at a date consistent with the time of Christ's death, the findings would have been compelling, but would have still have proven nothing concrete about the identity of the man in the image. As I have shown, the antipathies were rooted in the question of who

[74] Hall's ambitions were apparently rewarded. In 1989 the Oxford laboratory was allegedly awarded a million pounds in private funding, and Dr. Michael Tite was granted a permanent professorship (Wilson, 1998).

would claim the authority to shape, govern and disclose the scientific story of this artifact. I have suggested that such a contest sheds light on how an object of seemingly marginal scientific value became the focus of profound disputation, and why the disputes were, at times, as intensely politicized and rancorous as controversies involving issues of great social consequence.

But while the carbon dating results offered *epistemic* closure, the evidence neither terminated the debates nor resolved the myriad outstanding questions about the artifact. Indeed, as discussed, the carbon dating enterprise provoked an adversarial discourse within the shroud science community that endures to the present. Skeptics responded to the carbon dating results by subjecting them to a deconstructionist inquiry, and evoked the same issues of contention leveled by their opponents. Where dissenters like Gove denounced the pre-debunking science of the shroud as biased, contrived and specious, the post-closure discourse made comparable denunciations, calling the carbon dating enterprise unethical, carelessly executed, unreliable, and flawed.

As noted in chapter one, Bart Simon (1999) has argued that closure in controversy is often stabilized when one group translates the claims of their opponents into "fictions" or "artifacts." The cold fusion controversy terminated with cold fusion being branded a "fiction" by the scientific community, who considered the claims of its proponents unfounded. In the context of this controversy, we see a similar process, with some exception. Here both sides of the provenance question rendered each other's evidence into fictions, or "artifacts" of the artifact. Thus despite all of the exertions to establish a reliable and objective interpretation of the shroud's provenance, it would seem that

ambiguity prevailed over certainty, such that the shroud has emerged again as an object inscribed with manifold interpretations.

It is possible to draw a straightforward conclusion about the fact that the carbon dating results, rather than settling the debates in the shroud science community, provoked a counter-discourse that continues to the present. We might simply surmise that the conflicts leading up to the debunking of the shroud created such deep divisions, antipathies and ill will among the various contenders that no one was willing to concede, unconditionally, to the final results of the carbon dating venture. Perhaps some opponents were indeed lured by the mystical trappings of the artifact, and could not fathom its demystification.

While there are perhaps some elements of truth to this conclusion, the post-closure backlash invites a less cynical interpretation. I propose a different way of understanding why a discourse of uncertainty followed so quickly and forcefully in response to the truth claims of the debunking enterprise. I suggest that the post-closure dissent reveals a "secular" reverence for the ambiguities of the artifact - a kind of opposition to the idea that science can or should inexorably extinguish the shroud's enigmatic qualities. Note that this signifies an important *fluctuation* in the flow and dynamics of this controversy. Thus far I have argued that the evidentiary disputes among shroud scientists were in part compelled by a conviction that science possessed both the authoritative capacity and obligation to impart a truthful account of the shroud. Moreover, I have argued that the conflicts were compelled by scientists' deep-seated commitment to assigning a trustworthy scientific narrative of the artifact. Accordingly,

the struggles within the shroud science community involved both practical and rhetorical actions on the part of contenders in an effort to preclude any spurious narratives of this artifact. The carbon dating results purportedly signified a moment of truth in the protracted disputes over the shroud's ontological status. But in this moment, the post-closure resistance denied that a single, reductive narrative is possible, decisive, or even desirable.

There are therefore multiple facets of science foregrounded by this controversy. We saw, for example, science as the agent of epistemic certainty in the face of ambiguity, particularly in the context of Harry Gove's performative dissent. In another context, scientists revealed their affective susceptibilities, particularly when we consider scientists' ethical commitment to being truthful to, and about, the shroud. Within the post-closure phase of this controversy, we see science embracing uncertainty as inherent in the creative repertoire of their practices. Certainty and ambiguity are, in fact, not incompatible in science. Ambiguity is not only profoundly appealing in science, it is thought to be consistent with its character (notwithstanding Harry Gove's archetypal characterization of the ethos of science). Moreover, the "essence" of certainty is both objective and subjective. As William Beyer's (2011) notes, "though certainty promises to do away with ambiguity, certainty itself is ambiguous" (p. 51). Thus in his account of what he calls the "mythology" of certainty in science, Beyers argues that the "certain, the precise, and the predictable" are fundamentally subordinate to wonder, mystery and creativity in science, to the extent that:

> The science of certainty…is derived from the science of wonder. That is why Einstein speaks of the mysterious as being at the cradle of true…science. The

> science of wonder has room for both certainty and uncertainty...the role of ambiguity in science is akin to the role of metaphor in poetry. Metaphor is where the richness of poetry resides, and in science it is what accounts for the depth of scientific ideas and concepts. (Beyers, 2011, p. 40, 89)

The Shroud Controversy: Congruencies and Disparities

Controversy analysts have placed interpretive weight on disputes in which social actors have a moral, political, ideological, pragmatic, economic or epistemological stake. Thus scientific controversies generally embody certain characteristics - their dynamics and constituents include differential relations of power, conflicting interests, clashing value systems, the imposition of techno-scientific authority, and the risk, resistances and struggles engendered by this authority. These are, of course, crucial analytical priorities, and are indispensible to understanding the critical and complex intersection of science and the broader social world.

I have suggested that the Turin shroud controversy has perhaps been overlooked in the field because the implications of this particular battle are relatively inconsequential to the pragmatic and political concerns of controversy studies and STS more broadly. My aim in this dissertation has been to challenge the definitional restrictions of what "counts" as a scientific controversy, as well as the empirical priorities of the field. In broadening the empirical focus of controversy studies, I have sought to draft this particular battle into existing sociological dialogues and argue for its importance as a social phenomenon. This case study has demonstrated that the shroud conflicts disclose both anticipated and paradoxical elements of controversy that are worth analytical attention and are often not fully articulated by existing interpretations of controversy studies.

Further, I have sought to train a sociological gaze on the shroud controversy in order to understand its dynamics and nuances beyond the standard and limited cultural understandings of this complex dispute. My research has shown that the shroud controversy is generally construed in terms of rather narrow demarcations between science and religion, science and pseudoscience, skeptics and believers, or winners and losers. My general position has been that this controversy cannot be unproblematically interpreted in terms of any rigid dualistic categories. I don't mean to suggest that a sociological analysis imparts a more precise or authoritative interpretation than the existing cultural perceptions of the shroud controversy. Rather, I propose that as social analysts we are obliged to approach social phenomena as excavators employing different maps and tools, turning stones that might have remained unturned, and offering a distinct conceptual understanding of the phenomenon in question.

This case study has demonstrated that there are elements of the shroud conflicts that correspond quite well to what we might anticipate in conventional scientific controversies. The social actors entrenched in these disputes have competed, dissented, strategized, engaged in boundary work, enrolled allies, "constructed" antagonists, made rhetorical appeals to the public and otherwise conflicted in often foreseeable ways. This controversy, like most, was at times circumscribed by relations of power among actors and institutions, the various vested interests of contenders, politicized decision-making, territorialism over place and object and the involvement of institutions such as the media who arbitrated, and to some extent escalated, disputes by the use of particular framing mechanisms.

We are also witness to several episodes of "science as usual" in this controversy. Often the motives and actions of scientific actors were consistent with the general character and tenor of evidentiary disputes in science. Shroud scientists evoked conflicting empiricist repertoires, denounced those who violated the putative norms of science, asserted the epistemic authority of technique and instrumentation to resolve the enigmas of the artifact, "pathologized" rival claims, turned the microscope back on the failings of technique and scrutinized the ethics, impartiality and motives of their rivals.

But controversies are not caused, driven and circumscribed only by relations of power, conflicting egos, contending evidentiary claims, and vested interests. What *else* provokes, shapes and circumscribes large-scale disputes among heterogeneous collections of actors? What of the *object* of dispute itself? Clearly the object or issue of contention is essential to the dynamics and resolutions of controversies. Actors wage battle over *something*. But how closely do we attend to the agential role that objects play in the movements and convolutions of a controversy? Venturini (2010) reminds us that a cartographical analysis of controversy demands that we take seriously the role of both human and non-human actors entangled in the relations of conflict. As he notes:

> ...there is no such thing as an isolated actor. Actors are always composed by and components of networks. Consider any biotech cultivar: each single transgenic seed is the result of the coordinated work of an extensive network made up of scientific protocols, field trials, research investments, technical instruments, industrial patents. At the same time, each little seed contributes to a wider network which gathers global corporations, scientific laboratories, non-governmental organizations, national and international legislation. Actors are such because they inter-act, shaping relations and being shaped by relations (Venturini, 2010, p. 266-267).

To understand the subtle distinctions of the shroud controversy, I suggest that it is critical to look to the object of contention itself, and the essential (and *agential*) role it has played all along in shaping and circumscribing these disputes. At the outset, the shroud *intrigued and confounded the scientific imagination* and thereby played a significant role in compelling scientists to assert their authority and obligation to interpret its ambiguities. In other words, the shroud, as an agent, *enrolled* the actors in this controversy, and compelled them to recognize the instrumental value of engaging in the disputes and defending their methods of interpretation. As I have shown, shroud scientists were motivated to demonstrate the prowess of their methods and instruments to impart scientifically reliable evidence of the shroud's historical status. Recall that one of Harry Gove's intentions in joining the fray was to exhibit to the public and the scientific community the superior technical command of AMS. STURP scientists similarly acknowledged an instrumental value in applying an array of techniques and rendering practices as a means of making comprehensible the confounding properties of the shroud. Moreover, scientists engaged in this venture to provide a public understanding of how science properly engages with a controversial religious object. The object thus presented an opportunity for scientists to articulate the apposite role of science in matters of religious consequence. Interestingly, this required strategic alliances with non-scientific actors, or what Venturini calls entering into "hybrid forums" – the spaces of conflict and negotiation among actors that would otherwise happily ignore each other " (p. 261).

The shroud's socio-material complexities, coupled with its hybrid status as a religious artifact and an object of scientific interest, also *compelled in shroud scientists*

an affective reverence to this artifact. Accordingly, disputes were shaped not solely by evidentiary battles and instrumental aims. Rather, the conflicts were importantly driven by scientists' commitment to preventing the dissemination of a specious scientific narrative of the artifact. In other words, the shroud conflicts were shaped in part by a will to protect the integrity of the artifact. It follows that we must recognize the importance of the shroud in essentially inspiring an ethical value system among scientists that exceeded their instrumental and epistemic interests. Moreover, this allows us to recover the idea that scientists are also compelled by an ethical commitment to finding truth, an idea that is often displaced or understated in many accounts of controversies that largely place interpretive weight on interests, egos and ambition.

The Turin shroud has been a contentious object since it was first put on display in the 13th century. As I have noted, historical disputes were centered around the dangers of beguiling the faithful with a spurious relic. What is compelling about the contemporary shroud controversy is that the conflicts are fundamentally underlined by the same anxiety. Although the contemporary battles have been waged among scientific practitioners and articulated in empiricist language, the underlying concern of these conflicts involved the risk of deceiving the public with specious knowledge of the shroud's ontological status. Each episode of conflict described in this case study was rooted in exposing how disputants' evidentiary claims were misleading to the public and to the scientific community. Thus the contemporary shroud controversy is not entirely divorced from the historical conflicts that preceded it. The values and motives of the shroud disputes are, on close examination, not markedly different, nor does the involvement of modern science

seemingly alter the root concerns of the battle. The message of contenders has remained consistent: do not *deceive* by proclaiming this object to be something it is not.

Finally, in the course of these disputes, the shroud has undoubtedly been a *resistant actor*. Notwithstanding the many evidence claims offered about its provenance and material composition, it would seem that this artifact defied any attempts to arrive at a single reductive truth. The artifact's resistance shaped the conflicts themselves, and played a critical role in the post-closure backlash. Thus by recognizing the agency of the object of dispute in a controversy, we are able to see how the object circumscribes the relations of actors entrenched in it.

Emergent Stakes & Interests

Fundamental to most scientific controversies is the fact that social actors wage battle in large part because something of *value* is at stake. The intensity, duration, and convolutions of controversies are a consequence of social actors struggling over the potential forfeiture of rights, freedoms, and collective social values. Disputants also resist the impositions of scientific authority if that imposition brings political, moral, ideological or economic implications. Typically, the stakes of a dispute can be readily identified once a controversy has erupted (though it is important to bear in mind that there are often multiple stakes in disputes, and that stakes are often defined differently by the various stakeholders). In Epstein's (1996) account of the AIDs controversy, for example, the stakes of the disputes involved the credibility of knowledge claims by experts and laypersons as well as the "the very mechanisms for the assessment of credibility" (p. 3).

In the context of the shroud controversy, the stakes of the conflict were not readily discernible at the outset. When I began to trace the terms and motivations of the disputes among shroud scientists and non-scientific actors, it was difficult to determine *what* in particular was at stake by resolving the ambiguities of a religious object. But what this controversy revealed is that the stakes of a conflict are not always apparent but are, rather, *emergent* in the course of disputes and shaped by the dispute itself. In this case study, as the investment of social actors increased, the more explicit and profound did the stakes become. As I have indicated, as the shroud disputes escalated, and more actors were drawn into the fold, the stakes emerged in three forms. First, the disputes among shroud scientists revealed that what was at stake concerned the protection of the artifact from spurious scientific interpretations. Second, at stake in these conflicts was the safeguarding of the integrity of science itself in its pursuit of a controversial artifact. The third issue at stake was the dissemination of a credible and ethical account of the shroud's historical status and material properties to both the scientific community and the broader social milieu. These stakes, as I have noted, only emerged as the controversy proliferated.

Extending Interpretive Lenses:

Finally, as I noted earlier, my purpose in interrogating science's contentious entanglement with a religious object was a way to call attention to the value of broadening the empirical concerns of controversy studies. While the conventional interests of the field provide considerable intellectual insight into the complex and often volatile intersections of science and the larger society, I suggest that controversy studies has often tended to privilege those conflicts involving the intersection of mainstream

science with the political and pragmatic concerns of the public. Such a focus overlooks the sociological importance of the contentions among scientific and non-scientific actors that have raged with comparable intensity, complexity and tenacity around the contested cultural objects of our world. I suggest that the shroud controversy offers a different perspective on how we might understand scientists' relationship to their objects of interests, the ways in which they assert their intellectual authority, the unanticipated convolutions and paradoxes of scientific conflict, the resistances of the object of dispute, and the ways in which scientific knowledge concerning non-scientific objects is shaped and defended.

Conclusion

Recently, a new line of inquiry concerning how the shroud image was formed has foregrounded the authenticity question once again. In 2011, Italy's National Agency for New Technologies, Energy and Sustainable Development published an independent report claiming that the shroud image was possibly generated by a burst of ultraviolet energy. Scientists conducted experiments for five years using ultraviolet laser technology on strips of linen to produce a "very superficial Shroud-like coloration of linen yarns in a narrow range of irradiation parameters" (Boyle, 2011, par. 5). They concluded from these results that there is a very a low probability that the shroud is a medieval painting, and stated that the image remains "a scientific challenge" (Boyle, 2011, par. 4). [75] Thus it would seem that neither the human artifice theory nor the carbon dating evidence have

[75] There is an interesting parallel here between the scientific claim that the image was possibly wrought by intense UV energy, and the belief among the most devout shroud adherents that the image on the cloth is the result of a powerful burst of radiant energy in the moment of the resurrection.

sufficiently settled the shroud's enigmas. What is noteworthy about this most recent study is that it points to the enduring relevance of this artifact over a century following its first confrontation with the scientific gaze, when two Sorbonne scientists attempted to discern the mechanism by which the shroud image was created. Whether or not this recent finding will trigger another full blown controversy is yet to be seen. [76] It is clear, however, that this artifact persistently confounds scientific interpretation, and continues to inspire curiosity and contemplation.

When science intervenes on an object, science leaves a mark. Scientists' cultivations, dissections, reconfigurations, abstractions and the epistemic skirmishes wrought by these practices of knowing effectively inscribe a narrative on to the object and thereby alter it in some way. Certainly this is true of the science of the shroud, which, through its epistemic practices, has inscribed new and multiple ways of witnessing and comprehending this artifact. The "plot" of science's narrative of the shroud is a thorny one, replete with compelling tensions between certainty and doubt; regret and vindication; antipathy and harmony; natural and supernatural; rationality and affect. The Turin shroud remains a sacred artifact that still provides spiritual succor to the devout, but is simultaneously a scientific enigma that inspires contemplation and, as I noted earlier, secular reverence. It is perhaps incorrect to suggest that the shroud is a thing that cannot be comprehended. Rather, it is something that can be understood as a variegated social

[76] Certainly news of the Italian scientists' findings generated several hyperbolic media headlines. Consider a few of the most sensationalistic: "Italian study claims Turin Shroud is Christ's authentic burial robe"; "Shroud Of Turin, Jesus' Proposed Burial Cloth, Is Authentic, Italian Study Suggests"; "Scientists say Turin Shroud is supernatural"; "Shroud Of Turin: Scientists Reopen Debate About Relic's Authenticity" (Squires, 2011; Llorens, 2011; Day, 2011; Vale, 2011).

object whose multiple functions and meanings hold together.[77]

[77] Star and Griesemer have developed the related concept of "boundary objects," a central analytic concept of social worlds theory that refers to artifacts that bear multiple meanings and "inhabit several intersecting social worlds" but which simultaneously maintain a "common identity across sites" (Star and Griesemer, 1989, p. 393).

Conclusion

In the introduction of this dissertation, I opened with a brief anecdote about the James Ossuary, an archeological relic whose provenance became the source of a protracted and highly publicized controversy that eventually found its resolution in an Israeli court. I encountered this story in a McLean's article and saved it primarily because the controversy struck me as a compelling and somewhat peculiar event. As I have noted, it was the James Ossuary disputes that inspired me to contemplate the sociological significance of scientific controversies waged over unconventional objects of contention, and waged on unfamiliar battlegrounds.

My original intention for this study was, in fact, to follow the actors embroiled in the complex James Ossuary conflicts. Instead, the Ossuary story led me to consider whether a controversy over an archeological relic was an exceptional episode, or if, in fact, scientists have immersed themselves in comparable disputes concerning the provenance of devotional or historical objects. If so, what would be the character, singularities and outcomes of such disputes? Would such controversies simply reproduce the archetypical hostilities between science and religion? Would scientists position themselves as emissaries of "truth" and assert their intellectual authority in these disputes (as they tend to do in the contemporary antagonisms with Intelligent Design proponents?).

This case study is the end product of those moments of contemplation. I was relatively unfamiliar with religious relics, but I did have a vague recollection that at some point in recent history, a Catholic funeral shroud- purportedly a relic of Christ (and thus a

sublime object of veneration)- had become the subject of considerable public debate over its authenticity. When I began my research, I discovered that this Catholic artifact, the Turin shroud, was not only the source of significant scientific interest and intervention, but that it had become embedded squarely in a long standing and deeply convoluted set of battles among scientific and non-scientific actors. But would a dispute over a devotional artifact comply with the interests of controversy studies, and could a study of this controversy claim a legitimate place in the field?

The task of this project, then, has been to demonstrate how the shroud controversy both exemplifies the character and dynamics of most scientific controversies, but simultaneously reveals paradoxes and anomalies that render this particular conflict rather exceptional relative to what we might anticipate when scientists confront and defend their objects of interest. Further, I have sought to understand not simply *how* the shroud conflicts were waged, but *why* contenders became so invested in the battles, and why these battles endured and proliferated.

To this end, I began by exploring the 1978 events that essentially ignited the contemporary shroud controversy. I examined the STURP inquiry with a view to understanding how the Turin shroud became a subject of scientific interest at the outset. I placed much of my analytical focus on how scientists confront, interpret and "translate" a controversial religious object, and how they assemble and defend their evidence claims. Situating the analysis in a constructivist framework, I argued that the STURP team's use of linguistic and visual resources "captured" and crafted an artifact that acquired a new kind of legibility, and provided STURP scientists with the authority to make provisional

evidence claims about its material properties. But more importantly, the STURP team's interventions and translations of the shroud ignited, unintentionally perhaps, the longstanding disputes over the shroud's material complexities and historical origins.

In tracing the motivations, dynamics, and convolutions of the controversy that followed the STURP inquiry, I have argued that beyond the ostensible antagonisms that unfolded among social actors, there were elements of the shroud disputes that do not fall neatly in line with what we might anticipate in scientific controversies. Thus while the antagonisms among social actors to settle the historical status of the shroud were rooted partly in issues of control and competition (the anticipated motivations that typically underlie most conflicts) a deeper interrogation suggests that the conflicts were often fuelled by an intrinsic value of defending a truthful account of the artifact. I have further argued that the artifact itself played a considerable role in shaping the direction and convolutions of the controversy. My broader aim in this work has been to highlight the utility of taking into account the controversies waged over unconventional objects. I have suggested that such conflicts provide an opportunity to bring to light some nuances and complexities of controversy that are not well articulated in conventional approaches.

Limitations and Future Directions

This case study has focused principally on the disputes that took place among shroud scientists themselves, as well as the relevant non-scientific actors who were drawn into the fold. The shroud controversy, as a broad cultural phenomenon, has involved social groups that do not find a presence in my analysis. Specifically, shroud science has provoked a considerable discourse of skepticism on the part of scientists, academics and

scientific writers outside of the shroud science community. Skeptics tend to position themselves as the envoys of "sober second thought" with respect to the theories and conjectures proposed by shroud scientists and their adherents. As such, skeptics generally dismiss the claims of shroud science as pseudoscientific sophistry. As one vocal shroud science detractor noted, "science has proved the Shroud of Turin a medieval fake, but defenders of authenticity turn the scientific method on its head by starting with the desired conclusion and working backward to the evidence—picking and choosing and reinterpreting as necessary" (Nickell, 2005, par. 14).

While the perspectives of skeptics represent an important position in the overall shroud debates, it was beyond the scope of my aims for this case study to engage in this discourse in any depth. This discourse does, however, offer a potentially new line of inquiry into this controversy. One possible research question might ask why much of the cynical assessments of shroud science tend to find recourse in the archetypical demarcation between credible science and pseudoscience. Further, given that many skeptics tend to formulate and promote alternative (and presumably corrective) theories of the shroud, it would be useful to interrogate why skeptics deem it necessary to impart such remedial narratives to the broader culture.

In tracing the various phases of this controversy, I did not attend to the views of members of faith communities as this too exceeded the scope of my analytical concerns for this project. A possible future direction to pursue would include an inquiry into how members of faith communities apprehend the interventions of science on objects of faith, and how they reconcile scientific evidence claims with their own beliefs.

The central concerns of this project were not focused in any depth on speaking back to the relationship between science and religion more broadly. I have suggested that popular accounts of the shroud controversy tend to characterize the relationship between these two domains in antagonistic terms. This was especially apparent in media accounts of the "Chosen Three" controversy, where the media discourse reduced the conflicts to a tension between religious obstructionism and scientific rationality. Indeed, this kind of depiction of the shroud controversy is consistent with popular assumptions about the incommensurable antipathy between science and religion. A.A Sappington (1991) notes that "perceived tension between religion and science occurs not only in specific areas of conflict such as...the authenticity of relics; it also occurs perhaps more fundamentally in that the determinism and reductionism long characteristic of science may seem to leave no room for the divine" (p.115). To be sure, such perceptions preclude a nuanced engagement with the intricate ways in which science and religion have been historically entangled. Steve Fuller (2007) notes that, "the feeling that science and religion pull in opposing directions has been integral to the experience of 'being modern'", yet the premise for such polarizations has never corresponded "very clearly to substantive intellectual differences" (p.1). Certainly this is true of the shroud controversy, where the tensions and alliances among disputants were rarely rooted in any manifest antagonisms between faith and science. I have suggested that the shroud conflicts disclosed rather complex intersections of these domains. Thus while the Turin shroud, as I have indicated, is a "contested terrain," it is also one of convergence between two domains which were often bound toward similar aims.

Finally, this project is a springboard for a more extensive comparative study of similar socio-scientific controversies that have yet to be explored in depth. Specifically, given that the James Ossuary disputes have been similarly centered around the authenticity of a relic believed by many to correspond to the historical Jesus, there is considerable potential for interrogating the contrasting and comparative dynamics of these two controversies.

On Reflexivity and Captivity: A Final Note

In their article "Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies," Scott et al., (2008) argue that it is neither possible nor even desirable for the analyst to adopt a neutral position when interrogating a controversy. In many contemporary scientific controversies, the authors argue, analysts are often "drawn into the fray" and unwittingly "captured" by one set of social actors in the disputes (p. 474). The notion of capturing challenges the feasibility of analytical symmetry, and also inspires analysts to confront a moment of reflexivity with respect to their role, their partisanship, and their intellectual and political investment in the controversy.

Given the historical nature of this study, I was obviously at no risk of any capturing by any side. Neither was I influenced by the position of any of the contenders, and I have formed no opinion either way on the veracity of any of the shroud scientists' claims. Any speculation I might harbor about the Turin shroud's possible historical or metaphysical status is thankfully left to personal musings. My interest in exploring this controversy was to trace and interpret the terms and motives upon which the shroud

disputes were premised, and to delineate how the dynamics of this conflict offers unique insight into the social relations of controversy.

But despite embracing a symmetrical position, I was not entirely invulnerable to being a "captive" of sorts. Indeed, there is more than one way by which analysts can be drawn into the fray. While I did not adhere to a particular position in these disputes, I found myself captivated by, and empathetic to, the persistence of shroud scientists in pursuing this artifact despite the negligible gains wrought by their efforts. Indeed, shroud science is a site of struggle. Shroud scientists, particularly in recent years, have struggled to define and justify the place, value, purpose and epistemic command of this marginal field, not only within the stern conventions of science itself, but against the often vehement disdain of skeptics who publicly deride the efforts of its adherents.

This controversy, therefore, inspired me to embrace something akin to Weber's notion of *verstehen* - to seek an interpretive understanding of the meaning underlying the fervency of scientists' investment in this artifact. Whether or not there is any demonstrable value in settling the provenance of a religious object, there are sociological gains to be made by "diving into the magma" of the intricate and often confounding social relations that have been cultivated by this curious endeavor.

Appendix A: Early History of the Turin Shroud

Historians generally agree that the Turin shroud made its first appearance circa 1353 when Geoffrey I de Charney, a French knight, brought the cloth from Constantinople to Lirey, France. Although it is unclear how de Charney acquired the cloth, historians speculate that it was either stolen or claimed as spoil of war. Owing to the ambiguity surrounding de Charney's discovery, a local Bishop, Henry of Poitiers, conducted an investigation and claimed to have found the artist who painted the image (Tribbe, 2006). Years later, a similar charge was leveled by Bishop Pierre d'Arcis, who composed memorandum to Pope Clement VII insisting that public expositions of the Shroud were being carried out "with fraudulent intent and for the purpose of gain" (McCrone, 1999, p.2).

The shroud remained in the de Charney family's physical custody until 1463, when Margaret de Charney, granddaughter of Geoffrey I de Charney, transferred ownership of the shroud to Duke Louis I of Savoy in exchange for an estate. It remained under the custodianship of the Savoy family until 1983, at which time the cloth was bequeathed to the Roman Catholic Church by deposed King Umberto II of Savoy (Wilson, 1998, 2000; Click, 1988).

Shroud historian Ian Wilson (2000) contends that while there exists no continuous historical record of the shroud prior to 1353, the shroud appears in both religious art and historical records that pre-date its 1350 appearance. Several historical records suggest that nearly a century earlier, the Turin shroud was in the possession of Byzantine rulers and was held as part of a collection of Passion relics of the Church of St Mary of

Blacherene in Constantinople. Historians also point to an image appearing in a Prayer Manuscript inscribed by the Boldva Benedictine monastery between 1192-1195 depicting "shroud like" details of Jesus's body following the crucifixion (Wilson, 2000). Wilson speculates that the shroud was in the possession of the Knights Templars between 1204 and 1314 (Wilson, 1978).

There have been approximately thirty public expositions of the Turin shroud between the years 1452 to 2010. Exhibitions have been held to honour high ranking authorities of the Catholic church, celebrate religious holidays, mark royal weddings, observe the ascension of royalty, and celebrate Holy and Jubilee years. Private viewings of the shroud have been generally reserved for high ranking church officials and members of European royalty. The Archbishop of Turin is the official pontifical custodian of the shroud. It is housed in a reliquary in the Cathedral of St. John the Baptist in Turin, Italy.

Residence:[78]

1355: The Shroud is held in Lirey, France.

1418: The Shroud is transferred to the Castle of Montford near Montbard, France and later moved to des Buessarts Chapel in St. Hippolyte sur Doubs in eastern France.

1463: Upon transfer of ownership to the Savoy family, the Shroud is kept at the Sainte Chapelle at Chambery.

1471-1478: The Shroud is transferred to several locations: Vercelli, Turin, Ivrea, Moncalieri, Chambery, and Sisa-Avigliano-Rivoli.[79]

1535: The Shroud is taken from Royal Chapel of Chambery Castle and stored in Piedmont after Chambery is invaded by French troops. It is later transferred to Vercelli.

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[78] Source: Wilson, 1998, 2000.

[79] The shroud was often moved for protection during periods of war and civil unrest.

1561: The Shroud returns to Chambery and is stored in the Church of St. Mary the Egyptian.

1578: The Shroud is brought from Chambery to Turin, where it is stored in the Guarni chapel.

1939: With the outbreak of WWII, the shroud is secretly removed from the Guarni chapel and transported the Benedictine Abbey of Montevergine, in the province of Avellino, Naples.

1946: The shroud is returned to Turin and is stored royal chapel of the Cathedral of Saint John the Baptist in Turin, northern Italy, its permanent place of residence.

Appendix B: Organizations, Institutions & Individuals

Individual	Affiliation	Role
Ballestrero, Anastasio	Roman Catholic Church	Cardinal of Roman Catholic Church; Archbishop of Turin; Official custodian of the Turin Shroud (1977-1989).
Chagas, Carlos	Pontifical Academy of Sciences	President (at time of 1988 shroud radiocarbon dating)
Canuto, Vittorio	NASA Institute for Space Studies; Pontifical Academy of Sciences.	Theoretical astrophysicist; Scientific advisor to Carlos Chagas
Dinegar, Robert	STURP	Research chemist
Damon, Paul	AMS Laboratory, Arizona	Co-director
Garbottle, Garman	Chemistry Department, Brookhaven National Laboratory	Senior scientist
Gonella, Luigi	Turin Polytechnic Institute	Professor of Physics ; Scientific advisor to Cardinal Ballestrero
Gove, Harold	Nuclear Structure Research Laboratory, University of Rochester	Director; Professor of Physics
Hall, Teddy	Research Laboratory for Archeology and the History of Art, Oxford University	Director
Hedges, Robert	Radiocarbon Accelerator Unit, Research Laboratory for Archeology and History of Art, Oxford University	Senior scientist
Heller, John	Shroud of Turin Research Project (STURP)	Chemist

Jackson, John	STURP	Physicist
McCrone, Walter	McCrone Associates	Forensic microscopist
Meacham, William	Center of Asian Studies, Hong Kong.	Archeologist
Riggi, Giovanni	Leader of Italian scientific team at time of STURP inquiry	Microanalyst
Rinaldi, Peter	Holy Shroud Guild	Guild Councillor; STURP advisor
Rogers Ray	STURP	Chemist
Sox, David	British Turin Shroud Society	Episcopalian priest; Secretary of British Turin Shroud Society (at time of 1978 Turin inquiry)
Stevenson, Kenneth	STURP	Engineer, STURP public relations official
Tite, Michael	Research Laboratory of the British Museum	Director
Testore, Franco	Turin Polytechnic Department of Material Science	Supervised the 1988 sample extraction of the shroud
Vial, Gabriel	Centre International d'Étude des Textiles Anciens, Lyon	Supervised the 1988 sample extraction of the shroud
Woelfli, William	AMS laboratory, Zurich	Director

Bibliography

Accetta, J.S., & Baumgart , J.S. (1980). Infrared reflectance spectroscopy and thermographic investigations of the shroud of Turin. *Applied Optics, 19(12),* 1921-1929.

Akrich, M, Callon, M & Latour, B. (2002). The key to success in innovation part I: The art of interessement. *International Journal of Innovation Management,* 6(2), 187-206.

Allgaier, J. (2012). Networking expertise: Discursive coalitions and collaborative networks of experts in a public creationism controversy in the UK. *Public Understanding of Science,* 21(3), 299–313.

Amann, K., & Knorr-Cetina , K. (1990). The fixation of (visual) evidence. In M. Lynch & S. Woolgar (Eds), *Representation in scientific practice* (pp.85-121). Cambridge, London: The MIT Press.

Anderson, I. (1988, January 21). Vatican undermines tests on Turin shroud. *New Scientist,* 22.

Archbishop of Turin agrees shroud wasn't 'Jesus'. (1988, October 14). *The Record,* A20. Retrieved from Factiva database.

A U.S. scientist says he is convinced the holy shroud....(1980, September 20). *The Globe and Mail,* np. Retrieved from Factiva database.

Ball, P. (2008). Material witness: Shrouded in mystery. *Nature materials,* 7, 349. Retrieved from: http://www.nature.com/nmat/journal/v7/n5/full/nmat2170.html

Barbet, P. (1953). *A doctor at Calvary: The passion of our lord Jesus Christ.* New York: P.J. Kenedy.

Barnes, B. (1977). *Interests and the growth of knowledge.* London: Routledge & Kegal Paul Ltd.

Barnes, B, Bloor, D. & Henry, J. (1996). *Scientific knowledge: A sociological analysis.* Chicago: The University of Chicago Press.

Beyers, W. (2011). *The blind spot: Science and the crisis of uncertainty.* Princeton: Princeton University Press.

Binford, L.R. (1962). Archaeology as anthropology. *American Antiquity.* 28(2), 217-225.

Bloor, D. (1976). *Knowledge and social imagery*. London: Routledge & Kegan Paul.

Bloor, David (1997). Remember the strong program? *Science Technology and Human Values, 22*(3), 373-385.

Boyes, R., Wright, P., & Longley, C. (1988, October 14). Church says fake Turin shroud is still an icon. *The Times*, np, Retrieved from Factiva database.

Boyle, A. (2011, December). Was holy shroud created in a flash? Italian researchers resurrect claim. Retrieved from http://cosmiclog.msnbc.msn.com

Brante, T. & Elzinga, A.(1990). Towards a theory of scientific controversies. *Science Studies*, 3(2), 33-46.

Brooke, J.H. (1991). *Science and religion: Some historical perspectives*. New York: Cambridge University Press.

Callon, M. (1999). Some elements of a sociology of translation: Domestication of the scallops and the fishermen of St. Brieuc Bay, (Abr.ed.). In M. Bagiolo (Ed.), *The Science Studies Reader* (pp. 67-83). New York, London: Routledge,

Calvin, J. (1543). *A Treatise on Relics*, trans. By Count Valerian Krasinski, 1854; 2cd ed. Edinburgh: John Stone, Hunter and Company, reprinted Memphis, Tenn: General Books LLC, 2010.

Cantor, G.N. (1975). A critique of Shapin's social interpretation of the Edinburgh phrenology debate. *Annals of Science*, 323, 245-256.

Clark, K. (1988, January 17). Shroud of Turin controversy resumes. *Chicago Tribune*. Retrieved from http://articles.chicagotribune.com

Clark, B., Bellamy Foster, J. & York, R. (2007). The critique of intelligent design: Epicurus, Marx, Darwin and Freud and the materialist defense of science. *Theory and Society*, 36 (6), 515-546.

Click, P.C. (1988). High technology meets the spiritual: Objectivity, popular opinion and the shroud of Turin. *Journal of Popular Culture*, 21(4), 13-23.

Coleman, C. & Dysart, E. (2005). Framing the Kennewick Man against the backdrop of a scientific and cultural controversy. *Science Communication*, 27(1), 3-26.

Collins, H.M., & Pinch, T.J. (1979). The construction of the paranormal: Nothing unscientific is happening. In R. Wallis (Ed.), *On the margins of science: The social construction of rejected knowledge* (pp. 237-270). Keele: University of Keele.

Collins, R. & Restivo, S. (1983). Robber barons and politicians in mathematics: A conflict model of science. *The Canadian Journal of Sociology*, 8 (2), 199-227.

Collins, H.M. (1987). Certainty and the public understanding of science: Science on television. *Social Studies of Science,*17, 689-713.

Collins, H. & Pinch, T. (1993). *The Golem: What everyone should know about science.* Cambridge: Cambridge University Press.

Cracraft, J. (1982). The scientific response to creationism. *Science Technology Human Values, 7(40), 79-85.*

Crick, N & Gabriel, J. (2010). The conduit between lifeworld and system: Habermas and the rhetoric of public scientific controversies. *Rhetoric Society Quarterly*, 40(3), 201-223.

Culliton, B.J. (1978, July 21). The mystery of the shroud of Turin challenges 20th century science. *Science*, 201(4352), 235-239.

Cunningham, R. (2001). Virtual witnessing and the role of the reader in a new natural philosophy. *Philosophy and Rhetoric*, 34(3), 2001, 207-224

Damon, P. E., D. Donahue, D.J., Gore, B.H., Hatheway, A.L., AJull,' A.J.T., Linick, et al. (1989). Radiocarbon Dating of the Shroud of Turin. Nature, 337(16), 611-615.

Day, M. (2011, December). Scientists say Turin shroud is supernatural. *The Independent.* Retrieved from http://www.independent.co.uk/news/science/

Dearing, J. W. (1995). Newspaper coverage of maverick science: creating controversy through balancing. *Public Understanding of Science*, 4, 341-361.

Delborne, J. (2005). *Pathways of scientific dissent in Agricultural Biotechnology.* Retrieved from: http://digitalcorpora.org/corp/nps/files/govdos1/303/303951.pdf.

Delborne, J. (2008). Transgenes and transgressions: Scientific dissent as heterogeneous practice. *Social Studies of Science,* 38(4), 509-541.

Delborne, J. (2011). Constructing audiences in scientific controversy. *Social Epistemology*, 25(1), 67-95.

Drews, R. (1984). *In search of the shroud of Turin: New light on its history and origins*. New Jersey: Rowman & Allanheld.

Dutton, Denis (1984). Requiem for the shroud of Turin. *Michigan Quarterly Review*, 23, 243-55. Retrieved from: http://denisdutton.com/requiem.htm

Engelhardt, H.T.& Caplan, A.L. (1989). *Scientific Controversies: Case Studies in the resolution and closure of disputes in science and technology*. Cambridge: Cambridge University Press.

Epstein, S. (1996). *Impure science: AIDs, activism, and the politics of knowledge*. Berkley: University of California Press.

Evans, J.H. and Evans, M.S. (2008). Religion and science: Beyond the epistemological conflict narrative. *Annual Review of Sociology*, 34, 87-105.

Faulkner, W (2000). The power *and* the pleasure? A research agenda for "making gender stick" to engineers. *Science, Technology, & Human Values*, 25(1), 87-119

Freudenburg, W.R., Gramling, R., & Davidson, D.J. (2008). Scientific certainty argumentation methods (SCAMs): Science and the politics of doubt. *Sociological Inquiry*, 78(1), 2–38.

Friedman, M. (2012, March). After 7-year saga, a surprising end to antiquities fraud case. *The Times of Israel*. Retrieved from http://www.timesofisrael.com/

Fujimura, J.H. & Chou, D.Y. (1994). Dissent in science: Styles of scientific practice and the controversy over the cause of AIDs. *Social Science of Medicine*, 38(8), 1017-1036.

Fuller, S. (2007). *Science vs. religion? Intelligent design and the problem of evolution*. Cambridge, Malden: Polity Press.

Gatehouse, J. (2005, March 28). Je$us: how a great Canadian museum bought into the 'antiquities fraud of the century'. *MacLeans*, 27-36.

Giere, R.N. (1987). Controversies involving science and technology: A theoretical perspective. In Engelhardt & A.L. Caplan (eds.), *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology*. Cambridge: Cambridge University Press, pp. 125-150.

Gieryn, T. F. (1983). Boundary work and the demarcation of science from

non-science: Strains and interests in professional ideologies of scientists. *American Sociological Review*, 48,(6), 781-795

Gieryn, T. F., Bevins, G.M. & Zehr, S.C. (1985). Professionalization of American scientists: Public science in the creation/evolution trials. *American Sociological Review,*50, 392-409.

Gieryn, T.F. (1999). *Cultural boundaries of science: Credibility on the line.* Chicago, London: The University of Chicago Press.

Gieryn, T.F. (2006). City as truth-spot: Laboratories and field-sites in urban studies. *Social Studies of Science.* 36(1), 5-38.

Gilkey, L. (1982). The creationist controversy: The interrelation of inquiry and belief. *Science, Technology, & Human Values*, 7(40), 67-71.

Glass, R. (1988, April 10). Laboratories to carbon-date cloth from shroud of Turin. *The Dallas Morning News, A24.* Retrieved from Factiva database.

Golinsky, J. (2005). M*aking natural knowledge: Constructivism and the history of science.* Cambridge: Cambridge University Press.

Gove, H.E. (1987). Turin workshop on radiocarbon dating the Turin shroud. *Nuclear Instruments and Methods in Physics Research.* B29, 193-195).

Gove, H.E. (1989). Letter to the editor: The Turin shroud. *Archaeometry*, 31(2), 235-237.

Gove, H.E (1989). Progress in radiocarbon dating the Shroud of Turn. *Radiocarbon,* 31(3), pp. 965-969.

Gove, H.E. (1996). *Relic, icon or hoax? Carbon dating the Turin shroud.* London: Institute of Physics Publishing.

Greene, A. (2009). Scientist re-creates Turin shroud to show it's a fake. Retrieved from http://articles.cnn.com/

Gross, A. (1984). Public debates as failed social dramas: The recombinant DNA controversy. *Quarterly Journal of Speech*, 70, 397-409.

Gross, A. (1996). *The rhetoric of science.* Cambridge: Harvard University Press.

Gross, A. (2005). Scientific and technical controversy: Three frameworks for analysis.

Argumentation and Advocacy, 42(1), 43-47.

Gruber, R. (1988, October 14). Shroud found to be forgery; church agrees. *St. Petersburg Times*, np, Retrieved from Factiva database.

Gruber, R. (1988, October 14). Turin shroud still worthy of veneration, church says. *The Toronto Star*, A20, Retrieved from Factiva database.

Hacking, I. (1983). *Representing and intervening: Introductory topics in the philosophy of natural science*. New York: Cambridge University Press.

Hacking, I. (1999). *The social construction of what*? Cambridge: Harvard University Press.

Halfon, S. (2008). Depleted uranium, public science, and the politics of closure. *Review of Policy Research*, 25(4), 295-311.

Hård, M (1993). Beyond harmony and consensus: a social conflict approach to technology. *Science, Technology and Human Values*, 18(4), 408-432.

Harre, Rom (2002). Material objects in social worlds. *Theory, Culture & Society*, 19(23), 23-33.

Harris, A.R. (1990). Assent, dissent, and rhetoric in science. *Rhetoric Society Quarterly*, 20(1), 13-37.

Heller, J.H. & Adler, A.D. (1980). Blood on the shroud of Turin. *Applied Optics*, 19(16), 2742-2744.

Heller, J.H. (1983). *Report on the shroud of Turin*. Boston: Houghton Mifflin Company.

Hess, D. J. (1997). *Science studies: An advanced introduction*. New York: New York University Press.

Hilts, P.J & Rensberger, B. (1988, April 4). Church official curbs plan to carbon-date shroud of Turin. *The Washington Post*, p. A04. Retrieved from Factiva database.

Holy Shroud Tested (1902, April 27). French scientists claim it possesses photographic properties – their conclusions accepted. *New York Times*, p. 3.

Jackson, J.P., Jumper, E.J., & Ercoline, W.R. (1984). Correlation of image intensity on the Turin shroud with the 3-D structure of a human body shape. *Applied Optics*, 23(14), 2244-2269.

Jamieson, A. (2009, July 1). Was Turin shroud faked by Leonardo da Vinci? Retrieved from http://www.telegraph.co.uk/

Jasanoff, S., Markle, G.E, Peterson, J.C and Pinch, T. (Eds.). (1995). *Handbook of science and technology studies: The cultural approach to the study of science and technology studies.* Newbury Park, CA: Sage.

Jumper, E.J., & Mottern, R.W. (1980). Scientific investigation of the shroud of Turin. *Applied Optics*, 19(12), 1909-1912.

Jumper, E.J., Adler, A.D., Jackson, J.P., Pellicori, S.F., Heller, J.H., & Druzik, J.R. (1984). A comprehensive examination of the various stains and images on the shroud of Turin. *Archaeological Chemistry—III.* 447-476

Kava, B. (1988, April 25). Dating the shroud: Some scientists call 3 labs too few for reliable results. *San Jose Mercury News*, np. Retrieved from Factiva database.

Knorr Cetina, K & Amann, K (1990). Image dissection in natural scientific inquiry. *Science, Technology and Human Values,* 15(3), 259-283.

Knorr-Cetina, K. D. (1992). The couch, the cathedral, and the laboratory: On the relationship between experiment and laboratory in science. In A. Pickering (Ed.), *Science as practice and culture* (pp. 113-138). Chicago: University of Chicago Press.

Knorr-Cetina, K. (1995). Laboratory studies: The cultural approach to the study of science. In S. Jasanoff, G.E. Markle, J.C. Peterson, and T. Pinch (Eds.), *Handbook of Science and Technology Studies: The Cultural Approach to the Study of Science and Technology Studies* (pp. 140-166). Thousand Oaks, CA: Sage Publications, Inc.

Knorr-Cetina, K. (1999). *Epistemic cultures: How the sciences make knowledge.* Cambridge & London: Harvard University Press.

Kuhn, T. S. (1962). *The Structure of scientific revolutions.* Chicago, London: The University of Chicago Press.

Lambert, K. (2006). Fuller's folly, Kuhnian paradigms, and intelligent design. *Social Studies of Science,* 36(6), 835–842.

Latour, B. & Woolgar, S. (1986). *Laboratory life: The construction of scientific facts.* Princeton: Princeton University Press.

Latour, B. (1987). *Science in action: How to follow scientists and engineers through society.* Cambridge: Harvard University Press.

Latour, B. (1988). Visualization and social reproduction: Opening one eye while closing the other...a note on some religious paintings. In G. Fyfe & J. Law (Eds.), *Picturing power: Visual depiction and social relations* (pp. 15-38). London: Routledge.

Latour, B. (1990). Drawing things together. In M. Lynch & S. Woolgar (Eds.), *Representation in Scientific Practice* (pp. 19-68). Cambridge, London: The MIT Press

Latour, B. (1999). Give me a laboratory and I will raise the world. In M. Biagioli (Ed.), *The Science Studies Reader* (pp. 258-275). New York, London, Routledge.

Latour, B. (2005). *Reassembling the social: An introduction to Actor-Network Theory.* New York: Oxford University Press.

Law, J. & Wittaker, J. (1988). On the art of representation. In G. Fyfe & J. Law (Eds.), *Picturing power: Visual depiction and social relations* (pp. 160-183). London: Routledge.

Law, John (2007). Actor network theory and material semiotics. Retrieved on January 2, 2010 from http://www.heterogeneities.net/publications/Law-ANTandMaterialSemiotics.pdf

Limoges, C. (1993). Expert knowledge and decision-making in controversy contexts. *Public Understanding of Science*, 2, 417-426.

Llorens, I. (2011, December). Shroud of Turin, Jesus' proposed burial cloth is authentic, Italian study suggests. *The Huffington Post.* Retrieved from http://www.huffingtonpost.com

Lynch, M. (1985). Discipline and the material form of images: An analysis of scientific visibility. *Social Studies of Science*, 15(1), 37-66.

Lynch, M. (1988). Sacrifice and the transformation of the animal body into a scientific object: Laboratory culture and ritual practice in the neurosciences. *Social Studies of Science*, 18(2), 265-289.

Lynch, M. & Woolgar, S. (Eds). (1990). *Representation in scientific practice.* Cambridge, London: The MIT Press.

Lynch, M., & Woolgar, S. (1990). Introduction: Sociological orientations to

representational practice in science. In M. Lynch & S. Woolgar (Eds.), *Representation in Scientific Practice* (pp 2-15). Cambridge, London: The MIT Press

Lynch, M. (1990). The externalized retina: Selections and mathematization in the visual documentation of objects in the life sciences. In M. Lynch & S. Woolgar (Eds.), *Representation in Scientific Practice* (pp. 151-186). Cambridge, London: The MIT Press.

Lynch, M. (1998). The discursive production of uncertainty: The OJ Simpson 'dream team' and the sociology of knowledge machine. *Social Studies of Science*, 28(5-6), 829-68.

MacKenzie, D. (1978). Statistical theory and social interests: A case-study. *Social Studies of Science*, 8 (1), 35-83.

Maines, David R. (1993). Narrative moments and sociology's phenomena: Toward a narrative sociology. *The Sociological Quarterly*, 34(1), 17-38.

Martin, B. (1991). *Scientific knowledge in controversy: The social dynamics of the fluoridation debate.* Albany: State University of New York Press.

Martin, B. & Richards, E. (1995). Scientific knowledge, controversy, and public decision making. In S. Jasanoff, G.E. Markle, J.C. Peterson, & T. Pinch (Eds.), *Handbook of Science and Technology Studies: The Cultural Approach to the Study of Science and Technology Studies* (pp. 506-526). Thousand Oaks: Sage Publications Inc.

Martin, B. (1996). Sticking a needle into science: The case of polio vaccine and the origin of AIDS. *Social Studies of Science*, 26(2), 245-276.

Martin, B. Strategies for dissenting scientists (1998). *Journal of Scientific Exploration*,12(4), 605-616.

Martin, B. (1999). Suppression of dissent in science. Retrieved from http://www.uow.edu.au/~bmartin/pubs/99rsppp.html.

Martin, B. (2004). Dissent and heresy in medicine: models, methods, and strategies. *Social Science & Medicine*, 58, 713–725

Martin, B. (2008). Enabling scientific dissent. *New Doctor,* 88, 2-5.

McCrone, W.C, & Skirius, C.M. (1980). Light microscopical study of the Turin shroud I. *Microscope*, 28(3,4), 105.

McCrone, W.C. (1990). The shroud of Turin: Blood or artist's pigment? *Accounts of Chemical Research*. 23, 77-83.

McCrone, W. (1999). *Judgment day for the shroud of Turin*. New York: Prometheus Books.

McFarlane, D.A. (2004). Resistance as a social drama: A study of change-oriented encounters. *American Journal of Sociology*. 109(6), 1249-1318.

McMullin, E. (1989). Scientific controversy and its termination. In Engelhardt, H.T. & Caplan, A.L. (eds.), *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology* (pp. 49-91). Cambridge: Cambridge University Press.

Meacham, W., Alcock, J.E., Bucklin, R. Burridge, K.O.L., Cole, J.R., Dent, R.J., et al. (1983). The Authentication of the Turin shroud: an issue in archaeological epistemology [and comments and reply]. *Current Anthropology,* 24 (3), 283-311.

Meacham, W. (1986). Radiocarbon measurement and the age of the Turin shroud: Possibilities and uncertainties. In *Proceedings of the Symposium "Turin Shroud - Image of Christ?"*, Retrieved from http://www.shroud.com/meacham.htm.

Mendelsohn, E. (1989). The political anatomy of controversy in the sciences. In H.T. Engelhardt, H.T & Caplan, A.L (Eds.), *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology*. Cambridge: Cambridge University Press.

Merton, R. (1957). Priorities in scientific discovery: A chapter in the sociology of science. *American Sociological Review*, 22(6), 635-659.

Merton, R. (1973). *The sociology of science: theoretical and empirical investigations.* Chicago: The University of Chicago Press.

Monagan, D. (1980, October). New data suggests the image on the shroud is genuine. *New York Times*, p. C3.

Morgan, R.(1983). *Shroud guide.* Australia: Runciman Press.

Morton, T. (1988, October, 8). Research unhappy with sample tested. *Colorado Springs Gazette Telegraph*, D4.

Mulkay, M. (1976). Norms and ideology in science. *Social Science Information,*

15(4-5), 637-656.

Mulkay, M., & Gilbert, G.N. (1982). Accounting for error: How scientists construct their social world when they account for correct and incorrect belief. *Sociology*, 6(2), 165-183.

Nelkin, D (1982). *The creation controversy: Science or scripture in the schools*. New York, London: W.W. Norton & Company.

Nelkin, D. (1987). Controversies and the authority of science. In Engelhardt, H.T. & Caplan, A.L. (Eds.), *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology* (pp. 283-293). Cambridge: Cambridge University Press.

Nelkin, D. (1995). Scientific controversies: the dynamics of public disputes in the United States. In S. Jasanoff, G.E. Markle, J.C. Peterson, and T. Pinch (eds.), *Handbook of Science and Technology Studies: The Cultural Approach to the Study of Science and Technology Studies* (pp. 444-456). Thousand Oaks: Sage Publications Inc.

Nelkin, D. (2004). God talk: Confusion between science and religion. *Science, Technology and Human Values*, 29(2), 139-152.

Nickell, J. (2005, March). Claims of invalid "shroud" radiocarbon date cut from whole cloth. Retrieved from http://www.csicop.org/

Noble, D. (1999). *The Religion of technology: The divinity of man and the spirit of invention*. New York: Penguin Books.

Ostling, R.N., Coile, N, Moynihan, R. (1988, October). Religion: Debunking the Shroud of Turin. Time Magazine Retrieved from http://www.time.com/time/magazine/article/0,9171,968744,00.html

Oxley, M. (2010). *The challenge of the shroud: History, science and the shroud of Turin*. Central Milton Keynes: Author House.

Park, H.J. (2001). The creation-evolution debate: Carving creationism in the public mind. *Public Understanding of Science*, 10, 173-186.

Pasveer, B. (2006). Representing or mediating: A history and philosophy of x-ray images in medicine. In L. Pauwels (Ed.), *Visual Cultures of science: Rethinking representational practices in knowledge building and science communication* (pp. 41-62). Hanover: Dartmouth College Press

Pauwels, L. (2006). A theoretical framework for assessing visual representational practices in knowledge building and science communication. In L. Pauwels (Ed.), *Visual Cultures of science: Rethinking representational practises in knowledge building and science communication* (pp. 1-25).Hanover: Dartmouth College Press

Pellicori, S.F. (1980). Spectral properties of the shroud of Turin. *Applied Optics,* 19 (12), 1913-1920.

Peterson, J.C. & Markle, G.E. (1979). Politics and science in the Laetrile controversy. *Social Studies of Science*, 9, 139-66.

Picart, C.J.S. (1994). Scientific controversy as farce: the Benveniste-Maddox counter trials. *Social Studies of Science*, 24, 7-37.

Pickering, A. (1995). *The mangle of practice: Time, agency and science.* Chicago & London: University of Chicago Press.

Polanyi, M. (1956, June 16). Passion and controversy in science. *The Lancet*, 921-925.

Priestland, G. (1988, October16). From holy relic to a holy red herring. *The Sunday Times*, np, Retrieved from Factiva database.

Pylyshyn, Z. (2003). *Seeing and visualizing: It's not what you think.* Massachusetts: M.I.T Press.

Restivo, S. (1995). The theory landscape in science studies: Sociological traditions. In S. Jasanoff, G.E. Markle, J.C. Peterson, and T. Pinch (Eds.), *Handbook of Science and Technology Studies: The Cultural Approach to the Study of Science and Technology Studies* (pp. 95-110*)*. Thousand Oaks: Sage Publications Inc.

Richards, E. (1988). The politics of therapeutic evaluation: The vitamin C and cancer controversy. *Social Studies of Science.* 18, 653-701.

Ritter, J. (1988, October 19). Scientist here feels vindicated by finding on Shroud of Turin. *Chicago Sun-Times*, np, Retrieved from Factiva database.

Rogers, R. & A. Arnoldi. (2002). Scientific method applied to the shroud of Turin: A review. Retrieved from: http://www.shroud.com/pdfs/rogers2.pdf

Rogers, R. (2005). Studies on the radiocarbon sample from the shroud of Turin. *Thermochimica Acta.* 425, 189-194.

Rogers, R. (2008). *A Chemist's Perspective on the Shroud of Turin.* Barrie

M. Schwortz (ed). www.shroud.com.

Rufford, N. (1988, August). Turin shroud: Vatican steels itself for `fake' result. *The Sunday Times*, np. Retrieved from Factiva database.

Saltus, R. (1988, April 18). Scientists may finally learn age of shroud: Carbon dating of controversial relic gets under way soon. The Boston Globe. Retrieved from http://www.highbeam.com/doc/1P2-8058246.html

Sappington, A.A. (1991). The Religion/science conflict. *Journal for the Scientific Study of Religion*, 30 (1), 114-120.

Schwalbe, L.A & Rogers, R.N. (1982). Physics and chemistry of the shroud of Turin: A summary of the 1978 investigation. *Analytica Chimica Acta*, 135, 3-49.

Scott, P., Richards, E., Martin, B (1990). Captives of controversy: The myth of the neutral social researcher in contemporary scientific controversies. *Science, Technology, &Human Values*, 15(4), 474-494

Segerstrale, U. (2000). Politics by scientific means and science by political means: Trojan horses in the socio-biology debate. *Science Studies*. 13(1), 3-18.

Shapin, Steven (1982). The politics of observation: Cerebral anatomy and social interests in the Edinburgh phrenology disputes. In H.M. Collins (Ed.), *Sociology of Scientific Knowledge: A Source Book* (pp. 103-150). Bath, Avon: Bath University Press.

Shapin, S. (1984). Pump and circumstance: Robert Boyle's literary technology. *Social Studies of Science*, 14(4), 481-520.

Shapin, S & Schaffer, S. (1985). *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton, New Jersey: Princeton University Press.

Shapin, S. (1995). Cordelia's love: Credibility and the social studies of science. *Perspectives on Science*. 3(3), 255-275.

Shermer, M. (1991). Science defended, science defined: The Louisiana creationism case. *Science, Technology and Human Values*, 16, 517-539.

Shortell, T. (2011). The conflict over origins: A discourse analysis of the creationism controversy in American newspapers. *Mass Communication and Society*, 14, 431-453.

Shroud of Turin only 700 years old. (1988, October 14). *Newsday*, np. Retrieved from Factiva database.

Silverman, M.P. (1992). Raising questions: Philosophical significance of controversy in science. *Science & Education*, 1, 163-179.

Simmel, G. (1955). *Conflict & the web of group-affiliations*. New York: The Free Press.

Simon, B. (1999). Undead science: Making sense of cold fusion after the (arti)fact. *Social Studies of Science*, 29(1), pp. 61-85.

Sismondo, S. (1993). Some social constructions. *Social Studies of Science*.23, 515 553.

Sismondo, S. (1996). *Science without myth: On constructions, reality and social knowledge*. Albany, NY: State University of New York Press

Sox, D. (1988, October 15). How an age of mystery ended; Turin shroud. *The Times*, np, Retrieved from Factiva database.

Sparks, R. and Meacham, W. (1998). C-14 debate from the shroud newsgroup. Retrieved from http://www.shroud.com/c14debat.htm

Spirit of the Shroud; Shroud of Turin. (1988, October 14). *The Times*, np, Retrieved from Factiva database.

Squires, N. (2011, December). Italian study claims Turin Shroud is Christ's authentic burial robe. *The Telegraph*. Retrieved at: http://www.telegraph.co.uk/news/religion/

Stafford, B. M. (2007). *Echo objects: The cognitive work of images*. Chicago: University of Chicago Press.

Star, S.L. (1983). Simplification in scientific work: An example from neuroscience research. *Social Studies of Science*, 13, 205-28.

Star, S. & Griesemer, J. (1989). Institutional ecology, 'translations' and boundary objects: Amateurs and professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39. *Social Studies of Science*, 19(3), 387–420.

Steinfels, P. (1988, October 14). For some, shroud is still inspirational. *New York Times*, p. A10.

Stevenson, K.E. & Habermas, G.R. (1990). *The shroud and the controversy: Science,*

skepticism and the search for authenticity. Nashville: Thomas Nelson Publishers.

Sturm, S.P. (1982). Creationism, censorship and academic freedom. *Science, Technology and Human Values*, 7(40), 54-56.

Suro, R. (1988, October 14). Shroud of Turin not authentic, church says. *San Francisco Chronicle*, A1 Retrieved from Factiva database.

The shroud keeps its appeal. (1988, October 15). *The Omaha-World Herald.* p. 16.

Thorpe, C. (2006). *Oppenheimer: The tragic intellect.* Chicago: University of Chicago Press.

Tribbe, F.C. (2006). *Portrait of Jesus?: The shroud of Turin in science and history.* St.Paul: Paragon House.

Turner, V. (1974). *Dramas, fields, and metaphors: Symbolic action in human society* Ithaca, NY: Cornell University Press.

Vale, P. (2011, December). Shroud of Turin: Scientists reopen debate about relic's authenticity. *The Huffington Post.* Retrieved from http://www.huffingtonpost.co.uk

Venturini, T. (2010). Diving in magma: How to explore controversies with actor-network theory. *Public Understanding of Science*, 19(3), 258–273.

Volckringer, J. (1991). *The holy shroud: Science confronts the imprints*. Rex Morgan (ed). Australia: The Runciman Press.

Weaver, K. F. (1980, June). Science seeks to solve the mystery of the shroud. *National Geographic*, 730-752.

Wilcox, R.K. (2010). *The truth about the shroud of Turin: Solving the mystery.* Washington, D.D.: Regnery Publishing Inc.

Wilson, I. (1978). *The Turin Shroud.* London: Gollancz.

Wilson, I. (1998). *The blood and the shroud: New evidence that the world's most sacred relic is real.* London: Weidenfeld & Nicolson.

Wilson, I., & Schwortz, B. (2000). *The Turin shroud: The illustrated evidence.* Toronto: McArthur & Company.

Winner, L. (1993). Upon opening the black box and finding it empty: Social constructivism and the philosophy of technology. *Science, Technology and Human Values*, 18(3), 362-378.

Woolgar, S. (1982). Laboratory studies: A comment on the state of the art. *Social Studies of Science,* 12, 481-98.

Wright, P.(1988, January 16). New disputes on dating tests: The Shroud of Turin. *The Times*, np, Retrieved from Factiva database.