HOW DO PAST CRISES AFFECT PUBLICS' PERCEPTIONS OF CURRENT EVENTS?:
AN EXPERIMENT TESTING CORPORATE REPUTATION DURING AN ADVERSE
EVENT

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ABSTRACT

JOHN DREW ELLIOT, III: How Do Past Crises Affect Publics' Perceptions of Current Events?: An Experiment Testing Corporate Reputation During an Adverse Event

Previous research in crisis communication has shown that a corporation's history of crises has a damaging effect on reputation during a current crisis. This study uses an experiment with student subjects testing not only the effect of a corporation's own crises on reputation, but also the reputational effect of a corporation without a crisis history but in an industry with a history of similar crises, called extraorganizational crisis history. Findings of this experiment show that publics' knowledge of extraorganizational crisis history may protect an organization's reputation in a crisis. Theoretical and practical implications of this finding are discussed and further research is suggested.

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CHAPTER 1

INTRODUCTION

As David Umansky (1993) of the public relations firm Burson-Marsteller defined it, a crisis is “an intense, unstable state resulting in decisive change” (p. 32). In the context of an organization, Lerbinger (1997) defined a crisis as an event that can bring an organization into disrepute and has the possibility to damage an organization’s profits, growth, or very survival. There are many other definitions in the literature of what constitutes a crisis, but most can be distilled into a situation that is (a) unexpected, either by the organization, its publics, or both; and (b) a threat, either to the organization, its publics, or both.

A recent example of the effect a crisis can have on a corporation underscores the importance of studying better ways to communicate during an adverse event. In 2004, Dow Corning ended a decade of lawsuits regarding its silicone breast implants. The lawsuits were filed by plaintiffs’ attorneys on behalf of women who claimed that leaking breast implants had caused a large assortment of ailments. Dow seemingly ended the controversy by combining a settlement of the lawsuits with a filing of bankruptcy (Kever & Tolson, 2004). But spurred by new events, the FDA recently lifted the 1992 ban on silicone breast implants (Goldberg, 2007).

The first event was the appearance of a 1996 book written by the editor of the *New England Journal of Medicine*, Marcia Angell, exposing the faulty science used by plaintiffs’ attorneys during the breast implant controversy. The second was a scientific review by the

independent government advisory group the Institute for Medicine, which concluded that the implants do not cause disease (Sommerfeld, 2004). Finally, a double-blind study funded by Dow showed that silicone implants do not increase the risk for cancer (No cancer link for silicone implants, 2006).

Considering that the \$3.2 billion class-action settlement bankrupted Dow Corning, it is now obvious that the real victims may not have been women with silicone breast implants, but Dow's employees and shareholders, among others. Lawyers were able to win those cases by painting Dow as unfeeling, profit-hungry, and stubborn (Kever & Tolson, 2004). If Dow had been better able to protect its reputation, those victims may have been spared some of the damage to their careers and pocketbooks.

The Dow Corning breast implant controversy is just one example of the importance of protecting a corporation's reputation during a crisis situation. Protecting reputational assets is one of the chief reasons to study crisis communication (Allen & Caillouet, 1994; Benoit, 1997; Hearit, 1994). As Coombs and Holladay (1996) argued, one of the main factors that affects the damage an organization's reputation can suffer in a crisis is where publics place the blame for a crisis, also called causal attribution. The cause that publics perceive for a given crisis will go a long way toward determining where the publics place blame for the crisis and an organization's subsequent response.

Furthermore, an organization does not face a crisis in a vacuum: "when a crisis is an exception to the organization's performance history," people will place less blame on an organization regardless of the organization's handling of the crisis (Coombs & Holladay, 1996, p. 282). Conversely, the Federal Emergency Management Agency (FEMA) received little sympathy for its inadequate response to Hurricane Katrina, since it is in the business of

managing the federal response to natural disasters and other emergencies. (After all, its middle name literally is *emergency management*.)

While public relations scholars have established that a history of crises tends to harm an organization's reputation (Coombs & Holladay, 1996, 2001; Coombs, 1998, 2004), in these studies the previous crises always affected *the same organization*. This construct can be called *intraorganizational crisis history*, for the crises to which it refers occurred within one organization. This study, on the other hand, will examine the effects that a previous crisis suffered by a *different* organization has on reputation. This can be called *extraorganizational crisis history*. To add to the findings of the intraorganizational studies, this study will use largely the same method as the extant research, an experiment using student subjects.

This work begins by tracing the history of research into crisis communication from its early focus on the communicative activities of an organization. Then it will turn to later scholarship, which concentrated on cataloging types of crises and suggesting practical models for pairing a particular crisis communication strategy with the type of crisis being faced. Next, previous research concerning studies that looked at crisis communication strategies themselves will be reviewed, along with experiments in crisis communication. The rationale for using the experimental method will be discussed in the methods section, along with the method that was used in this experiment. The results of this research are presented next, with a discussion of the findings, suggestions for further research, and conclusion.

CHAPTER 2

REVIEW OF THE LITERATURE

Early Scholarship

Much of the early scholarship examining crisis communications came from the perspective of the speech communications academic discipline. In what now can be seen as a seminal work in crisis communications, Ware and Linkugel (1973) wrote an article in the *Quarterly Journal of Speech* that began to bridge the gap between speech and communication studies and nascent public relations scholarship. Looking mostly at modern American speeches by Clarence Darrow, Eugene V. Debs, Edward Kennedy, and others, the authors examined apologia as a communicative tool to lessen reputational damage. An apologia was defined as “the speech of self-defense” (p. 273), usually through a justification of one’s actions; apologiae may or may not contain an apology (Hearit 1994). Ware and Linkugel identified two reformatory factors: denial and bolstering; and two transformative: differentiation and transcendence.

The definitions of the terms that Ware and Linkugel used are not important for the purposes of this study. What is important to note is the use of rhetorical terminology and their emphasis on classification of communicative forms. Their article illustrated the emphasis that speech and communications scholars placed on types of communications radiating from an individual or organization that is faced with a crisis. This type of analysis was external and descriptive; it examined the efforts rhetors made to influence an audience when communicating in the aftermath of a crisis. In contrast to the rhetorical perspective,

when public relations scholars began to study crisis communications, some began to look at the issue from an organizational perspective.

The Organizational Perspective

From the organizational perspective, the emphasis was internal and (sometimes) prescriptive—it tried to decide what an organization *should* do in a crisis, not just critique what it *did* do. Wan and Pfau (2004) noted this distinction, saying “most reactive crisis communication strategies focus on apologia and rhetoric, [while] a proactive approach seems to emphasize relationship management” (p. 302). But the difference really lay in the focus of the scholarship, not in crisis communication strategies themselves. In the communications discipline, the researcher is a birdwatcher, listening and cataloging the taxonomy of bird-made noises. From the organizational perspective, on the other hand, the public relations scholar put himself in the place of the bird itself, and examined which birdcall would be most effective. Organizations had always had crisis communications strategies—good and bad. It was the point of view of the scholar that had evolved.

Crisis types.

One large step that public relations scholars took was a need to understand the crisis affecting the organization before choosing a communications strategy. In 1988 Benson challenged scholars to categorize crisis types and hypothesized that certain response strategies would be more suited to certain crises than others. As Coombs noted in 1998, “comparatively more attention has been devoted to describing and defining CCSs [crisis communication strategies] than to understanding the situation” (p. 178). From the

organizational perspective, studying the response before studying the situation puts the cart before the horse. Choosing your weapon before you know what is attacking you may be possible, but it is certainly not desirable. Therefore in this study, an examination of the crisis type literature precedes an examination of the response strategy.

As a reply to Benson's (1988) challenge, Coombs and Holladay (1996) developed a 2×2 matrix to determine crisis type. Crises were grouped first by external control, or whether a crisis is caused by something the organization did (internal) or whether it was caused by an outside agent (external). Crises can also be grouped by intentionality, or whether the crisis was committed purposefully (intentional) or not (unintentional).¹

Thus a crisis can fall into one of four categories. As named by the authors, the categories are:

- faux pas (external and unintentional);
- accidents (internal and unintentional);
- terrorism (external and intentional); and
- transgressions (internal and intentional).

The names of two categories—faux pas and terrorism—may no longer be effective. The term *faux pas* may be a misnomer. In this typology, the faux pas and accidents categories are really both accidents, one external and one internal. If an event is serious enough to be thought of as a crisis, it would be strange to call it a faux pas. For example, if a truck parked on a hill above a factory slipped its parking brake and rolled down the hill into the factory, injuring several workers, the expression *faux pas* is not likely to come to mind. Whatever it is called, the important element of this category is the belief that the organization has been

¹ Although a later empirical study by one of the authors (Coombs, 1998) questioned some of the findings of Coombs and Holladay's (1996) matrix, it is still useful.

the victim of an accident. In fact, *victim* may be the best name for the category, a term Coombs used in a slightly different context in another work (see below) (Coombs, 2004; Coombs & Holladay, 2002). Similarly, the term *terrorism* seems a bit strong here, although literally correct. In the world after the terrorist attacks of September 11, 2001, calling any crisis event that is external and intentional a terrorist act does not appear appropriate. These authors named their categories in 1996, and might choose different descriptions today.

In 1999 Coombs created a list of potential crises, dividing them into nine categories. He refined and expanded the list in 2002 to 13 crisis types (Coombs & Holladay, 2002). The types are rumor, natural disaster, malevolence/product tampering, workplace violence, stakeholder challenge, technical breakdown accident, technical breakdown product recall, megadamage, human breakdown accident, human breakdown product recall, organizational misdeeds with no injuries, organizational misdeeds with injuries, and organizational misdeed management misconduct. Coombs (2002) also provides real-world examples through case descriptions of each type of crisis.

In 2002, an experiment by Coombs using factor analysis convinced him to abandon his prior typologies and group ten crisis types into three categories: victim crises (the organization is the victim), accidental crises, and intentional crises. The victim cluster includes natural disasters, rumors, product tampering, and workplace violence. Accidents include stakeholder challenges, technical-error accident, and technical-error product recall. Intentional crises are the most serious and include human-error product recalls, human-error accidents, and organizational misdeeds (Coombs, 2002, 2004).

These typologies are not the only ones that public relations scholars have developed (*e.g.*, Sen & Egelhoff, 1991). The development and refinement of these typologies allowed

researchers to begin to examine the ways in which corporations had chosen to respond when a particular kind of crisis happened. By examining what organizations had done in the past, they would be more capable of suggesting what organizations should do in the future.

A matched response: The symbolic approach.

After determining the type of crisis that an organization faces, the next step is matching the communicative response with the crisis type. Public relations scholar W. Timothy Coombs, writing in 1998, noted that “a vibrant new perspective on crisis management has been developing over the last few years” (p. 177). He was speaking of the development of a new theory of crisis communications: the symbolic approach.

The symbolic approach built on two earlier theories in the field, neoinstitutionalism and attribution theory (Coombs & Holladay, 1996). The focus of neoinstitutionalism is on an organization’s legitimacy, or its right to continue operations (Allen & Caillouet, 1994). Since legitimacy is grounded in the perception of how well an organization conforms to rules, regulations, and conventions, a crisis can be a threat to legitimacy. Attribution theory, on the other hand, focuses on how publics decide who or what caused an event, *i.e.*, the attribution of causality. It is important to note that causal attribution, as with legitimacy, is concerned with the perception of cause (not what really caused the crisis). As Coombs and Holladay (1996) explained:

Logically, a crisis is an event for which people and publics seek causes and make attributions. More specifically, people evaluate organizational responsibility for a crisis when they determine the cause of a crisis. The more publics attribute crisis responsibility to an organization, the stronger the likelihood is of publics developing and acting on negative images of the organization. Greater attributions of responsibility lead to stronger feelings of anger and a more negative view of an actor’s image (Weiner, Amirhan, Folkes, & Verette, 1987). (p. 282)

Coombs and Holladay combined neoinstitutionalism and attribution theory into what they called the symbolic approach to crisis communications. They chose the term *symbolic* because “it emphasizes how communication can be used as a symbolic resource in attempts to protect the organization’s image” (Coombs, 1998, p. 177). The crux of the symbolic approach is that once an organization identifies what kind of crisis it is facing, it should choose a crisis communication strategy (CCS) that matches that particular type of crisis.

Stakeholder Theory

The organizational perspective is not the only way that researchers study crisis communication. As Martin and Boynton (2005) pointed out, now the “focus of crisis communication has shifted from the organization to those with an interest in the organization” (p. 6). Those interested parties are called *stakeholders*, which better reflects the definition of public relations as relationship management. While the symbolic approach does not ignore stakeholder concerns, its perspective lies within the organization (Coombs & Holladay, 2001). As stakeholder theory applies to corporate crisis communication, it means that crises can affect many publics. The organization may be very experienced in communicating with some of these publics (employees, shareholders, government regulators), while others may be novel for it (the community, foreign media, future generations). Corporations must prioritize their publics in a crisis, and how corporations give publics priority can be strategically and ethically challenging (Ulmer & Sellnow, 2000).

Ulmer and Sellnow (2000) used stakeholder theory to analyze the crisis communication activities of the fast food chain restaurant Jack in the Box, which was the source of a 1993 outbreak of E. Coli 0157 :H7 in the American northwest that killed three

children and infected about 400 people. The authors found that the corporation was guilty of several unethical tactics, including elevating the needs of internal publics—mainly shareholders—over external publics. One important point the authors make is that ambiguity is common in the initial stages of a crisis, but “it is ill-advised for organizations to capitalize on the inherent ambiguity of crisis situations in an effort to escape blame or legal liability” (p. 146).

At the heart of stakeholder theory is the belief that stakeholders are affected by corporations, but that corporations are also affected by stakeholders (Martin & Boynton, 2005). A corporation should—and indeed, must—live in harmony with its stakeholders if it is to derive the maximum benefit from society (profit) while providing the maximum benefit to society (safe products and services).

Organizational communication has been studied by communications scholars, public relations scholars, and others. Scholars have looked at it from the perspective of the listener, from the organizational perspective, or a combination of perspectives, as in stakeholder theory. From whatever perspective a crisis is viewed, and through whatever theoretical framework it is seen, what has interested most scholars in the end is what an organization says. The next section looks at these crisis communication strategies.

Crisis Communication Strategies

When scholars look at crisis communication strategies, one of the first elements they look at is the purpose of communication. From the organizational perspective, one of the main purpose of crisis communication—although not the only one—is to protect an organization’s reputation (Allen & Caillouet, 1994; Benoit, 1997; Hearit, 1994). As

aforementioned, Ware and Linkugel (1973) studied speeches and noted that apologiae involved bolstering, denial, differentiation, and transcendence. Benoit (1997) expanded that list to five “message options” or communicative strategies: denial, evasion of responsibility, reducing offensiveness of event, corrective action, and mortification, also known as apology.

Using real-world cases, Benoit (1997) placed crisis communication strategies on this continuum from denial to mortification. To employ these strategies effectively, he suggested that companies take the proper steps: prepare, analyze the crisis, and identify relevant audiences. Although Benoit did not propose it overtly, what he posited was essentially a matched response to the crisis situation; in other words, every response type on his continuum is an effective response, provided that it fits the situation at hand.

For instance, Benoit (1997) analyzed two cases in which corporations employed one of the image restoration strategies he identified: shift the blame, one of the strategies of denial. He argued that blame-shifting worked for Johnson & Johnson in the 1982 Tylenol product-tampering case, since Tylenol was able to shift the blame to an unknown culprit. However, when Exxon employed the same strategy in the Alaska *Valdez* oil spill, it backfired. When Exxon attempted to shift the blame to Captain Hazelwood, publics refused to accept the shift, reasoning that since Hazelwood was in Exxon’s employ, he was Exxon’s responsibility.

In 1994 Keith Michael Hearit studied cases affecting Chrysler, Toshiba, and Volvo to provide a catalogue of their communicative strategies. According to the narratives, Chrysler sold cars as new when they had been driven for months without the odometers connected, Toshiba sold sensitive milling equipment to the Soviet Union, and Volvo faced criticism

when it staged a series of advertisements based on an actual event in which a Volvo vehicle was left largely unscathed after a monster truck ran over it.

Hearit (1994) found that the corporations engaged in communicative strategies with three objectives: first, to provide a competing narrative that was more favorable to the corporation; second, to express concern while accepting minimal responsibility; and third, to dissociate the organization from the wrongful act. Hearit's account was useful in its emphasis on how organizations attempt to redefine an issue in a crisis, but it failed in two areas. First, it did not examine whether these attempts mitigated reputational damage or not. Second, he did not discuss the ethical nature of using these strategies.

Later, Hearit (1996) showed that some organizations use "kategoria," or an apologia that "contests the validity of a charge of wrongdoing by attacking the credibility of the accuser" (p. 234). Using a case study, he showed that General Motors chose this approach when it attacked the NBC television network's ethics after revealing that NBC's "Dateline" program installed incendiary devices and ill-fitting fuel caps in order to "prove" that the GM pickups would burst into flames in an accident. Hearit cautions, however, that using kategoria is not always an effective strategy.

Most of crisis communication literature reviewed here focuses on how corporations have communicated during a crisis. Corporations, of course, are not the only organizations that need to communicate in adverse situations. For instance, Ryan Martin and Lois Boynton (2005) examined how major newspapers assessed the communications efforts of NASA following the *Challenger* shuttle disaster and the *Columbia* shuttle disaster. First, the authors identified five criteria for successful public relations from the public relations literature: prompt response, truth/avoidance of absolutes, constant flow of information, concern for the

victims and their families, and choice of appropriate spokesperson(s). Then they conducted a content analysis of articles covering to the two tragedies. They found that the successful criteria were mentioned more after *Columbia* than after *Challenger*, and that NASA received more positive news coverage after *Columbia* than after *Challenger*.

The public relations trade literature also provides some insight into crisis communication strategies. Generally, the trade literature exhorts practitioners to have a crisis communication plan, not to use a “no-comment” strategy, to keep the media and other publics informed as quickly and as often as possible, and to train a good spokesman or spokeswoman (see, *e.g.*, Barnett, 2003; Duke & Masland, 2002; Gaschen, 2003; Geibel, 1996; Samansky, 2002; Umansky, 1993).

A good example of such a practically minded article is David Umansky’s 1993 work in *Public Relations Quarterly*. Umansky defines what an organizational crisis is, then defines eight characteristics of a crisis, such as insufficient information, loss of control, and intense scrutiny from the outside. Following his description of a crisis, he suggests ways to prepare for a crisis, including identifying vulnerabilities, creating management and communications systems and procedures, and training spokesmen/spokeswomen and media managers. Umansky concludes with eight tips on crisis communication itself, such as containing the problem, holding back complete trust of people within the organization who seem to know the most, and using direct communication with important publics.

Much of the crisis communication literature involves looking at how organizations have chosen to communicate during a crisis, then classifying and judging their activities. While these studies certainly provide the field with valuable information about crisis communication, they also have limitations. Researchers can study only what they have

observed, and they can observe only what has been tried. This fact means that not all crisis communication strategies can be evaluated using observation. The next section will discuss another way of evaluating crisis theory and strategy: the experiment.

Testing Crisis Theory Through Experiments

Most of the research in crisis communication is theoretical or based on case study, but scholars have also used experiments to test theory. No matter what method is used, the most prolific researcher in crisis communication is W. Timothy Coombs. As noted above, he has done a large amount of theoretical work, but he is especially prolific when using experiments in crisis communication research. In an article in the *Journal of Public*

Relations Research in 1999, Coombs excoriated the field for its lack of empirical research:

Writers offer advice about what crisis managers should do but rarely provide verifiable support for the recommendations. . . . Although lists and lessons are a good starting point for more research, relying on untested assumptions weakens the field of crisis management. It is by testing our assumptions that public relations and crisis management becomes a stronger social science. In turn, good social science benefits practitioners by advancing the body of knowledge and improving the practice. (pp. 125-126)

While the use of experiments has not increased much since then (Boynton & Dougall, 2006), Coombs certainly took his own advice to heart.

Recall that Coombs, along with Sherry J. Holladay (1996), merged neoinstitutional and attribution theory to propose the symbolic approach (q.v.), in which corporations use symbolic resources—communications strategies—to protect the organization's reputation (see also Coombs, 1998). One part of the symbolic approach deals with what they called stability. When an organization has faced similar past crises, the cause of the crisis is said to be stable. When a crisis is a new event for an organization, the organization is in an unstable

situation. Stability is thought to increase publics' propensity to attribute causality to an organization, thus damaging the organization's reputation. As Coombs and Holladay contend, "organizational crisis responsibility should be perceived as strongest if the cause is stable (i.e., the organization has a history of crises). . . . When a crisis is repeated (stable), publics are more likely to attribute responsibility to an organization" (p. 282).

In 1996 Coombs and Holladay tested their theory by using an experimental design with student participants. Students were divided into groups, each group being given a different account of a crisis to read. The crisis accounts were manipulated to test the authors' hypotheses. For instance, some students read that an organization had faced similar crises in the past, while others received no indication of past crises affecting that organization. The authors found that, as hypothesized, a poor performance history (a history of crises) negatively affected organizational image. In addition, they found that publics attribute greater causality to an organization when the crisis is a transgression than when it is an accident, since the organization seemingly has more control over a transgression than an accident. They also found that publics attributed less causality when an organization's response was matched to the type of crisis faced by the organization. For example, an organization facing an accident emphasized the unintentional nature of the crisis, with the hope that publics would see the crisis as unavoidable. Overall, the symbolic approach correctly predicted a public's response to the various situations.

A few years later, Coombs (1998) conducted an experiment, again using undergraduate students, testing whether publics attributed more blame to an organization based on the perceived locus (internal or external) and the organization's history of crises. In this experiment subjects blamed organizations more when personal control was highest and

when an organization had faced repeated crises. A history of crises was also found to have a more substantial effect on causal attribution when the crisis was an accident than when it was a transgression: “as expected, past crises color how people interpret the current crisis” (p. 187). But the study also found that when a crisis was perceived as a transgression, subjects attributed responsibility whether or not the organization had a history of similar crises. In other words, if the cause for a crisis was internal and intentional—say, senior managers were accused of sexually harassing employees—then it made no difference if the organization had never been accused of a similar crisis before. Publics will give the benefit of the doubt to organizations facing a new crisis, but only if the crisis is perceived as an accident. Conversely, “an organization with a history of accidents will need to treat a new accident as a transgression because both generate similar perceptions of organizational responsibility for the crisis” (p. 187). So once an organization experiences a series of accidents, it will no longer get the benefit of the doubt that an accident crisis type usually provides.

In 1999 Coombs tested crisis response strategies using public relations practitioners, specifically professional crisis managers. Although the author insisted that using crisis managers as subjects for the experiment was better than using students, he ignored the fact that crisis managers may be affected by their high level of experience as communications managers during crises. Regardless, Coombs found that organizational reputation was protected most when organizations display compassion toward victims. Contrary to accepted theory, the specificity of instructing information released by an organization—that is, information that stakeholders need to make informed decisions—was found to have no effect on reputation. Instructing information can be details about what happened, precautions the public should take (“evacuate a four-mile radius for 72 hours”), or what the organization has

done to correct the problem. Coombs said that although it is “widely accepted” that instructing information protected an organization’s reputation by showing that it is in control of the crisis (*e.g.*, see Egelhoff & Sen, 1992), Coombs found that not to be the case. This is not to say that instructing information is not important, only that it does not protect an organization’s reputation. This surprising finding underscores why these theories of crisis communication need to be tested, not just contemplated.

Another theoretical tenet examined by Coombs, this time with Sherry J. Holladay (2001), was the so-called “halo effect.” The halo effect, an outgrowth of Ledingham and Bruning’s (1998) relational management perspective, posits that an organization’s favorable relationship history with stakeholders and crisis history insulate it from reputational damage during a crisis. As Coombs and Holladay (2001) described it, “the favorable relationship history becomes a bank account of goodwill. Past efforts place deposits in the account, whereas a crisis leads to small withdrawals (Druckenmiller, 1993; Siomkos & Shrivastava, 1993)” (p. 324).

The results of Coombs and Holladay’s (2001) experimental study modify the halo effect. Using student subjects, they found that a positive performance history (a halo) and a neutral performance history (or no history) have no effect on reputation during and after a crisis. A *negative* performance history, on the other hand, does harm an organization during a crisis. The authors termed this phenomenon the “velcro effect” (p. 338). A negative history will cause crises to “stick to” an organization, whereas publics will give the benefit of the doubt to organizations without a negative history. As with all of Coombs’ studies, the experimental manipulations affect the same organization throughout the experiment.

Coombs and Holladay teamed up again in 2002 to conduct a student experiment testing situational crisis communication theory. This theory is their attempt to provide a practical model based on the symbolic approach. They examined two factors that affect causal attribution: personal control, or whether an organization had control over the event; and crisis responsibility, or whether an organization was the cause of the event. Using factor analysis, the authors found that personal control and crisis responsibility actually measured the same construct. In other words, while the concepts of personal control and crisis responsibility are different, they have *the same effect on causal attribution* and thus mean the same thing for the study of crisis communication.

Coombs (2004) later used another student experiment to test the central tenet of situational crisis communication theory, namely that attribution of crisis responsibility represents a direct threat to organizational reputation. Specifically, the experiment tested whether an organization's past crises intensified both crisis responsibility and reputational damage for all crisis types. Choosing real-world crises from the victim and accidental crisis types, Coombs manipulated the organizational crisis histories (neutral/unknown, no past crises, and history of similar crises) and tested the responsibility attribution and the reputational effects. A major finding was that a negative crisis history strongly affected reputation, but only weakly affected crisis responsibility. He also found support for the velcro effect, since little difference was found between the neutral history (no history given) and the positive history conditions.

In 2004, Wan and Pfau used Taiwanese student subjects to test inoculation treatments and supportive treatments and their effect on post-crisis organizational reputation. A supportive treatment is similar to bolstering; that is, telling publics what good things a

company has done for them and their communities. Inoculation, on the other hand, introduces a hypothetical threat and refutes it, supposedly encouraging publics to practice mentally the steps it will take to mitigate reputational damage when a crisis does strike. The researchers found that when publics initially hold a positive view of a corporation, both inoculation and supportive strategies mitigate reputational damage after a crisis about the same amount, and combining them does not provide a boost to the mitigation effect. The study also found more support for Coombs' velcro effect.

Intraorganizational Crisis History Versus Extraorganizational Crisis History

Public relations scholars and practitioners have benefited from the insights of all of the preceding literature. However, four works have informed this study to the greatest extent: W. T. Coombs' experiments from 1998 and 2004, and his work with Sherry J. Holladay in 1996 and 2001. In addition to establishing the relationship between crisis history and reputational damage and all involving the same researcher, those four studies have something else in common: They all operationalized crisis history as past negative events that affected *the same organization* (Coombs & Holladay, 1996, 2001; Coombs, 1998, 2004). Thus these studies looked at *intraorganizational* crisis history. What appears to have been left unstudied is whether a similar, previous crisis affecting a *different* organization has the same damaging effect on reputation. This can be called *extraorganizational* crisis history. In other words, suppose that an oil tanker belonging to the petroleum company BP spilled a

massive amount of oil into the ocean, and suppose that this happened just one year after the Exxon *Valdez* oil spill. Assuming for argument that BP has never had a large oil spill, will publics give BP the benefit of the doubt, or will they assign Exxon's negative crisis history to BP, intensifying BP's reputational damage?

CHAPTER 3

HYPOTHESES AND RESEARCH QUESTIONS

While all the facets of crisis communication are worthy of study, this study focused on the effect that crisis history has on reputation. Coombs' (2004) study revealed a "direct, negative relationship between crisis history and organizational reputation. A history of similar crises lowered perceptions of an organization's reputation" (p. 284). As a result, when an organization with a negative crisis history faces a crisis, it also faces all of its similar past crises as well (Coombs, 2004). This result matches earlier findings in the area (Coombs & Holladay, 1996, 2001; Coombs, 1998). Thus a detrimental effect on a company's reputation was expected for a repeated crisis:

H1: Intraorganizational crisis history will negatively affect an organization's reputation.

Coombs' 2004 experiment also discovered an important, if minor, finding. The last item on the survey he gave to students during that experiment read "The organization has a history of similar crises," and was scored on a Likert-type scale (p. 278). This item served as a manipulation check for the different scenarios that the students read, as it did in this study. As was expected, the "history of past crises" condition scored significantly higher than the "information indicating no past crises" and the "unknown history/no crisis history given"

conditions in Coombs' study. In other words, students were agreeing that the scenarios Coombs had given them read as Coombs wanted: The scenario that told of an organization with a history of crises was viewed by the students as such.

When asked whether an organization has a history of crises, the answer for the "information indicating no past crises" condition should be *no*, since the scenario affirmed that the organization had never faced a similar crisis. When asked the same question for the "unknown" condition, the answer should be *no way to tell* (not merely *no*), since the scenario relayed no information regarding crisis history. Yet when analyzing the data, Coombs found what can be called a corollary to the velcro effect: The scores on that item for the "information indicating no past crises" and the "unknown history/no crisis history given" were not significantly different. Coombs thought that "these findings seem to suggest a positive assumption about the crisis history: No mention of a crisis history is viewed the same as information indicating no past crisis" (p. 279).

This experiment included a "no crisis history" condition in one of the scenarios (the extraorganizational crisis history group, see below). It also included an "unknown crisis history" condition in another scenario (the unknown crisis history group). Thus, with the dependent variable being the responses to the manipulation check item ("the organization has a history of crises"), Coombs' (2004) finding was expected again:

H2: Subjects will view an organization with no crisis history the same as they view an organization with an unknown crisis history.

As discussed above, what the public relations literature does not appear to have addressed is the effect that this *extraorganizational* negative crisis history has on an organization's reputation. This is an important question. Coombs (2004) said that an organization will face all of its own past crises whenever it faces a new one, but could an organization face *all* similar crises, not just its own? Since this effect has never been tested, it was phrased as a research question rather than a hypothesis:

R1: Will extraorganizational crisis history negatively affect an organization's reputation?

It was expected that intraorganizational crisis history would damage an organization's reputation, and it was unknown whether extraorganizational crisis history would affect reputation. If a corporation's reputation is damaged by the activities of another company, then the next question is how the reputational damage in the two situations compare to one another.

R2: Will the reputational damage caused by intraorganizational crisis history be greater than damage caused by extraorganizational crisis history?

The answers may provide crisis managers with another tool to assess what crisis communication strategy to choose when faced with a crisis. The following chapter addresses the method used to test the hypotheses and explore the research questions.

CHAPTER 4

METHOD

Experimental Methodology in Public Relations

The literature reviewed for this study focuses on three types of academic literature: pure theoretical works, case studies testing theory, and experimental studies testing theory. In terms of the crisis communication literature as a whole, however, experiments are greatly over-represented in the preceding pages. In truth, the vast majority of crisis communication literature is either theoretical or based on case study (Boynton & Dougall, 2006; Cutler 2004).

The need for a theoretical underpinning is undeniable. A good theoretical framework is needed to understand and examine, then to influence and improve any field of study. The need for case studies is obvious as well. In the preface to their public relations case study textbook, Lamb and McKee (2005) correctly observed that “principles of best practice may be best learned through examining how real organizations have chosen to develop and maintain relationships in a variety of industries, locations, and settings” (p. xiii). However, it should be noted that at least one scholar contends that the method is often applied incorrectly in public relations literature (Cutler, 2004). Other public relations research methods include content analysis, survey research, focus groups, and interviews.

Unfortunately for public relations scholarship, the experimental method too often is overlooked. A study by Boynton and Dougall (2005), which looked at ten years of journal

articles in the *Journal of Public Relations Research* and *Public Relations Review*, found that only 46 out of 400 articles (12%) in those publications even mentioned the word *experiment*. In fact, just “21 [studies, or 6%,] reported the findings of original experimental research” (p. 4). The authors described the lack of experimental research as “methodical avoidance” by researchers (p. 1).

The dearth of experimental research in public relations is troubling, especially since it is used rather extensively in other areas of mass communication research. Consider, for example, that a small sample (five issues) of the *Journal of Advertising* yielded 42% that involved experimental design (Boynton & Dougall, 2006). In addition, Wimmer and Dominick (2006) found that 21% of articles published in all of mass communication research were experiments. Yet less than 6% of public relations research designs to study through experiment.

As the experimental literature included above has shown, using experiments in public relations research is not only possible, it is necessary. Some of the very ideas that seem so logical when laid out in theory do not hold up in the laboratory. Too little of this type of research has been done, so these inconsistencies are little more than conundrums at the moment. Much more work is needed by scholars in the discipline.

Experiments are the oldest research methodology in the mass communications field, and—except in public relations—are rising in popularity. Experiments have several advantages, including identification of causality; researcher control over the environment, the variables, and the participants; cost; and the opportunity for replication. (Wimmer & Dominick, 2006). As public relations scholar Don Stacks (2002) put it, experiments are “the only way we can definitely test whether something actually causes a change in something

else” (p. 196). More specifically, Stacks said that “experimental designs allow the researcher the control necessary to precisely specify and manipulate the source or message characteristics he or she is interested in comparing” (p. 265).

The Experiment

Much of the design for this experiment was based on the experiments performed by W. T. Coombs and Sherry Holladay (Coombs, 1998, 2004; Coombs & Holladay, 1996, 2001). Subjects were students in the School of Journalism and Mass Communication at the University of North Carolina at Chapel Hill and, based on course enrollment, part of a voluntary research pool administered by the school. They signed up to participate in this study as an option to meet a class research requirement. These students were enrolled in introductory journalism, public relations, advertising, or ethics courses in the school. Students were given the option of participating in a research study or completing another assignment. Students who choose the research participation option generally have their choice of studies in which to participate, but that depends on how many studies are being conducted during that semester. The *N* was 115 subjects for the study.

A student research pool such as the one used in this study is called a convenience sample, and is not an example of random sampling (Wimmer & Dominick, 2006). The population for this experiment can be defined as all adults in the United States of America. Random sampling would provide that everyone in that population would have an equal chance of being selected for the study. For obvious temporal and monetary reasons, using random sampling would have been very difficult for the instant study.

A true experimental design and random assignment for causal inference.

The experiment was conducted on March 5, 2007. As each subject entered the room to participate in the experiment, he or she was assigned to one of three groups by being handed a packet of information marked “1,” “2,” or “3,” that corresponded to the three experimental groups to be used. This random assignment was accomplished by pre-stacking the packets in an order determined by a computer generation of random numbers 1-3 inclusive. In order to provide balance to the randomization, the numbers were randomized by each triad, not along the entire sequence of numbers. In other words, when the first three subjects (the first triad) entered the room, they were randomly assigned to Condition 1, 2, or 3—and each condition was used once in each triad. Unlike a normal table of random numbers, there was no chance that, for instance, all three members of the first triad could have been assigned to Condition 1. All three conditions were used in each triad, but the chance of getting a particular condition was still random. Thus, the number of subjects assigned to each condition was as close as possible to the *N* of the other two conditions. This procedure ensured a true experimental design (Stacks, 2002).

Stacks (2002) noted that a true experimental design, as opposed to a pre-experimental or quasi-experimental design, is better for two reasons. First, conditions are assigned randomly. Second, “it ensures that the proper comparisons are present to test for instrumentation and testing” (p. 204). *Instrumentation* and *testing* are two sources of internal invalidity, as discussed below. Random assignment is discussed first.

Random assignment of conditions is important for several reasons. When subjects are assigned randomly to the experimental groups, it lessens the chance that explanations other than the treatment are the cause of observed effects, and thus it allows the researcher to

infer that the treatment was the cause of those effects. This ability to infer the cause of an observation, called causal inference, is the foremost reason that experiments are used in scientific research (Shadish, Cook, & Campbell, 2002; Stacks, 2002). In other words, while there may be threats to validity in the subject pool, the chances are that those threats are distributed evenly throughout the groups to which the subjects are assigned. In this way, random assignment to conditions controls for almost all sources of internal invalidity and facilitates causal inference (Shadish et al., 2002).

The second reason to use random assignment is that “it ensures that the proper comparisons are present to test for instrumentation and testing” (Stacks, 2002, p. 204). *Instrumentation* and *testing* are the two causes of internal invalidity that random assignment does not control. As defined by Stacks, instrumentation means that “changes in the calibration of a measuring instrument or changes in the observers or scorers used may produce changes in the obtained measurements” (p. 202). Testing is a source of invalidity in that scores of a second test may be affected by the act of taking a first test.

Although a true experimental design ensures that those two sources of invalidity can be taken into account, neither was an applicable concern for this experiment anyway. The life of the experiment was less than one hour. Thus the instrument, namely the survey, did not change over the life of the experiment, so instrumentation was not a factor. Likewise, the lack of a pre-test (see below) eliminated any invalidity due to testing that might have occurred. Thus, a true experimental design was practical and best for this experiment, since it eliminated all causes of internal invalidity and increased the ability to infer that any observed change in the subject was caused by the treatment and not caused by anything else.

Description of the sample.

The sample consisted of 115 student subjects ($N = 115$), of whom 87 (75.7%) were female and 28 (24.3%) were male. Sophomores and juniors represented 86.1% of the sample, while freshmen and seniors represented 13.9%. Most of the sample (87.8%) indicated that journalism and mass communication was their major course of study. Of the journalism and mass communication majors, the majority indicated that they were in the advertising (37.6%) or public relations (30.7%) sequence of study. The remainder were spread over the news editorial, visual communications, and electronic communication sequences.

The fact that the overwhelming majority of subjects was female is certainly not representative of the population for the study. This discrepancy, however, should not be interpreted as evidence of self-selection based on sex, since it closely reproduces the sex ratios of both the University of North Carolina in general and the School of Journalism and Mass Communication in particular. Additionally, since no significant differences in the reputational scores were found between males and females (see below), it can be assumed that the over-representation of females is not problematic in this study.

Description of packets and lack of a pretest.

Each packet consisted of a cover sheet with directions (see appendix B), an informed-consent form (see appendix C), a crisis scenario (see appendices D, E, and F), and a questionnaire (see appendix G). Subjects were told at the beginning of the session that they were to read the informed-consent form and sign it if they agreed, then read the scenario and answer the questionnaire based on what they read (Coombs, 2004). These directions were

typed on the cover page. There was no time limit for completion of the experiment; subjects took about 15 minutes to complete the experiment. Once subjects finished, they turned in their packets and exited the room.

The experimental design can be shown as follows (Shadish et al., 2002):

Table 1

Experimental Design

<i>R</i>	<i>X_a</i>	<i>O</i>
<i>R</i>	<i>X_b</i>	<i>O</i>
<i>R</i>		<i>O</i>

In this diagram, *R* stands for random assignment, *X_a* and *X_b* are the treatments (intraorganizational and extraorganizational crisis history groups, respectively), and *O* is the observation, or post-test. The third group, the one that received no treatment, was the control. The unknown crisis history condition group was the control group.

As can be seen above, there was no pretest. There are several reasons for the lack of a pretest. Pretests are usually preferred in experimental design, but in some situations they are either unnecessary, detrimental, or both (Shadish, et al., 2002). Pretests are detrimental “whenever pretesting is expected to have an unwanted sensitization effect” (p. 260). That was the case in this experiment, in which testing subjects on reputation scores before they read the crisis scenario would have alerted them to the importance of the reputational aspects of what they were about to read, possibly causing unnatural scrutiny and increasing the artificiality of the experiment.

In addition to being detrimental, pretests are also unnecessary in some situations. One of the chief reasons for pretesting is to be able to control for attrition. Since little or no attrition was expected in this experiment, a pretest was unnecessary (Shadish, et al., 2002). Similarly, pretests are unnecessary when it is reasonable to assume that the dependent variable to be tested in a pretest is a constant across all experimental groups. As discussed above, random assignment to conditions helps to ensure the probability that any confounding factors are distributed evenly among the groups.

To control for preexisting reputational attitudes, Coombs (2004) used a pretest and a complex evaluation to ensure that prejudicial attitudes could not confound his results. These precautions made sense, since he used real, well-known companies in his scenarios: Burroughs-Wellcome, Sharp Electronics, Caterpillar, and Radisson. However, the crisis scenarios used in the instant study were fictional, so it was impossible that subjects' preexisting attitudes could have contaminated the results. In addition, the questionnaire (see appendix G) contained a screening item, "I have either close family members or good friends who work in the construction supply industry" (yes/no). The purpose of this item was to screen anyone who might have confounding attitudes regarding the industry used in the crisis scenarios. Of the 115 subjects, 14 responded affirmatively to the screening question. Since no significant differences were found on reputational scores between those 14 and the remaining 101 (see below), these subjects were not removed from the sample.

The crisis scenarios.

The crisis scenario was the independent variable for H1, R1, and R2. All the scenarios consisted of a fictitious newspaper-style article relating an industrial accident at a

warehouse owned by Alexander Construction Supply Corp. (ACS). The scenarios reported that one worker died and ten were hospitalized after several stacks of steel corrugated roofing sheets fell over, pinning them underneath. Coombs (1998) found that accidents were the best situations to use in this type of experiment, since crisis history has no effect when the crisis is classified as a transgression.

Group 1 ($N = 39$) read the intraorganizational crisis scenario (X_a , see appendix D). Part of the article they read stated that ACS had encountered this type of accident before, but made no mention of any other organization. Group 2 ($N = 38$) read the extraorganizational crisis scenario (X_b , see appendix E). This manipulation consisted of the same story as Group 1, except Group 2's scenario stated that (1) ACS had never had a crisis of this type before, but that (2) Brown's Builders Supply (BBS) had a similar accident last year, in which two workers died and three were injured. As news reports often mention previous, similar events to provide context to a story, this technique improves external validity (Coombs, 1998).

Group 3 ($N = 38$), the control group, merely read of the accident at ACS without any mention of crisis history (intraorganizational or extraorganizational). No mention of BBS was in Group 3's scenario either (see appendix F). This scenario was the unknown crisis history condition. To control for confounding effects, all the scenarios were balanced so that they were equal in length (Coombs, 2004).

The independent variables can be summarized as follows:

Table 2

Experimental Group Assignments

	Group 1 (<i>N</i> = 39)	Group 2 (<i>N</i> = 38)	Group 3 (<i>N</i> = 38)
<i>Condition</i>	Intraorganizational	Extraorganizational	Unknown crisis history
<i>Manipulation</i>	ACS recently faced a similar crisis	(1) ACS has never faced a similar crisis, but (2) BBS (same industry) recently faced a similar crisis	No mention of crisis history in account of accident

The instrument: a survey questionnaire.

After the three experimental groups read their manipulations, they answered a post-test questionnaire (see appendix G) testing their attitudes about ACS. Reputation was tested, as opposed to responsibility for the crisis, because Coombs (2004) found that crises affect organizational reputation, not responsibility. Reputational scores were the dependent variable of the experiment, and were measured using the Organizational Reputational Scale, a five-item scale developed by Coombs and Holladay (1996) and based on a character scale developed by McCroskey (1966). This scale was refined and used in later research as well (see Coombs, 1998, 2004; Coombs & Holladay, 2001, 2002; Coombs & Schmidt, 2000), and its use in those studies has produced reliabilities ranging between .81 to .92 (Cronbach's

alpha) (Coombs, 2004). The last item of this part of the questionnaire asked subjects whether ACS had a history of crises. This item served as a manipulation check for the three crisis scenarios and was asked last to minimize any potential effects (Coombs, 2004).

The second section of the questionnaire was a series of demographic questions, in which respondents were asked their sex, year in school, sequence of study in the School of Journalism and Mass Communication (advertising, electronic communication, news editorial, public relations, visual communication), and whether they had taken specific courses within the school (news writing, principles of public relations, or principles of advertising). The final item on the questionnaire was the screening question, discussed above.

CHAPTER 5

RESULTS

This chapter reports on the results of an experiment involving 115 undergraduate students. The experiment's purpose was to test the difference that intraorganizational and extraorganizational crisis history have on organizational reputation. Once the data were collected and entered into a computer database, statistical analyses were performed using the Statistical Package for the Social Sciences (SPSS) software, version 15.0. This section begins with a report on the reliabilities associated with the survey instrument, then details the results of the manipulation check item. Next, the results of the hypotheses and research questions are given. Finally, the results for the tests for significant differences on the demographic variables are given.

Reliabilities

The Organizational Reputation Scale comprised the first five items on the questionnaire. For this experiment, the scale's reliability was measured at 0.80 using Cronbach's alpha, which was slightly lower but within the acceptable range and consistent with its reliability in the extant research (Coombs, 2004; see also Coombs, 1998; Coombs & Holladay, 2001, 2002; Coombs & Schmidt, 2000). In addition, an item analysis assessing a single construct was run on the Organizational Reputation Scale. Since the lowest corrected item-total correlation was above 0.5 (see table 3), and Cronbach's alpha would decrease if

any item were dropped, no single item differed significantly from the other items in terms of reliability (Green & Salkind, 2005). Thus the scale as a whole, and each item, was reliable.

Table 3

Item-Total Statistics for the Organizational Reputation Scale (N = 115)

	Scale mean if item deleted	Scale variance if item deleted	Corrected item-total correlation	Cronbach's alpha if item deleted
<i>-ACS is concerned with the well-being of its publics.</i>	14.44	6.863	.609	.754
<i>-ACS is basically DISHONEST.</i>	13.50	6.866	.585	.760
<i>-I do NOT trust ACS to tell the truth about the incident.</i>	13.93	5.925	.625	.748
<i>-Under most circumstances, I would be likely to believe what ACS says.</i>	14.02	7.193	.513	.781
<i>-ACS is NOT concerned with the well-being of its publics.</i>	14.12	6.371	.591	.758

Since the second, third, and fifth items of the Organizational Reputation Scale were reverse-coded, meaning that a highly reputable company would score low on these items, these scores were recoded to reflect the other two. As a result, a higher score meant a higher perceived reputation for each item. Scores on the scale were then summed to compute a new variable called the composite reputation score for each subject. Scores on the composite reputation score could range from 5 to 25. Again, a higher score meant a better reputation according to the scale. This approach follows previous procedures used in this type of research (Coombs, 2004; see also Coombs and Schmidt, 2000).

Manipulation Checks

The sixth item on the questionnaire, “ACS has a history of similar crises,” served as a manipulation check for the crisis scenarios. The intraorganizational crisis history condition stated explicitly that ACS had experienced a previous crisis, while the extraorganizational condition stated explicitly that ACS had *not* had a previous crisis. Subjects could choose from a low score of 1 (*strongly disagree*) to 5 (*strongly agree*). The N for this item was 114, since one subject out of the overall 115 did not mark a response for this item. The intraorganizational condition garnered the highest scores on this item ($M = 3.72$), followed by the unknown crisis history condition ($M = 2.62$), and the extraorganizational condition ($M = 1.89$), as expected. A one-way analysis of variance (ANOVA) was run on this item to test the significance of the differences in the three means. The differences were found to be significant, $F(2, 111) = 30.5, p < .001$. As the Dunnett C post hoc analysis in table 4 shows, each of the three means was significantly different from the other two.

Table 4

Post Hoc Tests Comparing Means of the Manipulation Check Item (N = 114)

Dunnett C

(I) Condition	(J) Condition	Mean difference (I-J)	Std. error
1- Intra	Extra	1.823*	.263
	Unknown	1.096*	.215
2 - Extra	Intra	-1.823*	.263
	Unknown	-.727*	.225
3 - Unknown	Intra	-1.096*	.215
	Extra	.727*	.225

* The mean difference is significant at the .05 level.

The manipulations were thus successful. In other words, the subjects were able to distinguish correctly among the three crisis scenarios, at least when it came to identifying whether the organization had a history of crises or not.

Tests of Hypotheses and Research Questions

Scores on the Organizational Reputation Scale, as measured by the composite reputation score variable, produced some puzzling results. Recall that the composite reputation score is a sum of each subject's scores on the five-item scale, and thus could range from a low of 5 to a high of 25. As expressed in Hypothesis 1, it was expected that an organization with a history of crises would suffer more reputational damage in a current crisis than an organization without crisis history or with an unknown crisis history. However, the mean score for the unknown crisis history condition ($M = 16.7$, $N = 38$) was lower than both the intraorganizational condition ($M = 17.2$, $N = 39$) and the

extraorganizational condition ($M = 18.6, N = 38$). A one-way ANOVA found that the means were significantly different, $F(2, 112) = 3.7, p < .05$. A Dunnett post hoc analysis showed that the significant difference was between only the unknown score mean and the extraorganizational mean (see Table 5).

Table 5

Post Hoc Tests Comparing Means of the Composite Reputation Score

($N = 115$)

Dunnett C

(I) Condition	(J) Condition	Mean difference (I-J)	Std. error
1 - Intra	Extra	-1.374	.699
	Unknown	.468	.695
2 - Extra	Intra	1.374	.699
	Unknown	1.842*	.713
3 - Unknown	Intra	-.468	.695
	Extra	-1.842*	.713

* The mean difference is significant at the .05 level.

Since there was no significant difference between scores on the composite reputation variable for the intraorganizational condition and the unknown condition, Hypothesis 1 was not supported. Hypothesis 2 posited that reputation scores for no crisis history, expressed here as the extraorganizational condition, and unknown crisis history would not vary significantly. Since the results show that there was a difference in scores between the

unknown and extraorganizational (no crisis history) condition, Hypothesis 2 was not supported either.

Research Question 1 asked whether extraorganizational crisis history would have a negative impact on an organization's reputation. The intent of Research Question 1 was to test the difference between the extraorganizational and unknown conditions, since it was expected that the scenarios that included crisis history would produce a lower reputation score than the unknown condition. As it happened, the unknown and extraorganizational conditions did produce significantly different scores, but in the opposite direction of what would be expected: The mean composite score for the unknown condition was well *below* the score for the extraorganizational condition.

Thus it must be concluded that extraorganizational crisis history did not have a negative impact on reputation. In fact, extraorganizational crisis history seemed to have a positive impact on reputation, as it garnered the highest overall composite reputation score. Subjects, it seems, viewed the corporation with an unknown history of crises more harshly than they viewed the corporation without a history but in an industry with a history of crises.

Research Question 2 dealt with the degree of the damage caused by extraorganizational crisis history as compared to damage from intraorganizational crisis history. Since there was no significant difference between the means for the intraorganizational and extraorganizational conditions, Research Question 2 was inconclusive. It should be noted, however, that the mean score for the extraorganizational condition ($M = 18.6$) was higher than the score for intraorganizational ($M = 17.2$) on the 5 to 25 scale. The direction of this difference indicates that extraorganizational crisis history may be less damaging than the intraorganizational kind.

Other Tests for Significance

T tests for the dichotomous demographic variables.

A bevy of t tests was performed on the data to ascertain significant differences in composite reputation scores among the dichotomous demographic variables. The dichotomous demographic variables were the ones which included only two possible responses from subjects. They were sex (male/female), introductory journalism courses completed (true/false for news writing, advertising, public relations, and none), and the screening item—whether subjects had friends or family in the construction supply industry (yes/no).

As an example, the t test for the sex variable is illustrated: First, cases were selected from the data set that corresponded to the students who read Condition 1 (intraorganizational). Then, an independent samples t test was run on these cases to test for significant differences between the mean scores of males ($M = 17.0$, $N = 10$) and females ($M = 17.3$, $N = 10$) for Condition 1. These results are reproduced in Table 6.

Table 6

T Test for Independent Samples Comparing Differences in Reputational Scores by Sex for Condition 1 (Intraorganizational) (N = 115)

		<i>t</i> tests for equality of means				
		t	df	Sig. (2-tailed)	Mean difference	Std. error difference
Composite Reputation Score	Equal variances assumed	-.248	37	.806	-.276	1.115
	Equal variances not assumed	-.228	13.717	.823	-.276	1.212

As can be seen in the table, the *t* test was not significant at the .05 level, meaning that there was no significant difference between the mean scores for males and females for the intraorganizational condition. Next, the cases were selected that corresponded to subjects who read the extraorganizational crisis scenario (Condition 2). The *t* test was repeated for these cases, and again no significant differences were found. Condition 3 cases were selected and tested next, and then all cases were selected and the *t* test run.

No significant differences were found based on sex for any of the conditions, and no significant difference was found when all cases were included in the analysis. In fact, when the four *t* tests were run on each of the six dichotomous demographic variables, no significant differences were found at all. Thus, the mean composite reputation scores did not vary significantly by sex, journalism classes taken, or whether the subjects had friends or family in the construction supply industry.

Significance tests for polychotomous variables.

The polychotomous variables, those items that included more than two choices, could not be tested using t tests. Instead, one-way ANOVAs were used to test for significant differences in the mean composite reputation scores for each of these. The polychotomous variables were class (freshman/sophomore/junior/senior/graduate student) and sequence of study (advertising/electronic communication/news editorial/public relations/visual communications/not a journalism major). Four one-way ANOVAs were run on each of these variables, three for each condition and one including all cases. Again, no significant differences were found in any of the relationships. Mean scores for the composite reputation score did not vary significantly by class or sequence of study.

Summary.

The results of this experiment are varied. The survey instrument was found to be reliable, as expected. The manipulation checks were positive, meaning that subjects understood that the different scenarios related differences in their crisis histories. Hypothesis 1, which stated that intraorganizational crisis history would negatively affect reputation, was not supported. Hypothesis 2, which stated that subjects would view an organization with no history of crises the same as an organization with an unknown history, was not supported. The results of Research Question 1 determined that extraorganizational crisis history did not have a negative effect on reputation. The results for Research Question 2, which aimed to determine the difference between intraorganizational and extraorganizational crisis histories' effect on reputation, was inconclusive, but suggested that extraorganizational crisis history may be less damaging than the intraorganizational kind. Finally, no significant differences

were found in any of the three conditions when the analysis controlled for the demographic variables.

These varied results certainly require interpretation and discussion. This analysis is contained in the next chapter, along with suggestions for further research and a summary and conclusion section for the entire study.

CHAPTER 6

DISCUSSION

The purpose of this study was to examine the relationship between an organization's crisis history and its effect on that organization's reputation in a current crisis. No support was found for the two hypotheses. The results for one research question yielded some answers, while the other research question remains inconclusive. In other words, the study provided some answers and raised many more questions. Some of these questions are theoretical in nature and some are more practical. This section will look at both the theoretical implications and the practical implications of this study. Next, suggestions will be given for further research in this area. The study's impact of on crisis communication theory is discussed first.

Theoretical Implications of the Study

This research study examined the difference that extraorganizational crisis history, as opposed to intraorganizational crisis history, might make on corporate reputation. In that respect, the experiment was inconclusive, since no significant difference was found between the attitudes of subjects who read that Alexander Construction Supply had previously experienced a crisis and those who read that the history belonged to Brown's Building Supply.

An interesting finding of the experiment was that extraorganizational crisis history protected a company's reputation more than an unknown crisis history. This finding runs contrary to Hypothesis 2, which postulated that the extraorganizational and unknown conditions would garner similar reputational scores. Hypothesis 2 was based on Coombs' (2004) study, which found that publics viewed an organization with no crisis history the same way they viewed an organization with an unknown crisis history. It is a corollary of Coombs and Holladay's (2001) velcro effect, which stated that a positive performance history (a halo) does not help an organization when faced with a crisis, but that negative performance history does adhere to an organization during a crisis. As explained next, though the results of the instant experiment found no support for these ideas, they did not necessarily erode support either.

Previous studies found no difference in reputational damage between a corporation with no crisis history and an organization with an unknown crisis history (*e.g.*, Coombs, 2004; Coombs & Holladay, 2001). None of these studies included extraorganizational crisis history, though, when presenting a no crisis history scenario. In other words, a no crisis history condition has always stated explicitly that the organization in question has never experienced a similar crisis before. The extraorganizational condition used in this study, however, stated additionally that another organization had faced a similar crisis. There was no way to tell from this experiment whether the cause of the difference in the perception of reputation was the difference between a no crisis history scenario and an unknown history scenario, or whether it was due to the added extraorganizational history. In fact, since extant research has shown that no crisis history and unknown crisis history are treated the same by publics (see Coombs, 2004; Coombs & Holladay, 2001), it would be logical to assume that

the difference was due to the extraorganizational history. But since this experiment did not test explicitly for that effect, it cannot be concluded with any known degree of certainty.

Explaining low scores for the unknown condition.

The most perplexing finding of this study was the direction of the difference between the unknown condition and the intraorganizational and extraorganizational conditions. Even if one assumes that the difference between the unknown condition and the extraorganizational condition was due to the mention of the crisis history of the other organization as explained above, the fact remains that the corporation in the intraorganizational condition scored better than the corporation in the unknown condition. Again—an organization with a crisis history earned better reputational scores than the organization with an unknown crisis history.

Although the difference between the reputation scores for the unknown condition and the intraorganizational condition were not significant, it is still surprising that the mean score for the unknown condition was lower than the intraorganizational score. Previous research found not only that there is a significant difference between the two, but that negative crisis history damages a corporation's reputation, while corporations with an unknown history tend to receive the benefit of the doubt from publics, thus protecting their reputations (Coombs, 1998, 2004; Coombs & Holladay, 1996, 2001).

The results of this study show that it is possible that crisis history matters less than previously thought. But since the extant research has repeatedly shown this not to be the case (see Coombs, 1998, 2004; Coombs & Holladay, 1996, 2001), it is more likely that the problem lies in the design of this study. Since no significant differences were found on the

demographic variables, it would seem unlikely that any difference between the sample and the population is the cause of the peculiar finding. Of course, the sample may differ in some other area than the ones tested—say, political philosophy or age—and that difference could be the cause.

It would be more correct to say that a difference could exist between the sample used in this study and the samples used in previous studies. Generalizability to the population is the idea behind all causal inference research, but comparing samples to samples is more straightforward than comparing a sample to the population. In terms of the kind of subjects used, three of the previous studies that found crisis history to have a negative impact on reputation used student subjects exclusively (Coombs, 1998; Coombs & Holladay, 1996, 2001), and the other one used a combination of students and “community members” (Coombs, 2004, p. 274). In terms of the number of subjects used, in Coombs’ 1998 and 2004 experiments, the N was much higher than this study’s 115 participants, at $N = 518$ and $N = 321$, respectively. In the other two studies, the N was much closer, at $N = 116$ (Coombs & Holladay, 1996) and $N = 174$ (Coombs & Holladay, 2001). Thus, the age and number of subjects used were similar.

As noted above, the age of the students in the previous experiments using student subjects and the age of the students in this experiment is quite similar (*e.g.*, Coombs, 1998; Coombs & Holladay, 1996, 2001). At least, that is true when age is thought of as the age of the participant when the experiment was conducted, the samples were similar. But age can also be thought of as generational, which may have made an impact on the results.

The three studies that informed this study’s assumptions about crisis history’s effect on reputation were Coombs (1998), Coombs and Holladay (1996) and Coombs and Holladay

(2001). The publications dates for these three studies range from 1996 to 2001, most likely meaning that the experiments themselves were carried out between 1995 and 2000. A 19-year-old student in 1995 would have been born in 1976, part of “Generation X” (Poindexter & Lasorsa, 1999). A 19-year-old student in 2007 would have been born in 1988, or 12 years younger than the subject in Coombs’ 1996 study. These subjects would certainly be considered part of separate generations, and that may have had some influence on the results. Perhaps the younger generation views crises less harshly because of the rise of 24-hour news networks, new media, or some other factor—and that may have contributed to a different view of reputation for the different generations.

Other than differences in the samples, four possible explanations for the disparate scores remain. Either the difference is due to a difference in the instrument, the procedures, chance, or the crisis scenarios themselves. Differences in the instrument may be ruled out, since this study employed the same five-item questionnaire that was used in the previous experiments (*e.g.*, Coombs, 1998, 2004; Coombs & Holladay, 2001, 2002; Coombs & Schmidt, 2000). Similarly, the procedures used in this study were carefully and purposefully replicated from the earlier research. Some differences exist, of course, since the focus of this study was slightly different than the others. For instance, this study used a shorter questionnaire than some of the earlier experiments. But since the procedures used here were substantially similar to the extant research, it is unlikely that procedural differences account for the disparate finding.

Chance is another explanation, and once again, one that can never be eliminated totally from consideration. As discussed in chapter 4, the Method section, random assignment is used in experiments to ensure that each condition has an equal chance of

containing subjects who vary from the mean subject in some significant way. It is important to note that random assignment does not ensure that group differences will be spread evenly among the conditions, only that each subject has an equal chance of being in each condition (Shadish, et al., 2002). As an example, take the sex of the subjects in this experiment. The sample as a whole contained 87 females and 28 males; it was 75.7% female. Among the three conditions, however, the percentage female varies: Conditions 1 and 2 contained 74.4% and 73.7%, respectively; but the ratio of females in Condition 3 jumped to 78.9%.

The post-experiment analyses showed that the reputational scores did not vary significantly either by sex or any of the other demographic variables. So having, for example, a higher ratio of females to males in the unknown condition did not influence the results to any significant degree. By testing for differences between conditions on the demographic variables, the experiment controlled for any possible influence they may have had on the results. What is not known is whether some other factor, one for which the experiment did not control, has confounded the results.

There is always a chance that the makeup of the conditions varied significantly from each other on some variable for which the experiment did not control. For example, perhaps the unknown condition was overly populated by environmental extremists, and perhaps those subjects bore great grudges against the construction supply industry. Is this explanation of the discrepancies among the conditions likely? It is not far-fetched to think that environmentalists would appear on a college campus. It is a little far-fetched, though, to think that the unknown condition contained significantly more environmental extremists than the intraorganizational or extraorganizational conditions. Therefore it is possible, but not likely, that chance could explain the low scores for the unknown condition.

The fourth explanation for the perplexing results of the experiment is that the difference had something to do with the crisis scenarios themselves. This explanation seems most likely. Two factors may be the cause of the difference. The first is the lack of large-scale impact on the community from the crisis. The second is the direct quote in the unknown condition. The lack of community impact will be discussed first.

In large crises, communities are almost always affected. Whether it is a chemical spill that taints drinking water, an industrial fire that requires evacuation of nearby neighborhoods, or a workplace shooting that injures innocent bystanders, crises usually have some impact on the world beyond the organization (these crises were used in Coombs, 2004). The crisis used for this experiment was no exception, but it did not affect the larger community on the scale of crises used in earlier experiments, such as those mentioned above.

When one person is killed and ten injured on the job, per this study's scenario, people outside of the corporation are certainly affected. Those affected would include the families and friends of the victims, as well as any religious, recreational, or social groups of which they were a part. There is less of a chance, however, that a subject reading a news article about the crisis would be apt to think "this crisis could affect me." This it-could-happen-to-me sentiment, or *personalization factor*, is low for the crisis scenarios used in this experiment, while it would be higher for the crises used in past experiments (*e.g.*, Coombs, 1998, 2004; Coombs & Holladay, 2001). For instance, in the workplace shooting scenario used by Coombs (2004), innocent bystanders (not just employees) were injured, raising the personalization factor. When a scenario has a high personalization factor, publics may take more notice of a corporation's actions, since those actions could affect their lives in some way. This researcher is unaware of any construct discussed previously in the crisis

communication literature that would approximate the personalization factor, although one may certainly exist.

If it is assumed that the personalization factor of a crisis scenario could affect subject responses in this type of experiment, how could that factor cause one condition's scores to be lower, when the same crisis was reported in all three scenarios? One explanation could be that since the intraorganizational and extraorganizational scenarios discussed more than one crisis, these two conditions may have led to higher combined feelings of personalization than the unknown scenario, which reported on just one crisis. Lower feelings of personalization might lead subjects to be more neutral about the reputation of the organization.

The data seem to provide support for this explanation. Recall that the lowest composite reputation score possible was 5, the highest 25. Thus a completely "neutral" score would be 15, obtained by a subject rating each of the five items on the organizational reputation scale a "3," the midpoint of the five-level Likert scale. As Table 7 shows, the mean score for the unknown condition was closest to a neutral 15.

Table 7

Composite Reputation Scores for the Three Experimental Conditions

Condition	<i>N</i>	Mean	5% trimmed mean	Std. deviation
1 - Intra	39	17.21	17.34	3.002
2 - Extra	38	18.58	18.73	3.125
3 - Unknown	38	16.74	16.71	3.090

Not only does the unknown condition score closest to neutral, but when the extreme scores (the top 2.5% and the bottom 2.5%) are "trimmed" from the data points, the mean for

the unknown condition decreases to 16.71, even closer to a neutral 15. Conversely, when extreme scores are dropped from the mean statistic for the other two conditions, they move farther from a neutral score. The correct way to interpret the difference in the scores, then, might be that the organization in the unknown condition received a *more neutral* score than the other two, not a *worse* score.

The last explanation for the unexpected scores for the unknown condition is the direct quote used in the scenario for the unknown condition. All three conditions contain an expert opinion on whether ACS stacked the roofing sheets too high. The opinion given in all three is intentionally ambiguous, since establishing crisis responsibility was not a goal of the experiment. The expert opinion was presented the same way—as an indirect quote—in both the intraorganizational and the extraorganizational conditions: *Twenty feet is a high, but acceptable height for material of that type to be stacked in a warehouse, one expert said.* But in the unknown condition, the expert was quoted directly: *“Twenty feet is mighty high, but it’s still around an acceptable height for steel sheets like that,” said Tom Currier, who designs warehousing systems for Jamison Inventory Systems in Eugene. “I’d have to see exactly what the setup was before I’d be willing to say whether Alexander was stacking them too high or not.”*

The reason that the expert opinions read differently in the scenarios was purely a space issue. To control for any effects of reading a comparatively longer or shorter scenario, the scenarios were written so that they would be equal in length (Coombs, 2004). The direct quote was used to provide bulk to the unknown scenario. To that end the unknown scenario also provided more information than the other two scenarios regarding funeral plans for the dead worker. In the other two scenarios this space was used to report on the previous crisis

experienced by ACS (in the intraorganizational scenario) or Brown's Building Supply (in the extraorganizational scenario).

Using a direct quote from an expert may have unintentionally led readers of the unknown condition to judge ACS more harshly than in the other conditions. In other words, subjects in the unknown condition may have attributed more responsibility for the crisis to ACS than did the subjects in the intraorganizational and extraorganizational conditions. While Coombs (2004) found that crisis history does not affect crisis responsibility, it is well established that crisis responsibility does have a negative effect on organizational reputation (Coombs, 1998, 2004; Coombs & Holladay, 1996, 2001). Thus, the direct quote in the unknown crisis history scenario may have unwittingly produced higher levels of causal attribution, which would account for the lower reputational scores for that condition.

Summary.

It is impossible to say why the results of this study contradict previous research in particular areas. Although some plausible explanations are offered above, more research into these questions is needed before the answers will be known with any certainty. As often happens in research, knowing what questions to ask, and how to ask them, is as important as any other part of the scientific process.

Practical Implications of the Research

Should a corporation in trouble point to mistakes made by other companies to lessen its own loss of reputation? That is one of several practical questions raised by this study. If publics' knowledge of extraorganizational crisis history can protect reputational assets, as

this study suggests, then informing publics of that history may be a good way to protect a firm's reputation. When publics see an accident as an aberration, they may naturally wonder why—if the corporation is truly not at fault—they have not heard of this sort of thing happening before. Thus, putting a crisis in the context of another organization's past crisis may help publics see the current crisis as an unfortunate accident, but one that could happen in the normal course of business nonetheless.

Bringing up other corporations' crises in order to protect one's own assets may seem ethically questionable at first blush. After all, if Exxon has an oil spill and released information proving that BP's CEO was having an extramarital affair, ethical questions would surely arise. Remember, however, that the extraorganizational crisis history as proposed in this study has two dimensions that the above scenario would violate. First, extraorganizational crisis history involves only crises that are classified as accidents, not transgressions. Second, BP's previous crisis would need to be not only an accident, but an accident that is similar in nature to the current crisis.

Thus, a more fitting scenario would be if Bayer were involved in a product-tampering crisis and pointed to Johnson & Johnson's actions in the Tylenol tampering case. In other words, the mood of the communications would need to be "another good company faced this crisis and maintained the public trust; we intend to do the same," not merely "other companies have had lapses too." A corporation employing this strategy ethically would need to provide a point of reference, not a scapegoat; informing, not excusing.

As Coombs (1998) noted, a corporation usually does not have to be the one to bring up its own past crises, as news stories often "contextualize a crisis in terms of past crises experienced by the organization" (p. 183). When it is another organization's past crisis,

though, the media may not be as perceptive about the past. The corporation may have to remind news organizations of similar happenings to influence how the story is framed.

Of course, this strategy assumes that extraorganizational crisis history really lessens reputational damage. This study far from proved that hypothesis, but it did suggest that it might be the case. More research must be done in this area before corporations begin to include this tactic as part of their crisis communication strategy, but it does provide a starting point for discussing extraorganizational crisis history and its effect on publics' perception of a corporation in a crisis.

This research did not provide conclusive evidence about the effect of crisis history on reputation, but it did show that publics are able to distinguish crisis history. The subjects in the study had no trouble discerning whether ACS had faced a previous crisis or not, according to the scenario they read. This finding means that crisis managers must be mindful of both their own and their industry's record when faced with a crisis. Publics will look for that information, so practitioners had better be aware of it, too.

Further Research

This study provides several suggestions for further research. A small but important finding revealed the continued reliability of the Organizational Reputation Scale as used in previous research (*e.g.*, Coombs, 1998, 2004; Coombs & Holladay, 2001, 2002; Coombs & Schmidt, 2000). Researchers should continue to use this instrument when measuring corporate reputation.

The most important course for future research, to be sure, is the effect of extraorganizational crisis history on reputation. Scholars must pursue this relationship.

Some interesting and tantalizing ideas were laid out, but they are far from settled matters. Several questions need to be answered. Paramount is whether extraorganizational crisis history really protects reputational assets—but that question raises puzzles of its own. Does it matter how long ago the other crisis occurred? Does proximity matter? Does being in the same industry matter? When do publics stop seeing a new crisis as unique to one organization and hold an entire industry accountable for its collective crisis history? Knowing the answers to these questions will be valuable to the field and will help practitioners develop more effective crisis communication strategies.

Another area ripe for exploration is the effect of what this study called a crisis' personalization factor on publics' perception of reputation. Will publics view corporations differently in two crises, one that they perceive as unable to affect them and one in which they can see themselves involved? In other words, do publics not care about a crisis because they do not care about the company involved, and vice versa? The answers to these questions may provide another level of discrimination to crisis typology, as discussed in chapter 2.

Conclusion

Despite the somewhat inconclusive results and findings that were incongruent with existing research, this study provided valuable insight into crisis communication. Rejecting research hypotheses should never be considered a failure in scientific investigation, but rather one more step in the pursuit of the truth. Often, discovering inconsistencies in a theory can result in a more nuanced and superior theory than if the results had fit perfectly into the accepted ways of thinking about phenomena.

This study confirmed some parts of crisis communication theory and challenged others. It answered some questions and brought some new questions to the fore. Perhaps more than anything else, it demonstrated that crisis communication theory requires more scrutiny by empirical research, and especially by experimentation, before the academy can offer much conclusive advice to practitioners in the field.

Appendix A:

IRB Approval

THE UNIVERSITY
of NORTH CAROLINA
at CHAPEL HILL

OFFICE OF HUMAN RESEARCH ETHICS
Medical School Building 52
Mason Farm Road
CB #7097

TO: John Elliot
Journalism and Mass Communication
CB: 3365

FROM: Behavioral IRB

Authorized signature on behalf of IRB

APPROVAL DATE: 2/13/2007

EXPIRATION DATE OF APPROVAL: 2/12/2008

RE: Notice of IRB Approval by Expedited Review

Submission Type: Initial

Expedited Category: 7. Survey/group chars

Study #: 07-0160

Other #: School of Journalism and Mass Communication

Study Title: The Effect of Crisis History on Organizational Reputation

This submission has been approved by the above IRB for the period indicated. It has been determined that the risk involved in this research is no more than minimal.

Federal regulations require that all research be reviewed at least annually. It is the Principal Investigator's responsibility to submit for renewal and obtain approval before the expiration date. You may not continue any research activity beyond the expiration date without IRB approval. Failure to receive approval for continuation before the expiration date will result in automatic termination of the approval for this study on the expiration date.

When applicable, enclosed are stamped copies of approved consent documents and other recruitment materials. The expectation is that you will copy these for use with subjects.

You are required to obtain IRB approval for any changes to any aspect of this study before they can be implemented (use the modification form at ohre.unc.edu/forms). Should any adverse event or unanticipated problem involving risks to subjects or others occur it must be reported immediately to the IRB using the adverse event form at the same web site.

Study Description:

Purpose: To examine how an organization's history of crises affects its reputation in a crisis.

Participants: 250 UNC-Chapel Hill students in the Journalism's research pool.

Procedures: Participants read one of three fictionalized news stories and respond to a brief questionnaire.

Details:

This study was reviewed in accordance with federal regulations governing human subjects research, including those found at 45 CFR 46 (Common Rule), 45 CFR 164 (HIPAA), and 21 CFR 50 & 56 (FDA), where applicable.

The University of North Carolina at Chapel Hill holds a Federal Wide Assurance approved by the Office for

Human Research Protections, Department of Health and Human Services (FWA # 4801).

If you have any questions about your study, please contact the IRB at 966-3113, or email the Behavioral IRB at aa-irb-chair@unc.edu.

Good luck with your research!

Lawrence B. Rosenfeld, Ph.D.
Office of Human Research Ethics
Co-Chair, Behavioral Institutional Review Board
CB# 7097, Medical School, Bldg 52
University of North Carolina at Chapel Hill
Chapel Hill, NC 27599-7097
aa-irb-chair@unc.edu
phone 919-962-7760; fax 919-843-5576

CC: Lois Boynton, Journalism/mass Communication, CB:3365 397 Carroll Hall, Faculty Advisor
Francesca Carpentier, (School of Journalism and Mass Communication), Non-IRB Review Contact

Appendix B:

Cover Sheet and Directions

Directions: This research study will take approximately 20 minutes. It has three parts. First, you will find an informed-consent form. Please read this form in its entirety, and sign it where appropriate if you agree to be a part of the study. Second, you will find a news article concerning a crisis event that you are to read. Third in this packet is a questionnaire concerning the news article. Please complete the questionnaire after you have read the article. Once you have completed the questionnaire, you may return the entire packet to the proctor and leave the room. Thank you.

Appendix C

Informed-consent Form

University of North Carolina-Chapel Hill
Consent to Participate in a Research Study
Adult Participants
Social Behavioral Form

IRB Study # 07-0160

Consent Form Version Date: 12 February 2007

Title of Study: The effect of crisis history on organizational reputation.

Principal Investigator: J. Drew Elliot

UNC-Chapel Hill Department: Journalism and Mass Communication

UNC-Chapel Hill Phone number: 919-260-2717

Email Address: drew.elliott@unc.edu

Faculty Advisor: Dr. Lois Boynton, Ph.D.

Study Contact telephone number: 919-260-2717

Study Contact email: drew.elliott@unc.edu

What are some general things you should know about research studies?

You are being asked to take part in a research study. To join the study is voluntary.

You may refuse to join, or you may withdraw your consent to be in the study, for any reason, without penalty.

Research studies are designed to obtain new knowledge. This new information may help people in the future. You may not receive any direct benefit from being in the research study. There are no known risks of participating in this research study.

Details about this study are discussed below. It is important that you understand this information so that you can make an informed choice about being in this research study.

You will be given a copy of this consent form. You should ask the researcher named above any questions you have about this study at any time.

What is the purpose of this study?

The purpose of this research study is to learn how corporate accidents (crises) affect public perception.

How many people will take part in this study?

If you decide to be in this study, you will be one of approximately 250 people in this research study.

How long will your part in this study last?

The study should take no more than 20 minutes. You will receive 1 hour of credit towards your Journalism research requirement. Remember also that there are other ways to fulfill your research requirement in addition to study participation.

What will happen if you take part in the study?

Study participants will be asked to read a newspaper article and then complete a short questionnaire about what they have read.

What are the possible benefits from being in this study?

Research is designed to benefit society by gaining new knowledge. You may not benefit personally from being in this research study.

What are the possible risks or discomforts involved from being in this study?

There are no known risks to participating in this study. As with any study, there may be uncommon or previously unknown risks. You should report any problems to the researcher.

How will your privacy be protected?

The researchers will make every effort to protect your privacy. Your name will only appear on this informed consent form and in the records for the Journalism Participant Pool. Your responses to the questionnaires will only be associated with a code number that we assign, but that number is not and will not be connected in any way with your name. Thus, your responses are anonymous. The data will only be accessible to the researchers, and will be stored separately from consent forms and anything that might identify you. Data from this study may be kept for seven years, in keeping with the requirements of academic journals, after which time the data may be destroyed. In any presentations, written reports, or publications, no one will be identifiable and only group results will be presented.

Will you receive anything for being in this study?

You will receive one hour of course research credit for taking part in this study.

What if you have questions about this study?

You have the right to ask, and have answered, any questions you may have about this research. If you have questions, or concerns, you should contact the researchers listed on the first page of this form.

What if you have questions about your rights as a research participant?

All research on human volunteers is reviewed by a committee that works to protect your rights and welfare. If you have questions or concerns about your rights as a research subject you may contact, anonymously if you wish, the Institutional Review Board at 919-966-3113 or by email to IRB_subjects@unc.edu.

Participant's Agreement:

I have read the information provided above. I have asked all the questions I have at this time. I voluntarily agree to participate in this research study.

Signature of Research Participant

Date

Printed Name of Research Participant

Appendix D:

Crisis Scenario 1

Intraorganizational Crisis History Condition, 235 words

Jackson, Ore., January 22, 2007 – An accident at the Alexander Construction Supply warehouse Monday killed one Jackson man and injured ten others, according to police. Rick Nichols, 38, died when a large stack of steel roofing sheets fell over and crushed him. The other workers were injured in the same accident.

The injured workers were taken to St. Mark's Hospital in Jackson, police said. Their names and the extent of their injuries are unavailable at this time.

It is not known what caused the stack of roofing sheets to fall over. Police said that the sheets were stacked about 20 feet high when they fell. Twenty feet is a high, but acceptable height for material of that type to be stacked in a warehouse, one expert said. Police will continue to investigate the incident.

This is not the first time a death has occurred at the Alexander Construction Supply warehouse.

In March of 2006 two workers died when a forklift operator traveling at top speed with a 2,500-pound load turned a corner, overturning the forklift and crushing the operator and another worker. The forklift operator was not wearing a required safety belt at the time of the accident.

A memorial service for Nichols will be held Thursday at 3 p.m. at St. Luke's Lutheran Church on Fairview Road. He will be buried at Shellrest Memorial Cemetery on Oak Lawn Road in Jackson.

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Appendix E:

Crisis Scenario 2

Extraorganizational Crisis History Condition, 236 words.

Jackson, Ore., January 22, 2007 – An accident at the Alexander Construction Supply warehouse Monday killed one Jackson man and injured ten others, according to police. Rick Nichols, 38, died when a large stack of steel roofing sheets fell over and crushed him. The other workers were injured in the same accident.

The injured workers were taken to St. Mark's Hospital in Jackson, police said. Their names and the extent of their injuries are unavailable at this time.

It is not known what caused the stack of roofing sheets to fall over. Police said that the sheets were stacked about 20 feet high when they fell. Twenty feet is a high, but acceptable height for material of that type to be stacked in a warehouse, one expert said. Police will continue to investigate the incident.

This is the first time since it opened in 1994 that an accident at Alexander Construction Supply has caused death or injury. Just last month, however, two workers died in an accident at Brown's Building Supply in Jackson.

Those workers died when a forklift operator traveling at top speed with a 2,500-pound load turned a corner, overturning the forklift and crushing the operator and another worker. The forklift operator was not wearing a required safety belt at the time of the accident.

A memorial service for Nichols will be held Thursday at 3 p.m. at St. Luke's Lutheran Church on Fairview Road.

###

Appendix F:

Crisis Scenario 3

Unknown Crisis History Condition, 236 words

Jackson, Ore., January 22, 2007 – An accident at the Alexander Construction Supply warehouse Monday killed one Jackson man and injured ten others, according to police. Rick Nichols, 38, died when a large stack of steel roofing sheets fell over and crushed him. The other workers were injured in the same accident.

The injured workers were taken to St. Mark's Hospital in Jackson, police said. Their names and the extent of their injuries are unavailable at this time.

It is not known what caused the stack of roofing sheets to fall over. Police said that the sheets were stacked about 20 feet high when they fell.

"Twenty feet is mighty high, but it's still around an acceptable height for steel sheets like that," said Tom Currier, who designs warehousing systems for Jamison Inventory Systems in Eugene. "I'd have to see exactly what the setup was before I'd be willing to say whether Alexander was stacking them too high or not." Police will continue to investigate the incident.

Police and ambulance units responded to a 911 call about 10 a.m. Monday, according to dispatch reports. A second ambulance was dispatched 20 minutes later.

Nichols was dead when the first units arrived, according to paramedics.

A memorial service for Nichols will be held Thursday at 3 p.m. at St. Luke's Lutheran Church on Fairview Road. He will be buried at Shellrest Memorial Cemetery on Oak Lawn Road in Jackson.

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Appendix G:

Questionnaire

I. INSTRUCTIONS: Think about the article you have just read. The items below concern your impression of Alexander Construction Supply (ACS) and the crisis. Circle one number for each of the questions. The responses range from 1 = STRONGLY DISAGREE to 5 = STRONGLY AGREE.

	<i>Strongly disagree</i>				<i>Strongly agree</i>
	1	2	3	4	5
<i>ACS is concerned with the well-being of its publics.</i>	1	2	3	4	5
<i>ACS is basically DISHONEST.</i>	1	2	3	4	5
<i>I do NOT trust ACS to tell the truth about the incident.</i>	1	2	3	4	5
<i>Under most circumstances, I would be likely to believe what ACS says.</i>	1	2	3	4	5
<i>ACS is NOT concerned with the well-being of its publics.</i>	1	2	3	4	5
<i>ACS has a history of similar crises.</i>	1	2	3	4	5

II. Demographics.

1. I am...

☐ male

☐ female

2. I am a...

☐ freshman

☐ sophomore

☐ junior

☐ senior

☐ graduate student

3. I have completed the following courses (new course numbers are listed first, followed by the old course numbers in parentheses:

- ☐ JOMC 153 (53), news writing
- ☐ JOMC 170 (170), principles of advertising
- ☐ JOMC 130 (130), principles of public relations
- ☐ I have not completed any of these courses at this time

4. I am pursuing the following sequence in the School of Journalism and Mass Communication:

- ☐ Advertising
- ☐ Electronic communication
- ☐ News editorial
- ☐ Public relations
- ☐ Visual communications
- ☐ I am not a JOMC major

5. I have either close family members or good friends who work in the construction supply industry:

- ☐ Yes
- ☐ No

That is all. Thank you for your time and cooperation!

###

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