

ABSTRACT

Interview and Test Procedures that Affect the Confidence/Accuracy Relation in Eyewitness Memory for Product Identification

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The study of eyewitness memory thus far has focused on situations involving identification of a suspect that has allegedly committed a crime. The present research concerns eyewitness identification of products an individual may have encountered in the past that has supposedly caused them personal harm. The interview tactics used in pre-trial preparation and a plaintiff's level of involvement with a particular product may affect the confidence/accuracy relation in claimants' product brand identification. Subjects packed various products into care packages and were tested on the brands of products they remember encountering either 10 minutes or 1 week earlier. Subjects provided confirmatory feedback for incorrect responses were more confident at both delay conditions. Subjects were also more confident in incorrect, falsely confirmed responses on target-present surveys than on target-absent surveys. Allowing subjects to select products for inclusion in the care package resulted in higher accuracy, confidence, and *G* scores as opposed to subjects who were provided products. However, asking subjects to justify their product selection at test did not alter accuracy, confidence, or *G*

scores. Applications of the findings to civil cases involving eyewitness identification of product brand identification are discussed.

Interview and Test Procedures that Affect the Confidence/Accuracy Relation in
Eyewitness Memory for Product Identification

by

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CHAPTER ONE

Introduction

For over 20 years, the American civil justice system has dealt with the longest running mass tort litigation in history—personal injury lawsuits brought by claimants exposed to asbestos, a known carcinogen. Claims are based on the premise that the dangers of inhaling asbestos dust were known long before individuals working with the mineral were informed. When claimants filing suit against asbestos companies retain counsel, they are required to identify which manufacturers' brands of asbestos-containing product may have been present in their workplace before the early 1970s. Because OSHA standards for asbestos exposure were put in place about 30 years ago, and the latency period for asbestos-related cancer is often 20 years or longer, plaintiffs must identify asbestos-containing products they worked with *decades* ago. In the landmark study investigating the effects of time on memory performance, Ebbinghaus (1885/1913), concluded that forgetting of previously encoded information occurred very rapidly—within hours. Three-quarters of originally encoded material was forgotten by the end of 6 days, followed by a slower rate of forgetting that persisted over an indefinitely long period of time. These findings have since been replicated using longer retention intervals and more relevant encoding material (Baird, 1971, 1991). The rate of decay depends on the type, function, and perceived relevance of the material in question (Wickens, 1999), but one may reasonably presume that a period of decades might contribute to at least some decline in memory performance.

This renders the product-identification reporting process highly vulnerable to manipulation, particularly with billions of dollars at stake in asbestos litigation. Some attorneys and their assistants have admitted to coaching claimants in preparing their depositions, encouraging and guiding their memory retrieval to reflect encounters with products of companies who have not yet filed for bankruptcy (Brickman, 2004). One guideline for doing so encouraged the claimant to assert that, even when there is no record of what products were used on a particular jobsite, “You will be required [to] do all this from memory. . .the best way to respond. . .is ‘Yes, I am SURE I saw it there!’ or ‘I KNOW it was that brand because I saw the name on the container’” (Olson, 1998, p. 2).

The justice system places—perhaps undue—importance on the common sense of individuals in the general population when determining the accuracy of an eyewitness account (Yarmey, 2001). Jurors tend to accept a witness’ testimony based on its plausibility within the circumstances involved in a case, and evaluate it based on their knowledge and life experience. One study suggests that attorneys, judges, and jurors do not have easy access to eyewitness research findings, requiring them to rely upon their common sense and intuition in deciding the accuracy of an eyewitness account (Schmechel, O’Toole, Easterly, & Loftus, 2006). However, there is a large body of empirical evidence indicating the fallibility of eyewitness memory (Loftus, 1996). In order for jurors to assess the accuracy of an eyewitness account, they need to understand memory’s complexity and malleability (Schmechel et al., 2006). A survey of eyewitness experts found that most would testify that the wording of retrieval questions, presentation of retrieval cues, exposure time to the encoded material, and the hypnotic suggestibility,

attitudes, and expectation of an eyewitness affect the reliability of eyewitness testimony (Kassin, Tubb, Hosch, & Memon, 2001). In addition, judges and jurors rely heavily on the confidence expressed by an eyewitness in determining the likelihood that the eyewitness is accurately reporting event details (Keogh & Markham, 1998). However, countless studies that focus on forensic application of the confidence/accuracy relationship conclude that confidence is a poor indicator of eyewitness accuracy (Penrod & Cutler, 1995). Even though confidence is commonly thought to be an indicator of eyewitness credibility, it is susceptible to distortion by social factors the justice system often overlooks, including post-identification feedback (Dixon & Memon, 2005).

In the laboratory, confidence in the accuracy of one's eyewitness testimony can be influenced by numerous encoding and retrieval conditions. Confidence can be manipulated during the product-identification process by interviewer feedback, particularly that which at least encourages—and at most, outright confirms—the individual's choice. Post-identification feedback effects inflate witness confidence, especially when the witness is incorrect (Bradfield, Wells, & Olson, 2002). Particularly in asbestos cases, corroborating evidence such as purchase receipts and work orders are not available many years after the fact, and the veracity of claimants' testimony cannot be determined. Therefore, in the absence of external corroborating evidence, a witness' confidence becomes particularly important. Given that confidence tends to be a poor indication of accuracy, and that interviewer-controlled factors such as feedback exert predictable effects on confidence especially when accuracy is poor, juries are forced to ascertain witness accuracy based on confidence, an unreliable and highly malleable aspect of witness testimony.

Early suits were brought by those suffering from cancer caused almost exclusively by working directly with asbestos. More recent claims have been filed by non-impaired individuals who did not work with asbestos, but were in the vicinity of those who were handling it. This is arguably the most important change in the state of asbestos litigation of late—that, between 1997 and 2000, the number of non-malignant claims grew to three times that of mesothelioma claims (Hensler, Carroll, White, & Gross, 2001). This phenomenon is due to the recent explosion of claims involving secondary, or premises, defendants.

Premises liability is a supposed failure of a building owner or tenant to maintain their premises in a safe condition (Brennan, 2003). Asbestos suits that name premises defendants claim that asbestos was present in products used during operations on-site or was merely present in the materials that comprised the building itself which the plaintiff occupied for some amount of time. One recent suit was brought against 73 defendants, 35 of which were operators and contractors who did not work with asbestos or asbestos-containing products directly (Warren, 2003). In another suit, the daughter of a man suffering from mesothelioma testified that her father sent her to the hardware store with shopping lists of products for a home-improvement project that occurred 30 years ago (Hampel, 2005). She recalled the joint compound brands Bondex and Georgia-Pacific from the shopping lists, but did not recall any of the brands of other products he used in the project, such as the drywall tape and paint. However, her father had already testified he had never used Georgia-Pacific products and the hardware store where she bought the joint compound never carried the Georgia-Pacific brand. Inconsistency between the testimonies of two individuals, both of whom encountered the same product during the

same exposure episode, demonstrates the need for evidence concerning encoding conditions that affect subsequent identification.

The vast majority of eyewitness memory studies have investigated eyewitness testimony following some criminal event. Research investigating eyewitness identification of products individuals have encountered in the past is relatively rare. Given the enormous costs of product liability lawsuits against manufacturers that have long-since stopped producing asbestos-containing products, the investigator believes more empirical evidence is needed concerning the unique case of product-identification testimony reported many years after exposure. The following studies investigated whether witness confidence reliably indicates accuracy for product brand identification similar to that given in civil testimony.

CHAPTER TWO

Costs and Consequences of Asbestos Litigation

Asbestos was Largely Manufactured and Used Over 30 Years Ago

Asbestos is a silicate material with a crystalline structure that forms on rock in sheets of distinct fiber units 25 nanometers in diameter (Virta, 2002). Mined aggregate fibers .1 – 100 micrometers in diameter and 1 centimeter long have been used since World War II in construction for fire protection, reinforcement, and insulation, in the textile and paper industries for thermal and electrical insulation and in friction products like brake pads, in plastics, rubber, coatings, sealants, adhesives, and asphalt road surfacing (Virta, 2002). According to the U.S. Geological Survey, yearly consumption of asbestos between 1948 and 1973 was 613,000 to 803,000 metric tons (USGS, 2007).

The earliest—and still most widely accepted—estimate of the total number of individuals exposed to carcinogenic levels of asbestos between 1940 and 1979 is 27.5 million (Nicholson, Perkel, & Selikoff, 1982). Selikoff, Churg, & Hammond (1964) explored the causes of death among 1522 members of the Asbestos Workers Union in New York and Newark, NJ from 1943 until 1962. The study found that the rate of death attributed to lung cancer was 6 times that predicted for the general male population. Another study of 21,000 asbestos workers' deaths from 1948 until 1963 found that asbestosis was the primary cause for 1/1000 deaths of workers in asbestos building products, 4/1000 deaths of those in asbestos friction materials, and 13/1000 deaths in asbestos textile material manufacturing (Enterline & Kendrick, 1967).

The risk of contracting asbestos-related pulmonary disease peaks at 20 years following the initial exposure to asbestos (Selikoff, Churg, & Hammond, 1965). Mesothelioma is the result of direct physical contact of asbestos fibers at the interface between lung and mesothelial tissue, which induces cycles of cell death and re-growth that in turn trigger tumor-suppression gene silencing (Christensen, Godleski, Marsit, et al., 2008). A study of 128 patients with mesothelioma found that mortality is dose-dependent upon the quantities of asbestos found in patients' lung tissue at autopsy (Christensen, Godleski, Roelofs, et al., 2008). The decades-long latency period of mesothelioma gives altered cell groups enough time to eventually develop into malignancy. Therefore, workers exposed to asbestos before the Occupational Safety and Health Act of 1970 was enacted discovered that they had the worst form of asbestos-related disease as recently as the 1980s and 1990s.

The Occupational Safety and Health Act of 1970 established the National Institute for Occupational Safety and Health, a regulatory agency that sets standards for safe working practices, enforces safe working parameters, and provides research towards advancements in the occupational safety field. The standard limits for safe asbestos exposure was first determined in the 1971 as part of the Federal Rule for the Emergency Standard for Asbestos Dust. The Emergency Standard for Asbestos Dust (1971) set a safe dosage standard of .1 fiber/cm³ of air in the immediate environment of a worksite. Since then, 43 federal registers and 36 standard interpretations have been enacted as part of the Occupational Safety and Health Administration final rules for asbestos exposure. However, the standards were set too late for the 27.5 million individuals who worked with asbestos prior to 1970. This, in light of the alleged asbestos product manufacturers'

knowledge of the dangers of working with the substance, has since led to over 650,000 personal injury claims filed against 8,400 different companies in at least 75 different industries (Carroll et al., 2005) in what has been described as the largest and longest-running mass tort in U.S. history.

Asbestos Litigation has Changed in Recent Years

The first asbestos claim set the precedent for modern mass tort litigation. It was filed in 1973 by Clarence Borel, an asbestos insulation worker between 1936 and 1969, against Fibreboard Paper Products Corporation (*Borel v. Fibreboard*, 1973). Mr. Borel was diagnosed with pulmonary asbestosis on January 19, 1969 and filed suit on October 20, 1969 against 11 different asbestos insulation manufacturers, among them Fibreboard and another manufacturer of asbestos-containing products, Johns-Manville. According to Mr. Borel, “I blowed this dust out of my nostrils by handfuls at the end of the day,” noting that he and his fellow workers thought “that the dust dissolves as it hits your lungs,” and testified that “yes, I knew the dust was bad but we used to talk [about] it among the insulators. . .how bad was this dust, could it give you TB, . . . and everyone was saying no, that dust don’t hurt you, it dissolves as it hits your lungs.” At his deposition he was asked, “then you did know it had some degree of danger but you didn’t know how dangerous it was?” Mr. Borel responded, “I knew I was working with insulation,” but when asked if he knew it contained asbestos, he replied, “yes, sir, but I didn’t know what asbestos was . . . if I had known [how dangerous it was] I would have gotten out of it” (*Borel v. Fibreboard*, 1973, p. 3). The claim held 11 defendants responsible for the gross negligence of failing to warn the workers of the danger to which they were exposed while working with the products, failure to inform the workers of

appropriate equipment and practices that would have protected them from asbestos exposure, and failure to ascertain the dangers of asbestos products and to remove them from the market once the danger was determined (*Borel v. Fibreboard*, 1973). The defendants countered that the danger was obvious but that it was the responsibility of the purchasers to warn the ultimate users (asbestos product installers, such as Mr. Borel) of the dangers of the products, not the manufacturers themselves. On September 10, 1973, the U.S. Court of Appeals for the Fifth Circuit ruled that the product manufacturers were liable for the failure to warn the claimant of the gravity of the dangers of working with asbestos insulation and affirmed the original trial court's damages in the amount of \$70,000 to be awarded to Mr. Borel's widow. Clarence Borel died of asbestos-related pulmonary disease in 1972.

Since then, the number of asbestos cases has exploded, due to a number of factors that have evolved slowly over the last two decades. The precise number of claims filed is not readily available, but one indicator of the recent trends in asbestos case filing is found in the data reported by the Manville Personal Injury Settlement Trust. The trust's 4th quarter claims filing summary reports that between 1997 and 1999, between 25,000 and 30,000 claims were filed against the Manville Trust. In 2000, the number of claims doubled to 60,000 and in 2001, over 90,000 claims were filed—against the Manville Trust alone.

The estimated total cost of asbestos litigation through 2002 is \$70 billion, a sum that is projected to climb to \$120 to \$150 billion over the next 50 years (Congressional Budget Office, 2005). Tort litigation costs totaled 2.2 percent of the GDP in 2003 – or \$866 per American citizen (Tillinghast, 2005). Lawsuits against companies are not

exclusively filed by patients of actively malignant diseases—up to 90 percent of claimants in recent cases are functionally unimpaired (Carroll et al., 2005). Efforts began in 2003 to curb the momentum of new cases filed, after 100,000 cases were filed that year alone (American Academy of Actuaries Mass Torts Subcommittee, 2006).

The Growth of Asbestos Litigation can be Traced to a Few Important Factors

McGovern (2002) proposes three different models of procedural law that attempt to explain the growth of asbestos litigation in terms of plaintiffs' and counsels' propensity to sue. Under the traditional model, law firms will conduct preliminary economic evaluation when deciding to pursue a case, based on the probability of return and the amount of expected return should they win the case. In the single-defendant mass tort model, multiple plaintiffs are named and the per-claimant cost to counsel decreases as the sheer volume of claims and cases overwhelms defendants, and cases are usually settled quickly with low settlement offers. In the multiple-defendant mass tort model, claims are filed against many defendants at a time, with little additional effort and cost over the single-defendant model, and expected returns increase exponentially. Under this model, the per-claim cost decreases dramatically as the preparation for each claim is virtually identical for each defendant. Most recent asbestos cases resemble the multiple-defendant mass tort model, and as a result, asbestos litigation has grown by orders of magnitude since the first cases were filed in the early 1970s.

Some say the standard of proof that burdens those filing suit against asbestos manufacturers have become lax. Factors in substantive law regarding exposure and procedural law concerning aggregation and plaintiff class certification may have also led to the recent growth of asbestos litigation (McGovern, 2002). In *Jenkins v. Raymark*

(1986) the U.S. Court of Appeals for the Fifth Circuit determined that the difference in circumstances involved in separate trials naming the same class of plaintiffs and defendants would overshadow the needs of the common class. The court affirmed the trial court's certification of a nationwide asbestos mass tort class, which set the precedent that many say opened the flood gate for cases filed in the late 1990s and early 2000s.

Case aggregation has also contributed to the recent increase in asbestos cases. Aggregating cases naming a common plaintiff class against a number of defendants forces the defendants to settle on cases for claimants named in the class that would not have been awarded compensation had the case been tried on its own (Brickman, 2001). In *Szabo v. Bridgeport Mach., Inc* (2001), the judges stated that one asbestos class' certification would "turn a \$200,000 dispute. . .into a \$200 million dispute. . .and may induce a substantial settlement even if the customer's position is weak" (§ 9). Nevertheless, many current cases are still aggregated, given that the alternative would involve, according to one judge, "days of the same witnesses, exhibits, and issues from trial to trial," therefore tying up the judicial system even more than it already is (*Jenkins v. Raymark*, 1986, ¶16).

*Asbestos Litigation has Spread to Defendants throughout Most Sectors
of American Industry*

At first, asbestos litigation focused on industries in which workers were exposed to high levels of asbestos through repeated, direct contact with asbestos laden products. These industries included primary manufacturing of asbestos products such as friction materials, pipe, textiles, roofing and insulating materials, secondary manufacturing of products such as heaters, boilers, furnaces, ovens, and electrical appliances, shipbuilding,

and construction (Nicholson, Perkel, & Selikoff, 1982). Recently, however, claims against non-traditional industries such as paper manufacturing, hospitality, retail, and food and beverage service have surfaced (Carroll et al., 2005). Employees of these peripheral industries hold that incidental exposure to asbestos in the occupational environment has caused them medical harm (Stiglitz, Orszag, & Orszag, 2006). For example, textile workers claim to have operated machines with gaskets that contained asbestos or in factories where the ventilation ducts contained asbestos (Carroll et al., 2002). Therefore, textile workers in those environments may have inhaled some amount of asbestos, but conceivably less than construction and shipyard workers in traditional asbestos industries.

Non-traditional claims rose 107% between 1999 and 2000 and 71% between 2000 and 2001, accounting for 60% of asbestos litigation expenses (Carroll et al., 2002, 2005). Wyckoff and McBride (2003) outlines a number of factors that can explain this phenomenon. In addition to mass tort class certification and multiple-defendant naming, these include an increase in compensation and settlement payouts and more frequent use of liability insurance to provide coverage for asbestos litigation. Traditional defendants in primary asbestos industries have largely turned to bankruptcy protection as a direct result of asbestos litigation. As a result, the nature of claims has changed, focusing on non-traditional industries that still have available funds and liability insurance to payout asbestos case settlements (Warren, 2003). “‘It has very little to do with whether anybody knowingly did anything wrong,’ says [WS] Bellows [Construction Corp.] Vice President Jim Stevens, ‘It’s more about who has deep pockets’” (Warren, 2003, p. B1).

Fishing for potential clients will also contribute to accelerating rates of the number of cases filed each year. Kazan (2003) describes mass mailing and screening techniques used by lawyers to fish for potential clients nationwide:

I received a mailing at my home. . . the letter was addressed to me or ‘current resident’. . . it sought sandblasters, shipyard workers, and railroad workers. . . I called the ‘800’ number to find out about my free screening. It turned out that the program was being conducted by a medical screening company headquartered in Pascagoula, Mississippi that works on contract for lawyers around the country. For-profit enterprises organize screenings, find cases, and sell them to the lawyers who sponsor the programs. (p. 4)

The Nature of Recent Claimants’ Asbestos-Related Injuries Has Also Changed

Because the severity of asbestos-related disease depends on the dose of asbestos inhaled, non-traditional claims are now being filed by claimants with medical impairments that are far less severe than plaintiffs who worked in traditional industries (Carroll et al., 2002). Steven Kazan, a lawyer who exclusively represents plaintiffs with mesothelioma, told the New York Times, “‘The overwhelming majority of [recent] cases are brought by people who have no impairment whatsoever’” (Berenson, 2002, p. A1). According to one estimate of the allocation of settlements against one defendant that is named on the majority of asbestos claims (who provided the information confidentially, as is customary where settlement amounts are concerned), 19% of settlement monies have been paid to claimants with some form of cancer related to asbestos exposure, 20% has been paid to claimants with mesothelioma, and the other 61% has been awarded to plaintiffs with non-malignant diseases (Hensler, Carroll, White, & Gross, 2001). Many of those now screened for medical impairments related to asbestos exposure held numerous jobs throughout their lifetime, including painting and sandblasting, with other, unique hazards to lung tissue, yet it is likely that asbestos defendants are now paying for

all occupational hazards to workers' lungs (Parloff, 2002). This is because asbestosis resembles over 130 other lung diseases, some of which are also caused by numerous toxic, airborne substances (Parloff, 2002).

Some lawyers representing unimpaired claimants say their clients deserve compensation even if they are not sick, claiming ““Juries throughout the United States have said that these people deserve compensation. . .you really have to have faith in the jury system”” (Berenson, 2002, p. A1). Hensler, Felstiner, Selvin, & Ebener (1985) interviewed jurors that decided to award four plaintiffs in a consolidated trial in eastern Texas \$1 million each, despite the fact that three plaintiffs were unimpaired and one was a patient with active asbestosis. The jury members thought that the three unimpaired plaintiffs deserved compensation because they would eventually become as ill as the one plaintiff with asbestosis. Evidence presented during the trial indicated the probability that the three unimpaired plaintiffs would develop an asbestos-related disease, but jurors treated the evidence as if the plaintiffs were already ill in determining the current case's award payout. This practice, as well as plaintiffs' and lawyers' desire to cash in on asbestos settlements before the defendants' money runs out, will ensure that asbestos litigation will go on for years to come, despite failed, recent legislative efforts to curb the ever-increasing number of claims filed the past few years. Stengel (2006) points out that until courts recognize that not all of the asbestos claims tried before them are valid, and that even impaired claimants may not be charging the responsible party, the asbestos litigation problem will worsen.

Efforts to reform state and federal laws through congressional legislation have failed. The Fairness in Asbestos Resolution Act of 2005 passed the House of

Representatives and was considered in the Senate, proposing establishment of the Office of Asbestos Disease Compensation within the Department of Labor. The Office would have compensated the pending 100,000 claims of those with asbestos-related malignancies and 1.5 million claims of those with other asbestos-related disorders (Holtz-Eakin, 2005). The Act would have established 9 levels of physical impairment caused by asbestos exposure, as well as a compensation system paid by the Asbestos Injury Claims Resolution Fund, which would graduate payouts according to the severity of the claimant's medical condition (Congressional Budget Office, 2005). All claims would have been decided upon within 90 days of initial filing, and attorneys could not have charged more than 2% of the award amount for representation (Cohen & Brooks, 2005). However, the Act failed in the Senate for numerous reasons, including corporate opposition and asbestos litigation lobbying against tort reform (Frank, 2006). Parties with political, judicial, financial, and academic interest in the matter agree that the issue of asbestos litigation is not going away any time soon, a dire predication given the untold costs associated with the problem to date.

Financial and Personal Costs of Asbestos Litigation Have Risen of Late

Asbestos litigation has cost an estimated \$70 billion to process over 730,000 claims through 2002 (Carroll, 2005). It is responsible for the bankruptcy of almost 80 companies, causing the loss of almost 60,000 jobs and \$25,000 to \$50,000 over the course of a laid-off employee's lifetime earnings, as well as a widespread decline in the creation of new industrial jobs as companies significantly reduce reinvestment (Stiglitz, Orszag, & Orszag, 2002). Employee layoffs impose an enormous cost on the federal government's budget resulting from lost income tax revenue and an increased utilization

of unemployment and need-based benefits such as Medicaid (Stiglitz et al., 2002). The individual workers in various industries have lost an average of \$8400 from their pensions due to asbestos settlements—an average 25% of the value of their 401(k) accounts—as the result of an incredible 92% decrease in one study’s index of stock value for bankrupt companies. Beyond these effects on the federal government, companies’ stock, and employees’ pensions, one study suggests that tort costs overall may impact the very competitiveness of U.S. products in the international marketplace (Tillinghast, 2005).

The situation has also taken a toll on the industrial workers themselves. Law firms interested in capitalizing on the asbestos lawsuit frenzy advertised free medical screenings for asbestos-related diseases in union publications (Pendell, 2005). This led to alleged mass false-positive diagnoses reported by attorney-appointed doctors, who, one screening audit reports, were paid up to \$725,000 for their service, rewarded \$1500 for each positive diagnosis resulting in a paid claim, and—not surprisingly—had an average false-positive rate of 63% (Parloff, 2003). Falsely diagnosed industrial workers experienced undue diagnosis-related anxiety, unnecessary invasive procedures, and carcinogenic x-ray exposure (Pendell, 2005). All told, the human and economic losses incurred from what has been called the greatest occupational disaster in our nation’s history are staggering.

Estimates of the Future Costs of Asbestos Litigation Are in the Billions

Projected costs of asbestos litigation in the future are no less bleak. Technical reports prepared by the RAND Institute for Civil Justice have reviewed numerous studies estimating the future of asbestos litigation. Estimates of the future costs of the problem

range between \$130 and \$195 billion and that only 25 to 75% of the total number of claims related to asbestos have been filed to date (Carroll, 2005). Stallard (2001) projected the number of future claims against the Manville Personal Injury trust. Using data from the Manville Trust itself, the National Center for Health Statistics and U.S. Census Bureau vital statistics, the Surveillance, Epidemiology, and End Results (SEER) Program data from the National Cancer Institute, and epidemiology data from the 1980s and 1990s, the study predicts that 462,095 claims will be filed between 2000 and 2049 against the Manville Trust alone. Academic and legal reviews of the matter all agree that this litigation will continue for at least another fifty years, and “lawyers can have a long and prosperous career in the field of asbestos litigation, assuming that Congress does not do something about it” (Kazan, 2003, p. 1). There are numerous reasons the problem is so resistant to resolution, among them:

. . .the class of people exposed is so enormous, the injuries so ill-defined, and the pool of potential defendants so bottomless. But the most vexing problem [is] that so many asbestos-related injuries will not occur for decades. How do you negotiate a settlement with plaintiffs who don't yet exist? (Parloff, 2002, p. 2)

Asbestos Litigation is Unique

Other mass torts have involved the tobacco industry, Agent Orange, and the fast food sector, but asbestos litigation is unique in that 8400 different defendants have been sued with enough money from insurance companies to keep the litigation going for decades (White, 2004). Other evidence supports this prediction. A possible reversal of the inability for plaintiff attorneys to apply what's known as *market share* doctrine to asbestos cases would make it even easier for claimants to identify any brand of asbestos-containing product, thereby receiving compensation from the entire market of asbestos

product manufacturers (Hensler, Feistiner, Selvin, & Ebener, 1985). While the number of mesothelioma cases is expected to drop over the next 50 years (Price, 1996) due to the decline in asbestos fiber use with the OSH Act of 1970 (Weill, Hughes, & Churg, 2004), the litigation will continue as predicted due to the number of claims naming non-traditional and premises defendants—and one other reason that has only been mentioned in the literature once thus far.

Attention will likely begin to shift from affected individuals in asbestos-manufacturing trades to those in asbestos stripping and removal work. Frost, Harding, Darnton, McElvenny, and Morgan (2008) surveyed 52387 asbestos stripping, or abatement, workers in Great Britain who were employed in the field between 1971 and 2005. The causes of 985 deaths were examined, including 384 cancers, 115 lung cancers, and 23 cases of mesothelioma. The rates of death overall, as well as those attributed to larynx, lung, peritoneum, pleura, and kidney cancer, mesothelioma, circulatory, cerebrovascular, and respiratory disease, and asbestosis were significantly higher than those predicted for the general male population. The study found no difference in mortality rate between workers who used a wet or dry removal technique, nor those who wore a positive-pressure respirator mask and those who did not. Among the risk factors examined, including workers' smoking habits and the size of the enclosure in which they predominately worked, the only differences in mesothelioma incidence was found according to the age at which first exposure occurred and the length of time workers were exposed to asbestos. Fourteen cases of mesothelioma were the cause of death for workers who were first exposed to asbestos under 20 years of age, and 9 cases were documented for those first exposed in their twenties. No cases of mesothelioma were

reported for groups first exposed in their thirties, forties, or fifties. Mesothelioma incidence was also elevated if the length of exposure exceeded 20-30 years. Overall, mortality rates from all causes was about 40% higher if workers spent more than 40 hours per week stripping asbestos than if they spent 20 hours or less per week doing the same. These findings indicate that current asbestos abatement equipment and techniques are not sufficient to completely remove the risk of working with the substance. This, along with the personal and financial motivations of plaintiffs to sue even marginal defendants and the continued use of early precedents concerning traditional asbestos companies' failure to warn the public about the known dangers of working with asbestos in deciding more recent claims with completely different circumstances, will all contribute to the continuation of asbestos litigation for many years to come.

CHAPTER THREE

Legal System Procedures Impel Research on Eyewitness Memory

The earliest studies on the contributions of experimental psychology to courtroom testimony were conducted in the early 1900s at Harvard's newly-established applied psychology lab. In his 1908 work, *On the Witness Stand*, Munsterberg's description of psychological research development is still relevant a century later:

If experimental psychology is to enter into its period of practical service, it cannot be a question of simply using the ready-made results for ends which were not in view during the experiments. What is needed is to adjust research to the practical problems themselves, and thus, for instance, when education is in question, to start psychological experiments directly from educational problems. (p. 8-9)

The legal system is concerned with events that happened at various times in the past, and cognitive psychologists are frequently called in for consults regarding the nature of eyewitness memory and the conditions under which the eyewitness report was gathered (Read & Connolly, 2007). The following studies continue a line of investigation that began in 2002, when the principal investigator of Baylor University's memory and cognition lab was called to provide expert witness testimony concerning product brand identification in a product liability case in Texas. Since then, members of the lab have been concerned with identifying factors that contribute to inaccuracies in eyewitness memory for product identification. Through research like the following experiments, we can build upon expert testimony such as the notion that memory does not function like a video camera, encoding information perfectly and able to replay past events with perfect accuracy for an indefinite amount of time.

Memories Are Not Perfect Records of Past Events

Previous studies of memory in general indicate that memory encoding is a selective process, known to differ across individuals. A number of factors can affect encoding and subsequent retrieval of eyewitness memory, including an individual's personality (Paddock, Terranova, Kwok, & Halpern, 2000; Ward & Loftus, 1985), level of anxiety and life stress (Pesta, Murphy, & Sanders, 2001), gender (Powers, Andriks, & Loftus, 1979), age (Ackil & Zaragoza, 1998), and the semantics involved with retrieval cues (Fruzzetti, Toland, Teller, & Loftus, 1992; Lane & Zaragoza, 1995; Loftus, 1974, 1975; Loftus & Palmer, 1974; Loftus & Zanni, 1975). Memory retrieval can be affected by neuropharmacological agents, hypnotic psychotherapeutic techniques, and guided imagery (Schefflin, Brown, Firscholz, & Caploe, 2002). The amount of time that has passed between encoding and retrieval is negatively correlated with accuracy and positively correlated with mistaken identification (Shapiro & Penrod, 1986). As such, memory can be described as a reconstructive process, vulnerable to errors that intrude during detail assimilation (Pollio & Foote, 1971). Most of the inaccuracy associated with memory is introduced during the reconstructive retrieval process, as an individual calls up memories of the original event (Bjork & Bjork, 1988). Finally, a mismatch between autobiographical focus and event facts can lead to inaccurate eyewitness memories when factual details were never encoded in the first place (Haber & Haber, 2000).

Encoding can be characterized in two distinct ways—intentional or incidental (Carmichael, Hogan, & Walter, 1932; Herman, Lawless, & Marshall, 1957). In memory research, subjects are directed to pay attention to some stimuli in an encoding condition. They know that the exposure task is part of an experimental paradigm and, aside from

subject-related nuisance variables such as motivation, attend to the encoding material, intentionally attending to it, learning it for use in some experimental task. On the other hand, incidental encoding occurs when material presented to subjects—for instance, during an eyewitness event about which subjects do not know they will be tested—is incidental with respect to the main events unfolding during the encoding event. That is, subjects are incidentally exposed to peripheral details that are not central to the main events of the encoding condition.

Intentional encoding leads to better memory performance than incidental encoding (Lampinen, Copeland, & Neuschatz, 2001; Migueles & Garcia-Bajos, 1999), most likely due to the effects of divided attention in the incidental condition (Reinitz, Morrissey, & Demb, 1994). However, eyewitness testimony outside the laboratory often involves reporting details about events that the witness experienced incidentally. Incidental encoding has been suggested as a more ecologically valid method for eyewitness research (Brewer & Williams, 2005). Accordingly, the current studies were interested in memory performance for incidental details (brand names of products) that are not central to the main encoding event (care package assembly). Subjects did not know that their memory for the brands of products would be tested, therefore providing a purely incidental encoding paradigm that closely resembles the product brand exposure of individuals in the real-world.

Evidence that Eyewitness Memory May Be Fallible

Eyewitness memory studies have described many factors that account for the possible inaccuracy of identifications made some time after the critical encoding event occurred. The variables manipulated in these studies fall into two distinct categories,

which were coined by Wells (1978): *estimator variables*, which are factors whose influence on eyewitness identification can only be estimated after the critical event has already occurred, and *system variables*, which are factors that are controlled by the justice system. Estimator variables describe features of the encoding situation itself, such as the severity of a criminal act. System variables describe retrieval conditions that can be manipulated by agents of the legal system, such as the amount of time between some critical eyewitness event and subsequent eyewitness identification. The following experiments manipulated both estimator and system variables. The effects of factors that cannot be controlled, like a plaintiff's level of involvement with a certain product, and those that can be controlled, such as the nature of interviewer feedback, on witness confidence for product brand identification was measured.

Besides the presence of interviewer feedback, another system variable under investigation is whether or not the target (correct answer) is present in an eyewitness identification lineup. When the target is absent from a simultaneous lineup, identifiers judge the relative similarity of each suspect in the lineup to the image they remember from the witnessed episode (Wells, 1984). That is, witnesses choose the "most correct" answer when the suspects—or answer choices, in our case—are presented in the lineup simultaneously. In civil cases, pre-trial preparation materials may or may not include the brand of product claimants actually encountered in the past. But, they may still feel pressured to make an identification based on financial or personal motivations. On the recognition task subjects in the present study will complete, the target product brand may or may not be present as an answer choice on the multiple choice survey. Presumably, civil suit claimants and subjects in the following studies will choose the most-correct

answer when the correct answer is absent from the product brand answer choice “lineup.” Methods have been developed to eliminate the relative judgment criterion witnesses adopt when identifying suspects in target-absent lineups. Sequential presentation of suspects in a line up discourages comparison of the suspects to each other according to their similarity to whatever the witness remembers from the criminal event (Lindsay & Wells, 1985). Subjects were more accurate in target-present lineups and less likely to make a false identification in target-absent lineups when sequential presentation was used, compared to simultaneous presentation (Lindsay & Wells, 1985).

Estimator variables, such as delays between encoding and test, have not been extensively studied in eyewitness memory research for a number of reasons. It is costly, both in time and money, to relocate and retest witnesses and the effect of delay—by itself—on witness accuracy is predictable (Wells & Quinlivan, 2009). More interesting are the effects of events that occur during the retention interval between encoding and retrieval, such as interviewer feedback, and how those manipulated variables exert differential effects on the relationship between witness confidence and accuracy at various times following the critical encoding event.

Witness Confidence and Accuracy is Subject to Influence

Many early studies on the confidence/accuracy (CA) relationship concur that witness certainty is commonly thought to indicate witness credibility (reviewed in Wells, 1993). In fact, the perception of witness confidence has been shown to account for over 50% of the variance in measures of believability of witness identification (Wells, Lindsay, & Ferguson, 1979). Suggested criteria for ascertaining witness reliability were even described in *Neil v. Biggers* (1972). The defendant was convicted of rape after

being identified in a line-up that occurred seven months after the plaintiff's attack. The court believed that the plaintiff's view of the defendant, amount of attention paid to the event, plaintiff's elaboration in the suspect's description, the passage of time, and confidence were all indicative of witness reliability, even if the identification lineup was conducted using suggestive interview tactics. The case concludes that, based on the victim's testimony alone, the lower court ruling should stand because the plaintiff had "no doubt" her identification was correct because her assailant had a face, she claimed, "I don't think I could ever forget" (p. 409).

Witness who appear confident are less likely to contradict themselves, hedge questions, hesitate answering, and tend to speak in longer, more elaborate sentences (Leippe, Manion, & Romanczyk, 1992). The way testimony is reported has a greater impact on believability and persuasiveness than what witnesses actually communicate, especially if it is difficult to evaluate the tenability of the facts of the testimony (Leippe et al.). Jurors tend to rely on witness confidence to judge the accuracy of witness testimony, without taking into account better predictors of accuracy, such as retention interval (the time lapse between event and identification) and decision time in making the identification (Penrod & Cutler, 1995).

However, countless studies of the CA relationship for eyewitness testimony have shown that confidence is a poor indicator of accuracy (Lindsay, Wells, & Rumpel, 1981). The CA relationship depends on factors such as the conditions of witnessing, information encountered between the encoding event and identification, the tone of language used during the identification process, commonly held social stereotypes, and implicit beliefs about memory quality that persist in the general public (Leippe, 1980). Additionally,

briefing witnesses by asking them to rehearse answers to potential questions that would be asked in court was found to inflate confidence, but not accuracy (Wells, Ferguson, & Lindsay, 1981).

A recent review of eyewitness literature identifies more factors that have been shown to affect the CA relationship, including lineup manipulation, recognition versus recall retrieval tasks, biased instructions, and interviewer feedback (Krug, 2007). One regression analysis determining the relationship between eyewitness errors and confidence, response time, and line-up fairness shows that only response time and line-up fairness accounted for significant variation in accuracy (Smith, Lindsay, & Pryke, 2000). Further, confidence was weakly correlated with accuracy, but once response time was accounted for, confidence did not contribute any additional variance to accuracy on its own. One study found that interviewer pressure to report details about a staged eyewitness event increased the number of details reported by subjects, both accurate and inaccurate, but had no effect on subjects' ability to gauge the accuracy of the reported details (Winningham & Weaver, 2000). A review of 271 criminal cases tried in Sacramento, CA revealed that 55% of plaintiff witnesses were eager to guess an identification when encouraged to do so by the interviewer (Penrod, 2003). Finally, an early review of 43 studies of the CA relationship determined that predicting accuracy with expressed confidence varies as a function of the quality of encoding conditions—which are rarely assessed in eyewitness identification—and recommends that the justice system cease relying on confidence as an indication of witness accuracy (Deffenbacher, 1980).

Confidence has also been proven a poor predictor of product brand identification accuracy. On both recall and recognition surveys, and after a 1 or 2 week delay following product exposure, one measure of the within-subject CA relationship, the Gamma correlation coefficient, was very low across both delay and survey task conditions (Krug & Weaver, 2005). A similar result was obtained when subjects in another product brand recognition study were either told the intent of the experimental product exposure (the intentional encoding condition) or not (incidental encoding condition), and were either tested 10 minutes or 1 week following product exposure (Terrell & Weaver, 2008). Accuracy in the incidental encoding condition fell to the rate of chance after a 1 week delay, while accuracy in the intentional encoding condition fell to 50% of that observed at 10 minutes. Subjects were well calibrated with respect to individual confidence in their responses at 10 minutes, but were overconfident across both encoding conditions after a week-long delay.

Evidence proving the accuracy of product identification witnesses—as in work orders and bills of sale—are often unavailable, especially in cases involving asbestos exposure with a long latency period between product use and disease manifestation (Weaver, Terrell, & Krug, 2004). Therefore, the absolute accuracy and reliability of witness' product brand identifications in these cases are rarely challenged (Weaver, Terrell, & Holmes, 2006). Eyewitness testimony protocols have, as yet, been largely unaffected by findings and recommendations of the field of forensic psychology—an alarming realization given the sheer volume of evidence indicating that eyewitness testimony is highly malleable by forces under the control of legal system personnel and courtroom triers-of-fact (Terrance, Thayer, & Kehn, 2006).

There are two distinct measures of the CA relationship (Nelson, 1996). Absolute calibration describes the relation between one's subjective confidence and absolute response accuracy on a task, expressed as a biased score. Relative calibration indicates the relation between one's confidence and relative response accuracy across multiple responses. One example of relative accuracy pertains to within-subject comparisons of the accuracy of one response compared to the accuracy of another response on the same survey. A measure of subject's relative calibration that quantified the association between the compared accuracy of two survey items and the compared confidence ratings of the same two survey items was calculated.

The best measure of witnesses' CA calibration—or how well witnesses gauge the accuracy of their memories—is, according to Nelson (1984), the Goodman-Kruskal gamma (G) correlation. A unit of measurement for G is comprised by a pair of observations taken from a subject. The G correlation indicates the measure of association between ordered data. Simply put, if one observation is incorrect, and a second observation is correct (indicating an increase in accuracy), does confidence also increase from the first observation to the second? If increased accuracy between two observations also corresponds to increased confidence between the same two observations, the pair of observations is concordant with respect to accuracy and confidence. Similarly, if decreased accuracy between two observations also corresponds to decreased confidence between the same two observations, the pair is concordant. If, in another case, increased accuracy between the two observations corresponds to decreased confidence in the observations, the pair of observations is discordant with respect to accuracy and

confidence. Likewise, a discordant pair of observations occurs if decreased accuracy between the two observations corresponds to increased confidence.

The ratio of the difference between a subject's concordant and discordant pairs of responses to the sum of the number of concordant and discordant pairs of observations for the subject gives a measure of association between the subject's accuracy and confidence, or the *G* score (Nelson, 1984). High rates of discordance between a subject's responses indicate poor CA calibration and results in a low value of the *G* score for that subject. By obtaining values of *G* for each subject within a particular group, we may identify conditions that lead to certain values of *G* across subjects.

A meta-analysis of previously-published data on CA calibration found that the population effect size of the CA relationship was $r = .29$, a medium effect as described by Cohen (1988) (Sporer, Penrod, Read, & Cutler, 1995). However, when the data pertaining to positive eyewitness identifications is separated from that for non-identifiers (or non-choosers), the relationship between confidence and accuracy differed, $r = .37$ for positive identifications and $.12$ in non-choosers. Therefore, it seems that moderating variables may contribute to differences in CA calibration across different conditions of encoding and retrieval.

The following studies identified factors that could be used to predict how likely witnesses are to be correct when they are confident in their responses. Finding patterns in CA calibration across various conditions would be significant, considering that the conditions of encoding and retrieval the studies attempt to mimic match those of tort liability cases that rarely involve corroborating evidence in discerning the absolute accuracy of witnesses' responses.

Interview Procedures Can Influence Plaintiff Testimony

When claimants filing suit against asbestos companies retain counsel, they are asked to identify which manufacturers' brands of asbestos-containing products may have been present in their workplace before the early 1970s. One former paralegal discloses the interview procedure her law firm used when asking clients about their previous exposure to certain product brands (Biederman, 1998). She states:

When you were talking with the guy, you would say, 'We know this product was there.' This is where you'd get them to implant false memories. We knew in many cases from insulators that certain products were on the job site. So you would say, 'Joe Smith says that Celotex was all over the place when they'd saw that thing in half.' They say, 'Oh yeah, I'd be under it.' You'd say, 'You know, it was Celotex they were cutting,' and they'd think, and they'd go, 'Yeah, yeah, it was Celotex.' (p. 2)

Another paralegal from the same law firm inadvertently distributed a copy of what is now called the Script Memo, which was a script paralegals would use in directing clients' testimony to include identification of product brands they may or may not have actually encountered (Brickman, 2004). Brickman (2004) also sums up numerous investigative reports indicating that paralegals also:

. . .instilled 'memories' into the minds of clients so they could 'recall' the products that were financially remunerative for them to name and 'not recall' the names of products that would have been financially counterproductive to identify. To assist in this process of creating 'memories,' paralegals showed clients pictures of the 'right' products, which were then listed on the Script Memo. Parties were then provided product details to study so that they could pass the 'test' that would be administered, i.e., a deposition, and obtain suitable financial rewards. (p. 142)

These practices lead clients to report past exposure to certain brands through encouragement and leading confirmatory feedback. Not only do clients believe they encountered certain products, but they may be coached into believing the same with a high degree of confidence. Claimants can then testify under oath confidently that they

were exposed to a certain product and their testimony can be treated as fact because corroborating evidence often does not even exist.

Post-Identification Confirmatory Feedback Can Affect Eyewitness Confidence

Research into the effects of confirmatory post-identification feedback on eyewitness memory has yielded insight into the mechanisms of confirming feedback on confidence in eyewitness identification. Memories can be thought of as an aspect of one's self whose quality is subject to one's own judgment and feelings. Those feelings can include the confidence with which one judges the accuracy of their memories, which can be influenced by knowledge of the quality of memory performance in the past (Shaw, McClure, & Wilkens, 2001). Information about the relative accuracy of one's memories compared with others' or the objective accuracy of memories against a set of correct responses shapes one's belief in the accuracy of their memories (Zimbardo & Leippe, 1991).

The effects of feedback on eyewitness identification itself began with Wells and Bradfield (1998). The study developed the post-identification feedback paradigm, in which subjects viewed surveillance video of an actual crime, and were asked to identify the perpetrator from a target-absent photo line-up. That is, all subject responses were incorrect. Then, one group received confirmatory feedback, while another did not. Their study coined the confirmatory feedback phrase almost universally employed by later studies, "Good, you identified the suspect" (p. 1). Both groups were then asked to rate various features of the witnessing and identification experience on a survey of questions, including confidence rating from 1 (not at all certain) to 7 (totally certain). The survey question concerning confidence was introduced to the field: "At the time that you

identified the person in the photo spread, how certain were you that the person you identified from the photos was the gunman that you saw in the video” (p. 366). The average certainty for the feedback group was 5.4, while that of the no-feedback group was 4.0. The average effect size in the differences in confidence and other measures of the witnessing experience between the feedback groups was $d = .75$, a large effect according to Cohen (1988).

A review of the eyewitness feedback literature that followed the study found an average Cohen’s d effect size of .79 between eyewitnesses’ confidence ratings following confirming feedback and that of subjects who not receive feedback (Douglass & Steblay, 2006). Confirming feedback also inflates the confidence of inaccurate witnesses more than accurate witnesses, suggesting that subjects are more susceptible to feedback if their response was incorrect to begin with (Allwood, Knutsson, & Granhag, 2006; Bradfield, Wells, & Olson, 2002). Subjects who received confirmatory feedback in another study repeated forcibly confabulated memory details with greater speed and confidence than subjects who did not receive feedback (Hanba & Zaragoza, 2007). In this study, confabulated memory details were elicited in response to survey questions concerning event details that were not present in the encoding condition. Confidence inflation in response to confirmatory feedback even occurs if subjects know, before the encoding event, that they will be identifying the suspect after viewing a crime scene (Douglass & Surrect, 2006). Identification accuracy in this intentional encoding condition was not higher than any other condition, but confidence was still inflated by confirmatory feedback. The same is observed if instructions are unbiased, indicating to subjects that the target—or actual perpetrator—may not be present in the line-up (Semmler, Brewer, &

Wells, 2004). Even though these findings were the result of experiments conducted in the laboratory, the same results were found in a study of 134 real eyewitnesses identifying suspects in criminal cases in the United Kingdom (Wright & Skagerberg, 2007).

Fewer studies have investigated the effects of feedback given after some time has passed following an eyewitness event and suspect identification. The effect of delay on the CA relationship following feedback is especially relevant in the current proposal, given the long retention interval of asbestos case plaintiffs between encoding (product exposure), product brand identification (recall the Script Memo), interviewer feedback (paralegal coaching), and confidence rating in plaintiffs' prepared testimonies.

Confirming feedback has been shown to inflate retrospective confidence compared to no feedback conditions even if it is given immediately following identification and confidence is rated 48 hours later, and if both feedback is given and confidence is assessed 48 hours following initial identification (Wells, Olson, & Charman, 2003). If subjects are told to be suspicious of the experimenter by an undercover research assistant posing as a fellow subject, namely that the experimenter is telling everyone that their identifications were correct, confidence inflation is eliminated between the confirming feedback and no feedback conditions at both immediate and delayed feedback/confidence rating conditions (Neuschatz et al., 2007). This confidence prophylactic effect occurs when confidence inflation can be prevented by some interviewer feedback manipulation.

The ability of eyewitnesses to discern the effect of feedback depends on whether or not the eyewitness was provided the feedback (Charman & Wells, 2008). Witnesses given confirmatory feedback consistently rated the influence of feedback on confidence

lower than those that did not receive feedback after identifying a suspect of a crime, even over a delay between identification and immediate feedback and, 48 hours later, retrospective confidence ratings (Charman & Wells, 2008). It seems that not only do subjects provided confirmatory feedback rate confidence higher than those provided no such feedback, but they also underestimate the influence of the confirming feedback on confidence in subject identification.

These studies investigated the effects of delay on confirmatory feedback effects on confidence, but found main effects of feedback itself, suspicion of experimenters' motives, and the influence of feedback across both delay conditions. None of the studies found a simple main effect of delay on these other factors concerning feedback and retrospective confidence for eyewitness identification. The studies involved delays of 48 hours between encoding and immediate identification and feedback administration/confidence assessment. None of the studies involved a delayed-identification condition, whereby the effects of confirmatory feedback on retrospective confidence could be assessed over a significant retention interval between some eyewitness event and identification.

The current studies are most concerned with eyewitness identification long after the witnessing event has occurred. In particular, the effects of confirmatory feedback, much like that employed in some law firms in preparing clients for product liability suits, on witness confidence will be assessed for product identifications made 1 week after incidental product exposure. It was expected that the effects of confirming feedback would inflate confidence for identifications made 1 week after the encoding event much more than that for identifications made immediately after encoding. Event details are

much less accessible after a delay between a witnessed event and identification, due to factors such as memory trace decay and interference from information and events individuals encounter in their daily lives during the retention interval. Therefore, the confirmatory post-identification feedback studies were expected to show that the effects of confirming subjects' delayed product brand identifications leads to inflated confidence compared to those who do not receive such feedback.

Feedback effects may be attributable to hindsight bias. Also known as the knew-it-all-along effect, hindsight bias is the tendency for subjects, when asked to remember what their past response to a memory prompt was, to recall their (original, incorrect) response as closer to the correct response than their answer actually was after they are provided the correct answer (Pohl, 2007), or to underestimate the influence of the feedback in determining whether their response would have been correct without the feedback (Fischhoff, 1975). In one eyewitness study, estimator variables such as subjects' ratings of the amount of attention they paid to a criminal episode and the quality of the view they had of the suspect were inflated by confirmatory feedback, in addition to confidence (Wells & Bradfield, 1999). Because these variables describing attributes of the encoding condition were rated higher in the confirmatory feedback condition, along with confidence, Wells and Bradfield (1999) suggests that simple hindsight bias may not be the exclusive mechanism by which feedback affects response judgments. Self-perception theory may explain higher subject ratings of attention and other encoding conditions following confirming feedback. Self-perception theory holds that if internal cues are weak (such as certainty, view quality, attention, etc.), witnesses rely on identification context—including the presence of feedback—to infer those qualities

associated with the witnessing event (Bem, 1967). Thus, witnesses use feedback to infer identification qualities, like certainty, when other cues, like memory quality, are weak, due to decay and interference over a retention interval following encoding.

Wells and Seelau (1995) suggest that “no feedback should be given until a clear statement of confidence is taken” (p. 780). However, given the statements of paralegals who formerly interviewed clients in asbestos law suits, it is unlikely this advice is followed. The encoding and identification interview procedures employed in the following studies approximated the product exposure and identification process involved in asbestos litigation. The ultimate goal of the study is to provide scientific evidence to the civil justice system that witness confidence in delayed product identification is vulnerable to interviewer tactics similar to those allegedly practiced by law firms in preparing their clients for their depositions and that high confidence expressed by witnesses is not predictive of witness accuracy.

The Level of Plaintiff Involvement with Products Varies

Plaintiffs filing suit against asbestos-containing product manufacturers report varied levels of involvement with the products to which they were allegedly exposed years ago. Cases brought against product manufacturers by claimants who were merely on the premises of asbestos-containing product use are on the rise. There may be differences in eyewitness memory for product identification between individuals who report a higher level of involvement with certain products and those who were less involved with asbestos defendants’ products.

For the purposes of this area of research, the actor/observer distinction in memory performance will operationally define the level of subjects’ event involvement in

eyewitness studies. The following experiments were not designed to study the actor/observer bias itself, but the actor/observer bias most closely resembles the differences in the independent variable of product-exposure involvement. Therefore, given this approximation of this study's novel variable under investigation (product involvement), and a sizeable literature on the actor/observer bias in eyewitness memory, we reviewed previous findings concerning the same. The following studies compared witness accuracy and certainty between subjects who participated in a staged, criminal eyewitness event and those that merely observed the crime from afar.

Witnesses' Level of Event Involvement May Affect Identification Accuracy

The results of eyewitness memory actor/observer studies fall into two categories with conflicting outcomes. Actors' event memories are more accurate than observers' under some conditions, while observers are more accurate than participants in others. Witnesses in one staged theft situation that were personally victimized were more accurate in identifying the confederate thief than witnesses that were asked to identify the same confederate in a no theft, control condition (Hosch & Cooper, 1982). In addition, confidence, in this case, was unrelated to accuracy, with no significant difference in mean confidence ratings across theft conditions.

Police recruits in another study were either active participants in or witnesses to two video training exercises depicting criminal situations, one of which was stressful and the other non-stressful, indicated by the perpetrator shooting at the officer or not, respectively (Stanny & Johnson, 2000). There were no significant differences in event detail accuracy between active participants and witness observers across stress conditions. However, participants recalled more event details from the non-stressful

training video than the stressful training video. There were no such differences in the amount of event detail recalled by witness observers, indicating that stress negatively affected event detail recall for participants, but not observers.

Police recruits were also surveyed in another study after participating in or witnessing a stressful or non-stressful role play scenario in which participant officers investigated a citizen's complaint about a suspicious individual loitering in a parking lot (Yuille, Davies, Gibling, Marxsen, & Porter, 1994). Recruits were then asked for their recollection of details surrounding the scenario 1 week later and 12 weeks later. One week after the event, there was no difference between participants and observers in the amount of event detail recalled. Likewise, there was no difference in the accuracy of recalled details across involvement conditions, but, as in the previous study, participant and witness officers' recalled event details were more likely accurate in the non-stressful than the stressful event—in which the suspicious loiterer was uncooperative and belligerent to the officer. Twelve weeks after the original event, participant officers recalled significantly more event details than witness observers. These findings suggest that participants outperform witnesses on event recall after a very long retention interval.

In contrast, other studies have shown that observers are more accurate witnesses than victims of a staged theft (Kassin, 1984). In one study, a confederate research assistant stole victimized witnesses' wrist watches, while other witnesses observed the confederate stealing a laboratory calculator (Hosch, Leippe, Marchioni, & Cooper, 1984). All subjects were then interviewed by a detective, who presented photo line-ups with the biased instructions, "we believe the person who stole the (calculator/watches) is present in this photo spread," or the unbiased instructions, "The person who stole the

(calculator/watches) may be one of the five individuals in this photospread” (p. 283). Victimized witnesses were less accurate when they were interviewed with biased instructions, possibly due to the pressure to make an identification and the anxiety resulting from being personally victimized. Along the same lines, participants witnessing a staged robbery live reported less event details and were less accurate than witnesses of the same event on video (Ihlebaek, Love, Eilertsen, & Magnussen, 2003). Observer witnesses had a more detached involvement with the event, while participants witnessing it live were not able to observe the entire event as they were ordered to hand over money or to get down onto the floor. Participant witnesses’ attention was focused on performing in the robbery event itself, and even though it was staged for experimentation, participants undoubtedly experienced some amount of stress during the event, interfering with their event memory accuracy.

In sum, the studies reviewed so far concerning the level of involvement of witnesses in some eyewitness event indicate that (1) stress exerts a deleterious effect on participant witness accuracy; (2) confidence was not related to accuracy; (3) there was no difference in witness accuracy between witness participants and observers, except when identification was made following a long retention interval (12 weeks) and if witness participants’ attention was focused on participating in the eyewitness event rather than on event details.

Eyewitness Events in the Current Studies Differ from Those Previously Studied

Product brand identification and product exposure events are different in many ways from suspect identification and criminal event witnessing. When claimants filing law suits against asbestos defendants identify brands of products they were exposed to

many years ago, they are identifying a detail of some incidental exposure event that, most likely, was a benign, everyday occurrence that did not seem important at the time.

Product exposures are not thought of as stressful events, unlike crimes that are emotionally-charged, stressful, distinctive events whose importance is probably not lost on the witness at the time it occurs. Further, suspect identification in a photo line-up is, for the average person, a distinctive event as well and carries its own set of stresses such as the desire to make an identification, the consequences if the identification is wrong, etc. We may also assume that claimants filing suit against asbestos defendants are also identifying products in a stressful environment, ill from asbestos exposure (but not in all cases), and subject to the tactics former paralegals have revealed that some plaintiffs' attorneys use in pressuring claimants to identify products they used many years ago. These are important distinctions between criminal and civil eyewitness testimony.

Colby and Weaver (2006) investigated the effects of all the variables discussed thus far. No significant differences were found in product brand identification accuracy between actors that were mixing baking ingredients according to a recipe and bystanders that were merely observing the actors doing so. Subjects' responses in both witness conditions were less accurate and familiar-brand false alarm rates were higher after a 1 week delay following the encoding event (recipe mixing). Observer confidence was greater than actors' overall, but there was no difference in confidence within either witnessing condition after a delay. Baking experience was analyzed as a covariant, and was shown to increase confidence across all witnessing and delay conditions, but not accuracy. Given that factors have been shown to increase confidence independent of accuracy and vice-versa, particularly as witnesses' level of involvement in a witnessing

event varies, we may infer that confidence is not only a poor indicator of accuracy for eyewitnesses that have observed a particular event, but that it may also be an unreliable estimate of the credibility of witnesses that actively participated in the event themselves.

There has been no other research to date investigating the effect of witnesses' level of involvement and delay on the CA relationship of delayed product brand identification. In particular, the following studies took the previous study's actor/observer paradigm a step further and looked at the effects of witnesses' specific interaction with products on subsequent brand identification. The product brand exposure experience of different witnesses in these studies will approximate the difference between claimants in asbestos suits that actually worked with products and those that were merely on the premises of sites where asbestos-containing products were being used. If the following studies showed differences between witness accuracy and confidence among various manipulations of witnesses' encoding experiences, and those product exposure experiences are similar to those reported by claimants filing suit against non-traditional and premises defendants in asbestos litigation, the results may be used as evidence that witnesses that are less involved with a certain brand of product are also less credible than witnesses that are more involved with a certain brand of product, who may prove to be more reliable than the former group. One paralegal, under conditions of anonymity, disclosed procedures she says the law firm of Baron and Budd used to prepare claimants for their product liability deposition (Biederman, 1998). She sums up the impetus for this line of research well: "There was a man; he was some sort of contractor. He had absolutely no exposure to asbestos—none. There was nothing in his work history," and continues:

Overall, workers in asbestos plants and insulators 'really did know the products. . .but when you got the electricians, . . .carpenters, . . .and brick masons, they didn't work with the products that much. . .these clients needed aggressive coaching. (Biederman, 1998, p. 2)

CHAPTER FOUR

Confirmatory Feedback Effects on Retrospective Confidence for Product Identification

Experiment 1

The first experiment established the confirmatory feedback paradigm for product brand identification, testing whether or not feedback affects confidence for incorrect product identification responses over a delay. Subjects in the feedback condition were told “good, you got it right” before they completed a retrospective confidence survey. Subjects in the no-feedback condition did not receive the feedback statement, but did complete the confidence survey in the same manner as the feedback group. Subjects completed target-absent surveys either 10 minutes or 1 week after the encoding condition. The dependent variable of interest is confidence in the incorrect response, not accuracy, as all subject responses were incorrect, by design, on the target-absent identification survey.

Methods

Participants. One hundred seventy-six Baylor University undergraduate students volunteered their participation in the experiment, earning experimental participation credit for introductory psychology courses in return. All participants signed an informed consent for research participation and received a copy of the form for their records. The study was approved by Baylor’s Institutional Review Board and met the American Psychological Association’s standards for minimal risk to the well-being of subjects. At

the conclusion of each research session, subjects were debriefed with respect to the objective of the care package assembly activity.

Materials. Subjects handled an assortment of products during a care package assembly task that involved sealing a toiletry item, a hand-held electronic game, a deck of playing cards, and a small spiral notepad into several sandwich- and gallon-sized Ziploc bags. Then, using bubble wrap as space-filling material, subjects packed the items into a cardboard box however they chose to prepare the package for shipping. The clear Ziploc bags allowed additional product exposure while subjects packed the items into the box. The brand of the toiletry item was the critical detail under investigation—the game, cards, and notepad did not bear brand names and were only included to reinforce the care package experience. The toiletry item available to subjects was one out of six possible products: hand lotion, mouthwash, shampoo, soap, sunscreen, or toothpaste. These products were chosen for their non-gender biased packaging and intended use. Finally, subjects were given a product identification survey asking for their identification of the brand of toiletry product they included in the care package. It asked subjects for their Baylor student ID number, gender, age, whether or not they suspected the actual purpose of the encoding experience (knew about the survey beforehand), and included one 4-alternative, target-absent, recognition question concerning the toiletry product's brand name (Appendix A). A second survey was administered asking subjects to rate their level of confidence in the product brand they identified from 0 to 100% in increments of 20% (Appendix B).

Procedure. Subjects entered a classroom in the Baylor Sciences Building and seated themselves at one of 5 care-package assembly stations set up around a large table in the center of the room. Subjects were given instructions on how to pack the care package, then assembled the toiletry item, game, cards, and notepad into a cardboard box, using bubble wrap to prepare the package for shipping. In the 10 minute condition subjects were then called, individually, into an adjacent conference room to complete the surveys. Subjects in the 1 week condition were dismissed for a week, completing the following steps at the same time in the same adjacent conference room 1 week later. Upon entering the conference room, subjects completed the product identification survey. Then the experimenter, after looking at the subject's answer on the survey, stated to subjects in the confirmatory feedback condition, "Good, you got it right!" Subjects in the no-feedback condition did not receive the feedback statement. All subjects then completed the retrospective confidence survey regarding their confidence in the brand they identified on the product identification survey.

Results

Subjects provided confirmatory feedback regarding their response on the identification survey reported higher levels of confidence in their (incorrect) response ($M = 70.45$, $SEM = 3.40$) than subjects who were not provided confirmatory feedback ($M = 47.73$, $SEM = 3.40$) across delay conditions [$t(86) = 4.73$, $SE = .00$]. Likewise, subjects provided confirmatory feedback reported higher levels of confidence than those who were not provided feedback within both the 10 minute condition [$t(42) = 2.13$, $SE = .03$] and the 1 week delay condition [$t(42) = 5.03$, $SE = .00$]. However, confidence did not differ between the delay conditions. An omnibus, 2×2 MANOVA test of the variability

between feedback and delay groups confirmed the descriptive statistics, in that a significant main effect of confirmatory feedback was found [$F(1, 84) = 22.30, p < .05$] but no effect on confidence was evident in the delay condition [$F(1, 84) = .14; n.s.$]. The interaction of feedback and delay conditions was also not significant. The value of Levene's test of the equality of error variances [$F(3, 84) = 1.40, n.s.$] allows us to fail to reject the null hypothesis that error variance is equal across groups. Therefore, the 2×2 MANOVA is reported under the assumption that the variance across groups is equal.

Discussion

In previous studies using the care package paradigm, subjects correctly identified the brands of products they included in the care package 70% of the time in both the 10 minute and 1 week delay groups (Holmes & Weaver, in press). However, the researchers were most interested in factors that influence confidence when subjects are wrong, so, subjects completed a target-absent survey, resulting in all incorrect product brand identifications. Confidence was expected to vary significantly between delay conditions, as previous studies have shown that confidence tends to increase over a retention interval. Interestingly, confidence was almost equal between delay conditions. So it would seem that subject's confidence in an incorrect response is not influenced by retention interval when the target is not present on the survey. Experiment 2 varied the target presence to test this finding. However, as expected, confidence in an incorrect response does vary according to the presence of confirmatory feedback, both within and between delay conditions.

Feedback effects increased over a delay. Given the week-long retention interval between care package assembly and test, information (such as the brand of product subjects included in the care package) could have been distorted or could have decayed altogether, indicating a weakening in the original memory trace of the encoding event. The self-perception theory posits that subjects use available context-specific information, such as confirming feedback, to infer the quality of their memory for events that happened in the past. That is, perhaps feedback influenced subjects' confidence ratings in the 1 week condition (Cohen's $d = 1.61$, a "large" effect size) more than the 10 minute condition (Cohen's $d = .66$, a "medium" effect size) because subjects' memories for the care package event in the delayed condition were weaker than those in the immediate condition, and delayed identifiers were more likely to base their confidence on the feedback.

Experiment 2

The second experiment tested whether the presence—or absence—of the target (correct) product brand in the choices given on the identification survey affected confidence for inaccurate responses over a week-long delay. The researchers were interested in determining whether the finding in Experiment 1 concerning the equal levels of confidence between delay conditions on a target-absent survey was due to the presence of feedback or were the result of target absence from the answer choices offered on the recognition survey. Therefore, Experiment 2 holds the delay constant at 1 week between care package assembly and test, provides all subjects confirmatory feedback following product brand identification, but varies the target presence on the identification survey.

Methods

Participants. Seventy-eight Baylor University undergraduate students volunteered their participation in the experiment, earning experimental participation credit for introductory psychology courses in return. All participants signed an informed consent for research participation and received a copy of the form for their records. The study was approved by Baylor's Institutional Review Board and met the American Psychological Association's standards for minimal risk to the well-being of subjects. At the conclusion of each research session, subjects were debriefed with respect to the objective of the care package assembly activity.

Materials. The same materials used in Experiment 1 were used in Experiment 2 as well. However, in this case, subjects completed one of two types of product identification surveys. The target-present identification survey (see Appendix A) included the correct response as an answer choice among three distracter choices. The target-absent survey included four product brand answer choices, none of which was the correct brand of product subjects included in the care package a week prior to test. The same confidence survey used in Experiment 1 was used as well (see Appendix B).

Procedure. The same procedure followed in Experiment 1 was repeated in Experiment 2, except that all subjects returned 1 week later to complete the identification and confidence surveys and all subjects were (falsely) told "good, you got it right" between identifying a product brand and rating confidence in their (incorrect) answer.

Results

Subjects whose product brand identification was incorrect on target-present surveys reported higher levels of confidence in their responses ($M = 76.55$, $SEM = 2.64$) than subjects whose responses were incorrect (by design) on the target-absent survey ($M = 52.41$, $SEM = 3.20$). A t -test of independent samples reveals a significant difference in confidence between the two means [$t(56) = 5.82$, $SE = .00$]. There was a large effect of target presence on the difference between the two means ($d = 1.6$). Equal variances in the two data sets were assumed, given the value of Levene's test for the equality of error variances [$F(2, 54) = 1.74$, n.s.].

Discussion

The results comparing memory performance (accuracy) over various retention intervals between encoding and test are conclusive. That memory impairment occurs as a result of memory decay and interference over time is well-documented, and because the main objective of the second experiment is to test whether target-absence affects confidence following feedback, a single delay condition of 1 week was used. In addition, all subject responses were incorrect in the target-absent condition, while subject responses in the target-present condition could have been correct or incorrect. A total of 49 subjects were in the target-present group, but only the 29 incorrect responses in this condition were included in the analyses. By delaying all identification by a week, sufficient numbers of incorrect target-present responses were elicited, allowing reliable analyses with all 29 target-absent responses.

The target-present condition was more similar to the actual encoding condition (by including one answer choice that was the correct product subjects encountered), than

the target-absent condition, which was less similar to the encoding event (because the product brand subjects actually encountered is absent from the identification survey). Subjects in the target-absent condition did not remember a correct memory trace that included any of the distracter choices on the target-absent survey. Perhaps confidence in target-absent responses was lower than the incorrect responses in the target-present condition because none of the distracter choices on the target-absent survey should be associated with any memory trace of the encoding event. Those target-absent responses could be said to reflect memory traces that are weaker—because they are non-existent—than memory traces and resulting responses in the target-present condition. It was expected that feedback would affect confidence for “more weakly remembered” responses on the target absent survey more than the “more strongly remembered” responses on the target-present survey. The opposite was found.

Recall that previous research has shown that the feedback effect on confidence for incorrect responses is more pronounced than for correct responses. When the target is present on the identification survey, subjects who respond incorrectly must reject the correct answer to make the incorrect response. When subjects were presented both correct and incorrect answer choices (in the target-present condition) and chose the incorrect response, confidence was higher than the target-absent condition. When the target was absent from the identification survey, in order to be incorrect, subjects did not need to reject the correct answer, but were forced to choose an answer among all incorrect choices. That is, target-presence on the survey allowed subjects to respond incorrectly when the chance to be correct was present, but target-absence imposed inaccuracy on other subjects’ responses. When subjects were incorrect “on their own” in

the target-present condition, confidence in their “own” inaccuracy was higher than confidence for responses in the target-absent condition that were forced to be incorrect by design of the survey.

CHAPTER FIVE

Product Involvement Effects on the Confidence/Accuracy (CA) Relationship

Experiment 3

The third experiment determined whether subjects' experience with particular products during care package assembly had an effect on their confidence and accuracy—and the relationship between the two—on a product brand identification survey 1 week later. Subjects either selected six products from two brands of each product to include in the care package (selected condition) or were provided the six brands of products that subjects in the selection condition chose in the previous session (provided condition). Subjects' assembly stations in the product-provided conditions included the brands of products subjects in the selection condition chose so that each brand of product was both chosen and provided equally as often over the course of the experiment. The dependent variables in question were accuracy of the product brand identification, confidence in subjects' responses, and the CA relationship, quantified by the Goodman-Kruskal gamma correlation, G .

Methods

Participants. Fifty-six Baylor University undergraduate students volunteered their participation in the experiment, earning experimental participation credit for introductory psychology courses in return. All participants signed an informed consent for research participation and received a copy of the form for their records. The study was approved by Baylor's Institutional Review Board and met the American

Psychological Association's standards for minimal risk to the well-being of subjects. At the conclusion of each research session, subjects were debriefed with respect to the objective of the care package assembly activity.

Materials. The same materials were used as in all previous experiments, except that the product identification survey (see Appendix C) included six 4-alternative, target-present recognition questions. It also asked for subjects' confidence in their responses on a scale of 0 to 100% in increments of 20% following each question.

Procedure. Subjects entered a classroom in the Baylor Sciences Building and seated themselves at one of 2 care package assembly stations set up on opposite sides of a large table in the center of the room. For the selection condition, six types of products were divided into two pairs of two different brands of each product and were placed on two separate tables at the back of the room. Each table held a pair of six different types of products (see Table 1). A hand-held game, cards, and notepad were provided at each assembly station along with a cardboard box and bubble wrap. Subjects were given instructions on how to pack the care package, then assembled the six toiletry items, the game, cards, and notepad into a cardboard box, using bubble wrap to prepare the package for shipping. Upon returning 1 week later to an adjacent conference room, all subjects completed the product identification survey.

Table 1. *Assembly Station Product Brand Selections, Experiment 3.*

Lotion	Mouthwash	Shampoo	Soap	Sunscreen	Toothpaste
Aveeno Jergens	Listerene Oasis	Dove Pert Plus	Coast Lever 2000	Coppertone Hawaiian Tropic	Aquafresh Rembrandt
Lubriderm Vaseline	Act Plax	Pantene V05	Dial Irish Spring	Banana Boat Ocean Potion	Colgate Crest

Results

Table 2 shows that subjects who selected their products for inclusion in the care package were more accurate ($M = .49$, $SEM = .04$) than those who were provided their products ($M = .33$, $SEM = .04$) and more confident ($M = 55.71$, $SEM = 3.29$) than the provided condition ($M = 39.28$, $SEM = 4.50$). As such, selection condition had a statistically significant effect on accuracy [$t(54) = 2.85$, $SE = .01$] and confidence [$t(54) = 2.95$, $SE = .01$]. Levene's test for the equality of variances was not significant for the differences in mean accuracy, but was significant for the confidence data [$F(2, 54) = 4.32$, $p < .05$]. Therefore, the t -test for the differences in mean confidence between the selection groups was performed with a correction for unequal variances. The mean within-subject G scores across six responses did not vary according to selection condition.

Subjects who selected their products were not only more confident in their incorrect answers ($M = 47.29$, $SEM = 2.6$) than those who were provided the same products ($M = 37.50$, $SEM = 2.6$), but were also more confident in their correct answers ($M = 64.34$, $SEM = 2.9$) than subjects in the provided condition ($M = 42.86$, $SEM = 4.3$).

A cross-tabulation of confidence levels from 0% to 100% in increments of 20% and accurate and inaccurate responses across all subjects revealed a strong association between confidence and accuracy for the selected-products condition ($G = .44$), but a weak association of the same for the provided-products condition ($G = .11$).

Table 2. *Mean Within-Subject Accuracy, Confidence, and G Across Selection Conditions, Experiment 3.*

Condition	Accuracy	Confidence	Gamma (G)
Selected	.49 (.04)	55.71 (3.29)	.41 (.13)
Provided	.33 (.04)	39.28 (4.50)	.25 (.15)

Discussion

According to previous research involving the actor/observer bias for eyewitness memory, it was expected that increased product involvement would lead to better encoding. Subjects who selected products were (presumably) more engaged in the encoding event, and more involved with the product itself, than were the subjects provided products to include in the care package. Indeed, subjects who selected six products for inclusion into the care package were more accurate and confident in their brand identifications 1 week later than subjects who were provided the same. These data confirm earlier results indicating that the quality of the encoding experience affects memory performance such that participants in one staged crime accurately recalled more event detail 12 weeks later than witnesses.

It was expected that the selection condition (higher level of subject involvement) would exhibit a higher value of G than the provided condition. That is, that subjects who

selected products would exhibit better calibration between the accuracy of their identifications and confidence in their responses. However, there was no significant difference between the mean within-subject G score across both conditions of product selection. Within selection conditions, subjects who selected products, even while reporting higher levels of confidence across both accurate and inaccurate responses, displayed better calibration than subjects in the provided condition. As expected, increased product involvement lead to not only enhanced response accuracy, but also better confidence/accuracy calibration over a retention interval.

Experiment 4

The fourth experiment tested whether asking subjects to justify their product selection affects accuracy and confidence for subsequent product brand identification a week later. The two conditions of product involvement will be whether subjects are asked to justify their product brand selection on the identification survey (justify condition) or are not asked to justify the same on the identification survey (no-justify condition). Subjects in the justification condition were asked to state why they selected the brands of products they included in the care package on the identification survey, along with their response as to the brands they selected and confidence in the accuracy of their answers. Subjects in the no-justification condition were not asked to justify their selections, but completed the identification survey with confidence ratings for each response. The dependent variables in question are accuracy of the product brand identification, confidence in subjects' responses, and the CA relationship, quantified by the Goodman-Kruskal gamma correlation, G .

Methods

Participants. Fifty-four Baylor University undergraduate students volunteered their participation in the experiment, earning experimental participation credit for introductory psychology courses in return. All participants signed an informed consent for research participation and received a copy of the form for their records. The study was approved by Baylor's Institutional Review Board and met the American Psychological Association's standards for minimal risk to the well-being of subjects. At the conclusion of each research session, subjects were debriefed with respect to the objective of the care package assembly activity.

Materials. The same materials were used as in Experiment 3 (see Appendix C), except that the product identification survey included, for the justify condition, a question asking subjects to justify why they selected a particular brand of product (see Appendix D). It also asked for subjects' confidence in their responses on a scale of 0 to 100% in increments of 20% following each question.

Procedure. The same procedure was used as in Experiment 3, except that all subjects selected six types of products according to the product brand selection schedule given in Table 1.

Results

Surprisingly, there were no statistically significant differences in mean accuracy, confidence, or within-subject *G* scores between justification groups.

Table 3. *Mean Within-Subject Accuracy, Confidence, and G Across Justification Conditions, Experiment 4.*

Condition	Accuracy	Confidence	Gamma (<i>G</i>)
Justify	.54 (.04)	56.54 (4.23)	.65 (.08)
No-justify	.49 (.04)	55.92 (3.41)	.41 (.13)

Discussion

It was expected that asking subjects to justify their product brand selection would result in subjects reflecting on their encoding experience of preparing the care package to a greater extent than subjects who were not asked to justify their selection. The justification condition, therefore, was expected to exhibit higher accuracy, confidence, and *G* scores than the no-justification condition. No such differences were found between justification conditions on any dependent variables. It seems that asking subjects to reflect on and explain some aspect of their encoding experience has no bearing on their memory performance or confidence in the same.

CHAPTER SIX

Conclusion

The four experiments utilized a novel paradigm for eyewitness testimony investigation, one in which subjects' incidental product brand exposure occurs while they perform some meaningful task, unlike traditional psychological experimentation procedures. Historically, eyewitness identification in the laboratory has involved slide shows of some criminal event and subjects are fully aware that the overall purpose of the slide show is part of a psychological experiment. The most commonly used slide show in eyewitness memory research was developed in 1985, which depicts a suspect breaking into an office and stealing a calculator from a desk (McCloskey & Zaragoza, 1985). However, the current studies employed a paradigm that resembles personal injury plaintiffs' initial exposure to harmful products—one in which the focus of the product exposure is not a psychology experiment, but rather a purposeful task in which they encounter certain products. After some amount of time, plaintiffs are asked unexpectedly about the brands they encountered during some episode of product exposure in the past. To the subjects of the present experiments (as well as claimants of personal injury suits), product brands and other details are seemingly unimportant at the time they worked with the products. This is not the case if subjects know they are working with products for the sake of psychological experimentation itself and will be asked about the product brands later. Similarly, claimants of personal injury suits did not know the product brands would be vitally important decades later and that their memory for those brands would be tested and scrutinized. The experience of subjects in this paradigm approximates the incidental

encoding condition in which harmful product exposure occurs outside the laboratory. Therefore, conclusions drawn from the data obtained using the care package assembly paradigm may be extrapolated to matters concerning product brand identification during personal injury lawsuits against companies that manufactured harmful products long ago.

In the case of confirmatory post-identification feedback, plaintiffs may be asked to identify particular brands of products they have encountered in the past and, in the course of pre-trial preparations, may be coached as to their response and asked how sure they are that they are correct. Experiments 1 and 2 established the first line of investigation into confidence malleability following confirmatory post-identification feedback for *delayed* identification. The interview techniques providing subjects with confirming feedback concerning their responses—right or wrong—mimicked the procedures prescribed by law firms in preparing a claimant for their deposition, namely, to encourage witness confidence in identifying product brands they may have encountered during some past critical event.

Subjects whose responses were confirmed, incorrectly, in Experiment 1 reported higher levels of confidence than those who were not told their answers were correct at test following both 10 minutes and 1 week delays. However, the confirmatory feedback effect was pronounced for the delayed identification group. These findings suggest that confidence in delayed product brand identification is susceptible to interviewer feedback, especially if the individual's response is incorrect because the correct answer is not given as an answer choice at test.

Experiment 2 was carried out to approximate the conditions by which claimants in civil litigation suits identify brands of products from photo books that may—or may

not—include as an answer choice the brand of product they actually encountered years earlier. Subjects who made incorrect responses when the correct answer was provided at test reported higher levels of confidence than those who were forced to be incorrect on a target-absent survey. Subjects who were wrong by rejecting the correct answer choice were more confident in their response accuracy than subjects who were wrong by design of the survey. This was surprising, as it was expected that the target-absent condition would rate confidence higher after confirmatory feedback than the target-present condition. These findings suggest that even if the correct product brand is included in materials provided claimants in preparing for their deposition, they may identify with high levels of confidence, a product brand that is incorrect.

Experiments 3 and 4 were conducted to test the effects of encoding factors that may influence the confidence/accuracy relationship. It was thought that the degree to which an individual is involved with a product may predict the chance that witness confidence is a reliable indicator of identification accuracy. That is, the more an individual is involved with a product, as in selecting, purchasing, or directly working with it, the more likely they will be well-calibrated with respect to confidence and identification accuracy. The less an individual is involved with a product, as in being on the premises of the harmful product or being provided the product to work with, the less likely their confidence will indicate identification accuracy. There was no precedent in the eyewitness memory literature for this particular area of study. Previous studies have carried out regression analyses on subjects' post-identification judgments of encoding characteristics such as subject estimates of the amount of time the perpetrator was shown

in surveillance video, but none have directly manipulated the encoding condition between subjects to identify the effects of such on confidence for delayed identification.

Experiment 3 established the methodology and preliminary findings concerning the product selection paradigm in this new line of eyewitness memory research. Subjects who selected products 1 week prior to test were more accurate, confident, and the correlation between the two measures was higher than subjects who were provided products for inclusion in the care packages. This suggests that individuals who have had an increased level of involvement with certain products will be more accurate, and accurately confident, in identifying the brands of products they have encountered than those who have had less involvement with the same products. Therefore, product liability claims involving plaintiffs who were merely on site of defendants' products may be less accurate in identifying the product brand they allegedly encountered years ago than individuals who selected or otherwise handled the products directly.

Experiment 4 tested whether asking subjects to justify their selection of particular brands of product corresponded to an increase in accuracy and confidence. It was thought asking subjects to reflect on the encoding experience by explaining their selection would correspond to increased accuracy and confidence. However, no such increase was found, as subjects who were asked to explain their product brand selection were just as accurate and confident as those who were not asked to reflect on their experience with the product. Claimants who may be asked to explain how and why they know they chose or otherwise worked with a particular brand of product may not be more accurate than those who are merely asked to identify and rate confidence. Presumably, elaborate justifications of how and why a plaintiff knows they encountered a particular

brand of product do not necessarily indicate they are correct, or even more likely correct than if they did not provide the explanation for their answer. An increased level of involvement alone is enough to indicate an increased chance that an individual's product brand identification is accurate, and that confidence reported by the more-involved individual is a good indicator of response accuracy.

The limitations of the studies can be addressed in future directions of this line of research. Claimants who have fallen ill as a result of direct exposure to harmful products likely report having encountered the products multiple times. Repeated exposure of the same products to subjects over time would more closely resemble this scenario. One criticism of this line of research is that we are testing, largely, college freshmen who are dissimilar from the claimants in product liability suits in many ways. When the field of applied cognitive psychology tests how memories are formed in the laboratory, we assume that the process of memorizing and memory retrieval occurs in the same manner across individuals. The differences lie in the aforementioned encoding variables, such as repeated exposures, and perhaps also familiarity and relevance of the products to the individual's everyday life. One could also carry these experiments out in the field using construction workers currently employed and working with products with potential, as-yet unknown dangers to their health. By recording the brands of products currently used on particular job sites, workers could be tested a week, months, or even years the jobs are completed. The conditions of those field experiments would even more closely resemble the conditions under which claimants worked with certain brands of products long ago.

Forensic psychology cannot presume that plaintiffs in product liability lawsuits knowingly confabulate their involvement with particular brands of products years ago.

However, the overall goal of this study is to inform triers-of-fact about the mercurial effects of confidence. While plaintiffs may report their testimony with high levels of certainty, confidence may be susceptible to interviewer feedback and encouragement and that the ability of confidence to predict identification accuracy may depend on the quality and level of involvement the plaintiff had with the product long ago.

APPENDICES

APPENDIX A

Identification Survey, Experiments 1 and 2

Baylor Student ID Number: _____ Age: _____ Gender: M/F

Please answer the following questions based on your experience of preparing the care package.

Did you know before the session began that you would be surveyed about the brands of products included in the care packages? (Be honest - its OK if you did, we just need to know! You'll still receive full credit for participating!) Circle: Yes / No

1.) What brand of **hand lotion** did you include in your care package?

Curel

Aveeno

Lubriderm

Vaseline Intensive Care

I did not include hand lotion in my care package

APPENDIX B

Confidence Survey, Experiments 1 and 2

Baylor Student ID Number: _____ Age: _____ Gender: M/F

Please answer the following question based on your experience of completing the product brand identification survey just now.

At the time of identification, how confident were you that your answer concerning the brand of **hand lotion** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

APPENDIX C

Survey, Experiment 3, Both Conditions and Experiment 4, No-justification Condition

Baylor Student ID Number: _____ Age: _____ Gender: M/F

Please answer the following multiple choice questions based on your experience of preparing the care package.

Did you know before the session began that you would be surveyed about the brands of products included in the care packages? (Be honest - its OK if you did, we just need to know! You'll still receive full credit for participating!) Circle: Yes / No

2.) What brand of **hand lotion** did you include in your care package?

- Aveeno
- Jergens
- Lubriderm
- Vaseline Intensive Care
- I did not include hand lotion in my care package

How confident are you that your answer concerning the brand of **hand lotion** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

2.) What brand of **mouthwash** did you include in your care package?

- Act
- Listerene
- Oasis
- Plax
- I did not include mouthwash in my care package

How confident are you that your answer concerning the brand of **mouthwash** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

3.) What brand of **shampoo** did you include in your care package?

Dove

Pantene

Pert Plus

V05

I did not include shampoo in my care package

How confident are you that your answer concerning the brand of **shampoo** you included in the care package was correct?

0%

20%

40%

60%

80%

100%

4.) What brand of **soap** did you include in your care package?

Coast

Dial

Irish Spring

Lever 2000

I did not include soap in my care package

How confident are you that your answer concerning the brand of **soap** you included in the care package was correct?

0%

20%

40%

60%

80%

100%

5.) What brand of **sunscreen** did you include in your care package?

Banana Boat

Coppertone

Hawaiian Tropic

Ocean Potion

I did not include sunscreen in my care package

How confident are you that your answer concerning the brand of **sunscreen** you included in the care package was correct?

0%

20%

40%

60%

80%

100%

6.) What brand of **toothpaste** did you include in your care package?

Aquafresh

Colgate

Crest

Rembrandt

I did not include toothpaste in my care package

How confident are you that your answer concerning the brand of **toothpaste** you included in the care package was correct?

0%

20%

40%

60%

80%

100%

APPENDIX D

Survey, Experiment 4, Justification Condition

Baylor Student ID Number: _____ Age: _____ Gender: M/F

Please answer the following multiple choice questions based on your experience of preparing the care package.

Did you know before the session began that you would be surveyed about the brands of products included in the care packages? (Be honest - its OK if you did, we just need to know! You'll still receive full credit for participating!) Circle: Yes / No

3.) What brand of **hand lotion** did you include in your care package?

Aveeno

Jergens

Lubriderm

Vaseline Intensive Care

I did not include hand lotion in my care package

Why did you select the product identified above?

How confident are you that your answer concerning the brand of **hand lotion** you included in the care package was correct?

0%

20%

40%

60%

80%

100%

2.) What brand of **mouthwash** did you include in your care package?

Act

Listerene

Oasis

Plax

I did not include mouthwash in my care package

Why did you select the product identified above?

How confident are you that your answer concerning the brand of **mouthwash** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

3.) What brand of **shampoo** did you include in your care package?

Dove

Pantene

Pert Plus

V05

I did not include shampoo in my care package

Why did you select the product identified above?

How confident are you that your answer concerning the brand of **shampoo** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

4.) What brand of **soap** did you include in your care package?

Coast

Dial

Irish Spring

Lever 2000

I did not include soap in my care package

Why did you select the product identified above?

How confident are you that your answer concerning the brand of **soap** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

5.) What brand of **sunscreen** did you include in your care package?

Banana Boat

Coppertone

Hawaiian Tropic

Ocean Potion

I did not include sunscreen in my care package

Why did you select the product identified above?

How confident are you that your answer concerning the brand of **sunscreen** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

6.) What brand of **toothpaste** did you include in your care package?

Aquafresh

Colgate

Crest

Rembrandt

I did not include toothpaste in my care package

Why did you select the product identified above?

How confident are you that your answer concerning the brand of **toothpaste** you included in the care package was correct?

0% 20% 40% 60% 80% 100%

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