

THE USE OF NARRATIVES  
IN SAFETY AND HEALTH COMMUNICATION

by

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AN ABSTRACT OF A DISSERTATION

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Department of Psychology  
College of Arts and Sciences

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Manhattan, Kansas

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## **Abstract**

Unintentional injuries represent the leading cause of death among Americans aged 1-44 years. While there have been many life-saving advances in engineering, attempts to save lives by changing people's behavior have been less successful. For instance, safety and health communications have sometimes led to increased knowledge and self-reported intentions to comply with recommendations, but traditional efforts to demonstrate changes in actual target behaviors have often failed.

Research in many settings has shown that narrative communications have exceptional power to persuade and affect peoples' decisions. This suggests that safety and health messages might be more effective if they include narratives, such as brief stories about people who have been injured. The purpose of this dissertation is to determine if safety communications that include stories about injuries result in superior behavioral compliance when compared with traditional abstract safety messages.

Teams of two participants assembled a swing set, using written instructions that contained relevant safety messages. Fifty-four teams were randomly assigned to three conditions: story-based safety messages, concrete nonstory safety messages, and traditional abstract safety messages. Compliance with safety messages was defined as the number of compliant components in the finished swing set. After adjustment for covariates, story-based messages resulted in a 20 percent improvement in compliance, compared with concrete nonstory and traditional abstract messages. Covariates included age, gender, (log) childcare experience, equipment assembly experience, presence of observer, and a final covariate related to timing of experimental sessions conducted by different experimenters.

A positive relationship was noted between behavioral compliance and immediate (but not delayed) recall of message content. Narrative transportation was also positively related to compliance, but only within the story-based condition. Behavioral compliance was not related to reminders or judgments about the likelihood of injuries.

The research is important because of its potential for improving safety communications and saving lives. Stories about injuries improved safety behavior even though the stories were brief and not designed to be entertaining or transporting. In contrast, the lack of correspondence between observed behavior and many surrogate measures suggests caution is in order when evaluating interventions using self-report measures, delayed memory, and other common dependent variables.

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Approved by:

Major Professor  
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Research in many settings has shown that narrative communications have exceptional power to persuade and affect peoples' decisions. This suggests that safety and health messages might be more effective if they include narratives, such as brief stories about people who have been injured. The purpose of this dissertation is to determine if safety communications that include stories about injuries result in superior behavioral compliance when compared with traditional abstract safety messages.

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## Dedication

I am fortunate to have some strong supporters in my corner.

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I guess the point is this: Working adults often don't have a support system that would allow them to take on a task like getting a Ph.D. I could not have spent so much time on this without knowing that everyone else would be OK and that they would pick up the slack for me until I was done. I owe them my thanks.

## **CHAPTER 1 - INTRODUCTION**

This dissertation examines whether compliance with safety messages can be improved by illustrating those messages with anecdotes of actual injuries. The significance of this research lies in its potential for improving safety and health communication methods.

Unintentional injuries claim more than 100,000 lives in the United States each year (National Center for Health Statistics, 2006). In fact, unintentional injuries represent the leading cause of death in Americans aged 1-44 years. Nonfatal injuries are even more common: According to the National Safety Council (2007), about 1 in 9 Americans sought medical attention for nonfatal injuries in 2004.

Unintentional injuries commonly occur in transportation, on the job, at home, and in leisure pursuits such as hunting, biking, and swimming. Safety and health professionals, engineers, health communicators, and a host of other specialists devote their careers to reducing the incidence of preventable injuries and illnesses. These efforts have met with many successes: Between 1950 and 1992, for instance, the annual age-adjusted mortality rate due to unintentional injuries declined by more than half (National Center for Health Statistics, 2006). By some accounts, the Twentieth-century safety and health movement resulted in more than 3 million lives being saved in the United States alone (Krieger & Montgomery, 1997).

Many of the Twentieth-century's greatest accomplishments in reducing preventable injuries and illnesses resulted from improvements in engineering and technology. The automobile safety belt and air bag are familiar examples of engineering solutions to safety issues. Other examples of Twentieth-century advances in safety and health engineering include chlorinated water, automation of manufacturing processes, and the elimination of lead from gasoline and certain paints.

In contrast to the clear successes of engineering, few conclusions can be drawn about the effectiveness of interventions designed to change people's safety and health behavior (e.g., DeRoo & Rautianen, 2000; Lincoln et al., 2000; Lipscomb, 2000; Rivara & Thompson, 2000; Runyan, Zakocs, & Zwerling, 2000; Segui-Gomez, 2000; Snyder et

al., 2004). Although a vast literature offers advice on how to conduct safety training and public health campaigns, there have been few rigorous experiments demonstrating the effectiveness of such interventions. In fact, the unproven effectiveness of behavioral interventions has led many professionals to conclude that the only effective way to reduce risks is to “engineer out” any opportunity for harm: “On the whole, passive engineered solutions work best – that is, technological innovation and legislative intervention appear to be more effective than so called active interventions based on socio/educational attempts to change behaviour” (Volpe, 2004, p. 4).

Still, engineering controls cannot eliminate all exposures to hazards. Furthermore, people often bypass or actively defeat even the best engineered safety devices. For instance, the National Safety Council (2007) estimates that by refusing to wear safety belts, over 5,000 Americans die needlessly in automobile accidents each year.

Since the publication of Heinrich’s (1931) classic accident prevention text, it has been widely accepted that most preventable injuries and illnesses are caused by the actions of people. Until engineering controls become universal and fool-proof (an unlikely event), safety and health professionals will continue to pursue better interventions for changing behavior. This goal seems even more important given recent evidence that fatal injury rates stopped declining after 1992 and are now actually on the rise (National Center for Health Statistics, 2006).

This dissertation draws on previous research in judgment, decision making, social persuasion, health communication, education, human factors, and related disciplines. The primary goal is to determine if observed behavioral compliance with traditionally impersonal and abstract safety messages can be improved by including narrative anecdotes describing relevant injuries. An additional goal is to identify some psychological mechanisms that may be related to any such effect.

In contrast to most safety and health communication research, this dissertation sought to achieve high internal validity by measuring observable behaviors in a controlled experimental setting. In order to accomplish this goal, product warnings were selected as the communication medium, and observed behavioral compliance was selected as the main dependent variable. Specifically, teams of participants assembled a swing set according to written instructions containing relevant warnings, and

performance was measured as the number of compliant swing set components. Despite the focused nature of the experiment, the findings of this dissertation have implications for a variety of safety and health communication methods. Besides product safety warnings, safety and health professionals communicate hazards through group training, one-on-one contact, mass media campaigns, etc. It is expected that the line of research begun in this dissertation will lead to future field studies involving other safety and health communication methods in naturalistic settings.

### **Safety and Health Communications**

Consider the following example of an actual safety message:

*Example 1:* “DO NOT mow wet grass.” (Ariens Company, 2003, p. GB-7).

This is a statement from the operator’s manual for a popular line of lawn mowers. The safety instructions in this manual consist of an exhaustive list of about a hundred short, abstract statements relating to different hazards such as the one described above. The statement does not explain why the operator should observe the precaution, nor does it convey the seriousness of accidents that might occur. To illustrate the weaknesses of this approach, ask yourself these questions: After reading this warning, do you know why you should not mow wet grass? Do you think you will remember and observe this warning in the future? Can you think of any reason why you would even care about this warning if you had purchased a mower and were reading the manual?

Now consider the following example that communicates risk more in a concrete manner using a true story.

*Example 2:* A worker at a golf course was cutting grass with a walk-behind lawn mower. It was early morning and the grass was wet with dew. He slipped on the wet grass while mowing. As he slid, one foot went under the mower. He kicked off the mower with his other foot, but it was too late. The foot that slid under the mower was slashed to the bone, and many of the worker’s tendons were cut in two. He was rushed to a hospital and underwent lengthy surgery to repair the damage. (Adapted from Occupational Safety and Health Administration [OSHA], n.d.-a)

This example communicates the risk of mowing wet grass in a more intuitive fashion by providing a vivid mental image and by making the risk seem real. Now do you know why you should not mow wet grass? What are the chances you will remember and observe this warning in the future? Do you care?

The value of stories as tools for communication and decision making has been verified by research in many settings. Furthermore, investigators have recommended using stories in messages related specifically to safety and health (e.g., Cole, 1997; Cullen, & Fein, 2005; Green, 2006). Still, there appear to be only a few published experimental studies of the effects of stories in safety and health communication, and these studies mainly examined participants' *attitudes* toward safety and health issues. Although a very small number of studies have also examined *self-reported intentions* or *self-reported behavior change*, I have been unable to find any published experiments examining the effects of stories on *observed behavior change* with safety and health recommendations. Furthermore, I have been unable to find any published reports investigating the effects of narrative communication in connection with safety warnings related to *products and equipment*. My discussions with leading researchers have lent support to the my suspicion that there may be no published reports of research on the behavioral effects of narrative product/equipment warnings (Henry P. Cole, personal communication, September 5, 2006; Elaine T. Cullen, personal communication, October 3, 2006; Baruch Fischhoff, personal communication, March 13, 2007; Melanie C. Green, personal communication, March 13, 2007; Terri Heidotting, personal communication, January 13, 2007; Roger Schank, personal communication, March 14, 2007; Michael S. Wogalter, personal communication, October 8, 2006).

### **What is a Story?**

Before examining how the word *story* will be used in this dissertation, it may be helpful to consider how this and related terms have been defined by others.

#### ***Story***

In *Mirriam-Webster Online* (n.d.), relevant definitions of story include “an account of incidents or events”; “a statement regarding the facts pertinent to a situation

in question”; “anecdote;” and “a fictional narrative shorter than a novel” (<http://www.m-w.com/dictionary/story>).

Regarding use of the word story in a research context, Schank and Berman (2002) have stated, “a story is a structured, coherent retelling of an experience or a fictional account of an experience. A satisfying story will include the following elements: themes, goals, plans, expectations, expectation failures (or obstacles), and perhaps, explanations or solutions” (p. 288).

Sarbin (1986) has emphasized the importance of the temporal sequence or pattern of a story. In this view,

A story is a symbolized account of actions of human beings that has a temporal dimension. The story has a beginning, a middle, and an ending [or, as Kermode (1967) suggests, the sense of an ending]. The story is held together by recognizable patterns of events called plots. Central to the plot structure are human predicaments and attempted resolutions. (Sarbin, 1986, p. 3)

Pennington and Hastie (1991) have emphasized that causal connections are necessary in any coherent story:

Stories involve human action sequences connected by relationships of physical causality and intentional causality between events. In its loosest form, a story could be described as a "causal chain" of events in which events are connected by causal relationships of necessity and sufficiency. (p. 525)

Other researchers have chosen not to routinely use the word story, but have instead used terms such as *narrative*, *anecdote*, *exemplar*, and *scenario*. In some cases, these words have been used synonymously with story, but in other cases a different meaning has been apparent. For audiences unfamiliar with jargon, I prefer the word story because it is simple, and I suspect that most people have some sense of the term’s meaning.

## *Narrative*

According to *Mirriam-Webster Online* (n.d.), a narrative is “something that is narrated”; or “the representation in art of an event or story” (<http://www.m-w.com/dictionary/narrative>).

This definition suggests that a narration is simply the manner in which a story is told. Abbott (2002) has made the same subtle distinction between a story and a narrative: While a story progresses from the beginning, through the middle, to an end, a narrative does not necessarily have to follow in that order. According to Abbott, then, a narrative is the telling or representation of a story, and the same story may be narrated in different ways.

While Abbott and *Mirriam-Webster Online* have distinguished between stories and narratives, most researchers have not. For instance, Sarbin (1986) maintains a “...*narrative* is coterminous with *story* as used by ordinary speakers of English” (p. 3).

Many other researchers have used narrative to mean essentially the same thing as story, while elaborating further on the characteristics that make for a good narrative. For instance, in the Transportation-Imagery Model of Green and Brock (2002), a narrative raises important issues and it establishes credibility not necessarily because it is true, but because it is *plausible*. In other words, a good narrative has the appearance of truth. Bruner (1986) further argues that a good narrative “...deals in human or human-like intention and action and the vicissitudes and consequences that mark their course” (p. 13). Finally, according to Dal Cin, Zanna, and Fong (2005), narratives develop with some suspense and uncertainty.

For most researchers, then, a narrative does not seem to differ materially from a story. If there is a difference, it is merely that a narrative is the telling of a story. In this dissertation, the terms story and narrative may be used interchangeably.

## *Anecdote*

*Mirriam-Webster Online* (n.d.) defines an anecdote as “a usually short narrative of an interesting, amusing, or biographical incident” (<http://www.m-w.com/dictionary/anecdote>).

Among researchers, the term anecdote appears to have essentially the same meaning as story and narrative. For instance, Slater and Rouner (1996) use anecdote when referring to stories about what happens to a person in a particular situation. Other researchers have also made use of this word when referring to evidence presented in the form of one or more explicitly-described cases (e.g., Berger, Johnson, & Lee, 2003; Fagerlin, Wang, & Ubel, 2005; Freymuth & Ronan, 2004; Hoeken, 2001; Saks & Kidd, 1980-81; Strange & Leung, 1999).

The terms anecdote and story may be used interchangeably in this dissertation when referring to an account of a single case or incident.

### ***Exemplar***

In *Mirriam-Webster Online* (n.d.), an exemplar is “one that serves as a model or example”; or “a typical or standard specimen” (<http://www.m-w.com/dictionary/exemplar>).

As used by researchers, exemplars are examples that are sometimes, but not always, in the form of stories. Brosius (1999) defines exemplars as “...short quotations (verbal or visual) from concerned or interested people that illustrate a particular problem or particular view on a problem” (p. 213). In a somewhat broader definition, Zillman and Brosius (2000) use the term exemplar to mean an example—usually a report of a particular case that has occurred.

Limon and Kazoleas (2004) have integrated the ideas of several authors in their definition of exemplars: “Exemplars refer to ‘qualitative evidence’ or ‘qualitative supporting information’ and include narrative materials such as personal anecdotes, analogies, examples (case histories), stories, and testimony...” (p. 291).

As demonstrated above, exemplar seems to be a broader, less precise term than story, narrative, or anecdote. For the sake of clarity, therefore, exemplar will not be routinely used in this dissertation.

### ***Scenario***

A scenario is defined by *Mirriam-Webster Online* (n.d.) as “a sequence of events especially when imagined”; or “an account or synopsis of a possible course of action or events” (<http://www.m-w.com/dictionary/scenario>).



Cole (e.g., 1997) has used the terms scenario and simulation interchangeably with the word story. However, it is clear from his experimental procedures that Cole's scenarios and simulations involve active imagination and role playing.

With the exception of Cole, most authors distinguish scenarios and simulations from other types of stories based on whether or not the task includes explicit instructions to imagine oneself in the events. For instance, Gregory, Cialdini, and Carpenter (1982) use the term scenario to describe a scripted mental simulation in which participants actively imagine themselves experiencing some event. The term scenario will not be used in this dissertation except when referring to activities in which participants are instructed to imagine themselves taking part in the story.

### ***Concrete Depictions of Events***

In the writings of some researchers, concrete information is equated with stories, and abstract information is equated with statistical or other nonstory descriptions (e.g., Anderson, 1983; Sherer & Rogers, 1984). In contrast, other researchers argue that concrete information consists simply of descriptive, illustrative details that evoke mental images, regardless of whether these details are embedded stories (e.g., Sadoski, 2001).

This variety of meanings is reflected in dictionary definitions of the terms in question. In *Miriam-Webster Online* (n.d.), relevant definitions of concrete include, "characterized by or belonging to immediate experience of actual things or events...specific, particular...real, tangible" (<http://www.m-w.com/dictionary/concrete>). In contrast, relevant definitions of abstract include, "disassociated from any specific instance...difficult to understand...formal...theoretical" (<http://www.m-w.com/dictionary/abstract>).

As used in this dissertation, concrete messages are those that are descriptive, specific, explicit, precise, definite, and imaginable. Concreteness may be achieved by describing tangible objects, events and examples. These examples may be presented in the form of a story or in a nonstory form such as a list of possible outcomes. Abstract messages, in contrast, are more intangible, vague, and hard to imagine. Abstraction may occur when conceptual meaning is substituted for precise detail.

Consider the following safety messages relating to swing sets, which progress from abstract to concrete: (1) “Users should not mount the supportive structures of play equipment.” (2) “Children could be injured if they climb on the frame of the swing set.” (3) “A child could strangle and die if her clothing becomes caught on a bolt while climbing on the swing set.” (4) “A six-year-old girl slipped as she was climbing on the frame of a swing set. Her necklace became caught on a bolt as she fell, and she was accidentally hung by the neck. She died of strangulation.”

This progression of concreteness was accomplished through the inclusion of details that relate to tangible objects and particular events. The final example is in the form of a story, but a detailed nonstory description could also have been used.

### ***The Meaning of “Story” in this Dissertation***

In accordance with most of the definitions noted above, the terms *story*, *narrative*, and *anecdote* in this dissertation may be used interchangeably when referring to an account of (1) at least one character (2) in an explicit or implied setting, (3) involved in events (4) occurring in a sequence over time (5) with explicit or implied causal connections among events. The characters and events in the story may be either factual or fictional.

In contrast, the terms *scenario* and *mental simulation* will be used only when referring to activities in which there are explicit instructions for participants to role-play or actively imagine themselves in the narrative.

### **Overview of Story-based Communication**

This dissertation is based in part on the story-centered theory of communication and memory developed by Roger Schank and Robert Abelson (1977, see also Schank, 1990; Schank, 1999; Schank & Berman, 2002). Schank and colleagues have maintained that it is much easier to understand and learn new information when that information is conveyed in the form of a story rather than being presented as an abstract rule of thumb. Schank argues that stories affect more than just our *understanding* of information. According to Schank, much of our everyday *behavior* is explained by the stories we have saved in memory. These stories may originate from our own experiences or from experiences described to us by others. The accumulation of experiences in the form of

stories helps us adapt our behavior to changing situations. While we find it hard to think in terms of abstract rules, stories convey information in an intuitive way (Schank, 1999).

To understand how this relates to safety and health messages, consider how hard it can be to communicate the true significance of injury-prevention recommendations.

This problem is illustrated by a statement from an actual dump truck operators' manual regarding precautions to take before servicing the truck's hydraulic bed lift system:

*Example 3:* "CAUTION: When the dump body is raised to perform maintenance tasks, the dump body prop must be installed" (Deere & Company, 2005, p. OOU1079,00003C9 -19-19JUN01-1/1).

How likely is it that a busy worker will understand the significance of this statement and become motivated to comply with it? Contrast the impact of the statement above with force of the following story that illustrates the same hazard:

*Example 4:* A worker was fixing a dump truck because the bed of the truck would only raise part-way. He raised the dump bed until it stopped. Then he leaned underneath to fix the truck, placing his head between the partly-raised bed and the steel truck frame below. While working in this position, he began adjusting the controls. One of the controls released the dump bed, and it fell on him without warning. His head was crushed between the dump bed and truck frame. He died at the scene. This tragedy could have been prevented if the worker had used the dump body prop supplied by the truck manufacturer. The dump body prop will safely hold the dump bed in the raised position while a worker repairs the truck.

(Adapted from Occupational Safety and Health Administration, n.d.-d)

According to Schank (1999), once we fully understand a concept such as the one illustrated in this story, we may generalize and apply the concept to new cases. A worker who understands the rule illustrated in the story above might decide to install the dump body prop in a different situation. For instance, if the truck malfunctions and the dump bed becomes *stuck* in the raised position, the worker may install the prop to protect passersby if he has to leave the truck unattended in order to get help. Insights gained in this manner have great personal meaning for the one who experiences them, but that meaning is hard to convey to others.

Shank maintains that people do not usually understand or remember abstract rules they hear from other people. Without context, the validity of a rule cannot be judged and it cannot be stored or retrieved effectively in memory. People are more likely to take advice or understand the significance of what is being said when it occurs in the context of a convincing story:

We can tell people abstract rules of thumb which we have derived from prior experiences, but it is very difficult for other people to learn from these. We have difficulty remembering such abstractions, but we *can* more easily remember a good story. Stories give life to past experience... We are more persuasive when we tell stories. For example, we can simply state our beliefs, or we can tell stories that illustrate them. (Schank, 1990. p. 10)

Many of Schank's claims regarding the power of stories have been confirmed by other researchers. Graesser and Ottati (1995) reviewed research from many contexts and verified that (1) stories have a strong impact on cognition, (2) people naturally use stories to explain events and situations, and (3) memory for unusual stories is often exceptional. De Young and Monroe (1996) also conducted a thorough review and found that stories are particularly effective for communicating and understanding abstract concepts. The authors concluded that stories are effective because they engage the learner. Learners develop powerful mental images when reading or listening to interesting stories. Good stories encourage deep cognitive processing that is more likely to have an impact on future behavior. In this regard, "...stories serve as a singularly effective replacement for direct experience..." (De Young & Monroe, 1996, p. 171).

Similar conclusions were drawn by Cox (2001), who reviewed research supporting the benefits of using stories of medical cases to train doctors. Cox argued that stories are effective in part because they are better remembered than lectures about facts and principles. Furthermore, stories convey the complexities of situations and illustrate pitfalls:

Stories illustrate 'what can happen' in a case as a guide to 'what to do'... Each local situation provides relevance, context and circumstantial detail... The listener pays close attention and is vicariously involved with working out what is wrong... (p. 862)

Norman and Brooks (1997) also found evidence that stories play an important role in memory and communication in medical settings. Clinicians tend to base diagnoses on similarity to prior cases, rather than analytical causal rules such as those taught in medical school. This is apparently because anecdotes are more easily recalled than lists and abstract rules. In fact, there is evidence that anecdotes involving specific medical patients are vividly remembered by health professionals decades later.

Likewise, in the field of judgment and decision making, researchers have argued that many decisions are made more or less automatically based on the recognition of patterns that conform to memories of past cases (e.g., Pliske & Klein, 2003). Like it or not, many of our decisions seem strongly influenced by the stories we have lived or heard about.

Many theories have been proposed to explain why stories have such a profound impact on human behavior. In the sections that follow, a few of these explanations will be discussed in terms of their relevance to this dissertation. First is the notion that people become so absorbed in the experience of a story that they accept uncritically the beliefs and values conveyed in the story. The second idea to be explored is that a given story tends to remind us of past experiences that shape our understanding of the current situation. The third notion is that stories provide concrete details that we would not think of on our own. The fourth explanation to be considered is that stories affect our judgments about the likelihood of events by making those events stand out in memory, resulting in a belief that if something is possible it is also likely.

These are not the only explanations that have been offered by scholars to account for the apparent influence of stories on human behavior. However, these notions lead to hypotheses that can be examined in a laboratory setting. Furthermore, each of these theoretical explanations suggests practices that might be employed to increase the effectiveness of safety and health communications. It is for these reasons that the four possible explanations will be considered here.

## **The Persuasiveness of Stories: Transportation and Uncritical Acceptance**

Research in many settings confirms that personalized anecdotes have an

exceptional ability to persuade. According to Green and Brock (2000, 2002, 2005), the persuasiveness of stories lies in their ability to evoke vivid images, arouse strong emotions, and transport the audience into the scene of the narrative. Green and Brock's use of the term, *transportation*, relates to the experience of being lost or immersed in the story.

There is substantial evidence to support Green and Brock's claims. For instance, Oatley (2002) found that readers are strongly moved and motivated while transported into an engaging story. Polichak and Gerrig (2002) found that transported readers may experience emotions and reactions very similar to those they would have if they were participating in the actual event described by the story.

Green and Brock (2005) have distinguished the process of transportation from Petty's notion of cognitive elaboration (e.g., Petty, Rucker, Bizer, & Cacioppo, 2004). Cognitive elaboration involves thinking critically and logically about rhetorical arguments and evaluating new information in light of previously-held beliefs. In contrast, transportation is the experience of uncritical immersion in the story—an experience that suppresses memories and beliefs that are not congruent with the narrative.

To sum up the research of Green and her colleagues, plausible stories are persuasive because readers and listeners are caught up in the story to such an extent that the experience seems real. As a result, they may unquestioningly incorporate the story's central message into their own beliefs and values. If Green and Brock are correct, it seems likely that compliance with story-based safety messages will be positively related to the self-reported experience of transportation.

Although studies of transportation have mainly examined story-based messages, it is important to note that stories are not the only form of communication that can be engrossing. This leads to a possibility that nonstory communications might be about as persuasive as story-based messages if the text is interesting enough for the reader to become absorbed in the experience. If this were the case, it could be expected that compliance with nonstory warnings would also be related to the experience of transportation.

## **Reminders: Influential Echoes of the Past**

Schank and colleagues (1990; 1999; Schank & Abelson, 1977; Schank & Berman, 2002) have argued that we interpret new situations in light of familiar stories and experiences we have stored in memory. Schank maintains that stories are easily recalled because they are rich with associations relating to settings, problem situations, beliefs, decisions, etc. When confronted with new experiences, we search memory for stories that best match the situation at hand. These remembered stories then affect our behavior and our understanding of the current situation. Schank refers to these remembered stories as *reminders*.

According to Schank, when we experience a new situation or story that is consistent with episodes in memory, our old beliefs are strengthened and we behave in habitual ways. In contrast, we may change our behavior when we encounter new stories that conflict with the old ones. For instance, when we hear a new story that is familiar enough to be understood, but different enough to be interesting, we may question our beliefs. Schank uses the term *expectation failures* to describe information that is anomalous or surprising in the context of remembered stories.

Many researchers have found evidence supporting Schank's notion that new stories remind people of past events and that these reminders are important in persuasion and decision making (e.g., Strange and Leung, 1999). Not all research has supported these claims, however. For instance, Graesser and Ottati (1995) found evidence that people are *not* frequently reminded of stories during conversations. Still others have found that reminders are triggered by a variety of experiences. For instance, Larsen and Seilman (1988) found that just as many reminders were triggered by reading nonstory text, compared with stories.

To summarize, Schank has claimed that stories are persuasive in part because of the memories they evoke. Sometimes, a story calls up consistent episodes from memory that support the central message of the narrative. Other times, a story reminds us of conflicting beliefs and causes us to re-think the meaning of our old stories. In either case, we are most affected by stories that contain unexpected events. Based on the notion that reminders play a central role in the persuasiveness of stories, it is predicted that

compliance with story-based safety messages will be positively related to the recall of similar episodes from memory (episodic recall).

There is evidence that reminders often occur while reading nonstory text. This leads to the possibility once again that nonstory communications might be as persuasive as story-based communications as long as the text is interesting enough to evoke reminders. Thus, it is expected that compliance with nonstory safety messages will also be related to the recall of similar episodes from memory.

### **Applications to Everyday Life: The Benefit of Concrete Examples**

Some researchers have suggested that stories are effective because they contain concrete examples that are rich in easy-to-imagine details (e.g., Sherer & Rogers, 1984). Concreteness in communication can result from describing tangible objects, people, and events. In contrast, traditional safety and health communications often describe hazards and protective measures in abstract, hard-to-imagine terms.

It has been argued that concrete details are important because people often fail to imagine details that are not specifically described. For instance, Fischhoff (1994) has argued that people may not heed severe weather forecasts because an abstract prediction such as “a large winter storm” fails to convey details that actually affect peoples’ lives. Fischhoff argued that forecasts are more useful when they predict details that matter, such as the possibility of becoming stranded in the snow, experiencing a power outage, or suffering a heart attack while shoveling snow. Similarly, Fischhoff, Slovic, and Lichtenstein (1978) found that people have trouble filling in missing, but seemingly obvious, details. Auto mechanics, for instance, were unable to identify common reasons why a car would not start when these reasons were missing from a fault tree. Mehle (1982) and Gettys, Mehle, and Fisher (1986) also found that participants had trouble accurately including unspecified information in decision tasks.

Concreteness is a quality that is not limited to story-based communications. Mark Sadoski and colleagues (e.g., Sadoski, 2001; Sadoski, Goetz, & Rodriguez, 2000) have reported that adding concrete details to nonstory texts increases comprehension, interest, and recall. To better understand how concrete nonstory information can fill in knowledge gaps and evoke mental images, consider how the abstract dump truck warning presented



earlier might have been described with a concrete nonstory warning:

*Example 5:* Always use the dump body prop to keep the dump bed from falling and killing you. Do this every time you reach or lean under the bed.

The dump bed can fall on you without warning if you accidentally bump the controls while working underneath. The dump bed can also fall if a hydraulic hose breaks or if there is a hydraulic fluid leak anywhere in the system. If the dump body falls, you will not have time to escape. You will be crushed and killed between the dump bed and frame of the truck.

Protect yourself: Use the dump body prop whenever you place any part of your body under the raised dump bed. The dump body prop will safely hold the dump bed while you repair the truck.

As illustrated here, concrete descriptions can convey details that affect peoples' lives, even without a story.

To sum up, some scholars have proposed that stories are effective largely because their concreteness clarifies abstract concepts. It has been demonstrated that people may fail to imagine seemingly-obvious situations and outcomes unless they are explicitly described. This line of reasoning leads to a prediction that compliance with story-based safety messages will be related to participants' ability to describe specific details regarding hazards and injuries.

Again, there is no evidence that story-based messages comprise the only form of communication with the qualities noted above. This leads once more to the possibility that nonstory communications might be crafted in such a manner that they are equally persuasive as story-based communications. Thus, it is expected that compliance with nonstory messages will also be related to participants' descriptions of specific details regarding hazards and injuries.

### **Knowing When to Take Precautions: The Influence of Probability Judgments**

Safety and health communicators often try to persuade audiences by using statistics to describe the scope of a particular problem; for instance, *100 dump truck operators and mechanics die each year when they are crushed by falling dump truck*

*beds*. In fact, I used that strategy when referring to peoples' reluctance to wear safety belts earlier in this paper. It might be interesting to ask yourself whether this strategy was effective—did those statistics persuade you that it is unreasonably risky to take even *one* ride in a car without a safety belt? Contrast the safety belt statistics with the story about the mechanic whose head was crushed under the bed of a dump truck. Did that story persuade you that it is unreasonably risky to place your head (even *once*) under the unsupported bed of a dump truck?<sup>1</sup> A vivid description of a single incident is often more persuasive than a statistical account of a broader problem.

Apparently, a good story can make almost any outcome seem likely. Fischhoff (1975) asked participants to read a narrative account of British military history involving conflict with the Gurkas in India. For some participants, the story included the true historical outcome (military stalemate). Other participants read stories in which the outcome was described differently (e.g., a Gurka or British victory) or in which no outcome was provided. All aspects of the story were identical except the one-sentence ending. Fischhoff found that learning about any ending (even a false one) led participants to believe the ending was apparent at the outset. Stories may be convincing in part because they make improbable events seem self-evident.

Some scholars have argued that people believe an event is likely when they can see that some probable cause of that event is present. For instance, Reyna, Lloyd, and Brainerd (2003) noted that death seems more probable when specific causes are mentioned. The death of a 20-year-old man due to a car accident seems more likely than the death of a 20-year-old man with no cause specified. Hastie & Pennington (2000), among others, have pointed out that good stories contain many causal connections, and this could explain why stories have such a profound impact.

Can nonstory communications convey causes just as effectively? Krynski and Tenenbaum (2003) found that people often ignored statistics when making judgments because the statistics were usually provided with no explanation. In contrast, when Krynski and Tenenbaum explained the statistics so that causal connections with outcomes

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<sup>1</sup> Slovic, Fischhoff, and Lichtenstein (1978) have argued that statistical arguments involving low probabilities are most likely to be effective when stated in terms of the cumulative probabilities over repeated exposures.

were apparent, participants did incorporate statistics in their judgments.

In conclusion, a detailed story about a single death may have a profound impact on an audience's judgments of risk. This may occur because of the vividness of the story, the plausibility of causes and effects presented in the story, or other factors such as emotions evoked by the story (e.g., Johnson & Tversky, 1983). In any event, this line of reasoning leads to a prediction that compliance with story-based safety messages will be related to participants' judgments of the likelihood of injuries.

There is no reason to believe that stories comprise the only way to convey vividness, causality, and emotion. This again suggests the possibility that nonstory communications may be effective, provided they are crafted with these requirements in mind. It is expected, therefore, that compliance with nonstory safety messages will also be related to participants' judgments of the likelihood of injuries.

### **Stories Versus Nonstory Communications: The Jury is Still Out**

Researchers in a variety of fields have sought to test the common claim that stories are more persuasive than statistics and other nonstory evidence. Based on the arguments presented so far, the outcome might seem self-evident. In reality, however, research results have been mixed. Some researchers have indeed found evidence that stories are more effective than statistics and other nonstory evidence (e.g., Anderson, 1983; Hamill, Wilson, & Nisbett, 1980; Koballa, 1986; Reinard, 1988; Taylor & Thompson, 1982). Other investigators have reached the opposite conclusion, finding that statistics and nonstory evidence are superior (e.g., Allen & Preiss, 1997; Baesler & Burgoon, 1994; Hoeken, 2001; Hoeken & Hustinx, 2002; Lindsey & Yun, 2003). Still others have found mixed or inconclusive evidence (Baesler, 1997; Kopfman, Smith, Ah Yun, & Hodges, 1998; Reinhart, 2006; Wilson, Mills, Norman, & Tomlinson, 2005).

How is one to reconcile these conflicting findings? One approach was suggested by Baesler and Burgoon (1994), who argued that researchers have often confounded evidence type (story v nonstory) with vividness. In other words, experimental differences in the persuasiveness of statistical and narrative evidence may simply be due to the fact that statistical arguments are often presented in a dry, uninteresting style, whereas narratives are usually written more clearly, directly, simply, and in a lively manner.

Based on this argument, one might wonder if all types of evidence could be equally convincing if only they are presented well. In fact, Limon and Kazoleas (2004) have suggested that stories and non-story evidence can both be persuasive, and the key to making any type of evidence effective is simply to use a well-crafted argument.

### **Evaluation of Story-based Safety and Health Communications**

Increasingly, story-based safety and health communications are being evaluated by researchers and used by practitioners. The following review will highlight safety and health interventions that employ reading, viewing, and discussing narrative messages.<sup>2</sup>

No attempt will be made to review interventions that involve role-playing or imagining oneself in a scenario. Role-playing and scenarios are powerful active educational techniques used by many trainers and communicators (including me). However, scenarios and role-playing are complex interventions that involve many components besides the cover story. Studies of these techniques in safety and health communication have typically failed to separate the effects of the story from the active learning processes involved. As a result, conclusions about the effects of the story-based elements of scenarios tend to be confounded by activity components that are unrelated to stories, *per se*. In order to present a clearer picture, this review will address interventions in which stories and nonstory information are presented in equivalent fashion (which, in most studies, has involved passive educational techniques).

This review will also be restricted to interventions involving *routine safety and health behaviors* such as wearing safety belts or observing common work rules related to safety. Due to the vastness of literature, no attempt will be made to review studies involving non-routine safety and health decisions of the type that involve careful consideration of risk/benefit tradeoffs. Moreover, this review will not address policy decisions about societal risks, nor will it address medical decision making by healthcare

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<sup>2</sup> This dissertation examines safety—rather than health—behaviors. However, since very few studies of story-based safety interventions have been published, this review will be expanded to include story-based health communications since they represent the nearest approximation to the subject matter in question.

professionals and patients. Finally, nonpersuasive uses of narrative, such as the therapeutic effects of telling one's personal story, will not be addressed.

### ***Informational and Training Materials Used by Safety and Health Professionals***

The story-based safety and health materials reviewed in this section generally have not been subjected to formal evaluation. Rather, these are materials that are in widespread use by professionals in the field of occupational safety and health.

The National Institute for Occupational Safety and Health (NIOSH) has developed a number of story-based publications to inform employers and safety professionals about hazards in the workplace. For instance, NIOSH has incorporated injury stories in publications highlighting fork lift rollovers (NIOSH 2001), phosphine poisoning (NIOSH, 1999), electrocution (NIOSH, 1998), and many other workplace hazards (e.g., NIOSH, n.d.-c). Furthermore, the NIOSH Fatality Assessment and Control Evaluation (FACE) program publishes investigative case reports of workplace fatalities to help safety and health professionals understand how to recognize and control a variety of risks (NIOSH, n.d.-a).

Along similar lines, OSHA (n.d.-c) has made liberal use of case reports in its *Fatal Facts* series. Each publication in this series illustrates a workplace hazard with a story about a fatal injury, followed by recommendations for preventing similar incidents. The publications are directed toward a broad audience including workers, supervisors, employers, and safety and health professionals.

While reading the anecdote-based publications of OSHA and NIOSH, one's subjective experience is often consistent with the notion that the injury reports are effective tools for communication. However, it does not appear that these publications have been evaluated experimentally.

Ricketts, Marr, Slocombe, and Upham, (2003), and Ricketts and Aramouni (2004) developed occupational safety and health training materials that included brief accident reports illustrating hazards described in the text. The training materials have not been evaluated in a controlled experimental setting, but over a thousand responses to surveys indicate the materials have been viewed as helpful by trainees and trainers (Ricketts, 2007). Over 40,000 copies of these publications were distributed electronically and in

print during the first two years they were available. Follow-up surveys indicated these copies were in turn duplicated and used in large quantities for training in workplaces throughout the world (Ricketts, 2007).

Cullen and Fein (2005) described story-based safety training materials that consisted of videos developed for miners in the western United States. The training videos include interviews with working miners, many of whom tell personal stories about actual mine accidents. Although the videos were not compared with other training programs, the researchers did report data suggesting some pre- to post-test improvement of safety knowledge among the miners who watched them.

The final example to be related here is a commercially-marketed safety and health training program entitled *Coaching the Lift Truck Operator* (FLI Learning Systems, Inc., 1999). This training program incorporates accident reports in a variety of learning activities. Although there appears to have been no attempt to validate the program experimentally, it is enthusiastically used by many practitioners (including me), heavily promoted by the National Safety Council, and has been well-received by trainees.

To sum up, a growing number of story-based safety and health communication materials are available for widespread use by practitioners. The response to these materials by practitioners and trainees has been positive. However, it does not appear that these materials have been compared with control or alternative treatment procedures.

### ***Case Report and Correlational Studies of Story Effectiveness in Safety and Health Communication***

This section will discuss a sampling of reports describing story-based safety and health communications that were evaluated, but not in comparison to control- or alternative treatment groups. Most of these case reports support the notion that stories can be used effectively in safety and health communication. Although these reports are suggestive, the case-history nature of the data prevents any definitive conclusions.

There have been many reports of safety and health interventions that include stories as one component of a much more sophisticated intervention. For instance, the Witness Project is an intervention designed to increase the early detection of breast cancer among African-American women (e.g., Erwin, Spatz, Stotts, Hollenberg, &

Deloney, 1996). In the Witness Project, female African-American cancer survivors tell their personal stories in churches and community organizations. The intervention contains many inspirational and educational activities, such as hymns, prayers, readings from the bible, community events such as Witness Walks, and the use of lay health advisors to provide cancer-related information. Because of the complex nature of the Witness Project, it is not possible to determine the effect of stories apart from other components of the program. For this reason, evaluations of the Witness Project and other multi-method interventions will not be reviewed here.

A case report that *was* limited mainly to a story-based intervention involved a narrative video designed to help people stop smoking. Lopes, Sussman, Galiaf, and Crippins (1995) reported on the effectiveness of the video, entitled *Beginning the Journey*, which tells the story of how a young African-American woman quit smoking. There was no control group or alternative treatment, but a number of participants who viewed the video later reported they had quit smoking.

The video evaluated by Lopes et al. was specifically designed to change health-related behavior. In contrast, popular television dramas sometimes include plots involving health-related issues, and case reports have demonstrated that these episodes may inadvertently lead to changes in viewers' health behaviors (e.g., Brodie et al., 2001; Kennedy, O'Leary, Beck, Pollard, & Simpson, 2004; Richardson, Owen-Smith, & Howe, 2002). Given that television dramas may influence health behavior, some have suggested that popular television serials can be used to educate while they entertain. This approach is known as entertainment-education or edutainment, and it has been used to convey information about health issues, even to populations that are otherwise hard to reach.

For instance, popular native-language television dramas have been used to promote safe-sex behavior in areas of the world that have been ravaged HIV and AIDS (e.g., Singhal, Cody, Rogers, & Sabido, 2004). As reported by Glik et al., (1998), health messages have also been intentionally embedded in American television dramas to promote immunization (e.g., *ER*, *Frasier*, *Mr. Roger's Neighborhood*, *Guiding Light*, and *Days of Our Lives*). Furthermore, drama workshops with anti-drug and safe-sex themes have been evaluated with youth in the U.S. and England (Glik, Nowak, Valente, Sapsis, & Martin, 2002; Starkey & Orme, 2001). On the whole, case history reports of

entertainment-education suggest that pro-health changes in knowledge, attitudes, and self-reported behavior are possible among audiences and participants.

In recognition of the impact of popular television dramas on Americans, the Johns Hopkins Health Institutions have developed televised health news series, Web sites, and toll-free hotlines based on the programs *ER* and *Chicago Hope* (Langlieb, Cooper, & Gielen, 1999). Although these are informational (not narrative) interventions, their content is linked to the weekly themes of the dramatic programming. Langlieb et al. reported that large numbers of viewers have watched the programs, *After ER* and *Living With Hope*, and many people have used the associated Web sites and hotlines.

To sum up, correlational studies and case-reports have linked story-based communications with changes in safety and health behavior. Unfortunately, these studies do not permit conclusions about causation. The next section will review studies that do permit such conclusions. First to be examined are studies that compare story-based communications with a no-treatment or irrelevant-treatment control condition. Second, studies that compare story-based communications with a legitimate alternative treatment will be reviewed.

### ***Story-based Messages Versus No-treatment or Irrelevant-treatment Control Conditions***

This section will review studies in which story-based messages were compared with no-treatment or irrelevant-treatment control procedures. Any studies that also included legitimate alternative treatments will be omitted here and reviewed in the following section.

Roberto, Meyer, Janan-Johnson, Atkin, and Smith (2002) examined the effects of a public service radio announcement advocating the use of gun-trigger locks. The public service announcement included a dramatization about children who found a gun, played with it, but were unable to pull the trigger because it was protected by a trigger lock. The message also included a toll-free number for listeners to call and request a free trigger lock. The public service announcement was evaluated by interviewing potential listeners in a county where the radio spot was aired (treatment county) and in a county where it was not aired (control county). Posttest telephone interviews indicated no significant



differences between the treatment county and a no-treatment control county in regard to (1) knowledge of gun safety practices, (2) perceived severity of or susceptibility to accidental gunshot injuries, or (3) response efficacy and self-efficacy regarding the recommended gun safety practices. Roberto et al. did report one very substantial accomplishment: 17 percent of gun-owning households exposed to the message called the toll-free number and requested trigger locks. Since no such telephone number was provided in the control county, it is not possible to say whether the narrative portion of the PSA was responsible. The behavioral portion of this study is therefore best described as a case report, rather than a quasi-experiment.

Kyes, Brown, and Pollack (1991) and Wright and Kyes (1996) reported three studies that evaluated changes in participants' attitudes toward condoms as a result of reading a brief story. Two stories were expected to promote positive attitudes: (1) an explicit account of sexual activity that included using a condom, and (2) a non-erotic story in which the main character brought up the topic of condoms to a new sexual partner. Comparison conditions varied, but included at different times a no-treatment control and a separate story about sexual activity that did not involve condom use. Findings demonstrated that stories involving condom use or discussions about condoms resulted in more positive attitudes toward condoms and in some cases, greater self-reported intentions to use them, compared with the control conditions.

In a series of three studies, O'Donnell, San Doval, Duran and O'Donnell (1995a, 1995b) and O'Donnell, O'Donnell, San Doval, Duran, and Labes (1998) evaluated two story-based interventions designed to increase condom use among African-American and Hispanic patients at a sexually-transmitted disease (STD) clinic. All participants (experimental and control) received coupons they could redeem for condoms at a pharmacy as well as STD prevention information from clinic staff. In addition, participants in the experimental group viewed culturally-relevant dramatic videos that portrayed characters overcoming barriers condom use. Compared with the control condition, the videos resulted in positive changes in knowledge, attitudes, risk perception, and self-efficacy. Furthermore, participants who viewed the videos were more likely to redeem the condom coupons at the pharmacy. Most impressively, patients who watched the video had significantly lower rate of new STD infections compared with controls

after 17 months (22.5 percent v 26.8 percent). In a similar study, Solomon, and DeJong (1989) also reported that a culturally appropriate dramatic video resulted in greater rates of condom coupon redemption, compared with a no-treatment control group.

Sutton and Eiser (1984) described two studies that compared participants' self-reported decisions to stop smoking after they viewed a TV documentary based on an interview with a man dying of lung cancer. The irrelevant-treatment control group watched videos about either alcoholism or safety belts. Participants who watched the video about lung cancer were more likely than other participants to report fear, intentions to quit smoking, and greater utility of avoiding lung cancer. Furthermore, after three months, they were more likely to report that they had either tried to quit smoking or tried to cut down on the number of cigarettes smoked (although the number that *actually* quit was not determined).

Cantor and Omdahl (1999) reported a study in which they showed movie scenes to elementary school children and then taught fire safety and water safety rules to the children. Some children saw movie scenes that dramatized fatal accidents, while others saw scenes that were neutral in content. As measured by self report, students who viewed accident dramatizations rated the precautions they learned as more important and the risky activities they discussed as more dangerous, compared with students who viewed the neutral movie scenes.

As the final example in this section, Zeedyk and Wallace (2003) evaluated the effects of a popular children's television video in England that contained songs, rhymes, and skits about road safety. The video was distributed to half of the families in the study, and the other half served as a no-treatment control. After one month, the five-year old children under study exhibited no increase in knowledge of road safety as a result of viewing the video in their homes, although the parents firmly believed the video had made a difference.

### ***Summary: Story-based Communications Versus No-treatment or Irrelevant-treatment Control***

The studies just reviewed compared story-based communications with either a no-treatment or an irrelevant treatment control. In essence, they tested whether story-based interventions were superior to doing nothing (or doing something that is irrelevant).

These studies did not test whether story-based communications were superior to nonstory communications.

Overall, the results were encouraging. O'Donnell et al. (1998) reported the most impressive result, namely that a story-based intervention was associated with a decline in new STD infections over a 17-month period. Other studies by O'Donnell et al. (1995a & b), and Solomon and DeJong (1989) reported on the surrogate behavior of redeeming coupons for condoms, but did not examine the target behavior of actually using condoms (which, of course, would be impractical to observe), nor did they report on verifiable health outcomes such as the incidence of sexually-transmitted diseases. The other studies reported on knowledge and self-reported outcomes. Most of these studies found positive results.

### ***Story- Versus Nonstory-based Communications***

This section will review studies in which story-based messages were pitted against legitimate alternative interventions, such as messages based on statistics or rhetorical arguments. It will be seen that the results are not very informative. Not a single study employed direct observations of target behaviors. Furthermore, in regard to knowledge and self-reported outcomes, most found no differences between story-based versus nonstory-based communications. Where differences were found, there was a very slight edge for stories. Because of the amount of research to be reviewed, the studies will be organized according to the communication channel employed, i.e., live presentation by a speaker, printed materials, and video or television.

#### ***Studies of Interventions Confounded by Message Channel***

Marty and McDermott (1985) reported a study in which male university students were given information about testicular cancer. One group of participants listened as a former testicular cancer patient shared his experiences. The other group of participants read informational pamphlets produced by the American Cancer Society. On a self-report measure, participants who listened to the former cancer patient indicated they felt greater susceptibility to cancer and perceived the session as more valuable, compared with those who read the pamphlet. There were no differences between the two groups on a test of

memory over the information, nor were there any differences in self-reported concern about their health after the program. Behavioral outcomes were not examined.

Unfortunately, Marty and McDermott used a live speaker for the narrative message and a pamphlet for the informational message. This made it impossible to determine whether the attitude differences were due to the speaker's story or to the live format. In an apparent attempt to address these problems, Marty and McDermott (1986) again presented testicular cancer information to male university students, but employed three conditions instead of two. This time, one group of participants listened to a testicular cancer patient who described his experiences, another group read informational pamphlets from the American Cancer Society, and the third group listened to a live facilitator who discussed facts about testicular cancer and showed how to perform a testicular self examination using a diagram. Unexpectedly, participants who listened to the story of the cancer patient performed *worse* on a post-test measure of knowledge about testicular cancer, compared with the other groups. Finally, the groups did not differ in regard to perceived susceptibility to testicular cancer or opinions about the severity of the disease.

### ***Interventions Using Live Speakers***

Larkey and Gonzalez (2007) reported an evaluation of a story-based intervention promoting colorectal cancer screening among Latinos. The story-based intervention consisted of a fictional story about a family in which the father was scheduled to have a colonoscopy after blood was discovered in a stool test. In the alternative treatment, participants used a risk assessment tool to calculate a personal cancer risk level score. The interventions included comparable information, were delivered by health educators in a face-to-face format, and included a discussion of health recommendations. The interventions were brief, lasting 7-10 minutes. After the intervention, participants in the story-based condition reported they intended to add significantly more servings of vegetables and more minutes of exercise per week, compared with participants in the risk assessment condition. There were no significant differences in intentions to obtain colorectal screening, fear of colorectal cancer, or perception of personal risk. Actual (rather than intended) behaviors were not examined.

### *Interventions Using Printed Stimuli*

Some studies have confined their stimuli to written communications. For instance, Golding, Krimsky, and Plough (1992) compared the effects of technical versus narrative communication on knowledge and behaviors related to radon in homes. The intervention consisted of articles about radon published in local newspapers. In one city, the articles were written in a technical style. In another city, the articles presented fictional conversations among homeowners about radon in their homes. In a third (control) city, no newspaper articles on radon were published. The small size of the follow-up sample prevented the authors from drawing definitive conclusions, but the results suggested that both risk communication efforts resulted in an increase in knowledge about radon. On the other hand, neither communication format was linked to an increase in self-reported risk-reduction behaviors such as radon testing or mitigation.

Greene and Brinn (2003) asked female university students to read messages about tanning bed use. Some students read a narrative message about a young woman who developed skin cancer after using tanning beds. Other students read a message containing informative facts, but no narrative. A group of control students did not read any messages relating to tanning. Some participants in each of the three groups also performed a self-assessment of their own skin cancer risk as an additional persuasive experience. The results generally favored the non-narrative informational message. For instance, participants who viewed the informational message reported less tanning behavior in a follow-up telephone survey. These participants also indicated greater perceived susceptibility to skin cancer, and they rated the message as being more informative.

Sherer and Rogers (1984) reported a study in which university undergraduates read messages relating to alcohol abuse. Although several factors were manipulated, of interest in this review is the treatment the authors referred to as concreteness. In one condition, participants read case studies of two problem drinkers. In another condition, participants read a statistical presentation. Participants who read the case study message demonstrated better recall for the message information and an increased belief in the noxiousness of problem drinking, but there was no difference in intentions to change alcohol consumption.

Rook (1987) reported three studies in which participants read information about health-related issues. The first study will not be reviewed here because it involved the economic benefits of generic medications, rather than specific safety or health related behaviors. The other two studies involved the prevention of osteoporosis, and these will be discussed. In Study 2, some participants read an abstract version of information on osteoporosis in which the facts were presented as they apply to people in general. Other participants read a case history in which one person's experiences were described. Participants included a broad sampling of women, and it was found that case history information had a greater impact on health attitudes and behavioral intentions, compared with the abstract message.

In Study 3, Rook's participants were women who were known to be at high risk of osteoporosis. This time, the case history message was more persuasive than the abstract message *only* among women who originally reported less concern about the threat of osteoporosis. Among these women, the case history message was also associated with better memory for the health recommendations in a follow-up survey six weeks later. On the basis of these findings, Rook argued that case history information is most persuasive when applied to risks that are perceived as distant, and no differences should be expected when risks seem imminent. A possible explanation for this effect is that women who perceived the risk as imminent were already concerned and were thus willing to attend to any preventive recommendations (abstract or story-based). In contrast, women who perceived the risk as distant became interested in preventive recommendations only after the story-based message aroused their concern.

Rook (1986) reported on a replication of Rook (1987). (The replication was published a year before publication of the original study.) In Rook (1986), women read information about the prevention of osteoporosis. Again, one group read information presented as it relates to all women (the abstract version), and the other group read information presented in the form of one woman's experiences. Compared with the abstract message, the case history message led to positive changes in attitudes and self-reported intentions *only* among premenopausal women, for whom the threat of osteoporosis was distant. Interestingly, the authors found that all differences in outcomes between abstract and case history messages had disappeared after six weeks.

In a study reported by Cox and Cox (2001), female participants over the age of 50 read an advertisement recommending regular mammograms. Some participants read a story about a woman's experience with breast cancer. Other participants read an informational message based on statistical evidence. Cox and Cox found no significant differences between statistical and anecdotal messages in terms of self-reported attitudes or self-reported likelihood of having a mammogram. (As with most story-based health communication studies, only self-reported behaviors were examined.) Cox and Cox did find that evidence that narratives promoting preventive attitudes are more effective with a negative frame (e.g., a story of a woman who *died* of breast cancer because she did not get regular mammograms) rather than a positive frame (a story of a woman who was *saved* because her breast cancer was detected at an early stage).

### ***Interventions Using Film Narratives***

Some studies have examined the effectiveness of film narratives in safety and health communication. For instance, Limon and Kazoleas (2004), reported a study in which undergraduate university students watched a brief public service announcement regarding the risks of tanning. Some students saw the story of a young woman who was portrayed as dying of skin cancer. Other students viewed a message based on statistical arguments. The same introduction and conclusion were used for both films; the only difference was in the use of either the story or statistical arguments. A third group of students served as no-treatment controls. There were no differences in the persuasiveness of the narrative and statistical messages, but both of these messages had persuasive effects when compared with the no-treatment control condition.

In a study reported by, Cody and Lee (1990) participants watched a video about skin cancer risks and prevention. Some participants saw a video that focused on objective information, and others watched a video consisting of interviews with people who were suffering from malignant melanoma. Still other participants watched videos about an unrelated ailment (heart disease). It is difficult to interpret the results of the study because Cody and Lee performed a large number of statistical tests, apparently without controlling for experiment-wise Type I errors. Most of the comparisons were nonsignificant. However, the authors did report that that intentions to comply with skin

protection recommendations were more persistent over time for participants who watched the interview-based video.

Finally, Maibach and Flora (1993) evaluated an AIDS prevention film that included scenes of couples negotiating about safe sex, scenes of women convincing friends to use condoms or buying condoms at a pharmacy, and information about AIDS. The statistical presentation is quite difficult to interpret. However, it appears that compared with a purely informational video, the narrative film was associated with increases in self-efficacy, but not in self-reported behaviors. The effects of an additional scenario-based intervention will not be reviewed here.

### ***Studies in Which the Cover Story Involved Evaluating the Intervention***

The final group of studies in this review incorporated a cover story indicating the participants' main task was to evaluate the quality of persuasive messages. These studies have been separated from the rest of the literature because participants may have approached the tasks differently.

Slater and Rouner (1996) presented college students with alcohol education messages based on narrative arguments, statistical arguments, a combination of the two, or neither type of argument. The cover story indicated the messages were written by an undergraduate journalism major<sup>3</sup>, and participants were asked to evaluate them. The investigators found no differences in participants' self-reported health beliefs as indicated on pre- versus post-test measures.

Morman (2000) reported a study in which male university students read messages relating to testicular cancer. Participants were told they would be evaluating the messages, and they were asked to underline important passages. The study employed a 2 x 2 factorial design. One factor was message type (factual v narrative). The other factor was efficacy (high v low amount of information needed to perform testicular self examination). The factual message was based on information from the American Cancer

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<sup>3</sup> A large body of research on source credibility suggests that an alcohol education message purportedly written by an undergraduate journalism major would have to contain some *very* strong arguments in order to have a persuasive effect (see Tormala, Brinol, & Petty, 2007 for a review of source credibility effects).



Society and the National Institutes of Health. The narrative message included the experiences of a fictitious testicular cancer victim. There were no difference between fact-based and narrative-based messages in terms of attitudes and intentions toward testicular self examination.

In a study reported by Kazoleas (1993), college undergraduates read messages advocating safety belt use. The cover story indicated the messages were transcripts from a congressional forum, and participants were asked to evaluate their quality. Some messages used statistical arguments (such as safety belts reduce the risk of injury by 50 percent in a nonfatal vehicle accident). Other messages used qualitative evidence consisting of examples, anecdotes, and analogies. Despite the cover story, the author did examine the effects of the messages on participants' knowledge and attitudes. Participants who read messages containing qualitative evidence demonstrated greater recall of message information and more persistent attitude change, compared with participants who read messages based on statistical arguments.

Other researchers have asked participants to evaluate messages to determine whether there is a general preference or liking for story-based versus nonstory-based messages (e.g., Morgan, Cole, Struttman, & Piercy, 2002; Slater, Buller, Waters, Archibeque, & Leblanc, 2003). Since there appears to be little evidence that persuasion or behavioral compliance is related to how much one prefers a particular message, these studies will not be reviewed here.

### ***Summary of Results: Story- Versus Nonstory-based Communications***

Sixteen studies directly compared story- versus nonstory-based safety and health communications. The results can be summarized as follows (see also Tables 1 through 3):

1. No studies have observed *target behaviors* directly.
2. When other treatment activities and message channels were held constant, not a single study reported that story-based communications led to changes in any *self-reported behavior*, relative to non-story based communications.
3. In regard to measures of *self-reported behavioral intentions, memory* and *attitudes* the results indicate a virtual draw, although there may be a slight edge for story- over nonstory-based messages.

**Table 1 Story- Versus Nonstory-based Safety and Health Communications, Part 1**

<b>Study</b>	<b>Message Channel / Target Issue</b>	<b>Self-reported behavior</b>	<b>Self-reported intentions</b>	<b>Memory / knowledge</b>	<b>Attitudes/ beliefs</b>
Marty & McDermott (1985)	Mixed channels / Testicular cancer			Stories = facts	Stories > facts, but confounded with channel
Marty & McDermott (1986)	Mixed channels / Testicular cancer	Story = facts with channel constant		Facts > stories	Stories = facts
Larkey & Gonzalez (2007)	Live speaker / Colorectal cancer		Stories > risk assessment for some (not all) DVs		
Golding, Krinsky, & Plough (1992)	Printed stimuli / Radon risk	No conclusions were drawn		No conclusions were drawn	No conclusions were drawn
Greene & Brinn (2003)	Printed stimuli / Skin cancer	Statistics > stories = control	Stories = statistics > control		Statistics > stories > control
Sherer & Rogers (1984)	Printed stimuli / Alcohol abuse		Stories = statistics	Stories > statistics	Stories > statistics
Rook (1987) Study 2	Printed stimuli / Osteoporosis		Stories > abstract message on a combined persuasiveness score		

**Table 2 Story- Versus Nonstory-based Safety and Health Communications, Part 2**

<b>Study</b>	<b>Message Channel / Target Issue</b>	<b>Self-reported Behavior</b>	<b>Self-reported Intentions</b>	<b>Memory / Knowledge</b>	<b>Attitudes/ Beliefs</b>
Rook (1987) Study 3	Printed stimuli / Osteoporosis	Stories = abstract message	Equivalent main effect; significant interaction: Stories > abstract message on persuasiveness and recall for women with low (not high) prior concern		
Rook (1986)	Printed stimuli / Osteoporosis	Stories = abstract message	Equivalent main effect; significant interaction: Stories > abstract message on combined persuasiveness score for premenopausal (not postmenopausal) women Stories = abstract on recall		
Cox & Cox (2001)	Printed stimuli / Breast cancer		Equivalent main effect; significant interaction: Loss framed stories = loss framed statistics > gain framed stories		Equivalent main effect; significant interaction: Loss framed stories = loss framed stats = control > gain framed stories
Limon & Kazoleas (2004)	Film stimuli / Skin cancer				Stories = statistics > control

**Table 3 Story- Versus Nonstory-based Safety and Health Communications, Part 3**

<b>Study</b>	<b>Message Channel / Target Issue</b>	<b>Self-reported behavior</b>	<b>Self-reported intentions</b>	<b>Memory / knowledge</b>	<b>Attitudes/ beliefs</b>
Cody & Lee (1990)	Film stimuli / Skin cancer	Stories = facts = control	Stories = facts > control; Story effect more persistent	Facts > control Stories = control	Stories = facts = control
Maibach & Flora (1993)	Film stimuli / HIV/AIDS	Stories = factual information			Stories > factual information
<b>Studies Separated Because of Evaluation Cover Story:</b>					
Slater & Rouner, (1996)	Printed stimuli / Alcohol abuse				Stories = statistics
Morman (2000)	Printed stimuli / Testicular self examination		Stories = facts		Stories = facts
Kazoleas (1993)	Printed stimuli / Safety belt use			Stories > statistics & control	Stories = statistics > control; Story effect more persistent

This dissertation began with strong evidence from a variety of settings indicating that story-based communications have an extraordinary power to persuade audiences. It now seems that when pitted against valid alternative methods of communication, the practical effects of stories in safety and health communication disappear.

At this point, one might question whether it is worthwhile to continue studying the use of stories in safety and health communication. After all, the applied experimental research (when taken as a whole) has shown no advantage for stories. On the other hand, a number of observations suggest that story-based safety and health communications may yet prove to be effective.

First, there is abundant evidence from non-applied research attesting to the power of stories to change attitudes and affect decisions. Perhaps applied efforts simply need to be designed more effectively in order to demonstrate these same effects.

Second, an examination of stimuli used in the story-based safety and health communication literature suggests some of the messages have not been well crafted. Different results might be obtained with better stimuli.

Third, an examination of the dependent variables used in past research demonstrates that we have not yet examined the outcome that matters most, namely observed (not self-reported) changes in target behaviors (not surrogate behaviors). Until we learn whether story-based communications produce changes in actual safety and health behaviors, we have not studied the issue sufficiently.

Fourth, it is noteworthy that of the 16 studies that included a comparison between story-based and nonstory-based messages, 15 used stimuli related to health promotion. Only one study (Kazoleas, 1993) examined stimuli related to injury prevention. It could be argued that compared with illnesses, events surrounding injuries are more dramatic and often involve clearer connections between cause and effect—and this might result in stories that are more persuasive.

For instance, if an electrician in an attic grabs an electrical wire without first testing it and is electrocuted, it is clear that a specific behavior (grabbing a live wire) caused the death. It is also clear that this particular death would not have occurred if the worker had shut off and locked the circuit breaker and then tested the wire before touching it. In contrast, imagine a 65-year-old who dies of lung cancer after smoking for

40 years. Is it clear that smoking caused this person's lung cancer? Was there a history of cancer in the family? If smoking was the cause, would the cancer have been averted if the person had stopped smoking? If so, would it have been necessary for the person to quit 1 year before, 5 years before, 10 years before? Was there radon in the home? Did the person live in an air-polluted city? Had the person ever worked around asbestos? Was there exposure to pesticides? The links between cause and effect are often fuzzy when speculating about any particular person's health, and this may render stories less effective in health promotion, compared with injury prevention.

In order to address some of these unexamined issues, this dissertation will examine whether story-based communications produce changes in injury prevention behaviors, compared nonstory communications. So that target behaviors can be observed directly in an experimental setting, the main experiment will examine compliance with *product warnings*, using a research paradigm that has demonstrated behavioral effects in the past.

### **Research on Behavioral Compliance with Product Warnings**

In contrast to most other safety and health investigations, product warning research often involves direct observation of target behaviors in laboratory settings. Furthermore, although the setting is experimental, participants can be kept unaware of the real purpose of the study so that their behavior remains more or less natural. Wogalter and Dingus (1999) have described how product warning researchers use the *incidental exposure paradigm*, in which participants are given a cover story indicating that the purpose of the experiment is to perform some task. The experimenter does not explicitly mention product warnings, but warnings are included incidentally in the task instructions. In this way, researchers can measure compliance, but demand effects do not cause participants to pay undue attention to the warnings.

The chemistry laboratory task is perhaps the best known example of the incidental exposure paradigm in warning research (e.g., Wogalter et al., 1987). In the chemistry laboratory task, participants are given an assignment that involves mixing and weighing chemicals as quickly and accurately as possible. During the task, they encounter warning messages indicating, for instance, that they should wear chemically-resistant gloves.

Although participants believe their task is simply to mix and weight the chemicals, the dependent measure is actually whether or not they wore the gloves or followed other required precautions. The incidental exposure paradigm is an effective research strategy because the target behavior is directly observed in a controlled laboratory setting, and the participant is unaware that the experimenter is even interested in that behavior.

In contrast to applied field research, laboratory studies of warning compliance have achieved relatively consistent results by making large numbers of observations in highly controlled conditions. These characteristics suggest this paradigm could help resolve the conflicting results that have plagued story-based safety and health communication studies.

## **CHAPTER 2 - PILOT STUDIES**

Three pilot studies will be discussed briefly as they apply to the current dissertation. These preliminary studies were conducted to determine whether story-based injury prevention messages would affect participants' recall, beliefs, and behavioral intentions. It was reasoned that any changes on these self-report measures would suggest the types of messages to examine in later studies of actual behavior.

The research proposal for the pilot studies was approved by the Institutional Review Board at Kansas State University prior to the collection of data (proposal 3561, approved August 10, 2005).

### **Gasoline Warning Pilot Study**

The first pilot study was the simplest and perhaps the most successful. This study explored how stories affected participants' attention to a simulated gasoline pump warning sign. As will be noted, one reason for the success of this pilot study may be that (in contrast to the other pilot studies) participants seemed to be unaware that the safety message was the object of study.

#### ***Participants***

The participants were 36 undergraduate student volunteers at Kansas State University (11 males; 25 females). Participants received class credit for their time, and informed consent was obtained from all participants.

#### ***Stimuli***

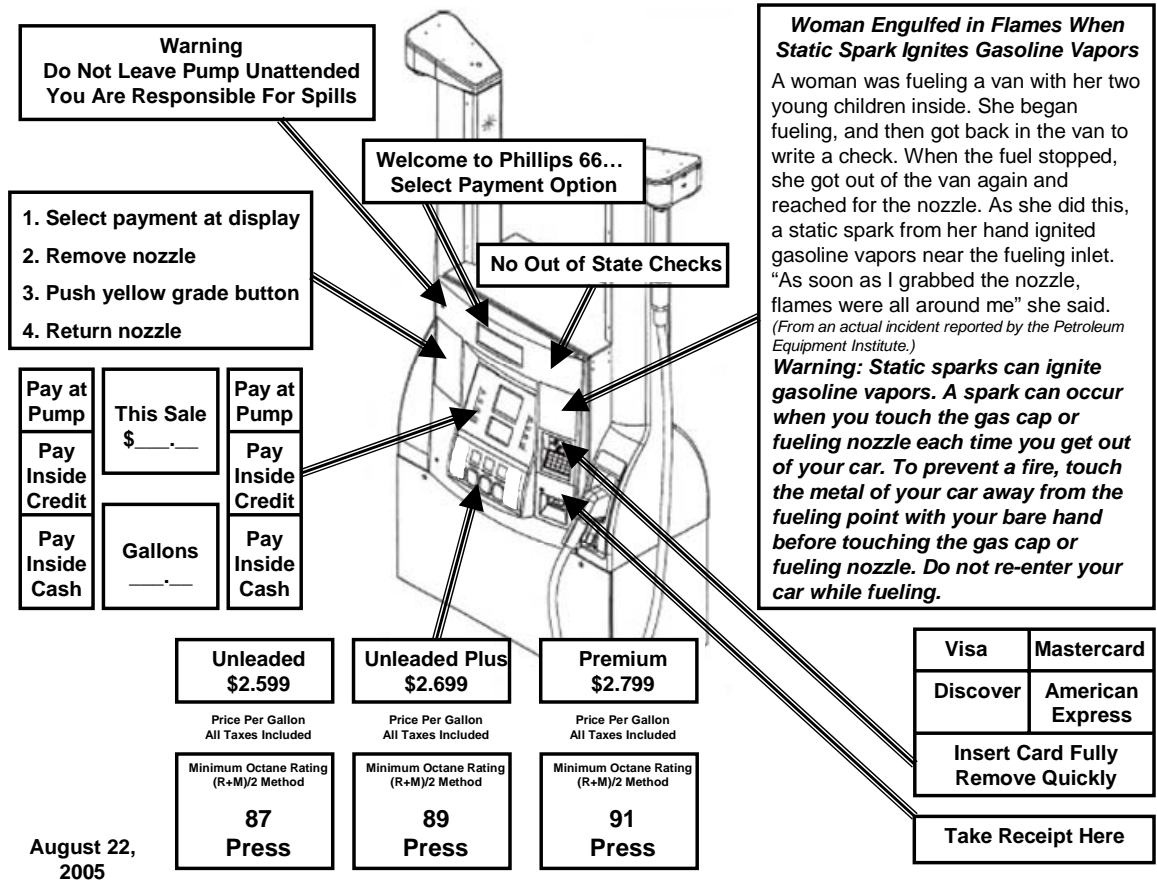
Each participant viewed a 12- x 9-in computerized image of a gasoline pump similar to image in Figure 1. The image contained prices, terms, a safety message, and other information based on observations at local service stations.

The safety message in each condition contained an experimental component, plus a common component. The common component was included to ensure that each condition contained enough information for participants to



understand the main causes, consequences, and preventive measures related to the hazard.

Figure 1 Example of Gasoline Warning Stimulus (Size Reduced)



The experimental components of the safety messages corresponded to the four experimental conditions: traditional abstract message, story-based message, concrete nonstory message, and statistical message. The text of these messages follows.

A traditional abstract message was taken word-for-word from a pump at a local service station on August 22, 2005:

*Static and Electrical Sparks.* Static and electrical sparks may cause gasoline vapors to explode. Do not get back in your vehicle while refueling. Reentry could cause a static electric spark. Discharge potential static electric buildup by touching car metal away from the fueling point with your bare hand before touching the fueling nozzle.

A story-based message described the true experience of a woman and her family who survived a gasoline fire:

*Woman Engulfed in Flames When Static Spark Ignites Gasoline Vapors.* A woman was fueling a van with her two young children inside. She began fueling, and then got back in the van to write a check. When the fuel stopped, she got out of the van again and reached for the nozzle. As she did this, a static spark from her hand ignited gasoline vapors near the fueling inlet. “As soon as I grabbed the nozzle, flames were all around me,” she said. (From an actual incident reported by the Petroleum Equipment Institute)

A concrete nonstory message provided the same information as the traditional message, but the text was less abstract:

*Static Sparks Can Ignite Gasoline Vapors.* Static electricity can cause a spark when you touch your car. The spark may ignite gasoline vapors near the car’s gas cap. There can be a spark every time you get out of your car. You can protect yourself. Touch the metal of your car away from the gasoline inlet, where there are no fuel vapors. Do not get back in your car until you have finished fueling.

A statistical message included a statement about the number of gasoline pump fires that occur each year:

*Static Sparks Can Ignite Gasoline Vapors.* Static sparks cause about 100 fires and explosions at gasoline stations every year. There can be a spark each time you get out of your car. The spark may ignite gasoline vapors near the car’s gas cap. You can protect yourself. Touch the metal of your car away from the gas cap, where there are no fuel vapors. Do not get back in your car until you have finished fueling.

The story-based, concrete nonstory, and statistical messages were similar in number of words ( $M = 70.33$ ;  $SD = 2.52$ ); Flesch-Kincaid grade level ( $M = 5.37$ ;  $SD = 0.06$ ); passive sentences (0 percent for each condition); and Flesch reading ease ( $M = 78.50$ ;  $SD = 2.96$ ). The traditional abstract message was intended to be representative of messages that exist on actual gasoline pumps. The readability statistics for the traditional

abstract warning were as follows: 49 words; 9.6 Flesch-Kincaid grade level; 0% passive sentences; and 47.6 Flesch reading ease.

### ***Procedure***

Participants were randomly assigned to one of the four message conditions and viewed only one image. Each participant completed the study individually. The experimenter provided the following instructions:

I will show you a drawing of a gasoline pump. I would like you to imagine that you are looking at the pump at a filling station. Look at the pump as if you were actually getting ready to use it. As soon as you are ready to pump the gasoline, I want you to say, "I'm ready."

The image of the pump was then shown. As soon as the participant indicated he/she was ready to pump the gasoline, the image was removed and the participant engaged in a separate reading activity for about 30 minutes. The separate reading activity is described below in the construction safety pilot study. After finishing the separate activity, each participant completed a series of questionnaires regarding proper procedures for using gasoline pumps, as well as questionnaires described in the construction pilot study.

### ***Results and Discussion***

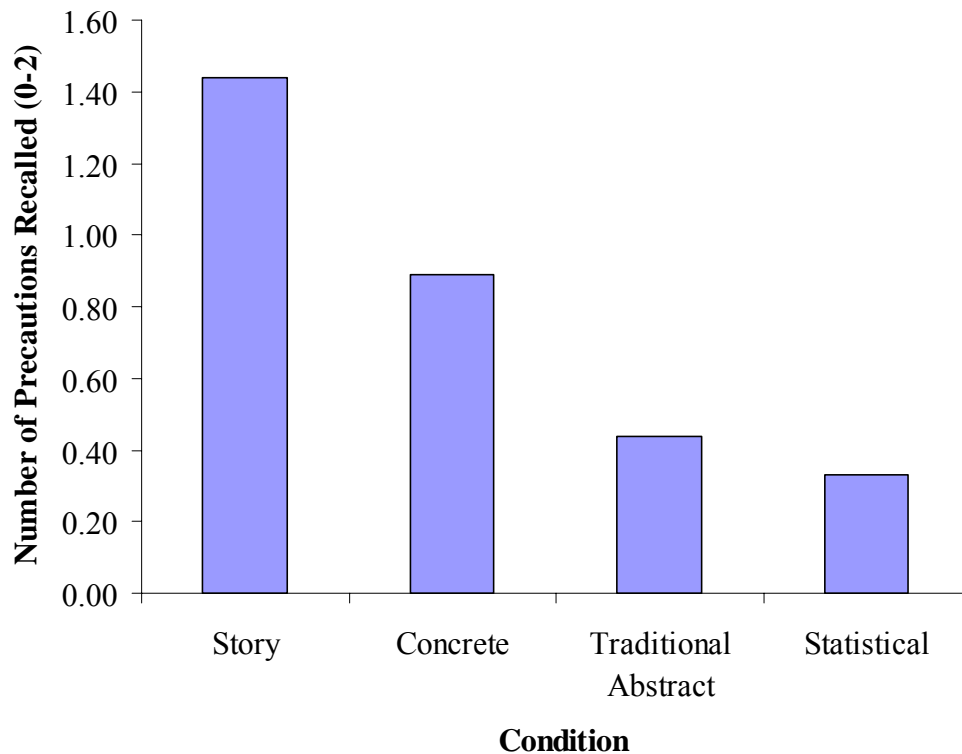
For one questionnaire item, participants described how they could protect themselves from fires at service stations. The answers were scored according to whether participants reported the two precautions that were listed in each version of the safety message. A total of two points was possible. The scoring key is included in Appendix A. One point was awarded if the answer stated to touch the vehicle away from the fueling point before fueling. A separate point was awarded for stating to not re-enter the vehicle during fueling. Each participant's response form was scored on two occasions approximately one week apart, and any discrepancies in scores were resolved prior to analysis. The experimenter was blind to the experimental condition while scoring all responses.

A one-way analysis of variance (ANOVA) demonstrated the message conditions had a significant effect on recall of the two precautions,  $F(3,32) = 3.76, p < .02$ . Partial

eta squared indicated a large effect size, with slightly more than one-fourth of the variation in recall of precautions accounted for by variation in the message conditions ( $\eta^2_{\text{partial}} = 0.27$ ).

The Tukey-a (HSD) procedure indicated the story-based message led to greater recall than either the traditional abstract or the statistical messages,  $p < .05$ . Although the concrete nonstory warning appeared to be the second-most effective treatment, there were no statistically significant differences between it and any of the other conditions. The results are illustrated in Figure 2.

**Figure 2 Mean number of precautions recalled**



Besides memory for warning information, this study examined participants' stated intentions to comply with the two precautions. Participants rated the likelihood they would observe the precautions whenever they filled a vehicle with gasoline in the future. Ratings were made on a 0-10 scale, where 0 indicated the participant would never take the precautions, and 10 indicated the participant would always take the precautions. The

average rating for participants who read the story warning was 6.22 ( $SD = 3.83$ ), while the average ratings for participants in the other conditions ranged from (4.22 to 4.33;  $SD = 2.65$  to 3.31). Although the results were in the expected direction, the differences were not significant (possibly due to the small number of participants and the relatively large within group variances).

Two other findings were also nonsignificant, but differed in that they were *not* in the expected direction. First, participants in the story-based condition did not rate gasoline fires as easier to imagine than participants in the other conditions. Second, when asked to estimate how many people are injured in fires at gasoline pumps each year, participants in the story-based condition made the lowest average estimates.

To sum up, this pilot study indicated that participants were more likely to recall information from a message that contained a vivid anecdote describing the experience of a single accident victim. Story-based messages did not lead to statistically significant increases in intentions to comply with the warnings, but the differences were in the expected direction. Nonsignificant trends also suggested the concrete nonstory message may have been nearly as persuasive as the story-based message.

### **Construction Safety Message Pilot Study**

The second pilot study involved the same 36 undergraduate students described above. After viewing the gasoline pump, participants viewed four different safety messages involving hazards in the construction industry. In this study, the stimuli were not consumer warnings, but brief messages similar to mass-media flyers or to handouts used in safety training. This pilot study is being described separately because of differences in the nature of the task and because participants commented that they approached the construction safety materials differently than the gasoline pump warning.

#### ***Participants***

The participants were the same as in the gasoline warning study.

#### ***Stimuli***

Sixteen different safety messages were developed, involving four different construction hazards and four different message conditions combined factorially. The

four construction hazards involved electrical power lines, nail guns, dump trucks, and tractors. The four message conditions were story-based messages, concrete nonstory messages, statistical messages, and visually illustrated messages. The three verbal messages were comparable in terms of Microsoft Word Readability Statistics (see Table 4). The visually illustrated messages contained a brief text that was comparable with the verbal messages in all respects except number of words.

**Table 4 Readability of Construction Safety Messages**

Message Type		Number of Words	Flesch-Kincaid Grade Level	Percent Passive Sentences	Flesch Reading Ease
Story	<i>M</i>	138.50	5.48	17.25	79.02
	<i>SD</i>	20.21	0.33	7.09	2.73
Concrete Nonstory	<i>M</i>	137.00	5.45	12.25	75.30
	<i>SD</i>	21.83	0.37	5.68	3.18
Statistical	<i>M</i>	142.50	5.50	20.00	75.35
	<i>SD</i>	17.41	0.37	7.16	3.58
Visually Illustrated	<i>M</i>	43.75	5.25	11.00	73.52
	<i>SD</i>	14.48	0.24	13.61	12.25

For each of the four hazards, every condition contained a common message to ensure comparability of information. For example, the common message for the power line hazard was as follows:

Avoid electrocution! Take these precautions whenever you work outdoors:

1. Look up before you start. Survey the site for power lines.
2. Remember that most power lines are not insulated, and most objects will conduct electricity.

3. Keep yourself and all materials at least 10 feet away from overhead power lines. Be especially careful with long objects such as ladders, pipes, TV antennas, etc.
4. If you must work within the 10 foot danger zone, call the utility company and ask if they will shut off the power.
5. If the power lines can not be shut down, ask the utility company if they will install insulation over the lines.
6. If the lines can not be shut down or insulated:
  - a. Have a meeting with everyone who is helping with the project. Discuss how you will keep people, tools, and materials out of the 10 foot danger zone.
  - b. Have someone guide the movement of equipment and materials whenever there is a risk of getting into the 10 foot danger zone.

The story-based message contained the common text plus the following account of a workplace death:

Five workers were building a chain link fence in front of a house. There was an overhead power line directly above the fence. One of the workers picked up a section of metal fence railing that was about 20 feet long. He lifted up the railing and stood it on end. When he did this, the railing touched the power line above his head. He cried out as he was being electrocuted. His friends tried to revive him, but his injuries were too severe. He died at the scene. This tragedy could have been prevented if the workers had kept themselves and their materials at least 10 feet from the power line at all times. (*From OSHA Accident Investigation Summary 40.*)

The concrete, nonstory condition contained the common text plus the following description of the hazard (the reference to an OSHA publication is fictitious, but was included to provide a source comparable to that used in the story-based message):

Watch out for overhead power lines when you work outdoors. Look above and around you. Be very careful if you are handling ladders, pipes, and other long objects. You can be killed if you touch a power line with something made of metal or wood. Keep yourself and all long objects at

least 10 feet away from power lines at all times. Power lines carry high voltage electricity. It can burn you severely and catch your clothing on fire. It can kill you by stopping your heart. Doctors may not be able to revive you. Never touch a power line. Keep all tools and objects at least 10 feet away. (*From OSHA Publication: Overhead Power Line Tips for Construction Workers.*) [This is a fictitious publication.]

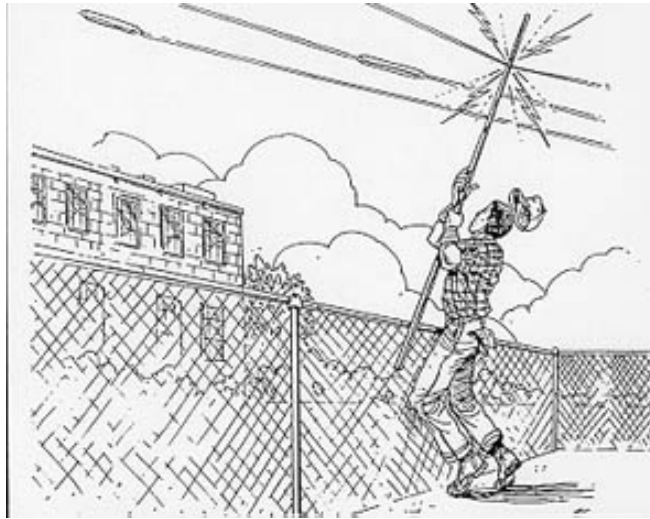
The statistical condition contained the common text, a statement about the number of electrocutions that occur due to power lines each year, and a concrete description of the hazard:

Over 100 people are electrocuted by overhead power lines in the U.S. each year. This is about the same as the number killed in floods. Watch out for power lines when you work outdoors. Be careful with ladders, pipes, and other long objects. You can be killed if you touch a power line with something made of metal or wood. Keep yourself and all long objects at least 10 feet away from power lines at all times. Power lines carry high voltage electricity. It can burn you severely and catch your clothing on fire. It can kill you by stopping your heart. Doctors may not be able to revive you. Never touch a power line. Keep all tools and objects at least 10 feet away. (*From OSHA Publication: Overhead Power Line Tips for Construction Workers.*) [This is a fictitious publication.]

Finally, the visually illustrated condition included the common text, a line drawing illustrating the hazard (see Figure 3), and a brief textual message:



**Figure 3 Visual Illustration of Power Line Hazard (From OSHA, n.d.-c)**



Watch out for power lines when you work outdoors. Be very careful with ladders, pipes, and other long objects. You can be killed if you touch a power line with something made of metal or wood.

### ***Procedure***

Each participant saw four safety messages representing the four different experimental manipulations. In this counterbalanced design, each participant saw (1) a story-based message about one construction hazard, (2) a concrete nonstory message about a second construction hazard, (3) a statistical message about a third construction hazard, and (4) a visually-illustrated message about a fourth construction hazard.

Each participant viewed four computerized images (Power Point slides) corresponding to the message conditions described above. Prior to viewing the messages, participants were told that they would be looking at safety information for workers who build houses. Participants were instructed to imagine that they were starting a new job building houses, and the information was being provided to help them stay safe on the job. Participants were told to read the four slides as if they were actually being trained for a new job. They were shown how to use the arrow keys to move from one slide to another. Finally, participants were instructed to tell the experimenter when they were finished.

As soon as each participant indicated s/he was finished, the images were removed and the participant completed a series of questionnaires. The gasoline questionnaires were completed first (see gasoline warning pilot study), followed by the construction warning questionnaires. Appendix A includes the scoring procedures for recall of safety information.

### ***Results and Discussion***

This study resulted in one significant finding. Participants were asked to think back over the gasoline warning and the four construction messages and indicate which of the five messages made the biggest impression on them. Although each participant viewed safety messages under five different counterbalanced conditions (story, concrete nonstory, statistical, traditional, and visually illustrated), almost half of all participants (17 out of 36) reported that a message presented in the story version made the biggest impression on them. This difference was significant,  $\chi^2(4) = 10.92, p < .05$ .

Of those who identified a story-based message as having the biggest impact, 29 percent referenced the gasoline fire, while 71 percent referenced one of the four construction anecdotes. Of those who identified one of the nonstory messages as having the biggest impact, 15 percent referenced the gasoline hazard, while 85 percent referenced one of the four construction hazards. Although the difference in proportions is not significant for a two-tailed test ( $z = 1.66, p = .097$ ), it does approach significance and suggests the gasoline story may have been more effective than the construction stories.

Conversations with participants suggested two reasons why the gasoline story may have been more effective. First, some participants indicated the construction stories were less personally relevant than the gasoline story—almost all participants had fueled vehicles many times, but few had any experience with construction activities. Second, there seemed to be an element of surprise associated with the gasoline story. Participants were not expecting that safety information would be the focus of the gasoline study. They also were not expecting to see the story of an actual victim conveyed in a gasoline warning. Furthermore, the central event of the story (a fire due to a static spark) seemed surprising to many participants. In contrast, the instructions for the construction messages

clearly indicated that participants would be viewing safety information, so they were prepared in advance to think about safety, and the stories were not as surprising.

Nonsignificant trends suggested the story-based and concrete nonstory messages were somewhat more effective than other messages. Participants in the story-based and concrete nonstory conditions reported (nonsignificantly) that (1) it was easier to imagine someone being injured by the hazard and (2) participants were more likely to take the precautions if they were to work in the construction industry. Participants in the concrete nonstory condition reported (nonsignificantly) the highest estimates of injuries related to the hazards, but participants in the story-based condition reported the *lowest* estimates.

### **Mower Safety Message Pilot Study**

Trends in the previous studies suggested the story-based and concrete nonstory conditions had the greatest impact on participants. Therefore, the mower safety study examined only story-based and concrete nonstory messages.

The design of the study was a 2 x 2 factorial. One factor was type of message (story v concrete nonstory). The second factor involved the opportunity for participants to relate reminders (opportunity to relate their own stories v no opportunity to relate stories). For the reminders factor, half of the participants reading each type of message were encouraged to immediately write down any stories about mower injuries from their past as the stories came to mind. This manipulation was based on Schank's (1990) notion that people stop paying attention to a message until they have a chance to tell their own stories. On the basis of Schank's theory, it was anticipated that participants would be better able to process the safety messages if they had an opportunity to record their own stories immediately.

This pilot study also differed from previous pilot studies in that participants completed a follow-up survey after approximately one month. The follow-up was based on Green and Brock's (2005) notion that story-based messages have a more lasting impact than nonstory communications.

### *Participants*

The participants were 23 undergraduate student volunteers at Kansas State University (8 males; 15 females). Participants received class credit for their time. Informed consent was obtained from all participants.

### *Stimuli*

Eight different safety messages were developed, addressing four different hazards related to mowing (i.e., rotating blades, fuel fires, lightning, and flying objects). Four of the messages (one involving each hazard) contained stories of workers who were injured. The other four messages (one involving each hazard) described the hazards and preventive measures in simple, concrete terms. Messages were made equivalent in terms of content and Microsoft Word Readability Statistics (see Table 5).

**Table 5 Readability of Mowing Safety Messages**

Message Type		Number of Words	Flesch-Kincaid Grade Level	Percent Passive Sentences	Flesch Reading Ease
Story	<i>M</i>	117.50	5.35	23.25	78.40
	<i>SD</i>	23.36	0.81	10.84	6.37
Concrete Nonstory	<i>M</i>	117.00	5.28	20.25	78.60
	<i>SD</i>	20.93	0.68	8.02	3.60

For each hazard, there was a common safety message to ensure comparability of information. For instance, the common message for the lightning hazard was as follows:

Lightning can strike 10 miles or more from any rainfall. In fact, most people who are hit are not even in the rain. You are in danger if you can see lightning or hear thunder. Protect yourself:

1. Stop mowing and get to shelter if you see lightning or hear thunder.
2. Get inside a sturdy building or hard-top vehicle with windows closed.

3. Don't start mowing again until at least 30 minutes after the last lightning flash. Many people are killed when they go outdoors soon too after the storm has passed.

The story-based message for lightning included the common message plus the following story:

A worker was mowing grass near a wooded area. He was a young man with a wife and two children. This was the first morning on his new job with a local landscaping company. As he worked, the sky became cloudy and dark. A light rain began to fall. There were distant flashes of lightning and far-off rumblings of thunder. Since the lightning seemed far away, he kept working. Without warning, a bolt of lightning struck a nearby tree, entered the worker's chest and exited through his feet. He was killed instantly, less than two hours after he began his new job. *From OSHA Accident Report ID 0418600*

The concrete nonstory message for lightening included the common message plus the following vivid description of the hazard and its consequences:

Never mow when you can see lightning or hear thunder. Be alert if you notice dark cloud bases and increasing winds. Head to safety if you hear thunder. Don't wait for the first flash of lightning. The first flash might be the one that kills you. Lightning strikes with great power. A bolt of lightning reaches a temperature of about 50,000° F. It can pass through your body and kill you instantly. If you aren't killed, you may be permanently disabled. Never take chances with your life when a thunderstorm is in the area. *From OSHA Publication: Mowing Safety*  
[This is a fictitious publication.]

In the mowing safety study, all messages were presented in booklet form instead of on a computer screen. Each booklet contained these instructions:

On the following pages, you will find safety information for workers who mow grass. Imagine that you are starting a new job for a landscaping company. You will spend a lot of time mowing grass. This safety information is being provided to help you stay safe on the job. Please read

the information carefully so that you will know how to protect yourself. Spend as much time reading and reviewing the information as you think you would if you were actually being trained for a new job.

Four booklets were prepared, corresponding to the four experimental conditions. Two booklets contained the story-based messages, and two contained the concrete nonstory messages. In addition, one of the story-based booklets and one of the concrete nonstory booklets included the following instructions: “If any experiences or stories related to mowing come to mind while you are reading, describe them briefly at the bottom of the page you are reading.” One point was awarded for each separate story if that story involved a mowing injury and included both of the following: a specific character (e.g., “me”, “my mom”, “a neighbor”, “a guy”) and a specific event that occurred at a particular point in time (e.g., “was hit by a rock”, “reached under the mower and was cut by the blade”). No points were awarded for generalized accounts that did not include a specific character and an event at a particular point in time, such as “I have heard about people getting hit by lightening when they mow.” Each participant’s response was scored on two occasions about one week apart, and no discrepancies in scoring occurred. The experimenter was blind to the experimental condition while scoring the responses.

### *Procedure*

Participants were randomly assigned to conditions, and each participant completed the study individually. The experimenter delivered oral instructions similar to those printed in the corresponding booklet, and each participant read a single booklet. Participants were instructed to tell the experimenter when they were finished.

As soon as each participant indicated he/she was finished, the booklet was removed and the participant completed a series of questionnaires relating to the safety information. Approximately one month later, the experimenter contacted each participant by e-mail with some follow-up questions.

Appendix A includes the scoring procedures for recall of safety information.

## ***Results and Discussion***

Only two participants recorded stories of mowing injuries. Therefore, the factor of reminders was dropped from all analyses. The resulting single-factor design demonstrated no significant differences between story-based versus concrete nonstory safety messages in regard to memory, intentions to comply with the warnings, estimates of the likelihood of injuries, or reported ease with which injuries could be imagined.

Nonsignificant trends tended to favor story-based messages in regard to recall of message information, while concrete nonstory messages tended to have a greater impact on self-reported attitudes and judgments.

The largest nonsignificant trend favoring story-based messages was observed in the follow-up data. Although the story and concrete nonstory conditions resulted in almost identical immediate recall of safety recommendations, at the one month follow-up participants in the story condition recalled an average of 6.09 ( $SD = 2.46$ ) safety recommendation, while participants in the concrete nonstory condition recalled an average of 5.51 ( $SD = 0.91$ ) safety recommendations. To put this in perspective, 12 recommendations could potentially have been recalled.

The largest nonsignificant trend favoring concrete nonstory messages was associated with how easily participants could imagine someone being injured while mowing. This variable was measured using a 0 - 10 scale, with 0 meaning “impossible to imagine,” and 10 meaning “you imagined it clearly and easily.” The average score of participants in the concrete nonstory condition was 8.89 ( $SD = 2.42$ ), and the average score for participants in the story-based condition was 6.60 ( $SD = 2.63$ ). Other nonsignificant trends also favored the concrete nonstory condition. For instance, there were nonsignificant differences suggesting a slight edge for the concrete nonstory condition in intentions to comply with the safety recommendations. Participants in the nonstory condition also tended to report a (nonsignificantly) higher likelihood of injury from the hazards.

This pilot study dispensed with conditions that had previously shown little promise (i.e., traditional abstract, statistical, and visually illustrated). The main question of interest was whether condition (story v concrete nonstory) would interact with

reminders (immediate opportunity to record versus no opportunity to record). Due to the small number of reminders reported, no conclusions could be drawn.

### **Discussion of the Pilot Studies**

The small number of participants resulted in low statistical power in each of the pilot studies. Most effect sizes ranged from about  $d = 0.40$  to  $d = 1.00$ . Power analyses were conducted based on procedures described by Cohen (1988). The highest power estimate occurred for the dependent variable of memory for precautions in the gasoline warning study. Even in this best case, the power estimate was quite low:  $d = 1.10$ , power = 0.44 for  $\alpha = .05$ .

Another limitation was that each experimental task was merely a mental exercise with no potential to identify how participants would actually behave in situations described in the messages.

The main significant finding was superior recall of safety recommendations for participants who viewed the story-based safety message in the gasoline pump study. Participants also indicated the story-based messages had a greater subjective impact than nonstory messages.

A number of nonsignificant trends were noted in all three studies. As expected, the story- and concrete nonstory conditions tended to result in the (nonsignificantly) highest self-reported intentions to comply with safety recommendations.

Other nonsignificant trends were surprising. For instance, in all three studies, participants in the story-based conditions reported a (nonsignificantly) *lower* probability of injury from the hazards, compared with participants in other conditions. This trend conflicts with a large body of research reported earlier indicating that anecdotes normally lead to exaggerated judgments of risk.

It was also surprising that in two studies participants who read story-based messages reported that it was (nonsignificantly) more difficult to imagine the injuries actually occurring. This trend seems surprising in light of the Transportation-Imagery Model, according to which the power of stories lies in their ability to make events in the text seem real (see p. 12 of the *Introduction*).



The pilot studies were designed mainly to establish direction for future efforts, and this was accomplished. The pilot studies did provide some evidence that story-based and concrete nonstory messages deserve further study as alternatives to traditional abstract safety messages. Additionally, the superior results for the gasoline pilot study suggested that research involving product warnings might be more fruitful than studies in which participants actively study messages in anticipation of having to recall them later.

## **CHAPTER 3 - EXPERIMENT**

### **Overview**

In order to examine the effects of stories and concrete nonstory information on observed safety behavior, participants assembled a product (a small swing set) using written instructions. The experiment was based on the incidental exposure paradigm, with safety messages embedded in the assembly instructions. Observed compliance with safety messages was the main dependent variable. By directly measuring the target behavior in an experimental setting, we avoided reliance on self-reported behaviors and other surrogate field measures of message effectiveness.

Due to the size of the components, teams of two participants were required to assemble the swing set. Thus, team (rather than individual) behavior was examined.

Finally, a number of important covariates were identified and included in statistical analyses. These covariates were age, gender, childcare experience, equipment assembly experience, presence of observer, and a final covariate related to timing of experimental sessions conducted by different experimenters.

### **Purpose**

The main purpose of this experiment is to determine if safety messages that include stories or concrete nonstory descriptions of injuries result in superior behavioral compliance when compared with traditional abstract safety messages. With this in mind, the main research question was as follows:

1. What is the effect on behavioral compliance when stories and concrete nonstory details about injuries are added to traditional abstract safety messages?

A second purpose of this experiment was to examine some psychological effects that may be related to increased compliance with safety messages. Specifically, the following research questions were examined:

2. What is the relationship between behavioral compliance and the self-reported experience of narrative transportation?

3. What is the relationship between behavioral compliance and reminders, i.e., recall of relevant stories from participants' past?
4. What is the relationship between behavioral compliance and participants' ability to provide describe concrete details related to the safety messages?
5. What is the relationship between behavioral compliance and participants' estimates of the likelihood of swing set injuries?
6. What are the effects on participants' ratings of product safety when stories and concrete nonstory details about injuries are added to traditional abstract safety messages?

## **Method**

### *Power analysis*

The experimental procedure was labor intensive. Each observational unit consisted of a team of two participants working for approximately 90 minutes at the experimental task and the associated questionnaire. Furthermore, follow-up surveys required an additional 30 minutes per participant. Since the experiment explored interventions that have a potential to save lives, it would have been ideal to design the study to identify the smallest possible effect. Unfortunately, this was not possible given the time and number of participants that would be required. Therefore, data from the pilot studies and other sources were used as the basis to estimate the minimum number of participants required for a reasonable test of the hypotheses.

Data from the pilot studies indicated that effect sizes ranging from about  $d = 0.40$  to  $d = 1.00$  could be expected in regard to measures of knowledge, probability judgments, and behavioral intentions. Due to the lack of other data, these effect sizes were used as the predicted effect size for behavioral compliance.

The power analysis was based on procedures described by Cohen (1988) for "pattern 3" ANOVA. In this pattern, two of the group means are located near one end of the distribution and one mean lies near the other end. This was the expected pattern in the present study because it was anticipated that the mean for the traditional abstract message

condition would lie near the lower end of the distribution, and the means for the other two conditions would lie near the upper end of the distribution.

Using these procedures at  $d = 0.40$ , about 94 teams per condition (282 total teams) would be required to achieve a power of 0.80. About 120 per condition (360 total teams) would be required for a power of 0.90. These numbers were not feasible, given the resources and time that were available.

In order to balance the need for power with the constraints of available resources, it was determined that 18 teams per condition would be recruited (54 total teams). Based on *a priori* power analysis, it was anticipated that the 54 total teams would result in a power of 0.21 for  $d = 0.40$  and a power of 0.85 for  $d = 1.00$ .

## ***Participants***

### ***Issues Relating to the Selection of Participants***

The swing set that participants would assemble was most easily handled by a team of two people. Therefore, the experiment was designed to investigate compliance with safety messages in dyads. Team compliance with safety and health communication is relevant because many everyday tasks require the cooperation of two or more persons.

In order to ensure that at least two participants were present, three volunteers were solicited for each experimental session. When all three participants were present, one person was given a silent observing task or an unrelated duty involving a separate experiment. Likewise, when only one participant was present, that individual participated in a different experiment. The other experiments will not be reported here, but involved compliance with safety messages during one-person assembly tasks such as assembling electrical equipment (e.g., a trouble light).

General psychology students may be considered a reasonable source of participants for this experiment. Many of these students have considerable experience at babysitting and supervising children at play. Some have even assembled play equipment. Furthermore, child supervision and assembly of play equipment are responsibilities that many of these students will undertake in the near future as they rear their own children or work in settings such as schools and child-care.

### ***Final Selection of Participants***

Participants included 55 teams (one team was excluded, see below), consisting of 145 student volunteers from general psychology classes at Kansas State University. Participants received class credit for participating. Informed consent was obtained from all participants.

One of the teams was excluded from analysis because team members were “tipped off” to the true purpose of the study by a previous participant. One of the experimenters witnessed the previous participant informing the incoming team that the study was really about observing the printed warnings, and they should read and follow the messages contained in each of the black boxes. After overhearing this conversation, the experimenter allowed the team to complete the experiment so they could receive class credit, but the data collection forms were set apart and not analyzed.

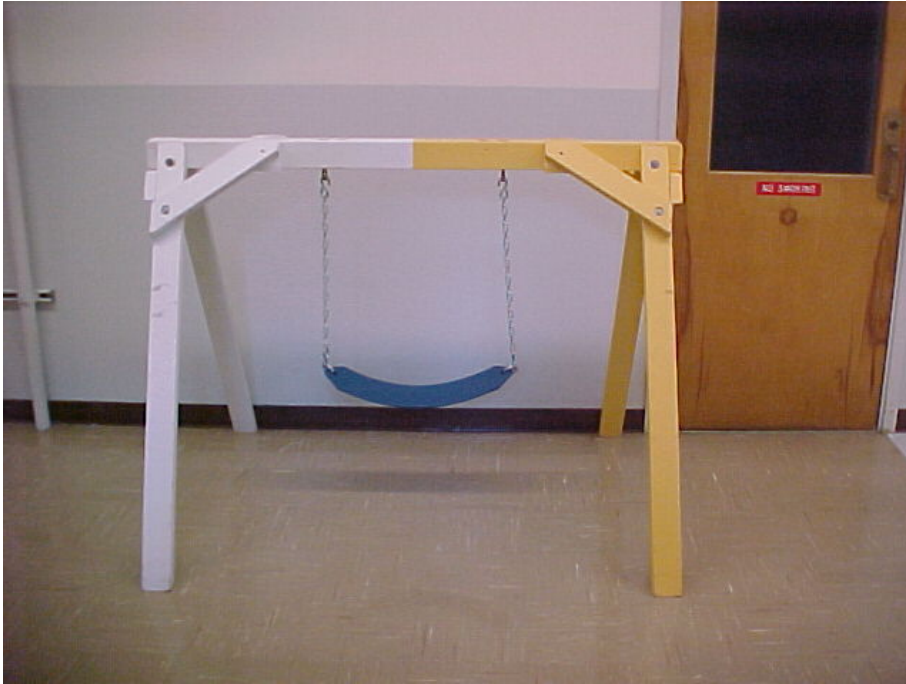
After excluding the team that was tipped to the purpose of the experiment, the remaining participants included 142 individuals comprising 54 teams, resulting in 18 teams assigned to each of the 3 conditions. By gender, participants included 44 males and 98 females. Mean age of participants was 20.01 years ( $SD = 3.53$ ). The distribution of childcare experience was highly skewed; the median response was 161.25 days providing childcare prior to the experiment. The median response for assembly experience was one item of play equipment assembled per participant prior to the experiment (this distribution was also highly skewed).

### ***Materials***

Stimuli consisted of swing set parts to be assembled, assembly tools, printed assembly instructions, and a questionnaire. The assembly task was designed to be of moderate difficulty and required the use of basic tools such as non-ratcheting box wrenches.

The assembly instructions contained a mixture of text and illustrations similar to instructions provided with actual consumer products. Copies of the assembly instructions and illustrations of the finished swing set and its component parts are contained in Appendices B, C, and D. Figures 4 through 12 illustrate the general design of the swing set and display some common hazards created by improper assembly.

**Figure 4 Swing Set Properly Constructed: About 4-ft. Tall, 6-ft. Long, 4-ft. Deep**



**Figure 5 Structural Components: Parts Pivot on Permanent Screws (Arrows)**



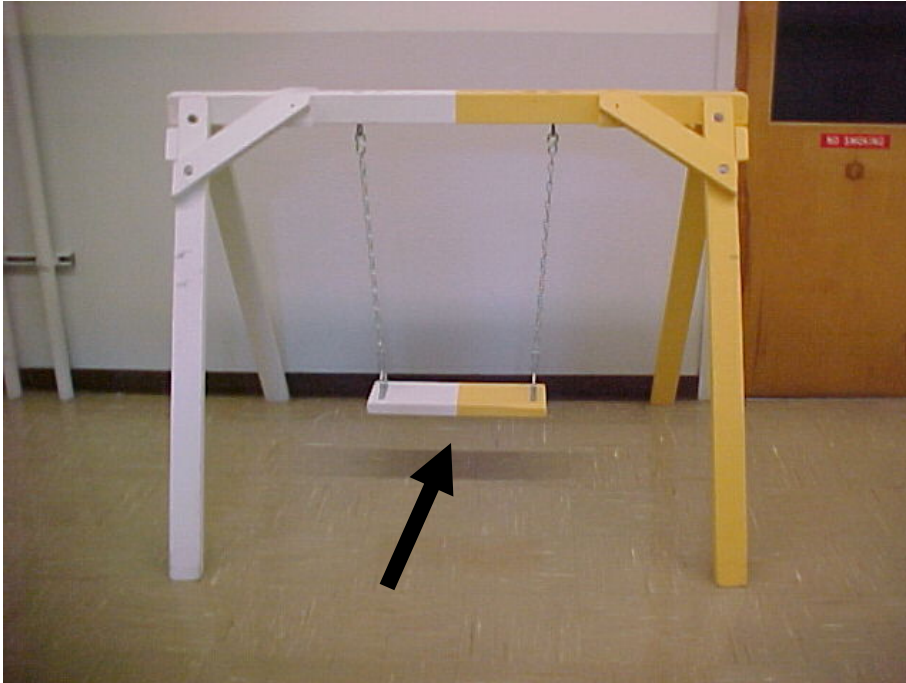
**Figure 6 Small Parts and Tools: See Appendix E For List of Assembly Materials**



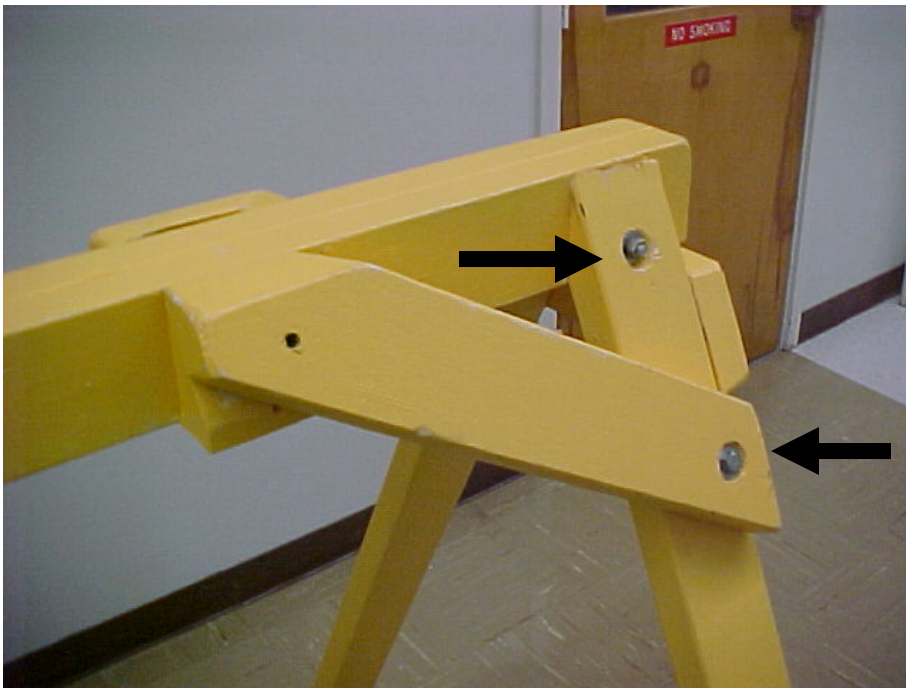
**Figure 7 Major Components Nest for Easy Transport and Storage**



**Figure 8 Swing Set With Hazardous Wooden Seat: See Assembly Instructions in Appendices for Explanation of the Hazard**

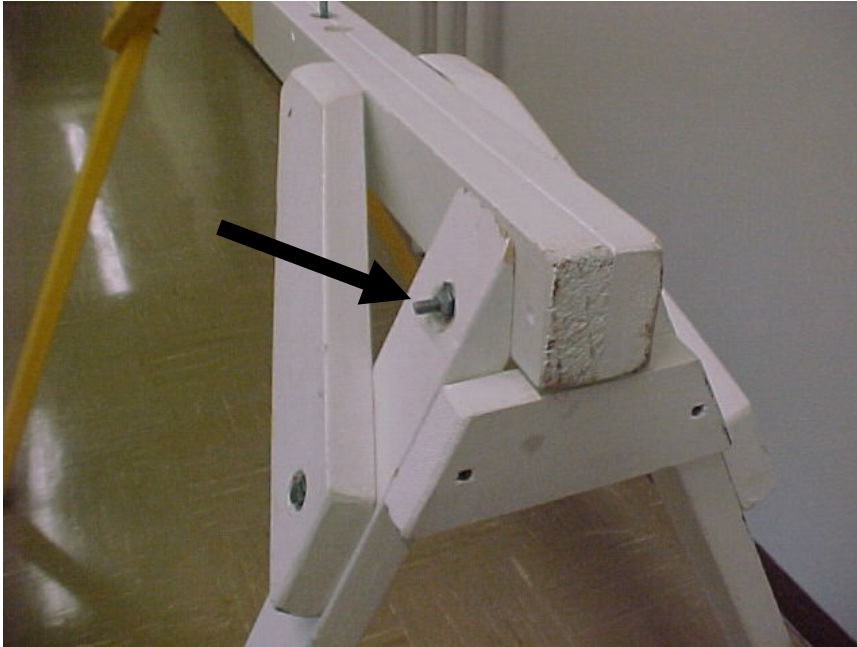


**Figure 9 Detail of End Assembly: Short Bolts Will Not Entangle a Child's Clothing**

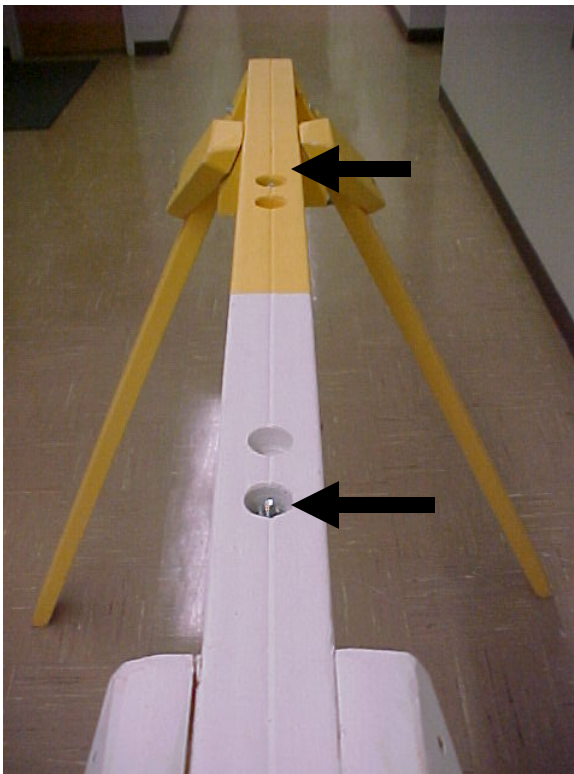




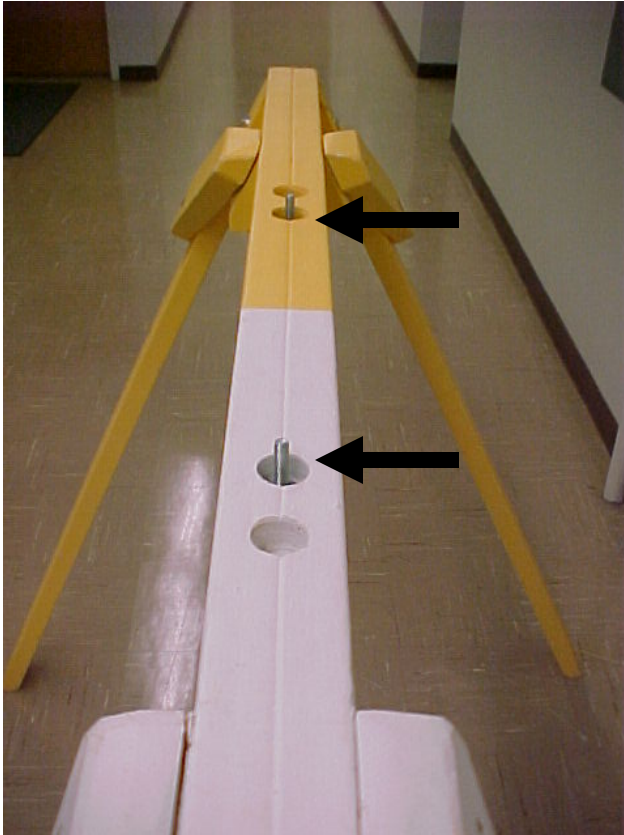
**Figure 10 Hazardous Long Bolt Is Capable of Entangling Clothing and Strangling a Child**



**Figure 11 Top Beam: Short Eyebolts Are Attached in Outer Holes to Reduce Side-to-side Movement of Swing**



**Figure 12 Top Beam With Hazardous Long Eyebolts Improperly Inserted in Inner Holes Will Allow Uncontrolled Sideways Movement of Swing**



In order to ensure realism and to disguise the intended purpose of the experiment, a number of safety messages were included at the beginning of the instructions that related to long-term use of the product, rather than its initial assembly. For instance, there were messages about periodic maintenance and rules for safe play. Without these messages regarding long-term use, it was believed that participants might quickly surmise that the real purpose of the experiment was related to compliance with safety messages during assembly.

Some important safety messages were contained in the text of the instructions, and others were included in explicit warnings that were set apart from the rest of the text. For instance, the following message was included in the text: “Important: do not attach more than one swing to this swing set.” Messages incorporated in the text were identical in each experimental condition and were included to determine if the explicit warnings

affected overall compliance with the instructions. Appendix G lists the safety messages that were included in each assembly manual, and describes how behavioral compliance was scored for analysis. Safety messages that were set apart from the rest of the text were manipulated according to the experimental conditions and will be described below.

Three different assembly manuals were used, corresponding to the three experimental conditions. The traditional assembly manual contained abstract warnings that are typical of assembly instructions for actual consumer products. These warnings contained terse commands, without explanations or examples of why the precautions are important. For instance, the following message warned participants about the dangers of using long (rather than short) bolts:

WARNING: Use short bolts that will not entangle children's clothing or necklaces. When tightened, the threaded end of the bolt should protrude no more than ¼ inch beyond the nut. If necessary, up to two (2) additional washers may be used as spacers to reduce the amount of thread that protrudes beyond the nut.

The story-based manual contained the same safety messages as the traditional abstract manual, with the addition of anecdotes describing how actual children had been hurt when warnings were ignored in the past. For instance, the abstract message about the hazards of long bolts (see above) was supplemented with the following story of a fatal injury:

Girl Strangled by Long Bolt on Swing Set. A 2-year-old girl was strangled when her necklace became caught on a long bolt while she was playing on her swing set. The girl's grandmother found her hanging by the neck from the frame of the swing with her necklace caught on the bolt. By the time her grandmother found her, the girl was limp and was not breathing.

Source: Chin, N., & Berns, S. (1995) Hanging Caused by a Toy Necklace. *Annals of Emergency Medicine*, 26, 522-525.

The concrete nonstory assembly manual contained the same safety messages as the traditional abstract manual, with the addition of descriptive text to provide explanations and concrete (but non-anecdotal) examples of how children might get hurt if the warnings were ignored. For instance, the abstract message about the hazards of long

bolts was supplemented with the following vivid nonstory description (note the fictitious reference similar to the actual reference used in the story-based condition):

Long Bolts on Swing Sets Can Strangle Children. A child can be strangled if her scarf, necklace, jacket drawstrings, and other clothing become caught on long bolts. Bolts should be short so they do not entangle clothing and result in accidental hanging. A child can die quickly when clothing tightens around her neck. By the time she is found, it may be too late to revive her. Source: Chin, N., & Berns, S. (1995). Toy Necklaces May Cause Hanging. *Annals of Emergency Medicine*, 26, 522-525. [This is a fictitious publication.]

The safety messages in the manuals were adapted from assembly manuals and consumer safety publications. The examples and anecdotes used in the story-based and concrete nonstory assembly manuals were adapted from reports described in the playground safety literature. Some details of the incidents were changed to balance the gender of the victims and to create narratives that were more coherent. For the concrete nonstory messages, details such as the titles of sources were modified to indicate that injuries are *possible*, rather than indicating that a particular child had *already suffered* a specific injury.

It has been hypothesized that messages containing personal pronouns such as “he” or “she” may result in text that is easier to comprehend (e.g., Flesch, 1948). With this in mind, personal pronouns were used in the concrete nonstory safety messages to control for any effect these pronouns might have in the story-based messages. Table 6 presents text difficulty measures of the explicit warnings in the three conditions as indicated by Microsoft Office software (all other text in the assembly manuals was identical).

**Table 6 Readability of Explicit Safety Messages**

Message Type		Number of Words	Flesch-Kincaid Grade Level	Percent Passive Sentences	Flesch Reading Ease
Story-based	<i>M</i>	64.20	5.60	17.80	77.41
	<i>SD</i>	16.38	1.13	14.30	8.44
Concrete Nonstory	<i>M</i>	63.90	5.42	18.20	78.45
	<i>SD</i>	13.64	0.97	14.30	8.05
Traditional Abstract	<i>M</i>	28.60	6.91	13.30	71.59
	<i>SD</i>	13.71	1.46	21.91	9.84

Participants were provided with each of the parts shown in the instructions. Extra parts were also included, and a statement on the cover of the instructions read, “The parts supplied with your model may be slightly different in appearance from those shown in these instructions.” By providing extra parts (including some hazardous components), participants were forced to make decisions relating to the safety messages. Appendix E lists the required parts and the parts actually supplied.

Appendix F contains the questionnaire used to collect self-report information from participants. Response Form A-1 gathered information about participants’ child-care experience, age, gender, and experience using tools and assembling products. Data from this section were used to check the effectiveness of the random assignment procedures and to provide covariates for the analyses.

The balance of the questionnaire was designed for collecting data regarding the psychological effects of the message conditions. Question (5) asked participants to describe any stories they could recall of children who have been hurt on swings (not including the stories in the assembly instructions). Questions (6) and (7) were designed to assess participants’ semantic memory of how playground accidents occur and specific physical hazards that may be present on swing sets.

Response Form A-3 asked participants to indicate their beliefs about the probability of injuries related to the hazards described in the safety messages. Participants

responded by drawing a vertical line on the approximately 100-mm visual analog scale, anchored by the probability statement “no possibility” at one end and “certain to happen” at the other. Immediately before completing response for A-3, a practice response form (A-2) was completed to ensure that participants understood how to use the response scale before answering the actual questionnaire items. Question (9) asked participants to indicate how the safety of the swing set compares with others.

Response Form A-4 was based on the Transportation Scale reported by Green and Brock (2000). The items of Green and Brock’s Transportation scale were modified to fit the context of the warnings in the present study. Specifically, the following modifications were made:

1. Items in the original Transportation Scale referenced “the narrative.” For instance, “I could picture myself in the scene of the events described in the narrative.” In contrast, items in the modified scale referenced “the warnings.” For instance, “I could picture myself in the scene of the events described in the warnings.”
2. The original scale asked for participants’ responses regarding named characters in the story. For instance, “I had a vivid mental image of [character name].” The modified scale asked participants to respond in regard to each of the warnings. For instance, “While reading the warnings, I had a vivid image of a child strangling when his/her clothing or necklace became caught on a long bolt.” So that responses related to specific warnings did not dominate the data, each participant’s average response for the 10 warnings items was entered once in the final transportation score.
3. The original Transportation Scale used a discontinuous 7-point response scale anchored by the statement “not at all” at one end and “very much” at the other. For the modified version, participants responded on a visual analog scale anchored by these same statements at each end. The visual analog scale was used in

order to maintain a mode of response consistent with the previous section of the questionnaire.

As in the original Transportation Scale, items (b), (e), and (i) of the modified scale were reverse-scored because responses on these items indicate reduced transportation. All other items were positively scored.

Appendix F includes the performance evaluation form used by experimenters to score participants' behavioral compliance. Also included is the follow-up e-mail questionnaire that was used to assess participants' long-term memory for the safety messages and to assess the extent to which they noticed other safety messages after the experiment.

Certain questionnaire items were added over time, resulting in some missing data for early participants. The modified transportation scale was added after six teams had already participated. Furthermore, the following items were added at the suggestion of the dissertation committee and major professor after 16 teams had participated: questionnaire items (4a) and (4b), and follow-up questionnaire item (4).

### ***Design and procedure***

The research proposal (4169) was approved by the Institutional Review Board at Kansas State University on January 24, 2007. Minor revisions (Proposal 4169.1) were approved on June 6, 2007.

Each team of participants was randomly assigned to one of three experimental conditions: story-based safety messages, concrete nonstory safety messages, or traditional abstract safety messages. Research was conducted by three different experimenters: Experimenter 1 (Ricketts) was a male PhD candidate and professional employee of Kansas State University, aged late 40's. Experimenters 2 and 3 were female upper-level undergraduate psychology students aged early 20's. One of these students was from Kansas State University and the other was a visiting summer research student from the University of Puerto Rico, Mayaguez Campus.

It was anticipated that an experimenter effect might occur due to differences in experimenters' genders, ages, professional status, and timing of experimental sessions. Because of his work schedule, Experimenter 1 conducted sessions mainly during

evenings and weekends early in the fall semester. In contrast, Experimenters 2 and 3 conducted sessions mainly during regular school hours throughout the spring and summer semesters. Thus, Experimenter 1 may have sampled a different subpopulation of students. Extreme care was taken to assign each experimenter to each of the three conditions an approximately equal number of times so that any timing/experimenter effects would influence the conditions equally.

Dependent variables included: (a) behavioral compliance, as indicated by the number of compliant safety-related elements in the assembled swing set, (b) reminders, as indicated by the number of related stories recalled from participants' past, (c) immediate memory for message content, as indicated by the number of injury mechanisms and swing set hazards recalled immediately after assembling the swing set, (d) generation of novel concrete injury mechanisms and swing set hazards that were not mentioned in the assembly instructions, (e) judgments about the probability of injury, as indicated by scale ratings, (f) transportation experience, as indicated by scale ratings, and (g) ratings of swing set safety, as indicated by scale ratings, and (h) delayed memory, delayed novel concrete injury mechanisms/hazards, and attention to other warnings, as indicated by a follow-up questionnaire.

Covariates for statistical analyses included participant age, gender, child-care experience, play equipment assembly experience, presence/absence of an observer (third participant), and a final covariate related to timing of experimental sessions conducted by different experimenters.

Participants volunteered by placing their names on a sign-up sheet in the lobby of the Department of Psychology. Upon arriving at the experiment site, participants were reminded about the length of time required for the study and then completed an informed consent form. Upon consenting to participate, they received the following instructions:

You are going to assemble a swing set according to written instructions. Imagine that you are assembling the swing set for a young child who is important to you. For instance, it could be your own child, a niece or nephew, a younger brother or sister, or someone you are babysitting. You could even imagine that you work for a preschool and you are assembling the swing for the children there.



Tell me what child or group of children you can imagine yourself assembling this swing for.

After identifying the particular children they were imagining, participants were shown the assembly instructions, tools, and swing set components. When three participants were present, a coin flip was used to determine which two participants would assemble the swing. (This step was left out when only two participants were present.) When three participants were present, the third participant either completed an unrelated task (not reported here) or served as an observer. When serving as an observer, the third participant received these instructions:

Instead of assembling the swing, you are going to observe the other participants and then complete a questionnaire. Pay close attention as they assemble the swing, because you will answer questions later. You may not talk to the other participants. They must assemble the swing with no help from you. Your job is to observe, but provide no help. This means no signals or hints of any kind.

The two main participants received these instructions:

Imagine that you have purchased this product and you are doing your best to assemble it properly for the child you have in mind. You must assemble the swing using only the materials and information you are given. (*When an observer was present: Do not speak to the third participant [the observer] during this experiment.*) Tell me when you are finished.

The experimenter then sat down and engaged in paperwork while the participants assembled the swing set. If participants asked questions related to assembly, the experimenter stated, "It is your decision. Just remember that your task is to assemble the swing set properly for the child you have in mind." The experimenter kept busy and did not stare at the participants. However, the experimenter did listen and glance up enough to determine whether the participants complied with items (2), (4), and (6c) of the performance evaluation. All other performance items were measured and recorded after participants had finished and left the room. Most participants seemed to become engrossed in the task. In fact, after beginning to work they appeared to pay little attention

to either the experimenter or the observer (if present). Most participants finished the assembly task in about an hour, and the questionnaires required an additional 15-30 minutes.

Participants completed the questionnaire after they finished assembling the swing. Because participants were not expected to be familiar with the visual analog scale, they completed a practice response form in advance. The practice response form required participants to indicate the likelihood that they would visit particular cities during a specified period of time. The cities mentioned on the scale were selected to include one that most participants were almost certain to visit (their hometown), one that most participants were quite unlikely to visit (Tokyo), and others that were more uncertain (Kansas City and Chicago). During the practice response session, the experimenter explained how to mark the scale, observed the participants as they made their practice marks, and addressed any issues that arose.

After a period of two to four weeks, a follow-up survey was sent by e-mail to determine delayed memory for the swing set safety messages and memory for any other warnings noticed by participants after the experiment. For the first 16 teams, the follow-up survey was sent 28 days after participation. The response rate was low, presumably because by this time most participants had completed all of the research credits for their psychology class. Beginning with the 17<sup>th</sup> team, the follow-up survey was sent at 14 days, and the response rate improved considerably.

### ***Scoring of Responses***

Participants' behavioral compliance with safety messages was scored according to the rules described in Appendix G. Responses recorded on the questionnaire's 100-mm visual analog scales were measured in mm from left to right.

Scoring procedures were devised for the free-response questionnaire items as described below. The experimenter was blind to the experimental condition while scoring all responses.

### ***Reminders (Episodic Memories)***

Questionnaire item (5) asked participants to record any swing or playground injuries they could recall (not counting those mentioned in the assembly instructions).

One point was awarded for each separate story if that story involved a swing set or playground injury and included both of the following: a specific character (e.g., “me”, “a friend”, “my brother”, “a kid”) and a specific event that occurred at a particular point in time (e.g., “fell off a swing”, “the swing set tipped over”). For instance, the following account contains the required elements and was scored as a story: “My friend got hurt when she fell off a swing.” In contrast, the following account was *not* scored as a story because it does not describe a specific instance and refers instead to a generalized class of accidents: “I have heard about kids getting hurt when they fall off swings.” Each participant’s response was scored on two occasions about one week apart, and no discrepancies in scoring occurred.

### ***Memory and Novel Concrete Responses for Swing Set Injury Mechanisms***

Questionnaire item (6) asked participants to describe injuries that can happen when children play on swing sets. It is common in the safety literature to describe injuries according to four components: source of injury, injury event, nature of injury, and body part involved. Questionnaire item (6) was scored according to this scheme because (1) the method identifies *concrete* injury mechanisms, (2) the method is widely recognized by safety professionals, (3) standard procedures have been published for this method (Bureau of Labor Statistics [BLS], 1992) and (4) the author has extensive training and work experience in the classification of injuries according to this method. Participant-generated injury sources, events, natures, and body parts were tallied if they clearly related to the traditional categories described by BLS (1992), or if they related to concrete sources and events noted by the Consumer Product Safety Commission (1997).

Appendix H lists the injury sources, events, natures, and body parts that were scored for analysis. For injury sources, both primary and secondary sources were counted. Separate scores were tallied for items apparently recalled from the safety messages and for novel responses. Each participant’s response form was scored on two occasions and any discrepancies in scores were resolved prior to analysis.

### ***Memory and Novel Concrete Responses for Swing Set Hazards and Precautions***

Questionnaire item (7) and follow-up questionnaire items (1), (2), and (3) asked participants to describe hazards related to swing sets and precautions to reduce injuries.

Responses were counted if they clearly related to hazards and preventive measures mentioned in the assembly instructions (memory) or by the Consumer Product Safety Commission (1997) (novel responses). Appendix I lists the hazards and precautions that were scored for analysis. Each participant's response form was scored on two occasions about a week apart and any discrepancies in scores were resolved prior to analysis.

## **Results**

### ***Statistical Reporting Procedures***

The alpha level for all analyses was  $\alpha = .05$ . Exact  $p$  values will be reported for descriptive purposes only. Effect sizes such as  $\eta^2$  will be reported for significant results.

For each analysis, data were screened for violations of statistical assumptions according to methods described by Tabachnick and Fidel (2001). In order to avoid redundancy in the text, the results of these screenings will be reported only when a problem is noted.

### ***Check of Random Assignment Procedure***

Data were analyzed to determine if significant differences existed among the experimental conditions in regard to participants' age, gender, childcare experience, equipment assembly experience, assignment of experimenters/times, or presence of an observer (a third participant).

A univariate ANOVA demonstrated no significant differences among the experimental conditions for participant age,  $F(2,139) = 1.28, p = .280$ .

A 2 x 3 chi-square analysis was conducted with gender as one factor and experimental condition as the other factor. There was no significant difference in gender assignment among the experimental conditions,  $\chi^2(2) = 0.56, p = .754$ .

Participants reported a wide range of childcare experience ( $M = 896.46$  days of experience,  $SD = 1,955.36$ ), resulting in a positively skewed distribution. Measures of skewness and kurtosis were 4.08, and 20.83, respectively. A log transformation of childcare experience improved the statistical characteristics of the distribution ( $M = 2.11$ ,  $SD = 1.07$ ; skewness = -0.49; kurtosis = -0.40). A univariate analysis of variance (ANOVA) demonstrated no significant differences among the experimental conditions

for (log) childcare experience,  $F(2,133) = 0.14, p = 0.87$ . Six participants did not provide a response and were excluded from this analysis.

Participants also reported a wide range of experience related to assembling swings and other play equipment ( $M = 11.04$  items assembled,  $SD = 84.47$ ), resulting in a positively skewed distribution. Measures of skewness and kurtosis were 11.63, and 137.04, respectively. An attempt to improve the characteristics of the distribution by means of a log transformation was unsatisfactory (skewness = 1.76, kurtosis = 4.96). Given the lack of normality in the distribution, assembly experience was transformed into four categories as described in Appendix J, based on natural breaks in the distribution: none ( $n = 64$ ), low ( $n = 32$ ), medium ( $n = 26$ ), and high ( $n = 19$ ). One participant did not respond to this question and was excluded from the analysis. A 4 x 3 chi-square analysis was conducted with assembly experience category as one factor and experimental condition as the other factor. The analysis demonstrated no significant differences in assembly experience among the experimental conditions,  $\chi^2(6) = 3.46, p = .750$ .

A 3 x 3 chi-square analysis was conducted with timing/experimenter as one factor and experimental condition as the other factor. There were no significant differences in assignment of timing/experimenter to the experimental conditions,  $\chi^2(4) = 0.87, p = .929$ .

A 2 x 3 chi-square analysis was conducted with presence of an observer (third participant) as one factor and experimental condition as the other factor. There were no significant differences in the presence of observers among the experimental conditions,  $\chi^2(2) = 1.11, p = .574$ .

Overall, these analyses indicated no observed biases in regard to the assignment of participants and experimenters/times to experimental conditions.

### ***Behavioral Compliance With Safety Messages***

The main purpose of this dissertation was to determine if safety messages that included stories or concrete nonstory descriptions of injuries resulted in higher behavioral compliance when compared with traditional abstract messages. With this in mind, a univariate between-subjects ANOVA was calculated, with message condition as the independent variable and number of compliant swing set elements as the dependent

variable. Each team's performance comprised one unit of analysis, resulting in a behavioral compliance score for each of the 54 teams.

The analysis demonstrated significant differences among the groups  $F(2,51) = 3.45, p = .039$ .  $\eta^2$  indicated that about 12 percent of the variation in behavioral compliance was accounted for by variation in the treatment conditions. ANOVA results are summarized in Table 7.

**Table 7 Single Factor Between-subjects ANOVA for Effects of Message Condition on Behavioral Compliance with Safety Messages**

Source	<i>SS</i>	<i>df</i>	<i>MS</i>	<i>F</i>	$\eta^2$
Between	23.41	2	11.71	3.45*	.12
Within	173.25	51	3.40		
Total	196.67	53			

\* $p = .039$

The Tukey-a (HSD) procedure demonstrated that the story-based condition resulted in significantly greater behavioral compliance than the concrete nonstory condition ( $p = .038$ ). The difference between means shown in Table 8 indicates that the mean compliance score of participants who read story-based safety messages was about 22 percent higher than the mean of participants who read concrete nonstory messages. No other differences were significant. On average, participants who read story based safety messages achieved an average compliance rate of 87 percent. Participants who read concrete nonstory messages averaged 72 percent compliance, and participants who read traditional abstract messages averaged about 76 percent compliance. To put this in perspective, chance performance would result in a compliance rate of about 45 percent. As another measure of effect size, Cohen's  $d$  indicated a standardized mean difference of 0.86 standard deviations in the performance of participants in the story-based versus concrete nonstory conditions. The standardized mean difference between the story-based and traditional abstract conditions was  $d = 0.65$ .

**Table 8 Total Behavioral Compliance Scores for Each Message Condition**

		Experimental Condition		
		Story-based Messages	Concrete Nonstory Messages	Traditional Abstract Messages
Total Behavioral	<i>M</i>	8.70*	7.15*	7.55
Compliance	<i>SD</i>	1.58	1.98	1.94

\*Means significantly different,  $p = .038$

In order to present a clearer view of the differences among conditions, an analysis of covariance (ANCOVA) was conducted with message condition as the independent variable, total behavioral compliance as the dependent variable, and six covariates consisting of the factors that were used to check the random assignment procedure: age, gender, (log) childcare experience, assembly experience category, timing/experimenter, and presence of an observer (see p. 75-76 for a discussion of these covariates).

Since behavioral compliance was computed as a team score, it was also necessary to compute team scores for the covariates. In each team, only two participants actually participated in assembling the swing. Therefore, only the scores of the two assemblers were combined for each team.

Appendix J contains complete details regarding the calculation of team scores, so only a brief overview is included here. Team age was computed as the mean age of the two team members. For team gender, the genders of the two members of each team were combined to create three categories scored ordinally as 1, 2, or 3: male-male ( $n = 6$ ), male-female ( $n = 24$ ), and female-female ( $n = 24$ ). Team (log) childcare experience was computed as the mean of the (log) childcare experience scores of the two team members. For team assembly experience, the experience categories of the two members of each team were combined to create five ordinal categories based on natural breaks in the distribution: no experience ( $n = 9$ ), low experience ( $n=13$ ), low-medium ( $n=14$ ), high-medium ( $n=11$ ), and high ( $n=6$ ). No transformations were required for timing/experimenter or presence of observer because these were team-level variables.

Six teams were excluded from analysis because a team member had failed to answer one or more items on the questionnaire. This included five teams excluded because of missing values for childcare experience, and one because of missing values for assembly experience. Incomplete questionnaire responses were distributed evenly among the experimental conditions, leaving 16 teams in each condition (48 teams, total).

Adjustment for covariates resulted in a 50 percent increase in effect size for the message conditions,  $F(2,39) = 4.30, p = .02, \eta^2_{partial} = .18$ . After adjustment for covariates, about 18 percent of the variation in behavioral compliance was accounted for by variation in the message conditions (for comparison,  $\eta^2 = 0.12$  in the univariate ANOVA). ANCOVA results are summarized in Table 9.

**Table 9 Between-subjects ANCOVA for Effects of Message Condition on Adjusted Behavioral Compliance**

Source	$SS_{Adj}$	$df$	$MS$	$F$	$\eta^2_{partial}$
Between (Message Condition)	21.07	2	10.53	4.30*	.18
Covariate					
Age	7.36	1	7.36	3.01	
Gender	8.91	1	8.91	3.64	
(Log) Childcare Experience	0.03	1	0.03	0.01	
Assembly Experience	0.20	1	0.20	0.08	
Timing/experimenter	22.74	1	22.74	9.29**	.19
Presence of Observer	6.43	1	6.43	2.63	
Within	95.47	39	2.45		
Total	177.26	47			

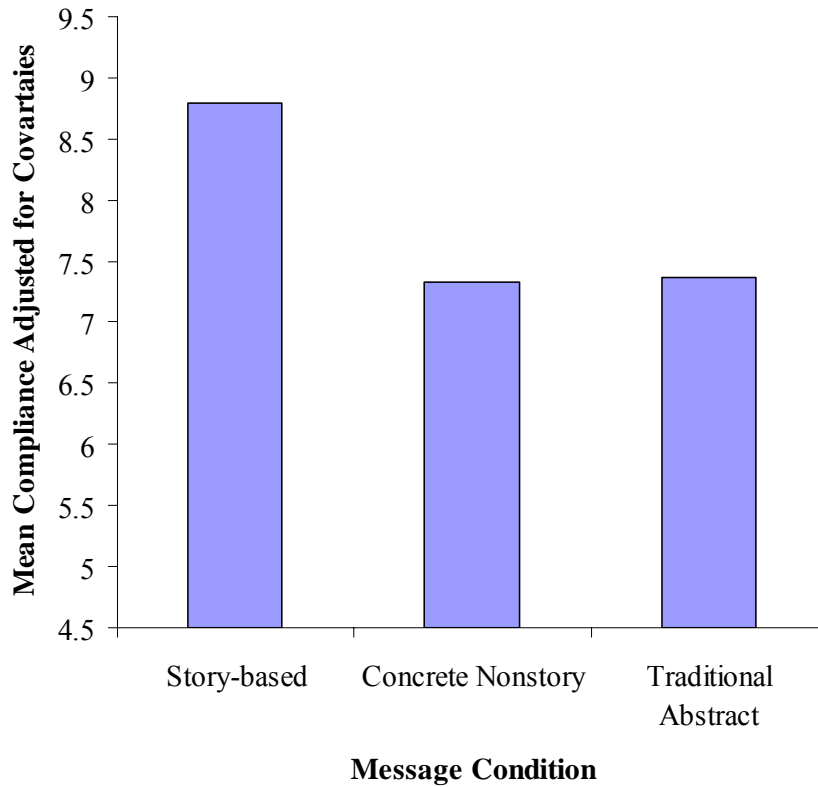
\*  $p = .02$ , \*\*  $p = .004$ ,  $R^2 = .46$ ,  $R^2_{Adj} = .35$

Figure 13 and Table 10 display means for total behavioral compliance, after adjustment for covariates. The adjusted means were compared with the Tukey-a (HSD) procedure modified for ANCOVA (Sheskin, 2004). The modified Tukey procedure demonstrated that the story-based condition resulted in significantly greater adjusted behavioral compliance than both the concrete nonstory and the traditional abstract



conditions ( $p < .05$ ). The differences shown in Figure 13 and Table 10 indicate that the adjusted behavioral compliance of participants who read story-based safety messages was 19 percent better, on average, than that of participants who read the other messages.

**Figure 13 Mean Behavioral Compliance Adjusted for Covariates.**



**Table 10 Adjusted Total Behavioral Compliance Scores for Each Message Condition**

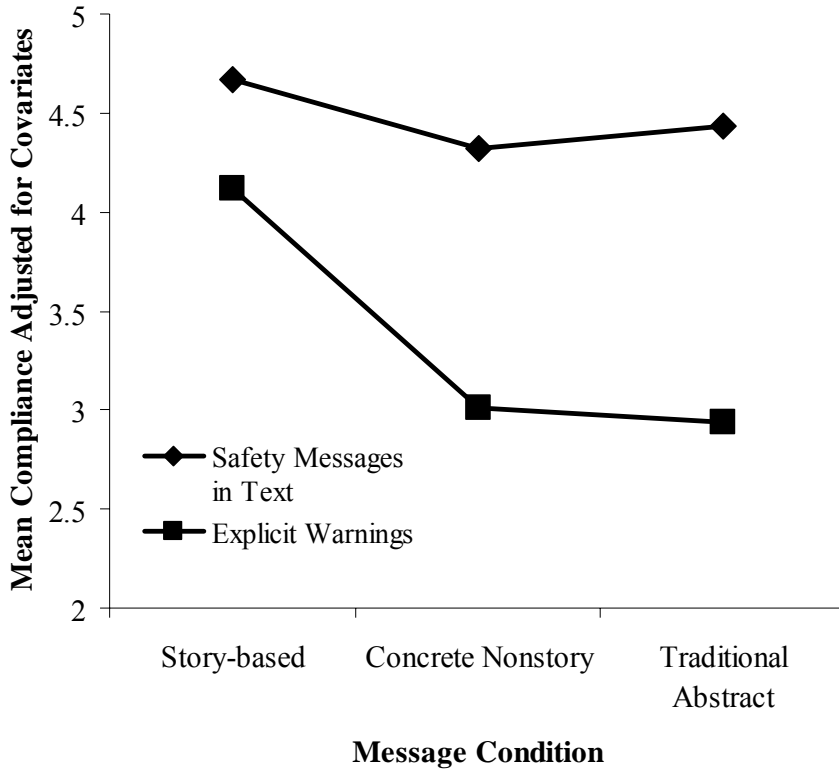
		Experimental Condition		
		Story-based	Concrete	Traditional
		Messages	Nonstory	Abstract
			Messages	Messages
Total Behavioral	$M_{Adj}$	8.79 <sup>1,2</sup>	7.33 <sup>1</sup>	7.37 <sup>2</sup>
Compliance Adjusted	$SE_{Adj}$	0.40	0.40	0.40
		for Covariates		

<sup>1,2</sup> Adjusted means with the same superscript were significantly different,  $p < .05$

The preceding analyses were based on each team's total compliance score, i.e., compliance with safety messages in explicit warnings *and* safety messages contained in the text. Thus, total compliance consisted of two components. In order to determine if the experimental conditions had an effect on both components, a between-subjects multivariate analysis of covariance (MANCOVA) was performed on explicit warning and textual safety messages with message condition as the independent variable, and the same six covariates used before. Again, due to incomplete questionnaire responses, there were 16 teams per condition (48 total).

Wilk's Lambda indicated that message conditions did not affect the two components of compliance differently, although the difference approached significance,  $F(4,76) = 2.24, p = .073$ . Since the difference did not meet traditional levels of significance, no further analyses were conducted. Nevertheless, the pattern of nonsignificant differences shown in Figure 14 suggests more of an effect for compliance with explicit warnings.

**Figure 14 Mean Adjusted Behavioral Compliance for Explicit Warnings and Other Safety Messages**



***Timing/experimenter Effects***

As previously noted, the random assignment procedure resulted in no significant differences among the experimental conditions in regard to assignment of timing/experimenter. However, an effect for timing/experimenter was anticipated and was apparent in the results of the ANCOVA reported earlier. In fact, the effect size for the timing/experimenter covariate was somewhat greater than the effect size for the independent variable of message condition. Further analyses will be reported here to rule out the alternative explanation that timing/experimenter effects were responsible for the differences among message conditions.

To analyze timing/experimenter effects in a univariate context, a between-subjects ANOVA was conducted with timing/experimenter as the independent variable and total compliance with safety messages as the dependent variable. The result was significant,

$F(2,51) = 6.22, p = .004$ . This indicated participants' total compliance scores were affected by timing/experimenter. ANOVA results are summarized in Table 11.

**Table 11 Single Factor Between-subjects ANOVA for Effects of Timing/experimenter on Behavioral Compliance**

Source	<i>SS</i>	<i>df</i>	<i>MS</i>	<i>F</i>	$\eta^2$
Between	38.55	2	19.27	6.22*	.20
Within	158.12	51	3.10		
Total	196.67	53			

\* $p = .004$

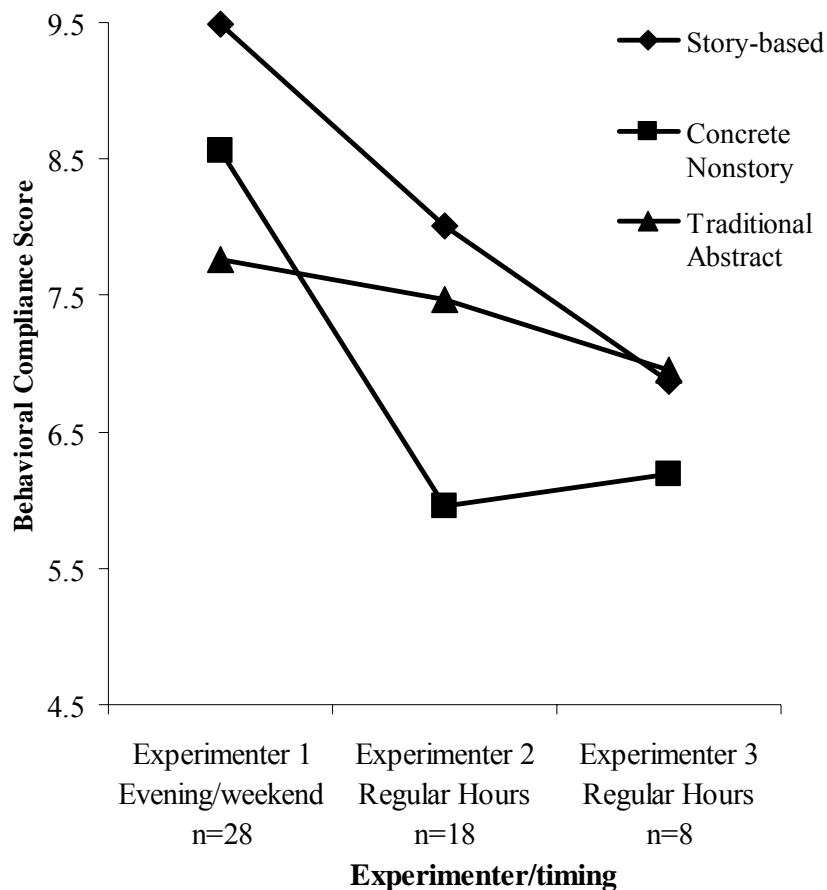
The three experimenters were described earlier (see p. 70-71). Briefly, Experimenter 1 differed from the other experimenters in age (late 40's), gender (male), and professional status (professional employee of the University). The other experimenters were female undergraduate students in their early 20's. Furthermore, Experimenter 1 conducted most of his experimental sessions in the evening and during weekends early in the fall semester. In contrast, Experimenters 2 and 3 typically ran their experimental sessions during regular class hours in the spring and summer semesters.

The Tukey-a (HSD) procedure demonstrated that average total behavioral compliance was significantly greater for the teams of Experimenter 1 than for the teams of Experimenter 2 ( $p = .015$ ) and Experimenter 3 ( $p = .021$ ). The difference between Experimenters 2 and 3 was not significant. On average, Experimenter 1's participants performed about 22 percent better than those of Experimenter 2 and about 29 percent better than those of Experimenter 3. This significant difference was expected, given that Experimenter 1 may have sampled a different population of students by conducting session mainly at night and on weekends, and because of differences in the experimenters' age, professional status, and gender.

Timing/experimenter effects do not threaten the validity of the findings as long as these effects do not interact with the experimental conditions. In order to determine whether timing/experimenter effects interacted with message type, a 3 x 3 univariate ANOVA was calculated with message condition as one factor, timing/experimenter as the

other factor, and total behavioral compliance as the dependent variable. The results indicated no significant interaction,  $F_{CONDITION \times EXPERIMENTER} (4,45) = 1.02, p = .410$ . Thus, it can be concluded that differences due to timing/experimenter did not have differential effects for the safety message conditions. These findings confirm the previous ANCOVA results in which the effects for safety messages were significant even after the influence of timing/experimenter had been statistically removed. Figure 15 displays mean total behavioral compliance scores for each message condition and experimenter.

**Figure 15 Behavioral Compliance Scores According to Timing/experimenter and Message Condition**



For each experimenter, the mean performance appears higher for the story-based message condition than for the concrete nonstory condition, mirroring the trend for the experiment as a whole. For Experimenters 1 and 2, there is also an apparent benefit for

the story-based condition over the traditional abstract condition. For Experimenter 3, performance for the story-based and traditional abstract conditions were essentially equal, although the number of teams for her was quite small.

An attempt was made to determine if the superior performance of groups led by Experimenter 1 was due to timing effects. Experimenter 1 conducted experimental sessions as follows: regular hours during summer ( $n = 1$ ); lunch hour during fall ( $n = 6$ ); evenings during fall ( $n = 16$ ); and Saturdays during fall ( $n = 5$ ). A Pearson Product Moment Correlation was calculated for Experimenter 1 with total compliance as the criterion variable and timing as the predictor variable (rank ordered according to divergence from regular class hours as follows: regular hours = 1, lunch = 2, evenings = 3, Saturdays = 4). A significant positive correlation was obtained, indicating that participant performance increased as timing diverged from regular class hours,  $r = 0.49$ ,  $p = 0.008$ .

A univariate between subjects ANOVA was conducted for Experimenter 1 with total compliance as the dependent variable and timing as the independent variable (with the four times as categorical variables). The result indicated that timing had a significant effect on total compliance,  $F(3,24) = 3.80$ ,  $p = .023$ .  $\eta^2$  indicated that about 32 percent of the variation in behavioral compliance for Experimenter 1 was accounted for by variation in the timing of experimental sessions. ANOVA results are summarized in Table 12.

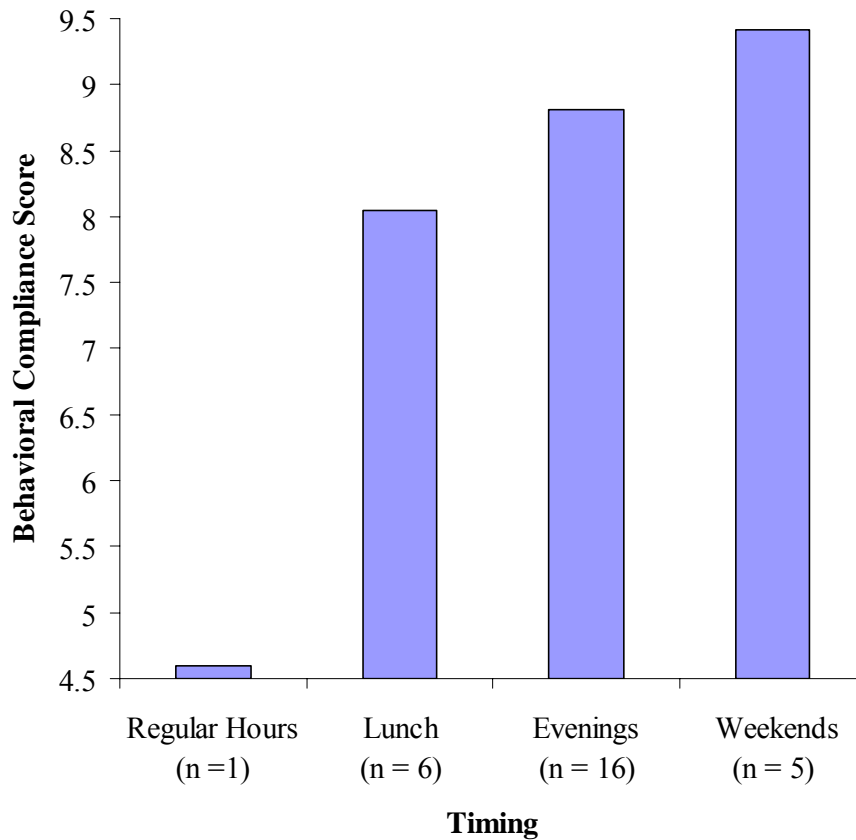
**Table 12 Single Factor Between-subjects ANOVA for Effects of Timing on Behavioral Compliance for Experimenter 1**

Source	<i>SS</i>	<i>df</i>	<i>MS</i>	<i>F</i>	$\eta^2$
Between	21.85	3	7.28	3.80*	.32
Within	45.97	24	1.92		
Total	67.82	27			

\* $p = .023$

Figure 16 illustrates the effect of timing on average total compliance for Experimenter 1's participants.

**Figure 16 Behavioral Compliance Scores for Timing of Sessions by Experimenter 1.**



With all three experimenters in the analysis, two ANCOVAs were conducted to examine the differential effects of timing and experimenters. In one ANCOVA, timing was the independent variable, total compliance was the dependent variable, and experimenter was a covariate. In the other ANCOVA, experimenter was the independent variable, total compliance was the dependent variable, and timing was a covariate. The timing score consisted of the four timing codes mentioned above. Since Experimenters 2 and 3 conducted experimental sessions only during regular class hours, all of their sessions were coded as “1.”

With experimenter as a covariate, timing had a significant effect on total compliance,  $F(3,49) = 3.51, p = .022, \eta^2_{partial} = .18$ . The covariate of experimenter was *not* significant in this analysis,  $F(1,49) = 1.59, p = .21$ . This indicates that timing had a significant effect on total compliance, after adjustment for the covariate of experimenter.

With timing as a covariate, experimenter did *not* have a significant effect on total compliance,  $F(2,50) = 0.63, p = .80$ . However, the covariate of timing *was* significant,  $F(1,50) = 5.69, p = .021$ . This indicates that experimenters did *not* have a significant effect on total compliance, after adjustment for the covariate of timing.

Taken together, these analyses provide strong evidence that timing/experimenter effects were due mainly to the effect of timing, rather than the effect of experimenters.

Finally, a multivariate analysis of variance (MANOVA) was conducted to determine if Experimenter 1 sampled a different population of participants, compared with the other two experimenters. The participants of Experimenters 2 and 3 were combined into a single category since their compliance scores and the timing of their sessions were similar. This resulted in two categories of experimenters: Experimenter 1 and Experimenters 2/3

A MANOVA was conducted with Experimenter as the independent variable and the following as dependent variables: age, gender, (log) childcare experience, assembly experience, and presence of observer. Six teams were excluded because of incomplete questionnaire responses (two teams for Experimenter 1 and four teams for Experimenters 2/3).

Wilk's Lambda indicated significant differences in the combined dependent variables according to experimenters,  $F(5,42) = 3.98, p = .005$ . Roy-Bargmann stepdown analysis indicated Experimenter 1's participants had significantly more play equipment assembly experience compared with the combined participants of the other experimenters,  $F(1,51) = 6.64, p = .013$ . Stepdown analysis also demonstrated that Experimenter 1's participants included more females, compared with the other two experimenters,  $F(1,49) = 5.69, p = .021$ . Differences in the other dependent variables were not significant in the stepdown analyses. Table 13 summarizes the results of the MANOVA and stepdown analyses. Mean assembly experience of participants is illustrated in Figure 17. (One team was excluded because of missing values for assembly experience for Experimenters 2/3). Figure 18 illustrates mean gender of participants, adjusted for the covariates of assembly experience and age (from the Roy-Bargmann stepdown analysis). One team was excluded because of missing values for age for Experimenters 2/3.



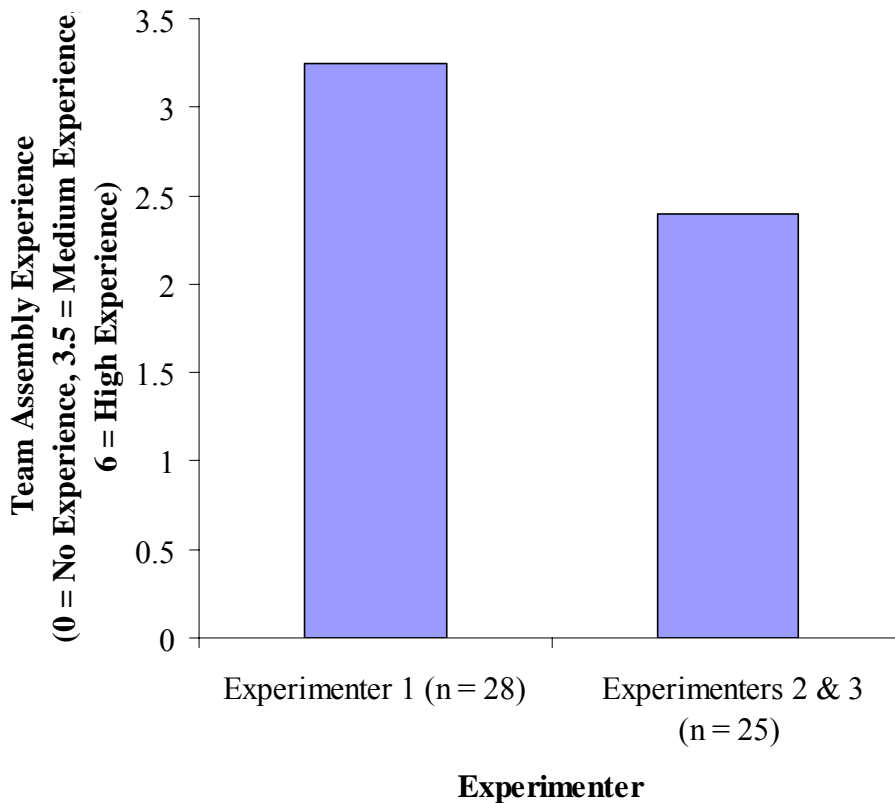
**Table 13 MANOVA for Experimenter 1 versus Combined Experimenters 2/3 on Characteristics of Participants**

Dependent Variable	$F_{\text{Univariate}}$	$df_{\text{Univariate}}$	$F_{\text{Stepdown}}$	$df_{\text{Stepdown}}$	$\alpha_{\text{Stepdown}}$
Assembly Experience	5.14 <sup>a</sup>	1,46	6.64**	1,51	.025
Age	3.81	1,46	3.48	1,50	.025
Gender	2.17	1,46	5.69*	1,49	.025
(Log) Childcare Exp.	2.10	1,46	0.25	1,43	.025
Observer Present	0.17	1,46	0.21	1,42	.025

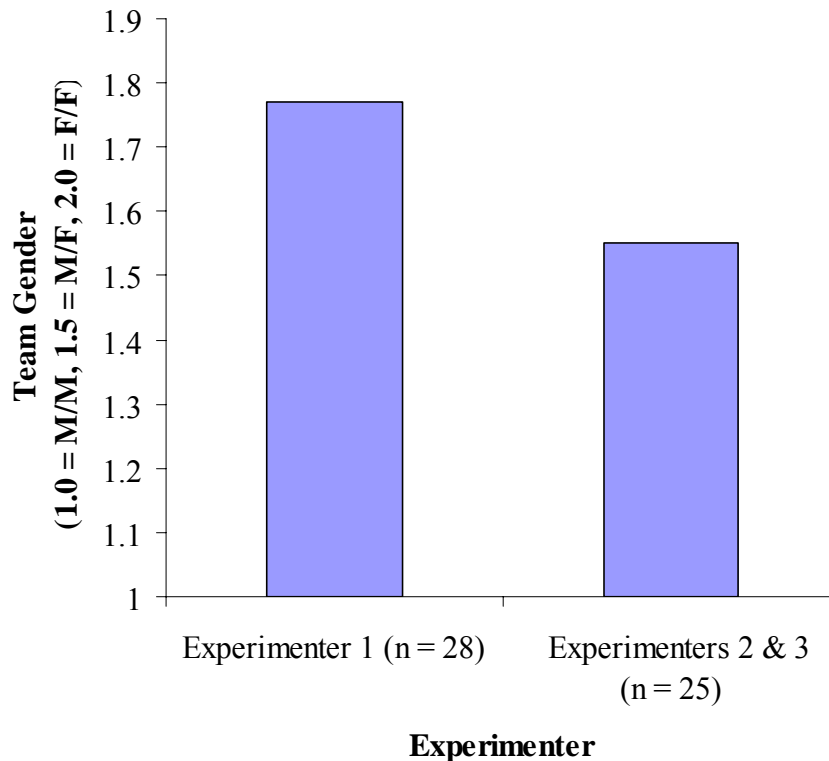
<sup>a</sup> Result can not be evaluated, but would be significant in a univariate context ( $p = .028$ )

\*  $p = .021$ , \*\*  $p = .013$

**Figure 17 Average Assembly Experience of Participant Teams by Experimenter**



**Figure 18 Adjusted Average Gender of Participant Teams by Experimenter**



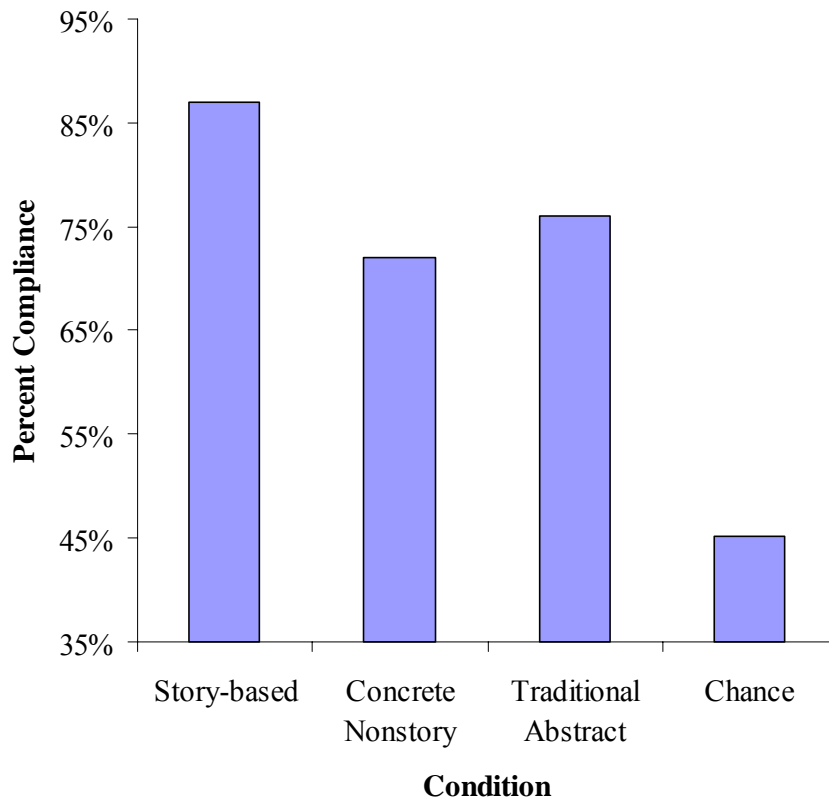
MANOVA indicates the Experimenters sampled different populations of participants. On average, the teams of Experimenter 1 included more females and they had more play equipment assembly experience, compared with the combined participants of Experimenters 2 and 3.

### ***Summary of Effects of Message Conditions on Behavioral Compliance***

Results demonstrated that story-based messages resulted in greater behavioral compliance, compared with traditional abstract messages and concrete nonstory messages. No differences in compliance were observed between concrete nonstory messages and traditional abstract messages.

To place the results in perspective, Figure 19 illustrates compliance rates as a percentage of correct responses. If responses had been made by chance, average compliance would be about 45 percent (some items had more than two possible responses).

**Figure 19 Compliance Rate as Percent of Correct Responses**



As anticipated, there was a strong effect for timing/experimenter, but this effect did not interact differentially with message conditions. Thus, the presence of timing/experimenter effects did not threaten the validity of the results. Analysis of the combined effect for experimenters and timing indicated the effect was associated mainly with timing of the experimental sessions, rather than differences among experimenters. By conducting experimental sessions mainly during evenings and weekends, Experimenter 1 sampled more females and participants with more play equipment assembly experience, compared with the other experimenters.

### ***Theoretical Correlates of Behavioral Compliance***

A second purpose of this experiment was to examine some psychological variables that may be related to increased compliance with safety messages. Since these psychological effects were derived from diverse theories in different contexts, each

hypothesized correlate will be examined separately. The results may suggest direction for future research involving these variables in the context of safety and health messages.

### ***Self-reported Transportation***

Research Question 2 asked: What is the relationship between behavioral compliance and the self-reported experience of narrative transportation? According to the Transportation-Imagery Model of persuasion, stories are effective because they transport the reader into the story, making events in the narrative seem real. If transportation was the reason for the effectiveness of stories in the present study, two findings would be expected. First, a positive relationship should be observed between transportation and behavioral compliance, at least for participants in the story-based condition. Second, scores on the modified transportation scale should be higher for participants in the story-based condition, compared with the other conditions.

Team scores for transportation consisted of the mean individual scores for the two members of each team. The modified transportation scale was not initially included in the experiment, but was added later. Thus, team transportation scores were missing for six groups, and these groups were excluded from analysis. The missing data were distributed evenly among message conditions, leaving 16 teams in each of the three conditions (48 teams, total).

The Transportation-Imagery Model relates to the experience of transportation while engrossed in a story. Thus, the Transportation-Imagery Model was initially tested only among the 16 teams that read story-based safety messages and had also completed the modified transportation scale.

To test the strength of the relationship while accounting for covariates, hierarchical multiple regression (also known as sequential multiple regression) was conducted with total behavioral compliance as the criterion variable and team transportation scores as the predictor variable after the effects of the following covariates had been removed: team scores for age, gender, (log) childcare experience, assembly experience, timing/experimenter, and presence of an observer.

The zero-order correlation between team transportation and team compliance was moderate and in the expected direction,  $r = .33$ . Addition of team transportation to the hierarchical multiple regression significantly added to the prediction of behavioral

compliance,  $F_{Change}(1,7) = 19.43, p = .003$ . Thus, there was a significant positive correlation between behavioral compliance and team transportation within the story-based condition, after adjustment for covariates.

These results supported the Transportation-Imagery Model. If it could also be shown that the experience of transportation was greater among participants who read story-based safety messages, one might speculate that transportation had something to do with the effect of the messages.

In order to determine if participants experienced the story-based safety messages as particularly transporting, an ANCOVA was conducted for all three experimental conditions, with message condition as the independent variable, team transportation scores as the dependent variable, and the same six covariates as used before. Prior to analysis, it was noted that a significant interaction existed between transportation and two covariates (gender and [log] childcare). This interaction resulted in a violation of the assumption of homogeneity of regression. As recommended by Tabachnick and Fidel (2001), these two covariates were removed from the analysis (the problem was not observed earlier with only 16 teams). After removal of these covariates, no further problems were observed with respect to the assumptions of ANCOVA. The following covariates remained in the analysis: team scores for age, assembly experience, timing/experimenter, and presence of an observer. Again, some teams were dropped from the analysis because of incomplete questionnaire responses, leaving 16 participants in the story-based condition, 15 in the concrete nonstory condition, and 16 in the traditional abstract condition (47 teams, total).

The ANCOVA result indicated message condition did *not* have a significant effect on team transportation, after adjustment for covariates,  $F(2,40) = 0.56, p = .57$ . This suggests the story-based messages were not experienced as transporting, compared with the other message conditions.

To determine if team transportation scores were related to compliance across all conditions, hierarchical multiple regression was conducted for the 47 teams that had transportation scores and complete questionnaire responses. The analysis was conducted with total behavioral compliance as the criterion variable and team transportation scores as the predictor variable after the effects of the following non-interacting covariates had

been removed: team scores for age, assembly experience, timing/experimenter, and presence of an observer.

The zero-order correlation between team transportation and team compliance was nominal,  $r < .01$ . Addition of team transportation to the hierarchical multiple regression was not significant,  $F_{Change}(1,41) = 0.12, p = .74$ . Thus, behavioral compliance and team transportation were *not* correlated across all conditions, after adjustment for covariates.

Overall, the results demonstrated a positive relationship between compliance and transportation among participants who read story-based warnings. However, participants did not experience greater transportation when reading story-based safety messages, compared with the other messages. Thus, transportation seems an unlikely explanation for the behavioral results reported in this study.

### ***Self-reported Reminders***

Research Question 3 asked: What is the relationship between behavioral compliance and reminders, i.e., recall of relevant stories from participants' past? According to Schank (1990), stories are effective in part because they remind us of past experiences which, in turn, affect our behavior. Nonstory communications, in contrast, evoke fewer reminders. Based on these notions, a positive correlation between reminders and compliance was expected, at least within the story-based condition. In addition, it was expected that story-based messages would lead to more reminders than concrete nonstory messages and traditional abstract messages.

For questionnaire item 5, participants reported stories of playground injuries remembered from their past. Team scores consisted of the mean number of stories reported by team members. The resulting team reminders were as follows: 0 stories ( $n = 9$ ), 0.5 stories ( $n = 20$ ), 1 story ( $n = 18$ ), 1.5 stories ( $n = 6$ ), and 3 stories ( $n = 1$ ).

To test the strength of the relationship while accounting for covariates, hierarchical multiple regression was conducted with total behavioral compliance as the criterion variable and team reminders as the predictor variable after the effect of the following covariates had been removed: team scores for age, gender, (log) childcare experience, assembly experience, timing/experimenter, and presence of an observer.

The influence of reminders was initially tested among the teams that read story-based safety messages ( $n = 16$ , after excluding 2 teams due to incomplete questionnaire

responses). The zero-order correlation between behavioral compliance and team reminders was rather small, but in the expected direction,  $r = .29$ . However, addition of team reminders to the hierarchical multiple regression did *not* significantly add to the prediction of behavioral compliance,  $F_{Change} (1,8) = 2.04, p = .19$ . Thus, for participants who read story-based safety messages, behavioral compliance and team reminders were *not* significantly related.

To determine the relationship between compliance and reminders across all conditions, an identical hierarchical multiple regression was conducted for all three conditions. Due to incomplete questionnaire responses for some participants, there were 16 teams per condition (48, total). The zero-order correlation between behavioral compliance and team reminders was quite small,  $r = -.02$ . Addition of team reminders to the hierarchical multiple regression was not significant,  $F_{Change} (1,40) = 0.49, p = .49$ . Thus, across all conditions, behavioral compliance and team reminders were not related.

In order to determine if message conditions affected reminders among the 48 teams with complete responses, an ANCOVA was conducted with message condition as the independent variable, team reminders as the dependent variable, and the same six covariates as used before.

The ANCOVA indicated that reminders of team members were affected by message conditions, after adjustment for covariates,  $F (2,39) = 5.00, p = .012$ . In fact,  $\eta^2_{partial}$  indicated that 20 percent of the variation in team reminders was accounted for by variation in message conditions. See Table 14 for a summary of the analysis.

**Table 14 Between-subjects ANCOVA for Effects of Message Condition on Team Reminders**

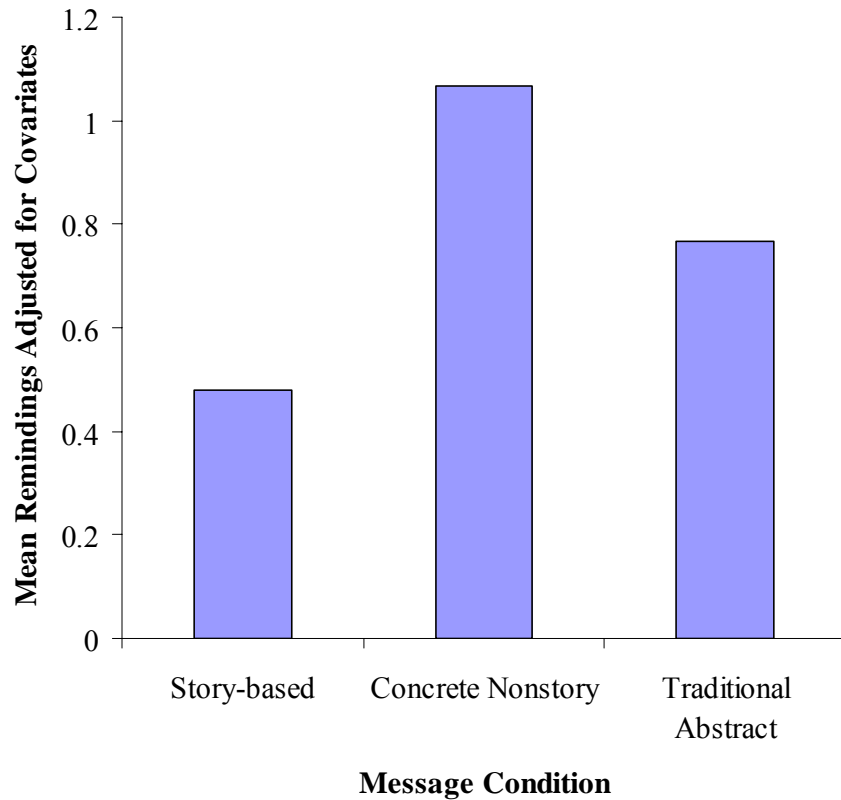
Source	<i>SS</i> <sub>Adj</sub>	<i>df</i>	<i>MS</i>	<i>F</i>	$\eta^2_{partial}$
Between (Message Condition)	2.63	2	1.32	5.00*	.20
Covariate					
Age	0.02	1	0.02	0.06	
Gender	0.95	1	0.95	3.61	
(Log) Childcare Experience	0.08	1	0.08	0.32	
Assembly Experience	0.28	1	0.28	1.05	
Timing/experimenter	0.01	1	0.01	0.02	
Presence of Observer	0.37	1	0.37	1.41	
Within	10.27	39	0.26		
Total	14.98	47			

\* $p = .012$ ,  $R^2 = .32$ ,  $R^2_{Adj} = .17$

Figure 20 displays means for team reminders, adjusted for covariates. The Tukey procedure modified for ANCOVA demonstrated that the concrete nonstory condition resulted in significantly more reminders than the story-based condition ( $p < .05$ ), after adjustment for covariates. Participants who read the concrete nonstory messages reported more than twice the number of reminders after adjustment, compared with participants who read story-based safety messages. No other differences were significant.



**Figure 20 Adjusted Team Reminders for Each Message Condition**



Taken together, these results provide no support for the notion that reminders were responsible for the superior performance of participants who read story-based safety messages. Reminders were *not* related to behavioral compliance. Furthermore, participants who read story-based messages reported *fewer* reminders than those who read concrete nonstory messages. These results support the findings of Larsen and Seilman (1988), who found that just as many reminders were triggered by reading expository text, compared with reading stories.

***Participants' Descriptions of Concrete Hazards, Injuries, and Preventive Measures***

Research Question 4 asked: What is the relationship between behavioral compliance and participants' ability to provide describe concrete details related to the safety messages? A positive relationship was expected, consistent with the notion that stories are effective in part because they convey concrete details that would not otherwise

be imagined. Furthermore, if stories are effective solely because of the details they contain, participants who read story-based messages should describe more concrete details, compared with the other message conditions.

Research Question 4 was evaluated by examining participants' memory and generation of novel concrete responses related to the content of the safety messages. Memory and novel responses were assessed immediately after the swing set was assembled and again after a delay of two to four weeks.

### ***Immediate Recall***

Memory for safety message content was assessed immediately after participants assembled the swing set. Team members' immediate recall of swing set hazards and safety precautions (questionnaire items 6 and 7) were averaged for each team. Team scores for memory were then correlated with behavioral compliance.

To test the strength of the relationship while accounting for covariates, hierarchical multiple regression was conducted with total behavioral compliance as the criterion variable and team recall as the predictor variable after the effects of the following covariates had been removed: team scores for age, gender, (log) childcare experience, assembly experience, timing/experimenter, and presence of an observer. Incomplete questionnaire responses were distributed evenly among the experimental conditions, leaving 16 teams in each condition (48 teams, total).

The zero-order correlation between behavioral compliance and team recall was moderate and in the expected direction,  $r = .48$ . Addition of team recall to the hierarchical multiple regression significantly added to the prediction of behavioral compliance,  $F_{Change}(1,40) = 12.96, p = .001$ . Thus, there was a significant positive correlation between behavioral compliance and team recall.

In order to determine if message conditions affected team recall, an ANCOVA was conducted with message condition as the independent variable, team recall as the dependent variable, and the same six covariates as used before.

The ANCOVA indicated team recall scores were *not* affected by message condition, after adjustment for covariates,  $F(2,39) = 0.37, p = .69$ . Thus, story-based messages did *not* lead to increased recall, compared with the other message conditions.

Taken together, these analyses support the notion that there was a relationship between behavior compliance and immediate recall of hazards and precautions. However, message conditions did *not* affect recall after adjustment for covariates.

### ***Immediate Generation of Novel Concrete Responses***

When responding to questionnaire items (6) and (7), participants listed a number of concrete injury mechanisms, hazards, and precautions that were valid, but had not been included in the assembly instructions or safety messages. These novel responses were analyzed because they relate to how much thinking participants did beyond the explicit information with which they were provided. Immediate generation of novel concrete responses was averaged for the two members of each team.

To test the strength of the relationship while accounting for covariates, hierarchical multiple regression was conducted with total behavioral compliance as the criterion variable and team novel response scores as the predictor variable, using the same six covariates as before. Incomplete questionnaire responses were distributed evenly among the experimental conditions, leaving 16 teams in each condition (48 teams, total).

The zero-order correlation between behavioral compliance and team novel concrete response was rather small, but in the expected direction,  $r = .21$ . Addition of team novel concrete response to the hierarchical multiple regression approached significance,  $F_{Change}(1,40) = 2.88$ ,  $p = .10$ . Thus, the positive correlation between behavioral compliance and team novel response while nonsignificant, was in the expected direction.

In order to determine if message conditions affected team novel concrete responses, an ANCOVA was conducted with message condition as the independent variable, team novel response as the dependent variable, and the same six covariates as before.

The ANCOVA result indicated team novel concrete responses were *not* affected by message condition, after adjustment for covariates,  $F(2,39) = 0.01$ ,  $p = .99$ .

Taken together, these analyses indicated a marginal relationship between behavioral compliance and participants' generation of novel concrete hazards and

precautions. However, message conditions did *not* affect novel responses after adjustment for covariates.

### ***Delayed Recall***

Delayed recall was measured by a follow-up questionnaire delivered by e-mail two to four weeks later. Delayed recall was scored in the same manner as immediate recall. The follow-up questionnaire was completed by 42 team members who had taken part in assembling the swing set. Due to incomplete questionnaire responses, 4 of these team members were excluded from analysis, leaving 12 participants in the story-based condition, 13 in the concrete nonstory condition, and 13 in the traditional abstract message condition. Since the teams were no longer intact, individual (rather than team) data were used for analysis.

In order to determine if message conditions affected team novel concrete responses, an ANCOVA was conducted with message condition as the independent variable, delayed recall as the dependent variable, and the same six covariates as in the other analyses.

The ANCOVA result indicated delayed recall was not affected by message condition,  $F(2,29) = 0.97, p = .39$ . Thus, message condition did not have an effect on delayed recall of safety message content.

### ***Delayed Generation of Novel Concrete Responses***

Delayed novel concrete novel responses were measured by the follow-up questionnaire discussed earlier. Delayed novel responses were scored in the same manner as immediate novel responses, and individual (rather than team) data were analyzed for the 38 participants with complete questionnaire responses..

In order to determine if message conditions affected team novel concrete responses, an ANCOVA was conducted with message condition as the independent variable, delayed novel concrete response as the dependent variable, and the same six covariates as in the other analyses.

The ANCOVA result indicated delayed novel concrete responses were not affected by message condition,  $F(2,29) = 0.30, p = .74$ .

### ***Summary of Participants' Descriptions of Concrete Hazards, Injuries, and Preventive Measures***

Overall, the results provided some support for the notion that behavioral compliance was related to participants' immediate (but not delayed) descriptions of swing set hazards, injuries, and preventive measures. At the same time, the results indicated participants' descriptions were no more complete as a result of reading story-based safety messages, compared with the other messages. Thus, it seems unlikely that stories were effective because they provided concrete details that would not otherwise have been imagined.

### ***Probability of Injury***

Research Question 5 asked: What is the relationship between behavioral compliance and participants' estimates of the likelihood of swing set injuries? A large body of research reported earlier suggests that the use of anecdotes in the media often result in exaggerated estimates of risk. On this basis, one might expect a positive relationship between compliance and estimates of swing set injuries. On the other hand, the consistent (but nonsignificant) findings of the pilot studies reported earlier suggests participants who read injury stories in the context of safety messages produced *lower* estimates of risk. These findings suggest a *negative* relationship between anecdotes and compliance when injury stories are presented to emphasize the importance of precautions.

Response form A-3 asked participants to make judgments about the probability of injuries related to the hazards discussed in the swing set safety messages. Team probability judgment scores consisted of the mean individual judgments for the two members of each team.

To test the strength of the relationship while accounting for covariates, hierarchical multiple regression was conducted with total behavioral compliance as the criterion variable and team probability judgment scores as the predictor variable, after the effects of the following covariates had been removed: team scores for age, gender, (log) childcare experience, assembly experience, timing/experimenter, and presence of an observer. Incomplete questionnaire responses were distributed evenly among the experimental conditions, leaving 16 teams in each condition (48 teams, total).

The zero-order correlation between behavioral compliance and team probability judgments was negative, but rather small,  $r = -.29$ . Addition of team probability judgment to the hierarchical multiple regression approached significance,  $F_{Change} (1,40) = 3.36, p = .07$ . Thus, the negative correlation between behavioral compliance and team probability judgments was nonsignificant, but in the same direction as might be predicted on the basis of the pilot studies (and in the *opposite* direction of what might be expected based on reported effects of anecdotes in the popular media)..

In order to determine if message conditions affected team judgments of probability of injury, an ANCOVA was conducted with message condition as the independent variable, team probability of injury scores as the dependent variable, and the same covariates as before.

ANCOVA indicated team probability judgment scores were not affected by message condition, after adjustment for covariates,  $F (2,39) = 1.43, p = 0.25$ .

To sum up, there was a nearly significant negative correlation between judgments of injury probability and behavioral compliance, but differences in probability judgments were *not* affected by safety message condition. Thus, the behavioral effects of stories on compliance were not explained by team judgments of the likelihood of injuries.

### ***Effects of Message Conditions on Ratings of Swing Set Safety***

Research Question 6 asked: What are the effects on participants' ratings of product safety when stories and concrete nonstory details about injuries are added to traditional abstract safety messages? Based on pilot study data, it was anticipated that participants who read story-based safety messages would rate the swing set as safer, compared with participants who read traditional abstract messages.

An ANCOVA was conducted with experimental condition as the independent variable, team ratings of the safety of the swing set as the dependent variable, and covariates consisting of team scores for age, gender, (log) childcare experience, assembly experience, timing/experimenter, and presence of an observer. Team ratings of safety consisted of the mean of the ratings of the two members of each team. Incomplete questionnaire responses were distributed evenly among the experimental conditions, leaving 16 teams in each condition (48 teams, total).

The ANCOVA result indicated ratings of swing set safety were not affected by message condition, after adjustment for covariates,  $F(2,39) = 0.49, p = .62$ .

## **Summary and Discussion**

This dissertation examined the effects of stories on targeted safety behaviors in a controlled experimental setting. The results supported the notion that story-based messages can have a powerful impact on safety behavior, compared with concrete nonstory and traditional abstract messages. Attempts to identify psychological variables associated with the effect were largely unsuccessful.

### ***Impact of Stories and Other Factors on Safety Behavior***

The results demonstrated that safety messages had a greater impact on target behaviors when those messages included brief stories about people who were injured in the past. Participants who assembled the swing set using instructions with story-based warnings achieved a compliance rate of 87 percent, on average. In contrast, participants who used instructions containing only the traditional abstract warnings had a compliance rate of 76 percent, and those who read instructions containing concrete descriptions of potential injuries (without stories) had a compliance rate of 72 percent. The difference in compliance between the story-based and traditional abstract message conditions was significant after adjustment for covariates. The difference between the story-based and concrete nonstory conditions was significant both before and after covariate adjustment.<sup>4</sup>

These results suggest the effectiveness of stories is not due solely to the inclusion of concrete, vivid details in the text. Concrete nonstory messages were less effective than stories despite the fact that both types of messages contained detailed, vivid descriptions of how injuries can occur, using language that was equivalent in terms of reading level, inclusion of personal pronouns, passive/active sentence structure, number of words, and number of sentences. Furthermore, concrete nonstory messages were no more effective than traditional messages that were abstract and pallid. Taken together, these findings suggest stories were effective for reasons other than mere concreteness or vividness.

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<sup>4</sup> To put these figures into perspective, a compliance rate of 45 percent would be expected by chance.

Besides the inclusion of stories in safety messages, other factors had an impact on safety behavior. Gender, age, (log) childcare experience, equipment assembly experience, presence of an observer, and research timing/experimenter were also important. In fact, the timing/experimenter effect was as important as, or more important than, the inclusion of stories in some analyses. In all three experimental conditions, participants performed better when trials were conducted by Experimenter 1 (Ricketts). Importantly this effect did *not* interact with message condition, indicating the superiority of story-based messages was not due to the timing/experimenter effect.

Results strongly suggested that the timing/experimenter effect was mainly due to differences in the population of participants sampled by the experimenters, rather than characteristics of the experimenters themselves. Experimenter 1 (a 49-year-old male professional employee of the University) conducted experimental sessions mostly during evenings and weekends in the first few weeks of the fall semester. In contrast, Experimenters 2 and 3 (female undergraduate students in their early 20's) conducted sessions mainly during regular school hours throughout the spring and summer academic semesters. Participants in Experimenter 1's sample reported significantly more experience at assembling play equipment and were significantly more likely to be female, compared with participants sampled by the other experimenters.

Analyses of the effects of timing of experimental sessions also suggested the timing/experimenter effect was *not* due to differences inherent in the experimenters. In fact, the effect of experimenters was *nonsignificant* after the effects of timing were statistically removed. For Experimenter 1, behavioral compliance increased as follows: regular class hours (lowest compliance), lunch hour (higher), evenings (higher still), and Saturdays (highest). Fully 32 percent of the variation in behavioral compliance for Experimenter 1 was accounted for by variation in the timing of experimental sessions! In analyses that included all three experimenters, timing of experimental sessions explained 18 percent of variation in behavioral compliance, after adjustment for the covariate of experimenters.

These analyses strongly suggest that timing/experimenter effects were due mainly to the effect of timing, rather than the effect of experimenters. Participants who volunteered for evening and weekend sessions early in the fall semester differed from



other participants in that they reported more equipment assembly experience and were more likely to be female.

Great care was taken to ensure that the effects of potentially confounding factors were randomly distributed over message conditions. In addition, gender, age, (log) childcare experience, equipment assembly experience, presence of an observer, and timing/experimenter were included as covariates to account for their effects in statistical analyses. After statistically removing the effects of these six covariates, the impact of message conditions was strengthened. This finding demonstrates the inclusion of stories had a genuine impact apart from the covariates.

To sum up, safety behavior was affected by many factors, and the inclusion of stories in safety messages was clearly among the most important of these.

### ***Cognitive and Emotional Impact of Safety Messages***

A number of cognitive and affective factors were examined in an attempt to gain some insight into how the safety messages affected behavior. Most of the analyses were nonsignificant, and added little to indicate how the stories exerted their influence.

#### ***Narrative Transportation***

Based on the Transportation-Imagery Model of persuasion, an attempt was made to determine if team members became absorbed in and emotionally affected by the stories of injury victims. The experience of narrative transportation was found to be no greater for participants who read story-based messages than for participants in the other conditions. This suggests that either the stories were not very engaging or the experimental task left participants with few available cognitive and emotional resources with which to become transported. On the other hand, when the story-based condition was subjected to a separate analysis, it was found that the experience of transportation *was* positively related to compliance.

These conflicting findings suggest that narrative transportation may increase the likelihood of compliance, while also suggesting that story-based messages did not result in increased transportation compared with other messages. In short, the only effects that were significant with respect to transportation occurred *within* the story-based condition and not between this and other conditions. This finding may not actually conflict with the

Transportation-Imagery Model because that model is based on narrative experiences rather than nonstory reading experiences. It is also important to recognize that much of the research on the Transportation-Imagery Model has been conducted in the context of stories that are meant to entertain. The stories used in this dissertation were meant to change behavior, but not to entertain.

### ***Reminders***

It has been suggested by Roger Schank and others that stories affect behavior in part by reminding us of related stories that we have in memory. Nonstory communications, in contrast, are less likely to remind us of old stories and thus should have less affect on behavior. The results of the present study do not support these notions. First, participants who read story-based messages reported only about half as many reminders as participants who read concrete nonstory messages. Furthermore, the number of stories recalled was not significantly related to behavior.

It is important to remember that the context of the present research is quite different from the setting in which Schank developed his notions of reminders. Schank has traced his interest in stories to early work on artificial intelligence. That work encouraged him to explore the nature of human memory. Among other observations, Schank noted that during engaging conversations, people are reminded of related stories from their past. While conversing, people do not necessarily listen to one another. Instead, each person seems to mull over his/her own reminders while the other person is talking. As soon as there is a pause, the person who has been “listening” jumps in to convey the new reminders in the form of another story. Thus, Schank’s reminders have a social or conversational function in addition to their effect on memory. In contrast, the story-based messages used in this dissertation did not serve any obvious social or conversational function. In short, the notions of Schank may not be applicable in the context of the current research.

On the other hand, this study was not the first to find problems with Schank’s notion of reminders. Graesser and Ottati (1995) found that reminders do not seem to occur very often in conversations, and Larsen and Seilman (1988) found that just as many reminders were triggered by reading nonstory text, compared with stories. Perhaps it is

time to better define the conditions under which reminders are triggered, as well as their presumed impact on behavior.

### ***Participants' Descriptions of Concrete Hazards, Injuries, and Preventive Measures***

There was a significant positive relationship between behavioral compliance and participants' immediate recall of message content. In addition, a positive correlation between compliance and novel concrete responses approached significance. On the other hand, story-based messages did *not* lead to greater recall or elaboration, compared with the other messages. Thus, there was no support for the notion that story-based messages were effective because they brought to mind concrete details that would otherwise not have been imagined.

Memory is often used as a surrogate measure of message effectiveness in safety and health research.<sup>5</sup> If memory had been used as a surrogate measure in this experiment (instead of observed compliance), there would have been no indication that the story-based messages were more effective. Memory, therefore, would *not* have been a valid surrogate measure for the target behavior in this experiment.

In this regard, it is important to remember that participants completed the initial memory task immediately *after* assembling the swing set. It is not possible to determine whether they recalled the information (1) because they had read the safety messages, (2) because they had successfully performed the assembly task, or (3) because of some other reason.

It is also important to remember that assembling the swing set was a well-defined task that took place in a brief period of time, requiring only short-term working memory.

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<sup>5</sup> Recall has also been used as a measure of message effectiveness in advertising and in the general persuasion literature. Contrary to what might be expected, attitude change has generally *not* been highly correlated with memory. For this reason, persuasion researchers are increasingly using alternative measures of persuasion such as the number of positive and negative thoughts reported in response to persuasive messages (e.g., Petty & Wegener, 1998). Similarly, due to the typically low correlation between recall and sales, some advertising researchers have suggested abandoning traditional measures of explicit memory in favor of implicit memory, as indicated by perceptual priming (e.g., Lee, 2002). Measures such as these were not used in this dissertation. However, such measures may be worth exploring in safety and health studies when target behaviors can not be directly observed.

Participants had no reason to expect they would be tested over the information afterward, so there was no reason for them to commit it to memory.<sup>6</sup>

Given the context of the experimental task, therefore, it is not especially surprising that memory was related to behavioral performance, but unrelated to message condition. Different results might be expected in research contexts that require information to be used after a time delay.

### ***Perceived Probability of Injury***

It has sometimes been argued that perceptions of risk are related to risk-reduction behaviors. Furthermore, a large body of research suggests that anecdotes about injuries lead to inflated perceptions of risk. This dissertation provided no support for either of these notions. First, there was a negative correlation between perceived probability of injury and behavioral compliance that closely approached significance. This finding was consistent with the nonsignificant trend seen in the pilot studies. Second, message condition had no impact on perceived probability of injury. In other words, story-based messages did *not* lead to increased perceptions of risk.

One can only speculate as to why there the correlation between perceived risk and behavioral compliance was (nonsignificantly) negative. It is important to keep in mind that participants made their risk judgments *after* completing the swing set. It is conceivable that those who complied with most of the safety messages were feeling confident that they had taken appropriate actions to reduce risk. In a state of confidence or greater positive affect, the risks described in the questionnaire might have seemed remote. On the other hand, participants who had not noticed the safety messages sometimes commented that they first learned about the hazards as they were filling out the questionnaire, and they began to worry about their performance at that time. This suggests that the questionnaire's mention of the hazards may have led to increased perception of risk and negative affect among those who performed poorly on the assembly task. If this was the case, the negative correlation between compliance and

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<sup>6</sup> See Hastie and Park (1986) for a discussion of how recall affects "memory-based" versus "on-line" judgment tasks.

judgments of risk seems reasonable, based on notions such as the affect heuristic (e.g., Finucane, Alhakami, Slovic, & Johnson, 2000).

### ***Ratings of Swing Set Safety***

Finally, participants' ratings of the safety of the swing set were not affected by the message conditions. This is an important finding. Taken together with the results discussed for probability of injury judgments, the findings indicate that explicit anecdotal injury reports did *not* make this product (or swing sets in general) seem unreasonably dangerous. To put this another way, there was no evidence to suggest that the anecdotal messages *in this study* created any mistrust of the product or class of products.

Why did the anecdotes fail to create mistrust of the product? One possibility is that there may be some threshold at which injury anecdotes exert their effect. If this is the case, perhaps the anecdotes used in this study did not exceed that threshold.

Another possibility relates to how the anecdotes were used in this study. Stories in the assembly instructions were designed to illustrate the importance of taking effective precautions. They were not intended to sensationalize injuries, imply negligence, or to suggest that the world is a dangerous place. One might even speculate that participants understood that the stories were provided to help them build a safer product (which, after all, was the intended purpose).

In contrast, stories about injuries in news reports or product recall announcements often implicate poor product design, faulty manufacturing, and perhaps most importantly, deception by untrustworthy corporations. It is worth noting that Paul Slovic and colleagues (e.g., Slovic, 2000) identified *trust* in organizations and experts as a key component in the public's acceptance of societal level risks. Consider, for instance, public perceptions of the nuclear power industry. The general public has always seemed somewhat uneasy with nuclear power. However, assurances of elaborate safety procedures were at one time enough for the industry to gain grudging acceptance by the public. The details of nuclear energy seemed beyond the understanding of common citizens, so people historically had no choice but to place their trust in nuclear power experts and the government agencies that regulate them. This trust was freely given so long as there were no serious accidents. Of course, a serious accident did occur. The story of Three-Mile Island destroyed public confidence in the nuclear power industry.

Furthermore, outrage over the incident seemed to be rooted in a sense that trusted experts had betrayed the public confidence.

Another story of public deception and betrayal is developing at the time of this writing—and it also seems to be leading to an exaggeration of risk. Throughout late 2007, the public has been bombarded with endless stories of hazardous children's toys imported into this country from China. It has been found, for instance, that some toys are contaminated with lead and other toxic substances. Outrage seems to be related at least in part to the perception that toymakers have a public trust to protect the welfare of children. When this trust is broken by the story of a dangerous toy, people may begin to wonder whether other toys are safe. The resulting spiral of distrust currently seems to be leading to a perception that *all* toys from China are dangerous.

In contrast, the stories in this dissertation did not imply any betrayal of trust on the part of the swing set manufacturer. On the contrary, the frankness of the warnings may have reassured participants that the manufacturer was being completely open about all of the important ways that children could be hurt while using the product. Slovic's notion that acceptance of risk is related to trust suggests that manufacturers have more to lose by denying risks than by informing the public openly. Additional research is needed to determine the circumstances under which information about hazards may serve to either destroy or build confidence.

### ***Explaining the Effects of the Intervention: Changing Their Behavior Without Changing Their Minds?***

How could story-based messages have exerted such a dramatic effect on behavior without resulting in a similar impact on cognition or emotions? One answer is suggested by a series of observations made by the three experimenters.

First, the subjective impression of the experimenters was that some participants were very conscientious. These individuals carefully read the instructions and safety messages, and they probably would have performed well in any experimental condition. At the opposite extreme, some participants appeared overly confident and did not seem to read any text, not even the safety messages. These participants only seemed to look at the pictures, and it is unlikely that any text-based safety messages could have gotten through to them.

Most participants fell somewhere between these extremes. These participants usually scanned the text quickly, including the safety messages, but relied largely on the drawings. These participants often assembled much of the swing set without observing the safety messages. Then at some point, a particularly relevant safety message might have caught their eye. After reading that message, they sometimes went back over the other safety messages and corrected their mistakes. It appeared to the experimenters that the story-based messages exerted an impact by being noticed more often than the other messages. Often, one of the participants would notice a story-based message and remark to the other team member that some child was killed because, for instance, the bolts protruded too far. Then the other participant might read the message and remark that perhaps they should read over the other warnings.

Thus, it appeared to the experimenters that story-based messages were more likely to capture participants' attention, compared with the other messages. This observation is also consistent with comments made by participants in the gasoline pump pilot study reported earlier.

The observation that stories captured participants' attention is similar to a notion expressed by Taylor and Thompson (1982). In discussing the effects of vivid versus pallid information, Taylor and Thompson noted that vividness may exert an effect only under conditions of *differential attention*, i.e., when research participants are presented with both vivid and pallid stimuli at the same time. One might apply this notion to the present case by asserting that the safety messages were more likely to be noticed because they were vivid enough to compete for attention with the illustrations. This possibility is supported by one effect that closely approached significance, namely that stories seemed to affect compliance with explicit safety messages (which included anecdotes) more than with the other safety messages that were sprinkled through the text without anecdotes.

Although these observations support the notion that differential attention may have played a role, other evidence suggests this may not have been the case. For instance, if story-based messages were more effective at capturing attention, why was their content not remembered better than the content of nonstory messages? Also, why were concrete nonstory messages unable to capture attention, given that they also contained vivid details?

One possible explanation for these results was suggested by my dissertation advisor (James Shanteau, personal communication, November 10, 2007). Perhaps the personal impact of a story motivates the reader to attend to the message. Attention then leads to better decision making and performance. If there is an anticipated need to use the information in the future, then the details will be committed to memory; otherwise, not. More research is needed to determine the interaction of motivation, differential attention, memory, and task context in story-based communication.

### **Limitations of the Research**

This experiment was an effort to examine the impact of stories on observed safety-related behavior involving a limited set of tasks in a controlled experimental setting. The study makes a contribution because there has been little direct behavioral evidence to back up claims in the literature regarding the supposed benefits of stories in safety and health communication. Still, there are limits regarding the extent to which one may generalize from this narrow experimental context. It is anticipated that this study will be followed by other controlled studies in a variety of settings. In the meantime, the following limitations must be remembered when interpreting the results of this study.

It is important to consider that effective strategies for product safety communication may differ from message strategies involving safety and health in other contexts. For instance, a product warning is designed to influence a specific set of behaviors in a well-defined task. In contrast, many safety and health messages seek to influence a broad range of behaviors in complex settings over an extended period of time. In this regard, the present experiment had a substantial effect on behavioral compliance even though there was little impact on beliefs and judgments. This finding suggests the observed impact on behavior might not generalize to other contexts and times. For instance, imagine that each of our participants were to design and build their own homemade swing sets six months after our experiment. Would participants who had been in the story-based condition be more likely to remember and comply with the safety messages from the experiment, compared with other participants? More research is needed to determine how changes in safety and health behavior transfer to different times and contexts.



It is important to consider that the present study examined compliance with safety messages that involved protecting *others* (i.e., children who were imagined to be using the swing set in the future). In contrast, most previous studies of story-based safety and health messages have addressed hazards and preventive measures as they relate to the participants' *own* safety and health. Further research is needed to determine if the effectiveness of story-based communications interacts with the target of the hazard (i.e., self v others). For instance, one might expect complex interactions involving individual difference variables. In this regard, it seems reasonable to predict that an empathetic person (say, someone who scores high on the personality trait of "agreeableness") may be more likely to take precautions when the hazard affects someone other than the self (such as a vulnerable child). In contrast, a person who is motivated more by self-interest (someone who scores *low* on agreeableness) might be more likely to take precautions when the hazard affects the self.

It is also important to note that participants were general psychology students, primarily Caucasian, mostly females, age mainly in the late teens to early twenties. Caution is in order when attempting to generalize to other populations such as middle-aged parents, experienced tradespersons, the elderly, middle school students, and others.

It must also be remembered that this experiment examined decision making and safety behavior in two-person teams. It is not clear to what extent the results can be generalized to the behavior of individuals who are working alone or to teams consisting of three or more persons.

Related to the issue above, it is possible that the results may have been strongly influenced by interpersonal dynamics. For instance, the more dominant members of some teams may have had a greater impact on team results, compared with less dominant "followers". While this may have occurred, it is not necessarily a limiting factor. Many everyday tasks are performed by teams and are influenced by social dynamics. The experimenters observed that some teams did maintain stable dynamics consisting of a dominant leader and a submissive follower. This seemed rare, however. In most teams, decision making was shared to varying extents, with each team member making some important decisions. Furthermore, leadership roles occasionally reversed completely during the course of the task. This typically happened when an initially submissive

partner “took control” after noting mistakes made by an inept leader. In sum, the social dynamics seemed to approximate interactions that may occur in other settings.

One might wonder whether participants took the same care in assembling the experimental swing set as they would have taken for children actually under their care. This limitation was addressed in part by asking participants to imagine they were assembling the swing for a particular child. Many participants mentioned the child repeatedly as they worked in the product. Upon noticing a mistake, for instance, one participant might say something like, “Do you think we should fix that?” In response, the other participant might say, “We’re putting this swing set together for my niece, Julie. You’d better believe we’re going to fix it!”

Experimenters also observed that most teams appeared to become completely engrossed in their task. Participants remarked frequently that the task was challenging and enjoyable. Many participants also asked specific questions during the debriefing, indicating concern about their performance and about particular safety issues. Across all conditions, participants achieved a compliance rate of 78 percent (chance was 45 percent), which seems encouraging given the pessimism commonly found in literature regarding safety and health behaviors in general. In sum, it appeared that most participants were fully engaged and tried to do their best.

Given the preceding statements, one might alternatively speculate that participants were *overly compliant* because they knew their behavior was under scrutiny. Again, observations by experimenters suggest this was not a common problem. It was often noted that participants appeared to skim over warning information in much the same way that people do in everyday settings. It was also common for participants to assemble much of the swing set incorrectly before noticing that the warnings were relevant. This behavior suggested participants did not anticipate that experimenters were interested in their compliance with warnings.

To sum up, this experiment represents an attempt to examine the impact of stories on specific safety behaviors in a controlled environment. When interpreting the results, one must consider the controlled nature of the setting, the specificity of the task, and the demographic composition of the participants.

## **CHAPTER 4 - Conclusions**

This research demonstrated that safety messages had a greater effect on observed behavior when those messages contained brief stories about people who had been injured in the past. This behavioral effect occurred even though the stories were not designed to be entertaining or transporting.

The research also demonstrated that behavioral effects occurred without accompanying changes in recall or judgments about the likelihood of injuries. No clear explanations of the results emerged by examining psychological constructs involving self-reported transportation, reminders, judgments of injury probability, or memory.

### **Implications for Safety and Health Professionals**

The research has practical implications for safety and health communicators. As a practitioner myself, I would like to discuss these implications briefly.

First, the results indicate safety and health messages may be more effective when they include stories about people who have been injured in the past. However, it is important to keep in mind that the stories used in this research were not overly graphic, they did not sensationalize the suffering of the victims, and they were designed to support (not distract from) the central theme of the warnings. The messages merely served to illustrate what can happen when precautions are not observed. Although not examined in this study, one might speculate that too much graphic or sentimental detail could cause a defensive reaction among message recipients. More research on this topic is needed.

Along these lines, it is important to note that the injury stories used in this study did *not* cause distrust of the product (the swing set) or the class of products (swing sets in general). Safety and health professionals often find themselves engaged in a balancing act. On the one hand, they must bring attention to hazards and convince people to take precautions. On the other hand, they must not cause people to panic. Judicious use of brief, but not overly graphic, injury reports may represent one way of arousing enough interest to stimulate action without creating undue alarm.

In order to keep participants from immediately seeing through to the real focus of the study (attention to safety messages), several warnings related to long-term use and maintenance of the product were inserted at the beginning of the assembly instructions. Although these warnings would be important for product *users*, they were completely irrelevant for product *assemblers* (our participants). It appeared that these irrelevant warnings did succeed in keeping participants unaware that safety compliance was the behavior of interest. However, observations suggest this common practice of including “user precautions” in assembly manuals should be abandoned outside of the research setting.

For instance, all three experimenters noted that participants typically began by scanning the initial safety messages. However, upon discovering that the warnings were unrelated to assembly, many participants seemed to shift their attention away from the warnings in order to focus on parts of the manual that were more relevant to the immediate task. In fact, many participants later made comments such as, “I started to read the warnings, but they weren’t about putting together the swing set, so I stopped reading them.” This suggests the *user* precautions served as good distracters for research, but should *not* be included at the beginning of an actual product *assembly* manual. A better place for such messages might be in a separate user’s manual or on the product itself.

After seeing a few irrelevant warnings, participants seemed to (1) pay most attention to the assembly illustrations, (2) pay somewhat less attention to the assembly text, and (3) pay least attention to the explicit warnings that were set apart from the rest of the text. A trend that closely approached significance also suggested that compliance was better for safety messages included as part of the text, compared with messages that were set apart as explicit warnings. Taken together, these observations suggest the following practices for designing product assembly instructions:

1. Compliance with safety messages may be enhanced if product assembly manuals include only those warnings that are essential to the task at hand. If company attorneys demand a laundry list of all possible hazards for liability reasons, consider including them in a separate flyer dedicated to remote risks and other legally-necessary messages.

2. If possible, include the most important safety messages where they are most likely to be noticed and heeded. This may require placing them in the illustrations and/or in the assembly text. If the public has grown weary of traditional warnings, it may be necessary to make them appear more relevant to the task at hand.
3. Even the best safety messages will not prevent all injuries. As documented by Don Norman (1988) errors will be minimized if products are designed so that their proper use is readily apparent. In this dissertation, the potential for a strangulation hazard was created by design, i.e., the swing set's components required bolts of different lengths. If participants accidentally substituted a long bolt in place of a short one, the long bolt would protrude too far and might entangle a child's clothing. In a genuine consumer product, the strangulation hazard might have been avoided altogether by designing the swing set so that it could be properly assembled with bolts of the same length. If this was not possible, the diameters of the bolts might have been designed to covary with length so that only a bolt of the proper length would fit in a particular hole. If the bolts could not be designed in these ways, then perhaps novel types of warnings could be used to make the hazard clear. For instance, safety messages on adhesive tape could be placed over holes requiring shorter bolts. The assembler would have to actively remove the warning tape in order to insert the bolt, increasing the likelihood of noticing the message.

Similar recommendations have been made by other researchers. Many innovative ideas can be found in the book, *Warnings and Risk Communication* (Wogalter, DeJoy, & Laugherty, 1999).

Although this dissertation examined the use of stories in product warnings, my true interest as a practitioner involves the use of stories in safety training. My observations over the past 15 years as a safety communicator lead me to make the following suggestions for trainers. None of these observations has been tested in a research setting, so I will simply describe how I use stories, rather than making assertions about how stories *should* be used.

Perhaps the most important observation is that trainees may become fatigued by too many injury stories. A variety of training techniques (including some stories) seems to work best for me. I prefer to begin training with one relevant injury story and then ask trainees if they would care to share any similar stories of their own. In most groups of experienced workers, several trainees will have relevant stories to tell (and these will often have more impact than my own stories). After a story from me and two or three stories from the audience, the trainees will usually be ready to engage in the training process. At this time, I normally stop telling stories for a while and focus instead on discussing new information.

In regard to exploring new information, I personally do not like to lecture to trainees because this often causes them to disengage. Instead, I prefer to draw on the existing experience and knowledge of participants by posing realistic problems and asking them for solutions. When it is necessary for participants to learn new procedures or lists of information, I provide these in written form, break the class into small groups, and give each group a realistic scenario problem to solve. I instruct the participants to first solve the problems using their own experience. Then I challenge them to improve on their solutions by using at least three ideas from the new information. Finally, I bring the groups back together to discuss their solutions. During the discussion, participants typically report novel uses of the new information that I could not have anticipated. Thus, the information is learned and applied, usually with no need for me to lecture.

At this point, it is normally time for me to tell another story, and off we go again. However, this time after discussing a story, we might apply new information by incorporating a hands-on exercise instead of a mental scenario.

The lesson is this: Stories serve to make training seem relevant, but it is not necessary to have a separate story for every concept you wish to share. A variety of activities will help participants remain engaged. For a one-hour training, two or three stories from me is usually enough. For a full day of training, one or two stories an hour is plenty.

Communicators can obtain injury stories from a variety of sources (e.g., NIOSH, n.d.-a; OSHA, n.d.-b). The stories must usually be re-written to eliminate unnecessary details and to clarify cause-effect relationships. I have found that the best training

discussions result from stories having the following characteristics: (1) a clear cause and effect, (2) a cause that could have been controlled by trainees if they had been in a similar situation, (3) a relevant setting and problem, and (4) a character with whom participants can identify. The best stories are also those that evoke counterfactual thinking, i.e., the victim would still be alive if only s/he had observed the safety practice in question.

In regard to *less* effective stories, I have found that participants get sidetracked by events described in such a manner as to be outside their control. For instance, if trainees are *line workers* and a story implies an accident was caused by poor management practices, outrage and complaining (rather than learning) are likely to result. In contrast, when trainees are *managers* the same story can promote valuable introspection. I have also found that stories tend to be ineffective when the victim is described in a way that implies lesser competence (e.g., a “new,” “young,” or “untrained” employee). In these cases, experienced workers often dismiss the injury as something that would never happen to them.

Stories tend to work best when they are brief and contain only enough information to make the event and victim seem real. Participants are distracted from the true point of the story in the presence of endless details about the physical wound, the victim’s suffering, or the grieving survivors. The humanity of the victim and survivors can usually be invoked simply by saying, “co-workers tried to save him, but it was too late. He died on the way to the hospital.” When some details are left to the imagination, trainees fill in gaps about the victim and survivors with images that are more relevant to their own lives.

Again, these are impressions based on my professional experience. Perhaps future research will shed light on their objective validity.

### **Implications for Researchers**

This dissertation has several implications for researchers. First, it *is* possible to study the behavioral effects of story- v nonstory-based safety communications in a controlled laboratory setting. In fact, laboratory manipulations of target behaviors may be especially well suited to the task of discovering the psychological mechanisms behind

narrative safety and health persuasion. This is in contrast to the difficulty of interpreting field studies of self-reported dependent variables and surrogate behavioral measures.

Second, the experiment demonstrated the importance of controlling for covariates related to safety behavior. Effect sizes in this study were dramatically improved by adjusting for age, gender, (log) childcare experience, assembly experience, presence of an observer, and timing/experimenter. In addition, covariates were invaluable for understanding the nature of the timing/experimenter effect. This effect did not interact with message condition, and thus did not threaten the validity of the results. Still, it was informative to learn that the effect was related mainly to time of day and day of week, and that participants sampled by the different experimenters varied in important characteristics. Without these details, one might question what the results really meant.

Third, the study demonstrated that interventions may have large effects on target behaviors without accompanying effects on surrogate measures such as memory. This suggests that researchers must select dependent variables carefully and exercise caution when interpreting the effects of surrogate measures.

Fourth, by studying teams, researchers benefited by listening to the conversations of team members. For instance, discussions between team members suggested that stories were effective because they motivated participants to take time and read the safety messages. Insights such as this might not have been possible if we had studied (presumably silent) individuals.

Fifth, when conducting power analyses for future studies of observed behavioral compliance, this dissertation suggests effect sizes ranging from  $d = 0.65$  to  $d = 0.86$  (prior to adjustment for covariates). The power analysis conducted before the swing set study incorporated a much lower effect size of  $d = 0.40$  based on self-report data from the pilot studies. This low *a priori* estimate of effect resulted in an unreasonably large estimated requirement for sample size. Practical considerations dictated that the study be conducted with fewer participants. Power analyses for future research may produce more realistic estimates of sample size by using the effects observed in the swing set study.

The results of this research must be interpreted according to the context of the experimental task. Participants assembled a swing set according to written instructions and then answered some questions about the experience. Results might have been



different if there had been a delay of hours, days, or months between reading the safety messages and measuring the dependent variables. This is important because many interventions are designed to affect long-term safety and health behaviors (e.g., daily habits such as wearing safety belts or practicing safe sex). The results might also have been different if participants were required to generalize and apply the information in a different context (e.g., applying the safety messages to a different swing set assembled at some later date without instructions). As with all research, the results must be replicated in other contexts to determine the strengths and limitations of the conclusions.

The results of this dissertation suggest a number of specific topics for future study:

1. The dissertation examined safety messages in the context of *assembly instructions*. In a similar laboratory setting, one could examine how injury stories affect compliance with *safety training*. For example, safety messages could be incorporated into printed training materials. At different intervals after reading the training materials (e.g., no delay, 24-hour delay, and 1-week delay), participants could assemble a product using instructions from which all safety messages have been omitted. Memory-based behavioral compliance would suggest the effects of stories after a delay, which is more consistent with goals of traditional safety training.
2. Future laboratory studies could measure compliance to determine how to create the most effective injury stories and to examine relevant psychological variables related to narrative persuasion. By manipulating the content and style of story-based messages, one could make causal inferences about issues such as narrative transportation (e.g., Green & Brock, 2002), counterfactual thinking (e.g., Tal-Or, Boninger, Poran, & Gleicher, 2004), identification with and empathy for the victim (e.g.,

Oatley, 2002), importance of clear causal connections (e.g., Pennington & Hastie, 1991), and many other factors<sup>7</sup>.

3. One could also examine how the target of the hazard (self v others) affects behavioral compliance. This could be accomplished by manipulating laboratory tasks and messages that so that in some cases the participant is the target of the hazard and in other cases another person is the potential victim.
4. There are many ways to extend the study of safety behavior to field settings. To illustrate how this could be done, here are two narrow examples using topics examined in this dissertation:
  - a. The swing set study could be adapted to a field setting by providing playground safety information to school maintenance personnel or parks & recreation staff. Periodic field inspections of the playgrounds could then document any reduction in hazards.
  - b. The gasoline warning pilot study could be extended to a field setting by any organization that operates its own fuel pumps (e.g., many large universities). This could be accomplished by randomly assigning fueling locations to safety message conditions, attaching the experimental warnings to the fuel pumps, and recording compliance behavior by means of surveillance cameras or inconspicuous observers.
5. While assembling studies for the literature review, I was struck by the fact that “narrative communication” is an overarching category that encompasses a diverse range of approaches. The conflicting research findings of the past may be resolved in part by developing a taxonomy for classifying narrative methods. For instance, consider the differences between this dissertation and interventions based on social learning theory. First, stories in this dissertation were used to *supplement*, rather than

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<sup>7</sup> Additionally, members of the dissertation committee suggested that future studies could examine whether story effectiveness is influenced by communication channel (e.g., oral versus written messages) and whether behavior can be influenced by the story headlines alone (without the accompanying story text).

*replace*, important nonstory information about hazards and preventive measures. In contrast, interventions based on social learning theory often dispense entirely with nonstory text and use the central character to *model* appropriate behavior. Also consider that in this dissertation, each story described a tragedy that materialized (*loss frame*). In social learning theory, on the other hand, the central character may avoid tragedy by taking appropriate actions (*gain frame*). As another example, contrast the brief, non-entertaining anecdotes used in the present dissertation with the literary works used as stimuli in studies involving the Transportation-Imagery Model. It seems a stretch to conclude that all narrative researchers are studying the same phenomena. Furthermore, some researchers have failed to describe their interventions in sufficient detail for others to understand what type of narrative communication took place. In some cases, it is not even clear whether the intervention involved a true story or some other type of exemplar (e.g., simple quotations or testimonials without a plot). As a result, reviews such as the one contained in this dissertation may be comparing apples to oranges. A clear understanding of narrative communication will emerge only if researchers establish and observe some common procedures for reporting the exact nature of their diverse interventions.

6. Finally, I would like to pass on a word of caution for researchers who wish to examine how story-based messages affect actual *injury and illness incidence rates*. One might argue that injury and illness rates matter more than behavior for evaluating safety and health interventions. In this regard, it is important to recognize that there are many factors outside a potential victim's control that determine whether an injury or illness will materialize. Occupational safety and health professionals have often been frustrated by the effects of the aging workforce, severe weather, genetic variations in the pathogenicity of microorganisms, and other uncontrollable factors that cause annual injury and illness rates to fluctuate. These uncontrollable events affect safety and health on scales as

small as individual workplaces (e.g., ice forming overnight in the parking lot due to an undetected break in a city water line) and over broad geographic regions (e.g., Hurricane Katrina). The modern trend among safety and health professionals is to measure success by focusing on factors that are targeted by the intervention *and* within reasonable human control. Such measures typically involve observed behavioral compliance with recommended safety and health practices. Concentrating on proactive behavior also makes it possible to anticipate (rather than react to) problems by attending to leading (rather than trailing) indicators of change. Furthermore, safety and health professionals have long noted that due to demand effects, workers typically under-report injuries and illnesses after an intervention. This problem of under-reporting can be avoided by measuring preventive behaviors, rather than injury and illness outcomes. It is also important to note that serious injuries and illnesses are rare events, making it difficult to find statistically significant differences in the low incidence rates of treatment and control groups. The difficulty of using incidence rates to evaluate interventions is illustrated by the fact that almost 2 million research participants were required to verify the effectiveness of Salk's polio vaccine (Dawson, 2004). In sum, researchers who wish to use injury and illness incidence rates as measures of message effectiveness should be prepared to collect massive amounts of data, preferably over broad geographical regions, and for long periods of time if they wish to dampen the statistical impact of uncontrollable events and demand effects.

### **Final Comments**

Increasingly, stories are being recommended for many persuasive purposes. This dissertation demonstrated that injury stories can be used to increase the effectiveness of safety messages. Apart from this dissertation, however, there is little hard evidence regarding the effects of stories on *objective measures* of safety and health behavior. Thus, it would be premature to draw strong conclusions from this single study.

If we are to continue recommending the widespread use of stories in important communications, we must accept the responsibility to study actual target behaviors by direct observation in controlled settings. When consistent trends become apparent in the laboratory, field experiments can determine whether laboratory results generalize to other contexts. Stories must also be pitted against legitimate nonstory alternative messages, and all interventions must be described in sufficient detail for others to determine the type of narrative communication that took place. Finally, care must be taken to ensure that competing messages are made equivalent in all respects except the minimal characteristics that uniquely define a story (e.g., a specific character involved in a progression of events through time). One way to help ensure message equivalence on potentially confounding factors is to involve researchers with opposing views through adversarial collaborations. If rigorous studies conducted in competing laboratories eventually yield a consistent pattern of findings, we may finally be able to say with confidence whether stories do or do not represent a uniquely effective form of communication.

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## Appendix A - Scoring Procedures for Recall in Pilot Studies

**Table 15 Pilot Study Scoring Procedures, Part 1**

Safety Topic	Recall Item (Points in Parentheses)
Gasoline Pump Safety (2 points)	Touch the car before fueling (1): Do not re-enter car while fueling (1)
Mowing Safety (12 points)	Never mow when grass is wet (1) Always PUSH the mower (.75) and: a. Never PULL (.25) For walk-behind mower, mow ACROSS hills (.75) and: a. don't mow UP/DOWN (.25) When re-fueling, shut off mower (0.33) and: a. let cool (0.33) b. for 5 minutes before re-fueling (0.34) Don't fill gas tank completely full (0.67): a. three-fourths full is maximum (0.33) Clean up spills immediately (1) Get to shelter if you see lightning (0.5) or: a. hear thunder (0.5) Get inside (.25): a. sturdy building (0.375) b. hard-top vehicle (0.375) After storm, wait (0.33): a. 30 minutes (0.33) b. after last lightning flash (0.33) Wear safety glasses (1) <i>Continued on next page</i>

**Table 16 Pilot Study Scoring Procedures, Part 2**

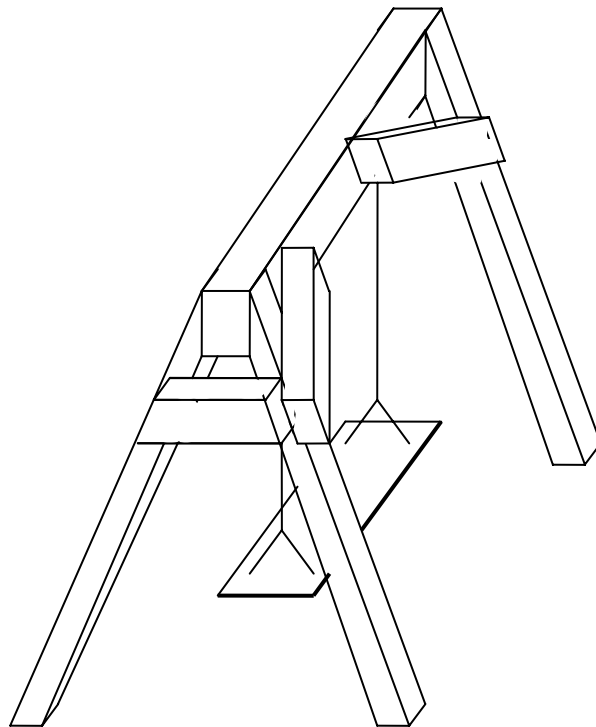
Safety Topic	Recall Item (Points in Parentheses)
Mowing	<i>Continued from previous page</i>
Safety	Pick up debris (0.5) <ul style="list-style-type: none"><li>a. in the mowing path (0.4)</li><li>b. before you mow (0.1)</li></ul>
	Shut off the mower to cross a sidewalk, driveway, or road (1)

**Appendix B - Swing Set Assembly Instructions: Traditional  
Abstract Warnings**

# Wooden Preschooler Swing

## Assembly Instructions

*Prototype Model 43-256, Model 43-257, and Model 43-258*



Important: This product is not intended for children under the age of two. Maximum weight limit for this swing is 50 lbs.

Note: The parts supplied with your model may be slightly different in appearance from those shown in these instructions.

## General Warnings



**WARNING:** After the swing set is in use, check all hardware for wear and damage at least once a month. If you find problems, do not allow children to play on the swing set until it has been repaired.



**WARNING:** To avoid injury, do not allow children to climb on the frame of the swing.



**WARNING:** To avoid injury, do not allow children to jump from a moving swing.

# Parts List

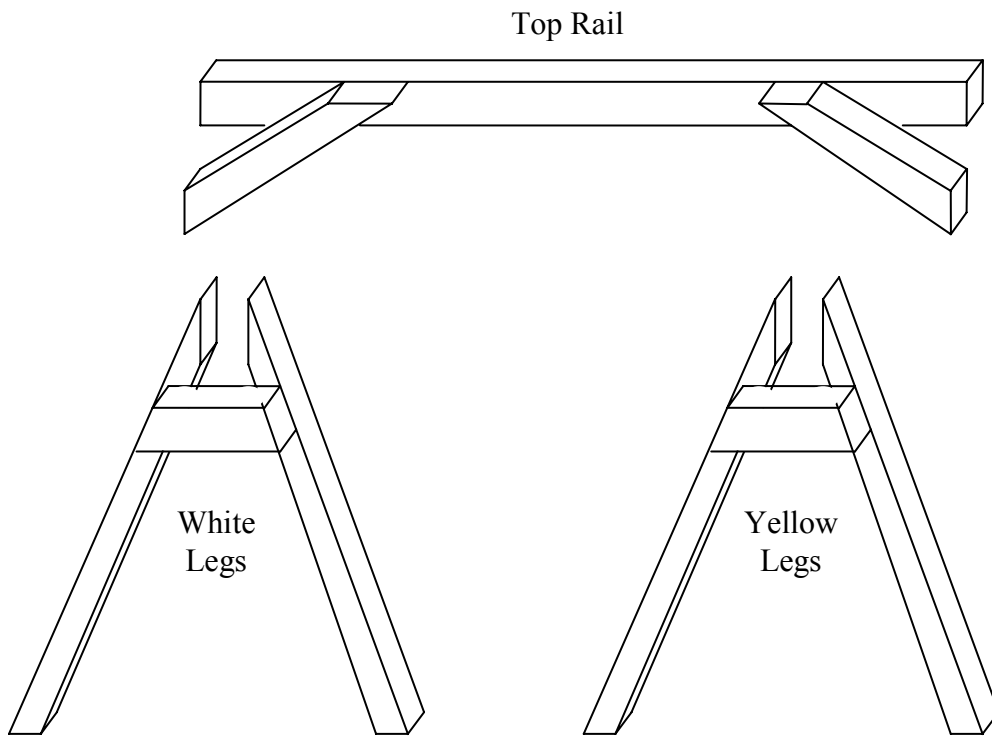
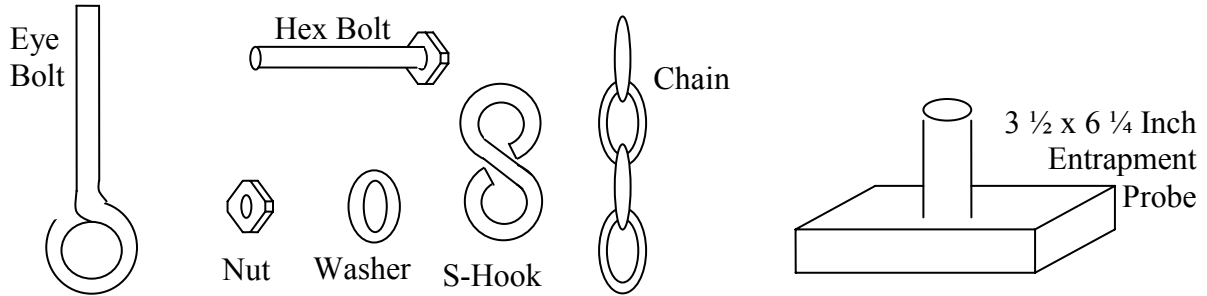


Figure 1



**WARNING:** This product must be assembled according to the instructions in this manual. Failure to follow all instructions could result in collapse of the swing set during use.



# Assembly

## Step 1

Place the white end of the top rail onto the top brace of the white legs as shown in Figure 2.

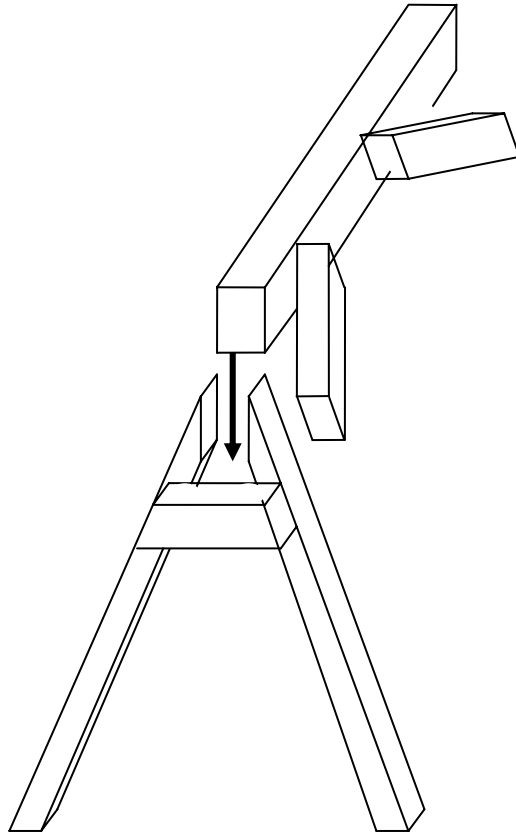


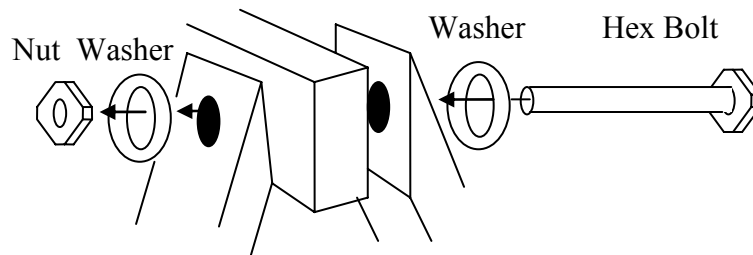
Figure 2



**WARNING:** To avoid injury, do not allow children to use this swing set on hard surfaces such as concrete, asphalt, wood, or packed dirt.

## **Step 2**

Attach the white end of the top rail to the white legs, using one hex bolt, two washers, and one nut as shown in Figure 3. Attach the nut, but do not tighten it during this step.



**Figure 3**

## **Step 3**

Place the yellow end of the top rail onto the top brace of the yellow legs. Refer back to Figure 2.

## **Step 4**

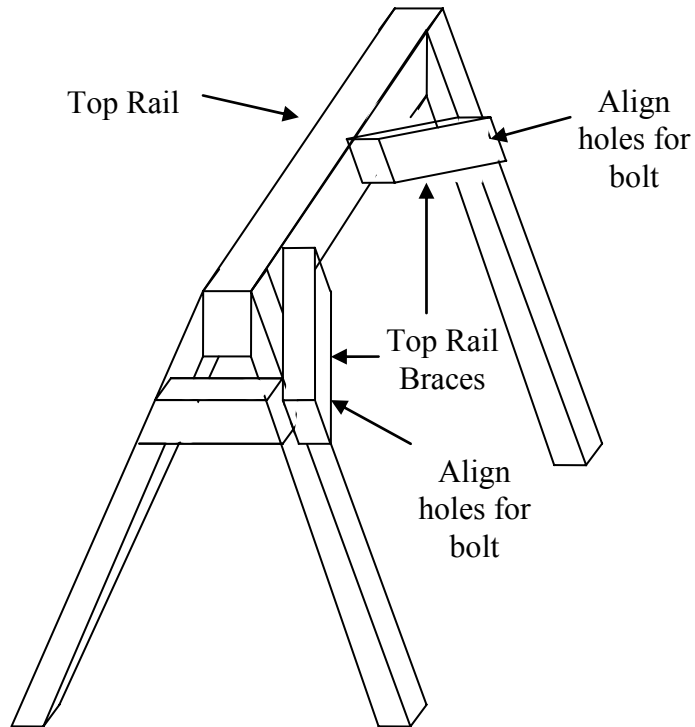
Attach the yellow end of the top rail to the yellow legs, using one hex bolt, two washers, and one nut. Refer to Figure 3. Attach the nut, but do not tighten it during this step.



**WARNING: Use short bolts that will not entangle children's clothing or necklaces. When tightened, the threaded end of the bolt should protrude no more than ¼ inch beyond the nut. If necessary, up to two (2) additional washers may be used as spacers to reduce the amount of thread that protrudes beyond the nut.**

## **Step 5**

Attach the four top rail braces to the legs as shown in figure 4. For each brace, use one hex bolt, two washers, and one nut. Attach the nuts, but do not tighten them during this step.



**Figure 4**

## **Step 6**

Use a wrench to tighten all nuts installed in Steps 2, 4, and 5. Tighten the nuts snugly, but not so tight that you damage the wood.



**WARNING: Make sure openings between the top rail and braces are too small to entrap children. Check openings using the 3 ½ x 6 ¼ inch entrapment probe. No openings should be large enough for the probe to pass through. (Note: The probe is the same size as a cross-section of a small child's chest and shoulders.)**

## **Step 7**

Insert eyebolts in top rail as in Figure 5. Use one washer for each eyebolt.

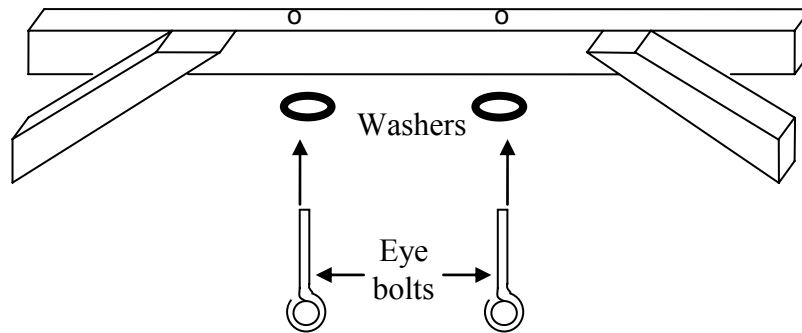


Figure 5

## **Step 8**

Attach eyebolts to top rail, using washers and nut as shown in Figure 6. Tighten snugly, but do not damage the wood.

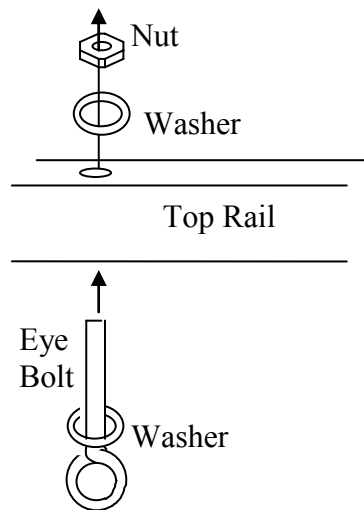


Figure 6



**WARNING:** The two eyebolts should be spaced at least 20 inches apart. This will reduce twisting and side-to-side motion of the swing.

## **Step 9**

Attach swing chains to eyebolts, as shown in Figure 7. Important: do not attach more than one swing to this swing set.

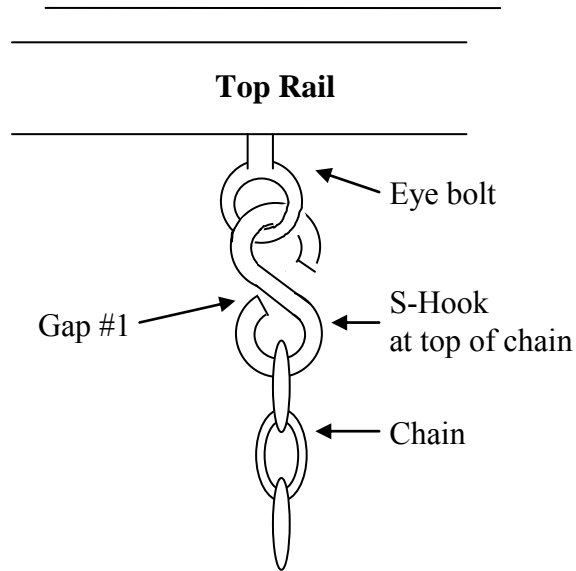


Figure 7

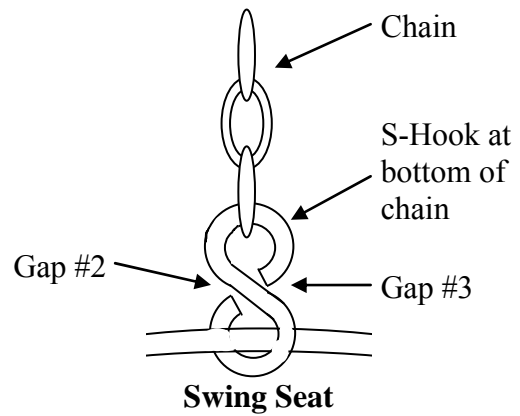


Figure 8



**WARNING:** Make sure S-hooks are completely closed to avoid catching children's clothing and jewelry. S-hooks are considered closed if the 3 gaps shown in Figures 7 and 8 are less than the thickness of a dime. Check the S-hook gaps with a dime to be sure.



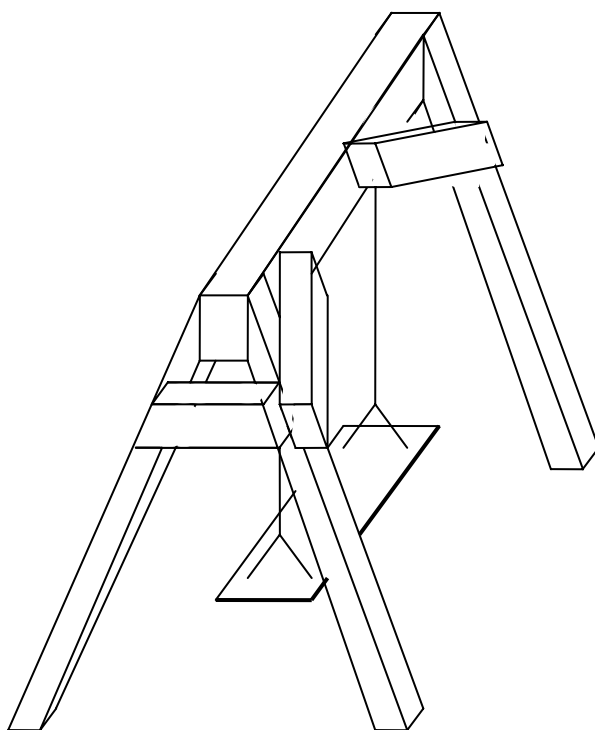
**WARNING:** To avoid injury, do not use heavy, rigid seats. Use lightweight, flexible seats instead.

## **Appendix C - Swing Set Assembly Instructions: Story-based Warnings**

# Wooden Preschooler Swing

## Assembly Instructions

*Prototype Model 43-256, Model 43-257, and Model 43-258*



Important: This product is not intended for children under the age of two. Maximum weight limit for this swing is 50 lbs.

Note: The parts supplied with your model may be slightly different in appearance from those shown in these instructions.

# General Warnings



**WARNING: After the swing set is in use, check all hardware for wear and damage at least once a month. If you find problems, do not allow children to play on the swing set until it has been repaired.**

Boy Paralyzed When Damaged Chain Link Gives Way. A 5-year-old boy was playing on a swing set. As he was swinging, the chain holding the swing snapped and he fell backwards. The boy's neck was broken in the fall. Doctors say he will never walk again. An investigation revealed that the chain broke because it was badly worn from months of heavy use. Source: Tucker and Prentice Case Summary 1:131.



**WARNING: To avoid injury, do not allow children to climb on the frame of the swing.**

Boy Injured In Fall From Swing Set Frame. On August 16, 2005, a six-year old boy was climbing on the frame of a small swing set. Suddenly, he lost his grip and fell to the ground, breaking his upper arm bone (the humerus). Source: Consumer Product Safety Commission Case #50841365.



**WARNING: To avoid injury, do not allow children to jump from a moving swing.**

Girl Jumps Out of Swing, Hits Head, And Loses Consciousness. On April 18, 2005, an eight-year old girl was playing on a swing at school. She jumped out while swinging, but instead of landing on her feet, she fell and hit the back of her head on the ground. The impact knocked her unconscious. She was rushed to an emergency room, where she was diagnosed with a serious closed head injury. Source: Consumer Product Safety Commission Case #50446332.



# Parts List

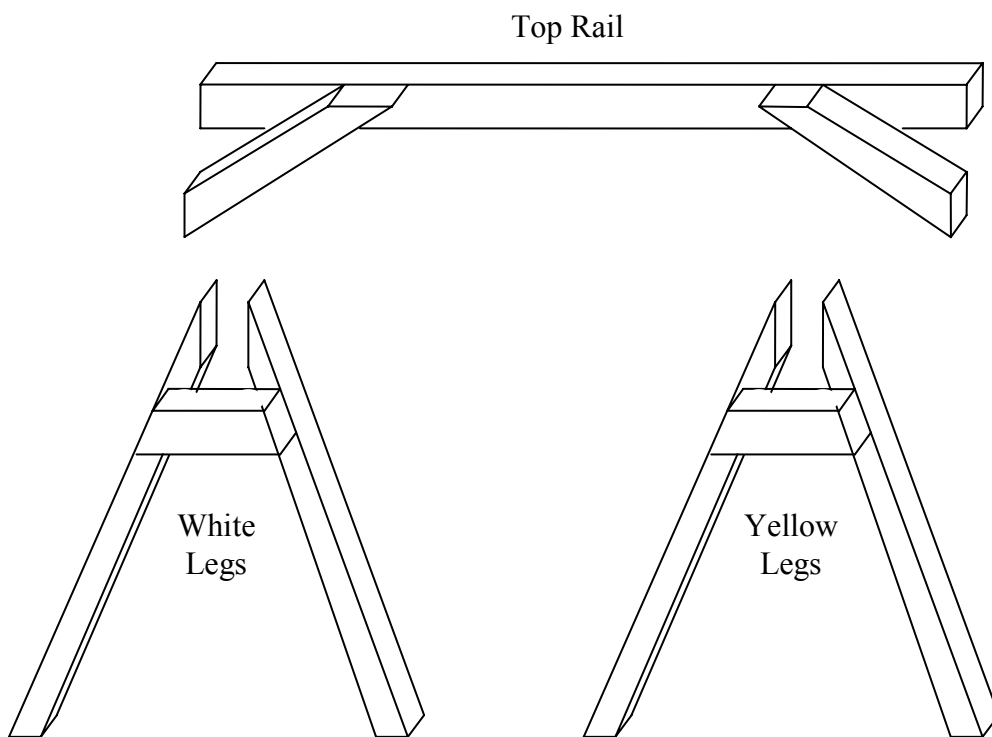
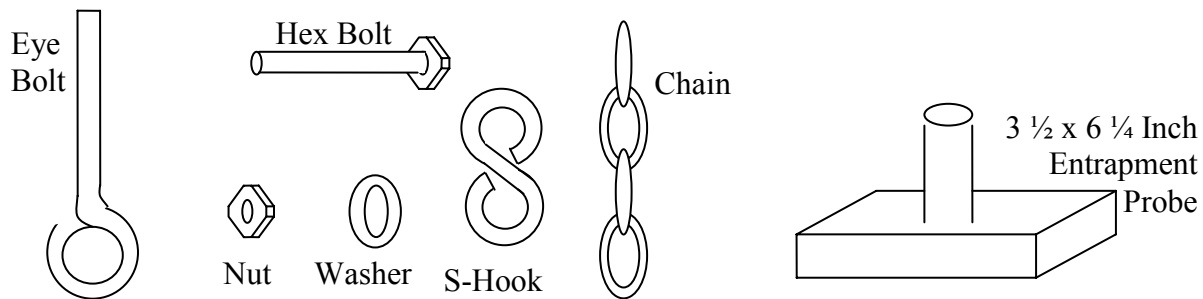


Figure 1

**WARNING:** This product must be assembled according to the instructions in this manual. Failure to follow all instructions could result in collapse of the swing set during use.

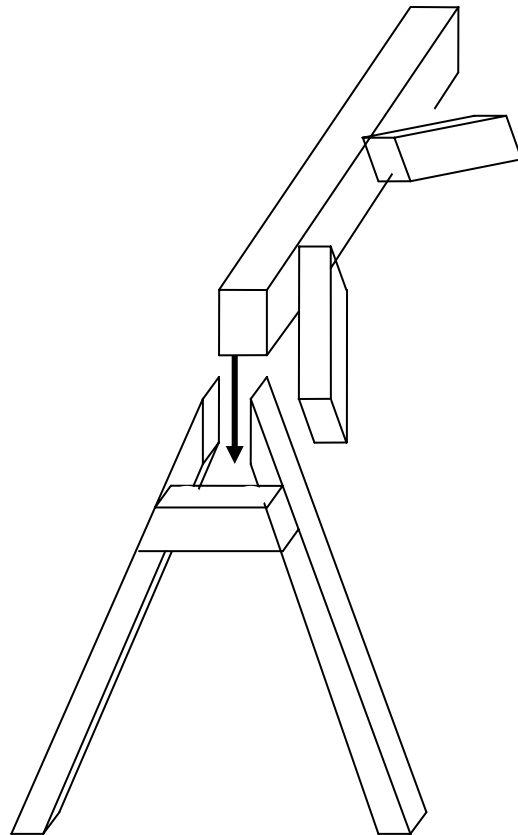


Girl Seriously Injured in Swing Set Collapse. On July 4, 2005, a young girl was sitting on a swing in a park. Without warning, the entire swing set collapsed. The frame of the swing set crashed down, breaking her nose and several other bones in her face. Her injuries required extensive medical treatment. An investigation found that the swing set had not been assembled properly. Source: Consumer Product Safety Commission Case #50711237.

# Assembly

## Step 1

Place the white end of the top rail onto the top brace of the white legs as shown in Figure 2.



**Figure 2**

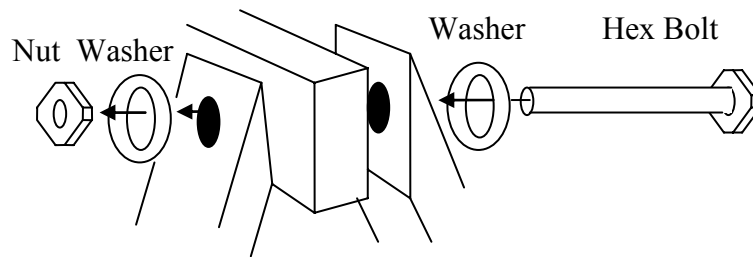


**WARNING: To avoid injury, do not allow children to use this swing set on hard surfaces such as concrete, asphalt, wood, or packed dirt.**

Boy Dies In Fall From Swing. A two-year-old boy was being pushed on a swing by his older cousin. The swing was located over a hard dirt surface. As he was swinging, the two-year-old fell backwards and landed on his head. He immediately lost consciousness and was rushed to a hospital, where he died a few hours later. Source: Plunkett, J. (2001). Fatal pediatric head injuries caused by short-distance falls. *The American Journal of Forensic Medicine and Pathology*, 22, 1-12.

## Step 2

Attach the white end of the top rail to the white legs, using one hex bolt, two washers, and one nut as shown in Figure 3. Attach the nut, but do not tighten it during this step.



**Figure 3**

## Step 3

Place the yellow end of the top rail onto the top brace of the yellow legs. Refer back to Figure 2.

## Step 4

Attach the yellow end of the top rail to the yellow legs, using one hex bolt, two washers, and one nut. Refer to Figure 3. Attach the nut, but do not tighten it during this step.

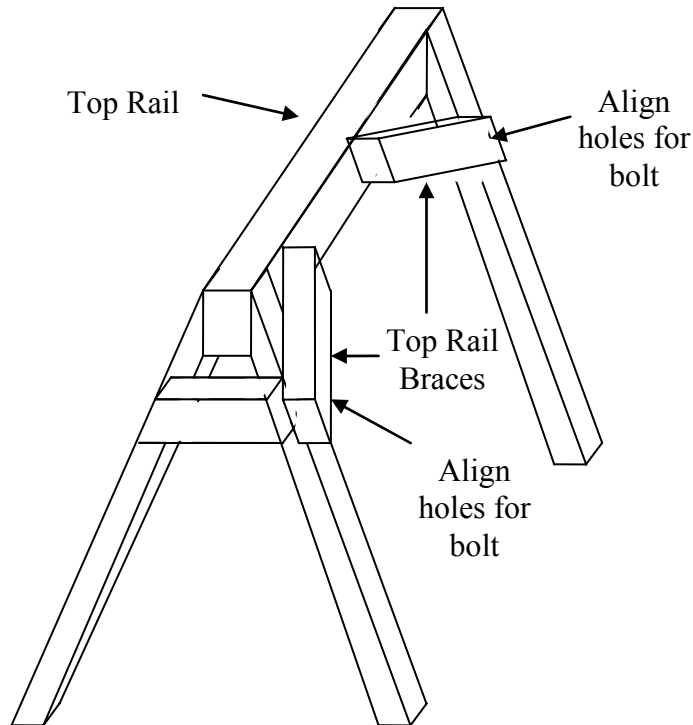


**WARNING: Use short bolts that will not entangle children's clothing or necklaces. When tightened, the threaded end of the bolt should protrude no more than ¼ inch beyond the nut. If necessary, up to two (2) additional washers may be used as spacers to reduce the amount of thread that protrudes beyond the nut.**

Girl Strangled by Long Bolt on Swing Set. A 2-year-old girl was strangled when her necklace became caught on a long bolt while she was playing on her swing set. The girl's grandmother found her hanging by the neck from the frame of the swing with her necklace caught on the bolt. By the time her grandmother found her, the girl was limp and was not breathing. Source: Chin, N., & Berns, S. (1995) Hanging Caused by a Toy Necklace. *Annals of Emergency Medicine*, 26, 522-525.

## **Step 5**

Attach the four top rail braces to the legs as shown in figure 4. For each brace, use one hex bolt, two washers, and one nut. Attach the nuts, but do not tighten them during this step.



**Figure 4**

## **Step 6**

Use a wrench to tighten all nuts installed in Steps 2, 4, and 5. Tighten the nuts snugly, but not so tight that you damage the wood.



**WARNING: Make sure openings between the top rail and braces are too small to entrap children. Check openings using the 3 ½ x 6 ¼ inch entrapment probe. No openings should be large enough for the probe to pass through. (Note: The probe is the same size as a cross-section of a small child's chest and shoulders.)**

Strangling Boy Rescued from Opening in Playground Structure. On April 10, 2000 in Phoenix, Arizona, a four year-old boy got stuck in a 4 ½ by 4 ½ inch gap on a playground structure. He had squeezed into the opening feet-first, and then wriggled the rest of the way. He got stuck because his head was too large to pass through the opening. When a playground monitor saw the boy hanging by his neck, he rushed over and held up the boy's body. This saved the boy from strangulation until emergency workers could free him. Source: Consumer Product Safety Commission Case #50711237.

## **Step 7**

Insert eyebolts in top rail as in Figure 5. Use one washer for each eyebolt.

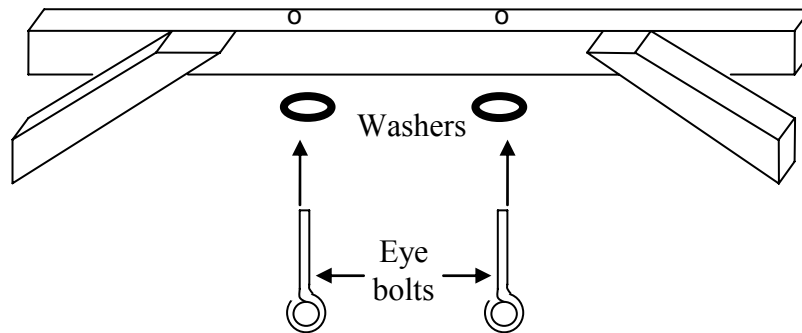


Figure 5

## **Step 8**

Attach eyebolts to top rail, using washers and nut as shown in Figure 6. Tighten snugly, but do not damage the wood.

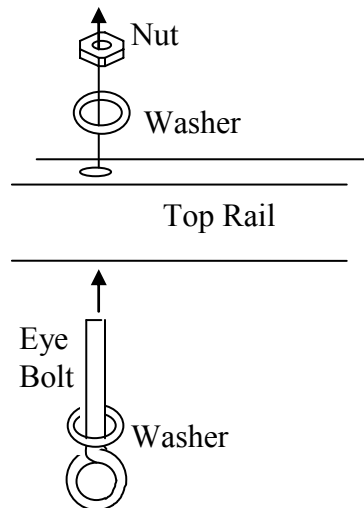


Figure 6



**WARNING: The two eyebolts should be spaced at least 20 inches apart. This will reduce twisting and side-to-side motion of the swing.**

Girl Injured When Swing Collides With Swing Set Frame. A young girl was swinging in a park on May 14, 2005. The chains of the swing were spaced too closely together. This caused the swing to twist and veer from side-to-side. While trying to stop, she swung sharply to the left and slammed into the swing set frame. She suffered serious head injuries because of the collision. Source: Consumer Product Safety Commission Case #50538156.

## **Step 9**

Attach swing chains to eyebolts, as shown in Figure 7. Important: do not attach more than one swing to this swing set.

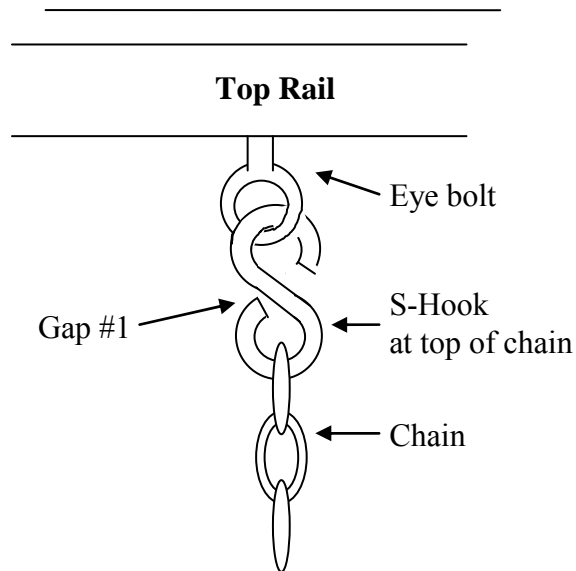


Figure 7

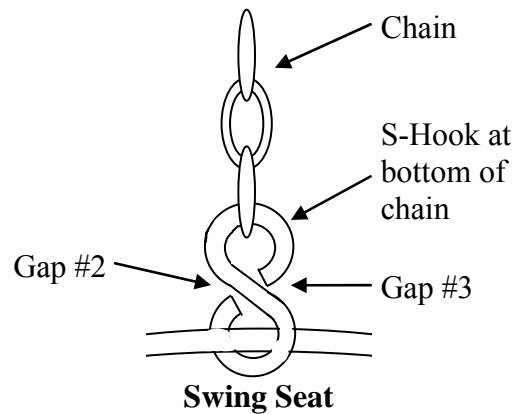


Figure 8



**WARNING: Make sure S-hooks are completely closed to avoid catching children's clothing and jewelry. S-hooks are considered closed if the 3 gaps shown in Figures 7 and 8 are less than the thickness of a dime. Check the S-hook gaps with a dime to be sure.**

Girl Loses Finger When Her Ring Gets Caught On an Open S-hook. An 11-year old girl was jumping from a swing. As she jumped, the ring on her left little finger got caught on the seat's S-hook. The ring was firmly caught, and her finger was torn from her hand when she jumped. The finger could not be reattached because of the extensive damage. Source: Hankin, F.M., Janda, D.H., & Wittenberg, B. (2000). Playground equipment contributing to a ring avulsion injury. *Injury: International Journal of the Care of the Injured*, 31, 635-637.



**WARNING: To avoid injury, do not use heavy, rigid seats. Use lightweight, flexible seats instead.**

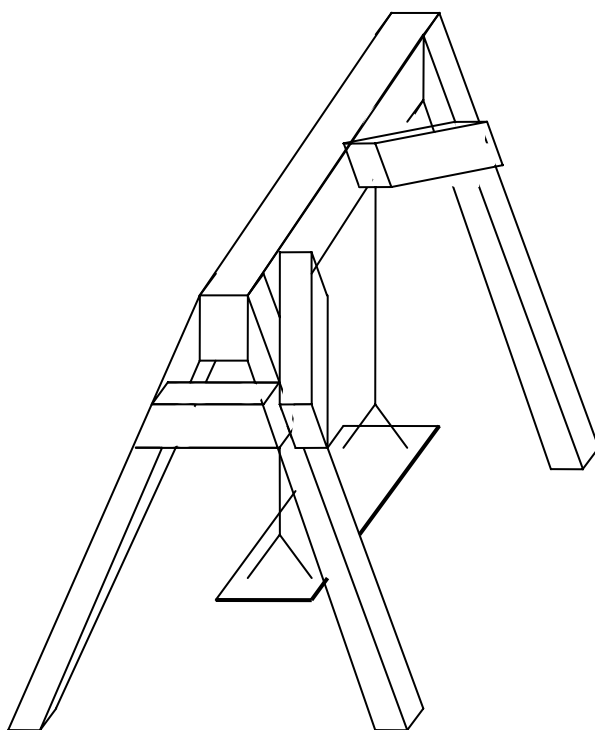
Girl Killed When Struck By Swing. A four-year-old girl died in Fayetteville, North Carolina on April 26, 1999. She was killed when the heavy seat of a playground swing struck her in the head. Source: Consumer Product Safety Commission, National Injury Information Clearinghouse, #9937019758.

**Appendix D - Swing Set Assembly Instructions: Concrete Nonstory  
Warnings**

# Wooden Preschooler Swing

## Assembly Instructions

*Prototype Model 43-256, Model 43-257, and Model 43-258*



Important: This product is not intended for children under the age of two. Maximum weight limit for this swing is 50 lbs.

Note: The parts supplied with your model may be slightly different in appearance from those shown in these instructions.



# General Warnings



**WARNING:** After the swing set is in use, check all hardware for wear and damage at least once a month. If you find problems, do not allow children to play on the swing set until it has been repaired.

Damaged Swing Set Parts Can Break and Cause Serious Injuries. Over time, swing set parts become worn and damaged. For instance, bolts and nuts may become loose; wooden parts may crack; and chains, S-hooks, and eyebolts may wear thin. These parts hold the swing together. If they break while a child is swinging, the child may suffer a fall resulting in paralysis or even death. Source: Tucker and Prentice Injury Prevention Recommendations 1:131.



**WARNING:** To avoid injury, do not allow children to climb on the frame of the swing.

Falls From Swing Set Frames Can Result In Tragedy. The frame of a swing set is not designed for climbing. A child can easily lose his grip and fall from the frame of a swing set. This can result in injuries such as broken bones and concussions. Source: Consumer Product Safety Commission Recommendation #50446332.



**WARNING:** To avoid injury, do not allow children to jump from a moving swing.

Children Can Be Injured If They Jump From Swings. When a child jumps from a swing, she usually expects to land safely on her feet. Unfortunately, children often tumble out of control when they hit the ground. It is possible for a child to injure her head or to break bones in her arms and legs if she lands awkwardly. Serious—even life threatening—injuries are possible. Source: Consumer Product Safety Commission Recommendation #50446332.

# Parts List

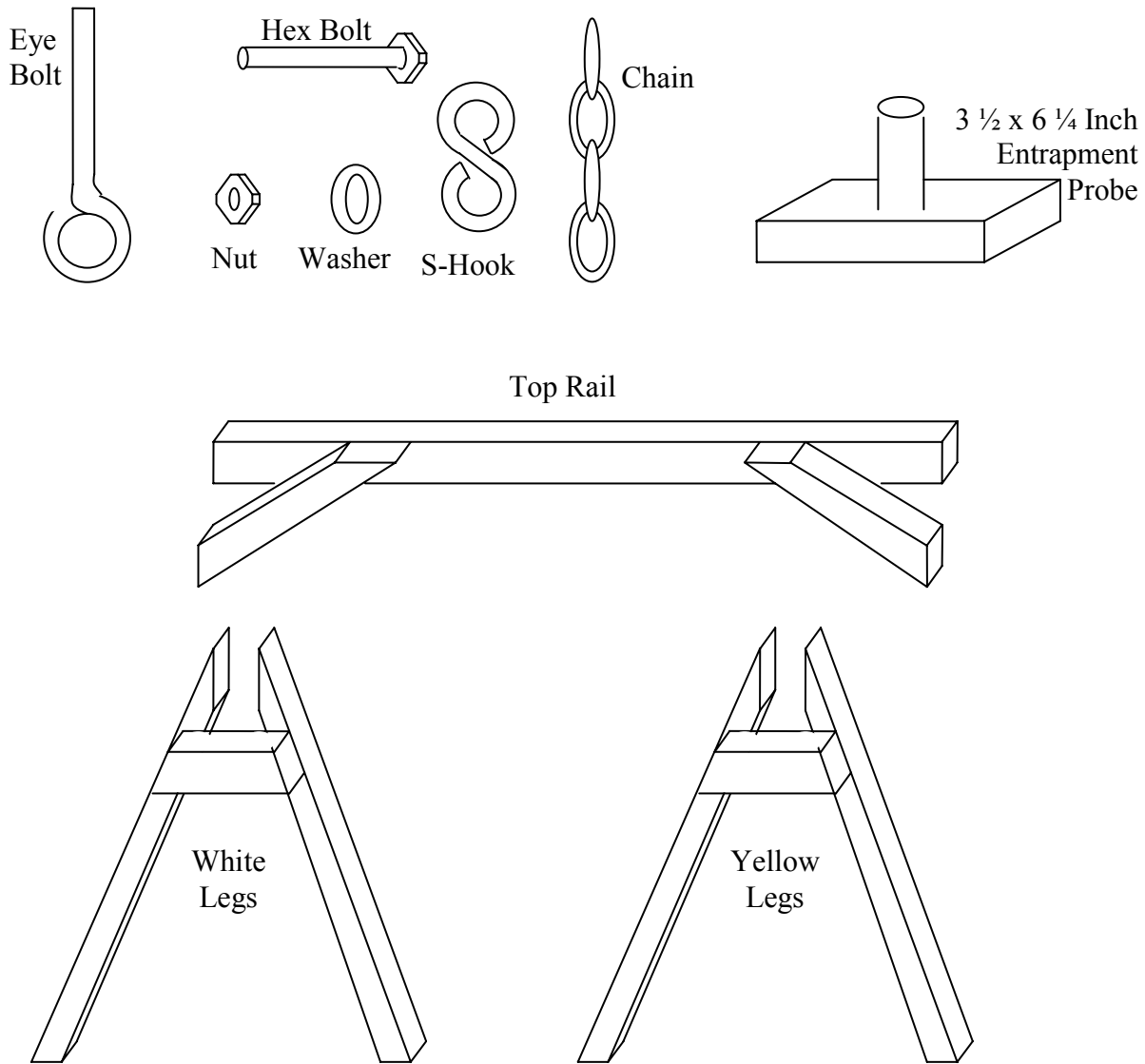


Figure 1



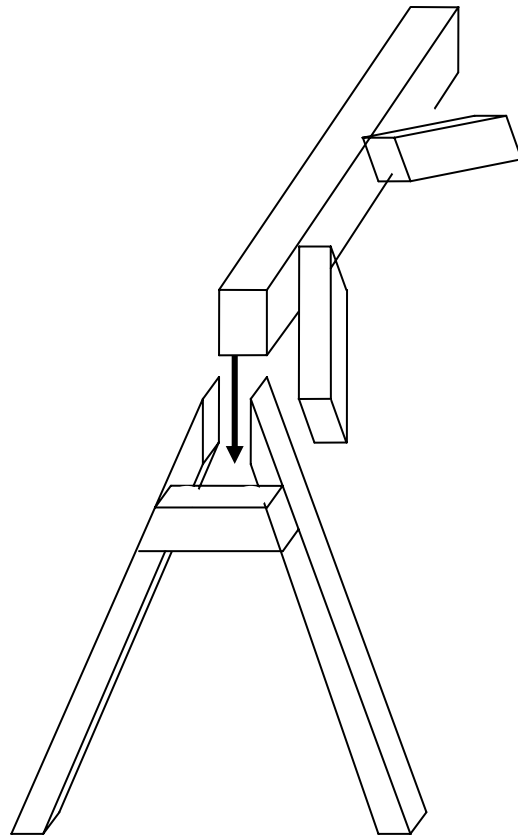
**WARNING:** This product must be assembled according to the instructions in this manual. Failure to follow all instructions could result in collapse of the swing set during use.

Collapsing Swing Sets Can Injure Children. If a swing set collapses while a child is playing, the child may fall and the frame of the swing may even crash down on her. The child may suffer broken bones, internal injuries, and disfigurement. She may require extensive medical treatment. Death is even possible. Follow all instructions in the assembly manual. Swing sets may collapse if not assembled properly. Source: CPSC Recommendation #50711237.

# Assembly

## Step 1

Place the white end of the top rail onto the top brace of the white legs as shown in Figure 2.



**Figure 2**

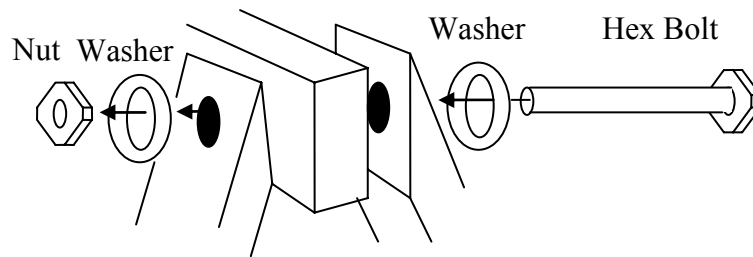


**WARNING: To avoid injury, do not allow children to use this swing set on hard surfaces such as concrete, asphalt, wood, or packed dirt.**

Children Can Die In Falls From Swings. A child can be killed if he falls from a swing and lands on a hard surface such as packed dirt. Children can suffer head injuries, broken bones, and internal injuries. Serious injuries can result even in short-distance falls. Very young children are at especially high risk. Source: Plunkett, J. (2001). Fatal pediatric head injuries can be caused by short-distance falls. *The American Journal of Forensic Medicine and Pathology*, 22, 1-12.

## **Step 2**

Attach the white end of the top rail to the white legs, using one hex bolt, two washers, and one nut as shown in Figure 3. Attach the nut, but do not tighten it during this step.



**Figure 3**

## **Step 3**

Place the yellow end of the top rail onto the top brace of the yellow legs. Refer back to Figure 2.

## **Step 4**

Attach the yellow end of the top rail to the yellow legs, using one hex bolt, two washers, and one nut. Refer to Figure 3. Attach the nut, but do not tighten it during this step.

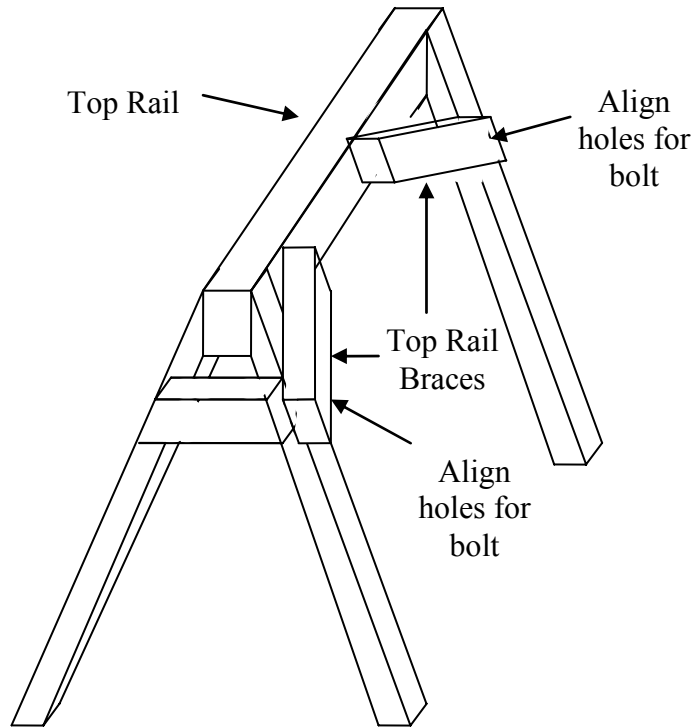


**WARNING: Use short bolts that will not entangle children's clothing or necklaces. When tightened, the threaded end of the bolt should protrude no more than ¼ inch beyond the nut. If necessary, up to two (2) additional washers may be used as spacers to reduce the amount of thread that protrudes beyond the nut.**

Long Bolts on Swing Sets Can Strangle Children. A child can be strangled if her scarf, necklace, jacket drawstrings, and other clothing become caught on long bolts. Bolts should be short so they do not entangle clothing and result in accidental hanging. A child can die quickly when clothing tightens around her neck. By the time she is found, it may be too late to revive her. Source: Chin, N., & Berns, S. (1995). Toy Necklaces May Cause Hanging. *Annals of Emergency Medicine*, 26, 522-525.

## **Step 5**

Attach the four top rail braces to the legs as shown in figure 4. For each brace, use one hex bolt, two washers, and one nut. Attach the nuts, but do not tighten them during this step.



**Figure 4**

## **Step 6**

Use a wrench to tighten all nuts installed in Steps 2, 4, and 5. Tighten the nuts snugly, but not so tight that you damage the wood.



**WARNING: Make sure openings between the top rail and braces are too small to entrap children. Check openings using the 3 ½ x 6 ¼ inch entrapment probe. No openings should be large enough for the probe to pass through. (Note: The probe is the same size as a cross-section of a small child's chest and shoulders.)**

Children Can be Strangled if They Become Caught in Small Openings. Young children have small bodies and large heads. This makes it easy for them to become stuck in very small openings on play structures. For instance, a child might squeeze into an opening feet-first, and then wriggle through until his head gets stuck. If the opening is high and his feet do not reach the ground, he may die of strangulation by hanging. Children can suffocate quickly in cases like this, and it may not always be possible to save them. Source: Consumer Product Safety Commission Recommendation #50711237.

## **Step 7**

Insert eyebolts in top rail as in Figure 5. Use one washer for each eyebolt.

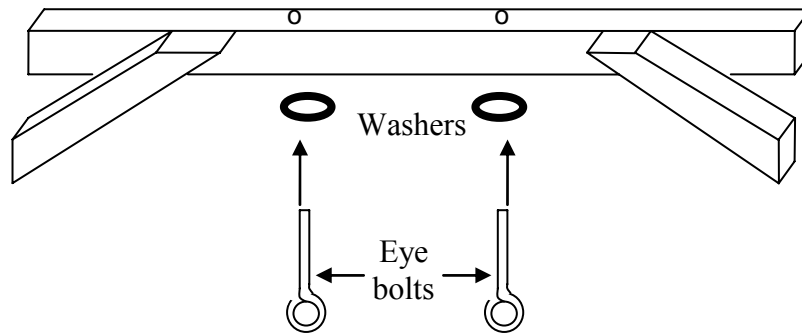


Figure 5

## **Step 8**

Attach eyebolts to top rail, using washers and nut as shown in Figure 6. Tighten snugly, but do not damage the wood.

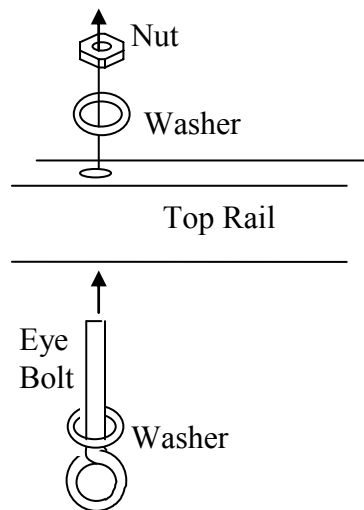


Figure 6



**WARNING: The two eyebolts should be spaced at least 20 inches apart. This will reduce twisting and side-to-side motion of the swing.**

Collisions Between the Swing and Frame Can Cause Serious Injuries. If the chains of the swing are spaced too closely together, the swing may twist and veer from side-to-side. This may cause a child to lose control while swinging. If the child cannot get the swing back under control, she may slam into the swing set frame. The collision may cause head injuries and broken bones. Source: Consumer Product Safety Commission Recommendation #50538156.

## **Step 9**

Attach swing chains to eyebolts, as shown in Figure 7. Important: do not attach more than one swing to this swing set.

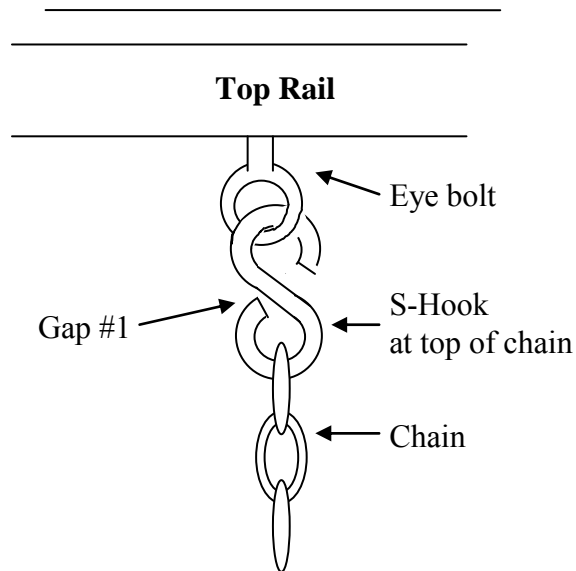


Figure 7

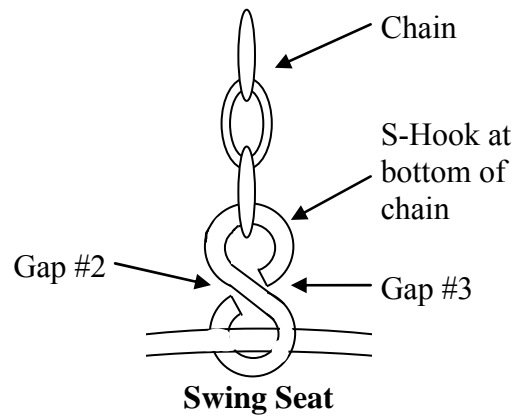


Figure 8



**WARNING: Make sure S-hooks are completely closed to avoid catching children's clothing and jewelry. S-hooks are considered closed if the 3 gaps shown in Figures 7 and 8 are less than the thickness of a dime. Check the S-hook gaps with a dime to be sure.**

Tragedy Can Strike If Clothing Gets Caught on Open S-hooks. Open S-hooks can catch a child's rings, necklaces, and clothing. Serious injuries can result. For instance, a child's finger could be torn from her hand if her ring gets caught on an S-hook while she jumps from a swing. An accident like this can disfigure a child for life. Source: Hankin, F.M., Janda, D.H., & Wittenberg, B. (2000). Playground equipment may contribute to ring avulsion injuries. *Injury: International Journal of the Care of the Injured*, 31, 635-637.



**WARNING: To avoid injury, do not use heavy, rigid seats. Use lightweight, flexible seats instead.**

Children Can Be Struck And Killed By Heavy Swing Seats. Blows to the head can easily injure a young child. In fact, it is possible for a child to be killed when struck by the heavy seat of a playground swing. Source: Consumer Product Safety Commission, National Injury Prevention Recommendations, #9937019758.

## Appendix E - Components Supplied in the Swing Set Assembly Kit

**Table 17 Swing Set Components**

Components Required	Components Supplied
(1) Top rail, including braces and (2) holes for attaching eyebolts.	(1) Top rail, including braces and (4) holes for attaching eyebolts.
(2) leg and brace assemblies	(2) leg and brace assemblies
(2) 3 ½ inch eyebolts	(2) 3 ½ inch eyebolts and (2) 4 ½ inch eyebolts
(2) 5 ½ inch hex bolts	(4) 5 ½ inch hex bolts and (4) 6 inch hex bolts
(4) 4 inch hex bolts	(6) 4 inch hex bolts and (6) 4 ½ inch hex bolts
(14) 5/16 inch washers	(about 25) 5/16 inch washers
(8) 5/16 inch hex nuts	(about 20) 5/16 inch hex nuts
(1) lightweight flexible seat, with chains and S-hooks attached	(1) lightweight flexible seat, with chains and S-hooks attached (1) heavy wooden seat, with chains and S-hooks attached
(1) entrapment probe	(1) entrapment probe
(1) dime (for S-hook check)	(2) dimes
(1) box wrench	(2) box wrenches
(1) deep socket wrench	(1) deep socket wrench
(1) tape measure	(1) tape measure
	(1) 12-inch ruler



## Appendix F - Data Collection Forms

### Participant Response Form A-1

1. Have you ever been responsible for children in any of the following situations?  
(Circle Yes or No)
  - a. Caring for your own children or caring for adopted or foster children: Yes/No
    - i. If Yes, please indicate:
      1. How many years: \_\_\_\_\_
  - b. Babysitting brothers or sisters: Yes / No
    - i. If Yes, please indicate:
      1. How many days per year: \_\_\_\_\_
      2. How many years: \_\_\_\_\_
  - c. Babysitting younger cousins or other relatives: Yes / No
    - i. If Yes, please indicate:
      1. How many days per year: \_\_\_\_\_
      2. How many years: \_\_\_\_\_
  - d. Babysitting children who are not related to you: Yes / No
    - i. If Yes, please indicate:
      1. How many days per year: \_\_\_\_\_
      2. How many years: \_\_\_\_\_
2. How old are you? \_\_\_\_\_ years
3. Please indicate your gender (Circle one): Male / Female
4. How many times in your life have you done the following (write the number of times in the blank space):
  - a. used wrenches and other hand tools: \_\_\_\_\_ times
  - b. assembled products using written instructions: \_\_\_\_\_ times
  - c. assembled swings or other play equipment? \_\_\_\_\_ times

5. Before today, have you ever witnessed or heard about a child being injured while playing on a swing set or other playground equipment? Yes / No
- a. If Yes, please describe what happened. If you have witnessed or heard about more than one swing or playground accident before today, please describe each accident. (Do not include any stories you might have read about today.)

6. Please describe how children can be injured or killed while using a swing set. Include as many types of accidents as you can think of:

7. If you were assembling a swing set, what specific hazards would you look for before you let a child use it? Please be specific. For instance, you might say, "I would make sure there are no wood splinters that could cut the child."

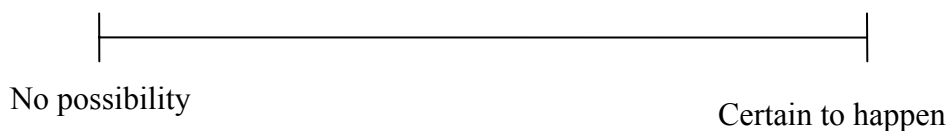
## Practice Response Form A-2

For the following questions, make a vertical (up-and-down) mark on the scale to indicate how likely it is that you will travel to a certain city or town some time in the next five years.

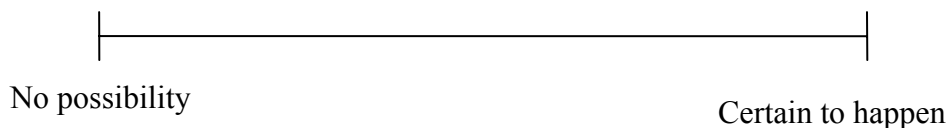
There are no right or wrong answers—please mark the scale according to what you actually believe.

For instance, if you believe you will definitely travel to a city, make a vertical line on right-hand end of the scale. If you believe there is no possibility that you will visit the city, make a vertical mark on the left-hand end of the scale. If you think it “might” happen, place a mark on the scale to indicate how likely you think it is that you will visit there.

- a. How likely is it that you will travel to your home town at least once in the next 5 years?



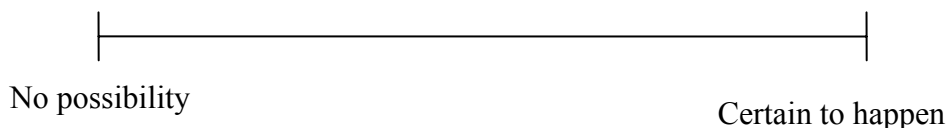
- b. How likely is it that you will travel to Tokyo at least once in the next 5 years?



- c. How likely is it that you will travel to New York City at least once in the next 5 years?



- d. How likely is it that you will travel to Chicago at least once in the next 5 years?



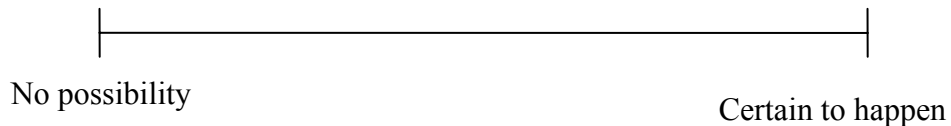
### Participant Response Form A-3

8. For the following questions, make a vertical (up-and-down) mark on the scale to indicate how likely it is that a serious accident will happen with the swing set you assembled.

There are no right or wrong answers—please mark the scale according to what you actually believe.

For instance, if you believe the accident is certain to happen, make a vertical line on right-hand end of the scale. If you believe there is no possibility the accident will happen, make a vertical mark on the left-hand end of the scale. If you think it “might” happen, place a mark on the scale to indicate how likely you think the accident is.

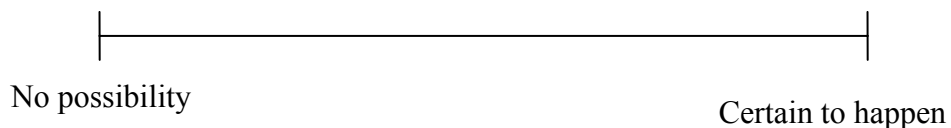
- a. If an adult *fails* to check the tightness and wear of the bolts, nuts, and S-hooks once a month, how likely is it that a child will be seriously injured while playing on the swing set?



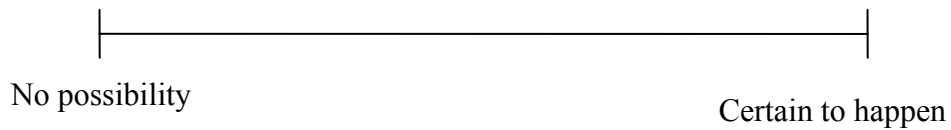
- b. How likely is it that a child will be seriously injured if she climbs on the frame of the swing set?



- c. How likely is it that a child will be seriously injured if he jumps out of the swing while it is moving?



- d. If an adult *fails* to follow the instructions when assembling the swing, how likely is it that the swing set will collapse and injure a child?



- e. How likely is it that a child will be seriously injured if she uses the swing set over a wood floor or other hard surface?



- f. How likely is it that a child will be seriously injured if the bolts on the swing set protrude more than  $\frac{1}{4}$  inch beyond the nuts?



- g. How likely is it that a child will be seriously injured if the openings between the top rail and braces are large enough for the entrapment probe to pass through?



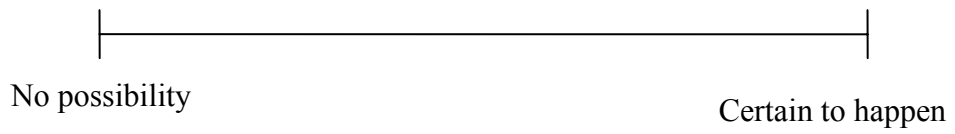
- h. How likely is it that a child will be seriously injured if the eye bolts holding the swing chains are spaced closer than 20 inches?



- i. How likely is it that a child will be seriously injured if the S-hooks are not completely closed?

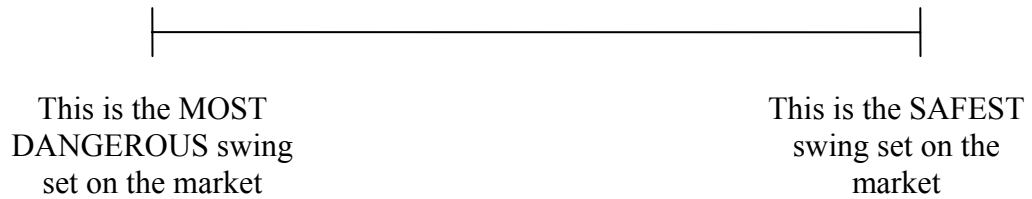


- j. How likely is it that a child will be seriously injured if the wooden seat is used?



\*\*\*\*\*

9. Compared to most other swing sets, how safe do you believe THIS swing set is?





**Participant Response Form A-4**

10. For the following questions, make a vertical (up-and-down) mark on the scale to indicate what you experienced while reading the warnings that were contained in the assembly instructions.

- a. While I was reading the warnings, I could easily picture the events described in the warnings taking place.

|-----|  
Not at all Very much

- b. While I was reading the warnings, activity going on in the room around me was on my mind.

|-----|  
Not at all Very much

- c. I could picture myself in the scene of the events described in the warnings.

|-----|  
Not at all Very much

- d. I was mentally involved in the warnings while reading them.

|-----|  
Not at all Very much

- e. After finishing the warnings, I found it easy to put them out of my mind.

|-----|  
Not at all Very much

f. I wanted to learn how the events in the warnings ended.



g. The warnings affected me emotionally.



h. I found myself thinking of ways the events described in the warnings could have turned out differently.



i. I found my mind wandering while reading the warnings.



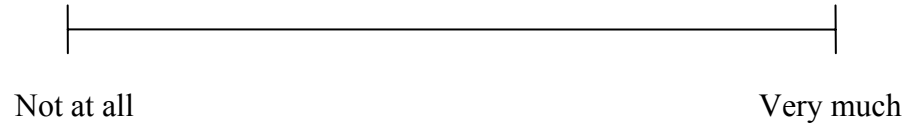
j. The events described in the warnings are relevant to my everyday life.



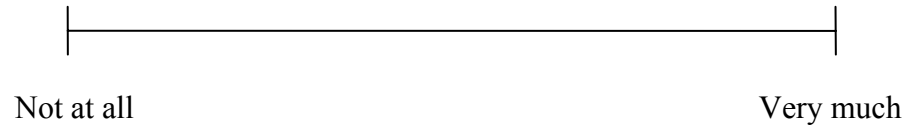
k. The events described in the warnings have changed my life.



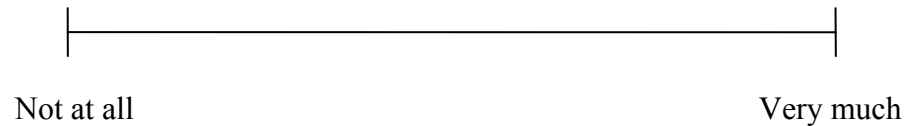
- l. While reading the warnings, I had a vivid image of a worn chain or S-hook breaking while a child was swinging.



- m. While reading the warnings, I had a vivid image of a child falling while climbing on the frame of a swing set.



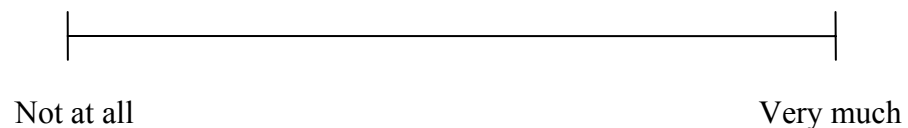
- n. While reading the warnings, I had a vivid image of a child being hurt while jumping from a moving swing.



- o. While reading the warnings, I had a vivid image of a swing set collapsing on top of a child.



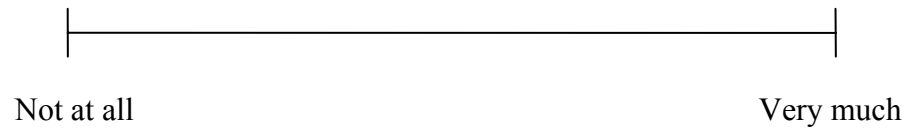
- p. While reading the warnings, I had a vivid image of a child falling on packed dirt, concrete, a wooden floor, or some other hard surface and getting hurt.



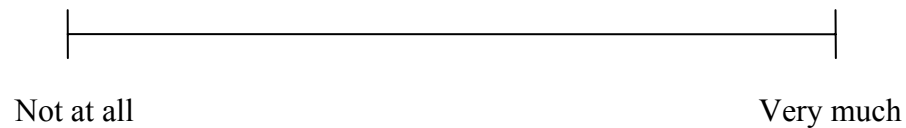
- q. While reading the warnings, I had a vivid image of a child strangling when his/her clothing or necklace became caught on a long bolt.



- r. While reading the warnings, I had a vivid image of a child's head becoming stuck in an opening on the frame of a swing set.



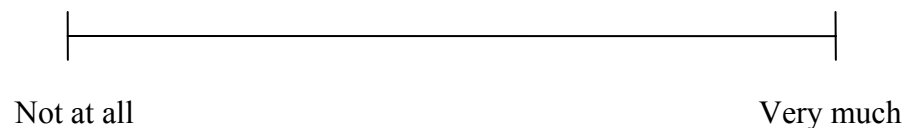
- s. While reading the warnings, I had a vivid image of a child's swing twisting and crashing sideways into a swing set frame.



- t. While reading the warnings, I had a vivid image of a child getting hurt when his/her jewelry or clothing became caught on an open S-hook.



- u. While reading the warnings, I had a vivid image of a child being struck in the head by a heavy, wooden swing seat.



## Performance Evaluation

Date \_\_\_\_\_ Experimenter: \_\_\_\_\_ Group # \_\_\_\_\_

1. Bolts protrude <  $\frac{1}{4}$  inch:
  - a. Yellow Legs to Top Rail: OK / Too Long (Circle one)
  - b. White Legs to Top Rail: OK / Too Long
  - c. Yellow Brace to Leg #1: OK / Too Long
  - d. Yellow Brace to Leg #2: OK / Too Long
  - e. White Brace to Leg #1: OK / Too Long
  - f. White Brace to Leg #1: OK / Too Long
  - g. Eye bolt #1: OK / Too Long
  - h. Eye bolt #2: OK / Too Long
2. Entrapment check: Attempted / Ignored
3. Eye bolt spacing: 20" or more / Less than 20"
4. S-hook dime check:
  - a. Chain #1, Gap 1: Attempted / Ignored
  - b. Chain #1, Gap 2: Attempted / Ignored
  - c. Chain #1, Gap 3: Attempted / Ignored
  - d. Chain #2, Gap 1: Attempted / Ignored
  - e. Chain #2, Gap 2: Attempted / Ignored
  - f. Chain #2, Gap 3: Attempted / Ignored
5. Seat: Rubber / Wood
6. Other failure to follow instructions:
  - a. Missing washers (how many): \_\_\_\_\_
  - b. Missing nuts (how many): \_\_\_\_\_
  - c. Tightened nuts too soon (how many): \_\_\_\_\_
  - d. Failed to tighten nuts: \_\_\_\_\_
7. Other observations:

## Follow-up E-mail

Remember the psychology study where you helped assemble a swing set? Thanks for participating—we learned some important new information. It would help even more if you could take a few minutes to answer the follow-up questions below.

If you still need research credit for your general psychology course, **YOU CAN EARN AN ADDITIONAL HALF-HOUR OF CREDIT** for answering these questions. If you already have all of your research credits, it would still help our research efforts if you could take a few minutes to answer the following questions.

Please send me an e-mail with your answers to the 4 questions below (write a separate paragraph for each question—4 paragraphs total). If you aren't sure you know the answers, just give it your best guess. I will then send you a confirmation, and if you still need research credits I will turn in a participation card for 30 minutes to your general psychology instructor.

1. If you were **ASSEMBLING** a swing set, **WHAT SPECIFIC HAZARDS WOULD YOU LOOK FOR** before you let a child use it? Please be specific and include as many details as you can think of. For instance, you might say, "I would make sure there are no wood splinters that could cut the child."
2. If you were **SUPERVISING** a child who was playing on a swing set, **WHAT RULES WOULD YOU MAKE THE CHILD FOLLOW?** Please be specific and include as many details as you can think of. For instance, you might say, "I would make sure the child doesn't swing too high."
3. If you were **SUPERVISING** a child who was playing on a swing set, **WHAT ISSUES WOULD YOU CHECK RELATED TO HOW THE CHILD IS DRESSED?** Please be specific and include as many details as you can think of. For instance, you might say, "I would make sure the child's shoes are tied so s/he doesn't trip and fall."
4. In the weeks since you participated in the experiment, describe any warnings you have noticed on other products or equipment. Describe the warnings in as much detail as you can.

Thanks.  
Mitch

## Appendix G - Scoring Procedures for Behavioral Compliance With Assembly Instructions

**Table 18 Scoring Procedures (One Point Per Safety Message), Part 1**

Safety Message in Text	Safety Message in Explicit Warning	Decision Task	Method of Scoring Compliance	
Hardware-a		Correct number washers	1/14 pt/washer = 1 point Chance = 0.4 point	
Hardware-b		Correct number nuts	1/8 point/nut = 1 point Chance = 0.5 point	
Tighten-a		Tighten all nuts	1/8 point/nut = 1 point Chance = 0.5 point	
Tighten-b		Tightening sequence	1/5 point/nut for first 5 nuts = 1 point total Chance = 0.5 point	
		No long bolts	Bolts protrude $\leq \frac{1}{4}$ inch with $\leq 2$ washers/end	1/8 pt for each bolt = 1 pt Chance = 0.375 point
		No entrapment openings	Check frame openings with entrapment probe.	1 point for attempting check. (All openings identical, so entire point for checking $\geq 1$ opening) Chance = 0.5 point

**Table 19 Scoring Procedures (One Point Per Safety Message), Part 2**

Safety Message in Text	Safety Message in Explicit Warning	Decision Task	Method of Scoring Compliance
Only 1 swing  Check S-hook gaps	Space eyebolts to avoid twist	Install eyebolts $\geq 20$ inches apart	1 point for proper spacing Chance = 0.25 point
		Attach only one swing	1 point for only 1 swing Chance = 0.5 point
	Check S-hook gaps	Check gaps with dime	1 point for attempting check. (All gaps identical, so entire point for checking $\geq 1$ gap) Chance = 0.5 point
	Lightweight seat	Use lightweight seat	1 point for using lightweight seat. Chance = 0.5 point
Messages Included for Realism, But No Score Possible in Assembly Task			
Weight limit		None	Not scored
	Maintenance	None	Not scored
	No climbing	None	Not scored
	No jumping	None	Not scored
	Follow instructions	No separate task	Not scored
	Fall surface	None	Not scored



## Appendix H - Scoring Categories for Injury Mechanisms

**Table 20 Most Often Mentioned Injury Categories, BLS (1992) Codes in Parentheses**

	Remembered items, explicitly mentioned in assembly manual	Novel items generated by participants
Primary and Secondary Source of Injury	Seat/swinging person (783/570) Chains (4223) Entire swing set structure (783) Legs/frame (783) Bolts (4213) Parts, unspecified (40) Clothing (entanglement) (9210)	
Injury Event	Fall, unspecified (10) Structural failure/collapse (044) Entangle (03, 38) Jump (12) Impact with seat/occupant (023) Impact with frame/legs (012) Entrap (38)	Pinch (03)
Nature of Injury	Fracture (012) Strangle/suffocate (091)	
Body Part Involved	Head (00) Finger (34) Neck (10)	

**Table 21 Moderately Often Mentioned Injury Categories, BLS (1992) Codes in Parentheses**

	Remembered items, explicitly mentioned in assembly manual	Novel items generated by participants
Primary and Secondary Source of Injury	Ground surface (623) S-hooks (4219) Jewelry (entanglement) (9222) Nuts (4213)	Hair (entanglement) (569) Rope (4224)
Injury Event	Impact with other objects (00)	
Nature of Injury	Death Other head injury (060) Paralysis (013) Concussion (062) Amputation/ Avulsion (031/033)	Cut, laceration (034) Abrasion/scratch (041) Crush (0971)
Body Part Involved	Leg (41) Arm (31) Face (03)	Hair (98) Limb, unspecified (98) Spine/tail bone (23)

**Table 22 Rarely Mentioned Injury Categories, BLS (1992) Codes in Parentheses**

	Remembered items, explicitly mentioned in assembly manual	Novel items generated by participants
Primary and Secondary Source of Injury	Eyebolts (4213) Washers (4213)	Nail (4212)
Injury Event		Burned on hot seat (323) Rope burn (052) Trip (215) Overexertion (22)
Nature of Injury	Puncture (037) Lost consciousness (4111) Internal injury (094) Disfigure	Heat burn (053) Sprain (021) Blister (042) Bruise (043) Friction burn (045)
Body Part Involved	Nose (033)	Ribs (22) Wrist (32) Hand (33) Foot (43) Back (23) Buttocks (253) Torso (29) Ankle (42) Toe (44)

## Appendix I - Scoring Categories for Swing Hazards and Safety Practices

**Table 23 Hazards and Safety Practices**

Remembered items explicitly mentioned in assembly manual	Novel items generated by participants
<p>Most often mentioned:</p> <ul style="list-style-type: none"> <li>All fasteners tight</li> <li>No hardware capable of entangling</li> <li>Check for wear and damage</li> <li>No jumping</li> <li>No entrapment openings</li> <li>No climbing</li> </ul> <p>Mentioned moderately often</p> <ul style="list-style-type: none"> <li>Assemble according to instructions</li> <li>No hard ground surfaces</li> <li>Observe weight limit</li> <li>No heavy/rigid seats</li> <li>Eyebolts/chains spaced to limit twisting</li> </ul> <p>Rarely mentioned</p> <ul style="list-style-type: none"> <li>All necessary hardware present</li> </ul> <p>Never mentioned</p> <ul style="list-style-type: none"> <li>Fasteners tightened in proper order</li> </ul>	<p>Most often mentioned:</p> <ul style="list-style-type: none"> <li>Stable/securely anchored</li> <li>Appropriate clothing/hair; no jewelry</li> <li>Provide rules for users</li> <li>No sharp edges</li> </ul> <p>Mentioned moderately often</p> <ul style="list-style-type: none"> <li>Located away from other hazards</li> </ul> <p>Quality materials</p> <ul style="list-style-type: none"> <li>Protective covering on hardware</li> <li>Install on level surface</li> </ul> <p>Appropriate distance from seat to ground</p> <ul style="list-style-type: none"> <li>Provide supervision</li> <li>No pinch points</li> <li>No lead based paint</li> </ul> <p>Rarely mentioned</p> <ul style="list-style-type: none"> <li>Appropriate distance between seats</li> <li>No trip hazards</li> <li>Chains of equal length</li> </ul> <p>No extra chain hanging from S-hook</p> <ul style="list-style-type: none"> <li>Equipment is appropriate size for user</li> <li>Appropriate distance between seat/frame</li> <li>Railings on platforms</li> <li>Respond to manufacturer's recalls</li> </ul>

## **Appendix J - Score Transformations**

### **Individual Score Transformations**

#### ***Childcare Experience***

Participants reported a wide range of childcare experience ( $M = 896.46$  days of experience,  $SD = 1,955.36$ ), resulting in a positively skewed distribution. Measures of skewness and kurtosis were 4.08, and 20.83, respectively. A log transformation of childcare experience improved the statistical characteristics of the distribution ( $M = 2.11$ ,  $SD = 1.07$ ; skewness = -0.49; kurtosis = -0.40). Six participants did not provide a response and were excluded from analyses.

#### ***Assembly Experience***

Participants reported a wide range of experience related to assembling swings and other play equipment ( $M = 11.04$  items assembled,  $SD = 84.47$ ), resulting in a positively skewed distribution. Measures of skewness and kurtosis were 11.63, and 137.04, respectively. An attempt to improve the characteristics of the distribution by means of a log transformation was unsatisfactory (skewness = 1.76, kurtosis = 4.96). Given the lack of normality in the distribution, assembly experience was transformed into four categories based on natural breaks in the distribution: none (0 times), low (1-2 times), medium (3-6 times), and high (8 or more times). The categorization procedure resulted in 64 participants in the none category, 32 in the low category, 26 in the medium category, and 19 in the high category (one participant did not respond to this question).

### **Team Score Transformations**

#### ***Team Gender***

The genders of the two assemblers in each team were combined to create three categories: male-male ( $n = 6$ ), male-female ( $n = 24$ ), and female-female ( $n = 24$ ).

### ***Team Assembly Experience***

The individual assembly experience categories of each assembler (see above) were assigned scores as follows: none = 0, low = 1, medium = 2, high = 3. The scores of the two assemblers in each group were then averaged to create a team assembly experience score. On the basis of these averaged scores, five categories of team experience were derived, based on natural breaks appearing in the distribution of scores: no experience (team average = 0), low experience (team average = 0.5), low-medium experience (team average = 1), high-medium experience (team average = 1.5), and high (team average = 2-3). The distribution of team scores was as follows: no experience (n = 9), low experience (n=13), low-medium experience (n=14), high-medium experience (n=11), and high (n=6). One team was excluded from the analysis a participant did not respond to the item.

### ***Other Team Scores***

All other team scores were calculated as the average of the scores of each team's two assemblers. These averaged team scores included age, (log) childcare experience, reminders, recall, novel concrete responses, probability of injury, transportation, and swing set safety rating.