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RESEARCH

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Analysis and Rebuttal of Development of an In-Water Intervention in a Lifeguard Protocol

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We review the paper by Hunsucker and Davison published in the *International Journal of Aquatic Research and Education* in 2010. The authors' two-part goal was to describe a protocol they named "in-water intervention" (IWI) that uses abdominal thrusts (ATs) and to report on its effectiveness at assisting drowning victims in waterparks. We identify serious shortcomings in the paper's methodology, interpretation and use of the literature, and ethical principles. We conclude that their primary assertions were unsubstantiated by the evidence they presented.

Methodologic Issues

The study design of the Hunsucker and Davison (2010) paper is a descriptive, retrospective review. As such, it cannot properly address treatment effectiveness. Without studying a comparison/control group, the authors cannot appropriately report associations, and without this being a prospective controlled experimental model, one cannot demonstrate causality (Grimes & Schulz, 2002). On the one hand, the authors appropriately state, "Obviously a treatment study would be necessary to pinpoint the mechanism that is involved, but that is outside the scope of this paper." Yet they promote the purported efficacy of IWI in saving lives and recommend its use over conventional resuscitation (CPR) protocols without having studied these protocols.

IWI Protocol

The authors fail to clearly detail their IWI protocol. They initially describe it as a "set of procedures" that includes "special emphasis on scanning, rapid rescue, and

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applying a resuscitation procedure in the water," but later they discuss it as only one component (d) out of six. The authors state that "extensive literature searches showed that relatively little scientific research on aquatic rescue techniques" had been published. While this statement is true, the study they conducted had a "resuscitation" variable and not a "rescue" variable. The literature contains extensive studies on resuscitation. This example of incorrect use of terminology and definitions is more than semantic.

The authors present ATs as a resuscitation intervention, but fail to address the absence of proven benefit in its use on drowning victims. They fail to acknowledge the fact that all of the resuscitation algorithms endorsed by the major standard-setting organizations of the world recommend use of AT *only* for removal of solid foreign bodies in the airway (American Red Cross, European Resuscitation Council, American Heart Association, Inc., and International Liaison Committee on Resuscitation, 2010).

By introducing a new protocol, the authors are obligated to provide a rationale for its use. Instead, the authors provide only their own beliefs, which guided the development of their protocol, without presenting any references, any data on their prior 10–15 years experience with it, or any evidence-based science. In fact, some statements have no apparent factual basis. For example, they state that "ATs were the only resuscitation technique that could be used effectively in the water, be easily taught, and had a reasonable expectation that they would be performed by the lifeguard." They then go on to conclude that, " . . . alternative procedures were examined and found to have major problems when being performed in deep water" without any scientific basis or rationale.

The authors fail to address when lifeguards should apply this potentially injurious intervention (IWI). Thus, they do not discuss assessing drowned patrons' level of consciousness, degree of respiratory distress, or respiratory arrest prior to giving them ATs. They state that the difficulty of recognizing respiratory failure makes it difficult to provide standard artificial respiration, but do not explain how this is made easier by providing ATs. Furthermore, the authors state that their " . . . lifeguards are always given a certain amount of discretion with regard to the protocol . . .". While lifeguards may have some discretion regarding rescue options, it is highly irregular that lifeguards would not have strict, clear resuscitation protocols.

The Hunsucker and Davison paper has no outcomes assessment. A critical outcome measure would be survival, but no apparent effort was made to determine if treated victims survived after they left the scene. Survival cannot be assessed solely at the scene since 80% of patients who have had resuscitation and remain unresponsive will go on to die 24–48 hours after the drowning (Quan & Kinder 1992). Additionally, the long-term neurologic outcome for survivors is not systematically determined.

A third outcome measure should have been whether lifeguards reliably used the IWI because the authors rationalized its use over standard resuscitation protocols, saying that the IWI was a "procedure that could be reliably performed in deep water." Interestingly, their data show that lifeguards did not consistently perform the IWI. It was used in only 20 of the 32 patrons in water > 5 feet deep. The authors fail to comment on this.

Their study did not include any kind of methodical follow-up to detect medical complications arising from the event or the resuscitation efforts.

Results/Data Reporting

Not surprisingly, given the lack of methodology, the data are incomplete. A reader cannot evaluate the most basic questions about rescues, resuscitations, and survival in those treated with IWI versus other resuscitative measures. There is no actual statistical analysis in any form. Despite stating that they used the Utstein reporting system, the authors do not present the data in the Utstein style (Idris et al., 2003). In fact, they do not describe a specific method of data collection, including any systems to insure complete capture.

Data on important outcomes, such as death, neurologic outcome, and frequency of use of the protocol are not presented. They dismiss the omission with " . . . information taken in the prehospital environment is often imprecise." Perhaps so, but basing the evaluation of a medical protocol on inadequate information is reckless.

The reporting consisted of a general and vague statement that " . . . the victim was usually uninjured with no underlying medical reasons for the loss of spontaneous respiration." Lifeguards cannot diagnose the known complications of AT— aspiration, diaphragmatic rupture, spinal fracture. This is a critical flaw since many major complications of abdominal thrusts may not be evident at the scene. In fact, the only complication that was clearly shown was that substituting IWI as a primary protocol delayed providing CPR.

Their data reveal that the great majority of patrons who received the IWI were submerged less than one minute, making this a very biased sample population, incapable of providing new useful information since 90% of those submerged less than 6 minutes survive with good outcomes (Quan & Kinder, 1992; Suominen et al., 2001). Only a small number (n = 21) of patrons actually received IWI. To show the IWI provided any reliable or meaningful improvement in survival would require hundreds to thousands of drowned and treated patrons. More important, it is unclear why any intervention was started in the water when terra firma was so close and the submersion interval was under one minute. Since the majority of the study victims were under thirteen years of age, it is difficult to understand why lifeguards could not move most patrons to land to receive CPR, if, in fact, they needed CPR. Why did any of these patrons receive a potentially dangerous intervention when the likelihood of their survival was over 90%?

The Discussion section is a series of speculations without apparent congruence between theory and data. For example, the authors justified their findings by comparing survival rates of their patrons with other studies of pool drowning (guarded public pools and U.S. pools), concluding that lifeguards trained by their company had a better success rate of preventing drowning death. This comparison is completely invalid since the victims, the water environments, the submersion durations, and many other variables differed. In addition, this comparison appears to represent bias, because it seems to relate a proprietary company's training to an objective study of a specific intervention, a study which was the purported reason for the paper.

Both of the final conclusions in this paper were, in our estimation, completely unsubstantiated. This paper supports neither the effectiveness of the IWI protocol nor the safety of using ATs. As a descriptive study, it lacks methodological soundness, such that its findings are neither useful nor applicable to any setting. Lacking "a systematic, ordered approach to the gathering of data," "an experimental method," adequately-described results that were observed, and "conclusions drawn from observed results," this paper does not meet the definition of scientific method that forms the basis for any study (Mosby's Medical Dictionary, 2009).

Ethical Concerns

The most disturbing aspect of this study is that Hunsucker and Davison ignored the ethical principles governing the conduct of human subject research. The study failed to adhere to all three recognized principles of human subject research—autonomy, beneficence, and justice as outlined in the Belmont Report (National Commission 1979) and codified in all current regulations regarding human subject research. It appears that experimentation was conducted on unknowing human subjects (failure to adhere to autonomy). It involved the use of a disproved and potentially dangerous procedure that ignored the international standard of care—CPR (failure to recognize beneficence). To make matters even worse, the majority of the victims treated in the study were children (failure to adhere to justice). Apparently, no institutional review board was involved, consent was not obtained, and procedures for the conduct of human research in the absence of prospective informed consent were not followed. The authors seemed to justify and conduct the experiment on their own without any oversight or outside review. There was no informed consent given by the victims/patrons, but it is also unclear if the lifeguards, instructors, facilities, or their insurers were aware that they were participants in an unsanctioned study. By failing to employ appropriate methodology, statistical analysis, and conduct of the study as previously described, the study cannot be of benefit and thus fails even the minimal required test of human subject research—that a study has social value and scientific validity.

The authors attempt to justify the use ATs by comparing their decision to that of the American Heart Association's Emergency Cardiovascular Care (ECC) committee's decision to recommend compression-only CPR. They state that, in their own views, " . . . the likely advantages in favor of this recommendation (ATs) outweigh the possible disadvantages." This comparison reveals enormous arrogance on the part of the authors. The AHA's ECC recommendation for compression-only CPR was based on review of scientific evidence by hundreds of resuscitation experts. The CPR decision flowed from a standard progression of clinical research, starting with multiple animal studies, followed by limited human studies, and then large clinical trials. None of these were conducted prior to the IWI protocol.

Additionally, the authors egregiously fail to establish any process for checking for complications of ATs, and seem to see no reason to do so, despite acknowledging that there have been " . . . very strong concerns about the use of ATs for drowning." How many serious complications arose from the unnecessary application of ATs resulting from protocols established by the authors?

The authors reference a paper reporting a low rate of complications from ATs (Lee, Kim, Shekherdimian, & Ledbetter, 2009); however, their use of this study to support the use of ATs is concerning on many levels. First, it was published in 2009, long after the patrons in this study were subjected to the AT protocol. Therefore, it provides no justification for the study that they conducted, even if it were pertinent thereto. As well, Lee et. al.'s study mainly involved ATs for foreign body obstruction and importantly indicated that the complications were mainly due to ATs being done incorrectly. Were there any assurances that the ATs were being done correctly in this protocol?

Hunsucker and Davison dismiss concerns about injury related to ATs, " . . . because the possibility of ancillary injuries from any medical procedure does not necessarily reduce the need for that procedure." In support of this statement, the authors reference a study of complications with bystander CPR (Lederer, Mair, Rabl, & Baubin, 2004). This comparison is inappropriate as CPR outcomes and complications have been reviewed, evaluated over years, and debated by thousands of international resuscitation experts. These experts include members of all of the representative organizations making up the International Liaison Committee on Resuscitation, including the American Heart Association (AHA), the European Resuscitation Council (ERC), the Heart and Stroke Foundation of Canada (HSFC), the Australian and New Zealand Committee on Resuscitation (ANZCOR), Resuscitation Council of Southern Africa (RCSA), the InterAmerican Heart Foundation (IAHF), and the Resuscitation Council of Asia (RCA).

The use of ATs/Heimlich maneuver for drowned victims has been extensively reviewed, twice by the Institute of Medicine (Rosen & Harley, 1995), multiple times by the Emergency Cardiovascular Care Committee of the American Heart Association (Quan, 1993), by the International Liaison Committee on Resuscitation and twice (in 2000 and 2006) by the Advisory Council on First Aid, Aquatics, Safety, and Preparedness (ACFASP) committee of the American Red Cross (Pia, Fielding, Wernicki, & Markenson, 2010). At each review, each body recommended against its use for drowning resuscitation, noted that ATs may be harmful, and limited its use only to airway obstruction caused by a solid foreign object. The International Lifesaving Federation condemned its use in a published medical position statement (International Life Saving Federation, 1996). No resuscitation guideline that we are aware of anywhere in the world supports its use in drowning.

Misinterpretation of Literature

Lastly, Hunsucker and Davison's use of published drowning literature is, in our estimation, often misleading and/or incorrect. They quote Szpilman & Soares (2004): "Moreover, many lifeguards are reluctant to perform mouth-to-mouth ventilation . . . ," implying that lifeguards in general, and specifically the lifeguards in this study, would be reluctant to perform mouth-to-mouth. The actual quote went on to say, " . . . moreover, many lifeguards are reluctant to perform mouth-to-mouth ventilation without a barrier device to minimize the risk of communicable disease." In fact, Spzilman et al. (2004) did not comment on how lifeguards with barrier devices would react. It is assumed that all lifeguards involved in this protocol would be required to have barrier devices with them at all times. In another example of misinterpretation of the literature, Hunsucker and Davison state, "Research has shown that lifeguards, following a trend even among medical practitioners are reluctant to perform mouth-to-mouth resuscitation even using universal precautions such as masks and gloves" (Horowitz & Matheny, 1997). Horowitz's study had nothing to do with lifeguards. In fact, Horowitz reported that respondents would perform mouth-to-mouth on a neighbor 84% of the time and on a child at a public swimming pool 88% of the time. These groups are the ones that likely would be encountered by a local pool lifeguard. Thus, when read correctly, Horowitz's findings discredit Hunsucker and Davison's stated beliefs.

Similarly, the authors take isolated information from various other studies out of context, string it together to convey ideas that have nothing to do with the original work and then use it to support the ideas of the authors. For example, Hunsucker and Davison state, "Abdominal thrusts lift the diaphragm and force enough air from the lungs to create an artificial cough" (American Heart Association, 2009). This increases the internal airway pressure (Langhelle, Sunde, Wik, & Steen, 2000) and might help break up the laryngospasm that has been described as part of the drowning process (Layon & Modell, 2009). Some medical evidence exists to support this view (Milstein & Goetzman, 1977)." The first two statements are found in these studies, but are clearly taken out of context. In fact, both references provided data supporting recommendations *against* the use of ATs. Langhelle's 2000 study demonstrated that "standard chest compressions are more effective than the Heimlich maneuver for treating complete airway obstruction by a foreign body" and recommended " . . . removal of the Heimlich maneuver from the resuscitation algorithm" altogether. The Layon and Model 2009 reference addresses laryngospasm, but as worded, Hunsucker and Davison incorrectly imply that Layon and Modell are suggesting that ATs " . . . might help break up the laryngospasm." In fact, this conjecture appears linked to Milstein and Goetzman, 1977, who described use of chest compressions, not ATs, to break laryngospasm in one neonate requiring intubation for a non drowning condition.

Conflicts of Interest

The authors fail to reveal important conflicts of interest. Lead author Hunsucker is the president and founder of NASCO, and Scott Davison is the Senior Vice President of Research and Development for NASCO.

On their Web site, the company claims, "We have the best safety record in the industry. Period" (National Aquatic Safety Company, 2010). The authors' company trained the lifeguards in this study. The authors also chose among the many facilities that they are affiliated with to include in the study. This clearly represents substantial financial and intellectual conflicts which, at a minimum, require disclosure but also may require exclusion from conduct of said research. The lack of disclosure and lack of independent oversight may be indicative of intentional, or at least unintentional, bias. Without appropriate disclosure and oversight, the retrospective review of apparently proprietary data couched as a "study" may, in fact, represent a rationalization for self-promotion.

In the spirit of full disclosure, some of the authors of this rebuttal (i.e., Wernicki, Fielding, Lees, Chambers, Pia, Quan, and Markenson) are members of the American Red Cross Advisory Council on First Aid, Aquatics, Safety, and Preparedness (ACFASP). The American Red Cross has a lifeguard program which competes with NASCO in certain markets. These authors only serve as volunteers without compensation; therefore, there is no financial conflict of interest. The authors do, however, serve in a scientific advisory capacity. The resultant potential for intellectual conflict is addressed by this disclosure.

Conclusion

The goal of this rebuttal is to perform a scientifically-valid critical review and to inform readers about what we consider dangerous methods and incorrect conclusions in the Hunsucker and Davison study. We encourage training organizations, facility managers, lifeguard instructors, and lifeguards to adhere to the standard, scientifically based protocols currently recommended and accepted nationally and internationally. The use of ATs in the resuscitation of drowning victims has been thoroughly reviewed and universally condemned by the medical community throughout the world. The Hunsucker and Davison paper reports on purported outcomes of implementation of an unethical protocol, one which itself was ill-conceived, poorly carried out, and replete with conflicts and potential for bias. The paper should not change the use of established scientifically valid and ethically obtained national standards for CPR that limit ATs to only treat foreign body airway obstruction. If you have any concerns, please discuss them with your medical director.

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