International Journal of Aquatic Research and Education

Volume 5 | Number 3 Article 4

8-1-2011

# Reply to “Analysis and Rebuttal of Development of an In-Water Intervention in a Lifeguard Protocol”

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Recommended Citation
Hunsucker, John and Davison, Scott (2011) "Reply to “Analysis and Rebuttal of Development of an In-Water Intervention in a Lifeguard Protocol”," *International Journal of Aquatic Research and Education*: Vol. 5 : No. 3 , Article 4.
DOI: https://doi.org/10.25035/ijare.05.03.04
Available at: https://scholarworks.bgsu.edu/ijare/vol5/iss3/4

RESPONSE

*International Journal of Aquatic Research and Education,* 2011, 5, 246-250


# Reply to "Analysis and Rebuttal of Development of an In-Water Intervention in a Lifeguard Protocol"

**John Hunsucker and Scott Davison**

Having just finished reading "Analysis and Rebuttal of Development of an In-Water Intervention in a Lifeguard Protocol" (A&R) by Peter Wernicki, Peter Chambers, Roy Fielding, Terri Lees, David Markenson, Francesco Pia, and Linda Quan (2011), we were both disappointed and surprised for two reasons.

1. The purpose of the paper, "Development of in-water intervention (IWI) in a lifeguard protocol with analysis of rescue history" (Hunsucker & Davison, 2010) was to present the first example, as far as our research has been able to find, of a rigorous examination of how a rescue protocol can be implemented and verified. The paper included a large retrospective study of the effectiveness of that rescue protocol. The entire water park rescue protocol was developed as part of the National Aquatic Safety Company's (NASCO) mission to "reduce the loss of life due to drowning." We were surprised to find that there was nothing in the A&R paper that would assist in fulfilling this mission or anything that addressed how to improve current protocols in order to save more lives. It was simply a criticism of IWI, which is only one of six parts of the protocol.
2. The second surprise was that we had hoped that an analysis of 56,000 rescues would open a learned discourse on rescue techniques for lifeguards. Instead the A&R authors have chosen to ignore the results of the rescue history and to concentrate on reducing the discourse to a personal level. Their paper can be summarized, at least in our minds, as "You are doing something we aren't, and we don't like it." We have addressed every single concern over the use of IWI in a rescue protocol and described it in our paper.

The A&R authors seem to have missed the whole reason for our original paper. If so, then as the authors, that fault lies with us. We will attempt to do better in subsequent publications; however, there were several other points brought up by the A&R authors that need be addressed:

1. The A&R authors felt that there were ethical violations committed when IWI was included in the rescue protocol. This was particularly interesting since one of us teaches professional ethics at the university. Unfortunately, the A&R authors don't show any indication that they know the history of how the IWI procedure was developed or tested. They also miss the point that this paper

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was not about the development of IWI per se, but was about how to test and validate a protocol. As far as we know, the actual history of the development of the IWI part of the protocol has never been published and would, perhaps, make an interesting follow-up paper. Since the A&R authors don't know how the IWI protocol was developed or how it parallels or does not parallel the development of other standard protocols, CPR for example, this is a major flaw in their reasoning process. In fact, even the most inexperienced researcher knows, or should know, that it is always a mistake to "assume."

2. The A&R authors seem to feel that we are recommending this IWI as a change to the CPR protocol or, as they say, "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" (Wernicki et al., 2011, p. 12). The American Heart Association (AHA) makes no recommendations about resuscitation in deep water of which we are aware and only asks that rescue breathing be initiated as soon as possible (Vanden Hoek et al., 2010, p. S847). We follow the AHA CPR guidelines on land and initiate rescue breathing as soon as possible. The A&R authors never address the fact that IWI takes only 4-6 seconds and does not delay CPR any more then the checks for pulse, breathing, or response that are already part of CPR. We made no recommendation that our protocol be universally adopted.
3. The A&R authors often show an amazing lack of knowledge about the actual lifeguard environment. As an example, they seem to ignore the fact, or are unaware, that the time for that part of a rescue starting with victim identification and ending with the initiation of on-deck CPR without IWI, is often on the order of two to four minutes (Hunsucker & Davison, 2010, p. 190). One of the few things that everyone agrees on is that the time to intervention is critical. To ignore this in the development of a protocol would certainly lead to an ineffective procedure.
4. The A&R authors criticize us for not conducting a large randomized trial and not following the drowning victims who stopped breathing (Loss of Spontaneous Respiration or LSR) after they left the hospital. The AHA 2010 CPR guidelines on evidence-based science state that "In reality, more questions exist than there are studies attempting to answer them; and when studies have been done, they are not typically large, randomized trials on human subjects. As a result, the writing groups were often confronted with the need to make recommendations based on results from human trials that reported only intermediate outcomes, nonrandomized or retrospective observational studies, animal models, or extrapolations from studies of human subjects who were not in cardiac arrest" (Sayre et al., 2010, p. S661). Are the A&R authors going to submit an "Analysis and Rebuttal" to the AHA for using such data? Again, this seems to show that they are unaware of how protocols are developed and also seem to be unaware of how the traditional CPR protocol was developed.

In our paper "Development of In-Water Intervention (IWI) in a Lifeguard Protocol with Analysis of Rescue History" (Hunsucker & Davison, 2010), we derived two conclusions from the rescue history data. One was that the rescue protocol was effective and the other was that IWI seemed to make a contribution. Both of these conclusions were contested by the A&R authors.


DOI: https://doi.org/10.25035/ijare.05.03.04

1. Admittedly, the term "effective" is a subjective term, but it is reasonable to propose that a very small fatality rate that is much better than that found in the general population would reflect an effective protocol. We used the Center for Disease Control (CDC) data and data collected from Texas public pools as reference points and for comparison with the fatality rate for the water park rescue protocol (Hunsucker & Davison, 2010, p. 193). The A&R authors showed their unfamiliarity with both data analysis and research techniques when they wrote that you can't make comparisons between a subset of a population and the population itself because they are different. The fact that the protocol studied has an approximately 100 times better fatality rate then the CDC or the Texas data show for the population studied that the rescue protocol was effective. We have not stated that this is the best protocol or that it cannot be improved, but simply said that it is effective. We wonder what fatality rate the A&R authors are using as the basis for their opinions.
2. The second conclusion that the A&R authors contested was our finding that the IWI seems to have contributed to a low fatality rate. The fact that over 40% of the LSR drownings responded to IWI alone with resumption of spontaneous respiration (Hunsucker & Davison, 2010, p. 197) didn't make an impression on the A&R authors. Of course, we can't say for sure that IWI was the cause of the victim recovering respiration, but the fact that it occurred after IWI in 14 out of 32 LSR rescues is a good indicator. It could easily have been the application of IWI in these drownings that helped their recovery. The attitude of the A&R authors seems to be, and we paraphrase here with apologies, "you are doing something we are not and we consider it dangerous." Yet, the A&R authors are not even consistent in the reasons they criticize IWI. In the review on "Sub-Diaphragmatic Thrusts and Drowned Persons" (Pia, Fielding, Wernicki, & Markenson, 2010), several of the A&R authors recommendation that abdominal thrusts not be used in drowning was due to issues associated with removing water from the lungs. This is not the intent of IWI, which is to help restore spontaneous respiration. We can show, using both animal and clinical research, that abdominal thrusts provide positive pressure (Pargett, Geddes, & Otlewski, 2008). Obviously, additional clinical research would be necessary to find the exact physiological mechanism that is being used by the body to restore respiration.

The A&R authors' line of reasoning seems to be based on "Accept our opinions because of who we are" and to this end, they submit a list of cited opinions along with an extremely one-sided analysis. This type of reasoning, i.e., "Accept our opinions because of who we are," may well be one of the main reasons why there have been so few advances in developing more effective means of rescuing drowning victims.

One other observation made by the A&R authors is their characterization of abdominal thrusts as a "potentially dangerous intervention" (Wernicki, et al., 2011, p. 8). We wonder how they are using this term. Do they mean that there is a strong possibility of injury to the victim? If so, both medical research and our data do not support their conclusion. Research has shown that the incidence of injury associated with the correct application of abdominal thrusts (Lee, Kim, Shekherdimian, & Ledbetter, 2009) is very low and is much less than, for example, the chest compressions (Lederer, Mair, Rabl, & Baubin, 2004) referred to by the A&R authors (Wernicki et al., 2011, p. 11), which cannot be done in the water. Do they mean that there is a more effective protocol in restoring respiration? Our study looked at 63.8 million people and the protocol resulted in only four fatalities. This is a fatality rate of 0.00635 per hundred thousand guests, which is roughly 100 times better than the CDC estimate of 0.6 per hundred thousand for all pools (Hunsucker & Davison, 2010, p. 193). To answer their question about the injuries associated with the use of IWI, there were none of which we are aware, as reported in our paper. If there had been any injuries from the IWI, the aquatic facility should have reported it back to us and we would have published it. In fact, the authors of the rebuttal continue to discount the fact that over 40% of the victims had respiration restored by IWI alone (Hunsucker & Davison, 2010, p. 197).

To this end, the A&R authors should simply publish the rescue data from other protocols that support their conclusions. If the results for these other protocols are better than the one described in this paper, we will change. If on the other hand, there are no rescue data supporting a greater effectiveness of the protocol that the A&R authors are using as a basis for comparison to our protocol, they are not engaging in scientific discourse, but simple criticism based on emotion.

To the authors of the rebuttal, we would simply say again, we were surprised by the A&R response. While there are issues with any analysis of a large set of data and there are certainly differing views about developing effective protocols for dealing with drowning victims, the A&R authors seem to have ignored the opportunity to add to the effort to improve lifeguarding and instead focused on a personal, rather than an analytical, level. Nothing in their response has helped to increase the effectiveness of rescue protocols. It is simply a reiteration of the old argument about not using abdominal thrusts to clear the airway of water, which was not the rational for the inclusion of IWI. The A&R authors are committing a logical fallacy, the irrelevance fallacy, which consists of trying to prove something different then what is in question. We expected a more reasoned and objective response from the A&R authors.

While we certainly feel that the A&R paper should have been published, it should not have been in the research section of *IJARE* for it really is an opinion piece, as is this letter, not research. In summary, the rebuttal is fundamentally flawed in that it makes an unsubstantiated assumption about the IWI development, misses the points of the paper that the rebuttal is directed toward, does not really address the only two conclusions of the original paper, and makes unsubstantiated and one-sided claims about the methodology used to analyze, check, and validate the effectiveness of IWI in the rescue protocol.

As a final comment, please note that we have no issue or argument with the American Heart Association about the use of CPR on the deck. We support their efforts to improve the protocol and increase the knowledge we have about both drowning and other respiratory emergencies.

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