motheoraces were repaired. I question how anyone can know that. Two of the six patients died. One 82-year-old woman developed a 20% pneumothorax on the 37th day after hospital admission. She refused chest drainage and died 4 days later. A 32-year-old woman who had been hospitalized for 25 days developed pneumothoraces on both sides of the lung, with one of them displacing 50% of the lung volume. A chest tube was inserted to treat the large pneumothorax, and the lung was reexpanded. The pneumothorax recurred on this side 6 days later, and this was also managed conservatively. The patient developed progressive respiratory failure necessitating mechanical ventilation, which exacerbated persistent air leakage. She then developed refractory hypoxia and died. Four other patients had persistent air leakage for 14 days to 1 month. These patients had substantial morbidity, and it is conceivable that surgical repair of the air leaks would have improved their outcomes. I agree with the authors that it is impossible to know, without a controlled trial, whether surgical repair would have benefited these patients. Expert opinions that were developed in a consensus of experts convened by the American College of Chest Physicians\(^\text{2}\) have advised that most or all of the six patients described by Siho\(\text{e}\) et al should have undergone thorascopic repair.

The contribution by Siho\(\text{e}\) et al is important in that it described pneumothoraces in SARS in detail and raised some of the provocative ethical issues surrounding the care of patients with SARS or other contagious diseases. In their original report\(^\text{1}\) and subsequent letter, Siho\(\text{e}\) et al poignantly described the considerable anxiety that clinicians experienced during this frightening epidemic. Fortunately, experience with SARS and other contagious diseases has demonstrated that strict adherence to modern infection control practices protects staff very well. The authors and I agree that pneumothoraces should be repaired surgically in patients with SARS if such repair is judged likely to improve their outcomes.

\text{Gregory A. Filice, MD}  
\text{Veterans Affairs Medical Center}  
\text{Minneapolis, MN}

---

\text{REFERENCES}\n
1 Siho\(\text{e}\) ADL, Wong RHL, Lee ATH, et al. Severe acute respiratory disease complicated by spontaneous pneumothorax. \text{Chest} 2004; 125:2345–2351

---

\text{Immersion in Fresh Water and Survival}\n
\text{To the Editor:}\n
Aristotle observed “One swallow does not make a summer.” Similarly, the article entitled “Survival After Prolonged Submersion in Freshwater in Florida” that was recently published in \text{CHEST} (May 2004)\(^\text{1}\) lacks predictive value. The report only demonstrates that one victim survived a serious submersion accident. Had the report been properly prepared, it would have been overwhelmed by the literature on drowning survival. Unfortunately, deficiencies in data collection and analysis make the report an anecdote of average value.

A claim for recovery from “prolonged submersion” requires a valid estimation of submersion time. We do not even know the submersion time. The majority of the episode was not witnessed. How much time did the child spend in the water? How much time was spent continuously under water? It is quite possible that the child was not always submerged.

Claims for the benefits of cold water should be supported by temperature measurements. Basic accident scene investigation requires water temperature determination, and it is not provided. Emergency medical technicians (EMTs) failed to measure the victim’s temperature at the scene or during transport. The hospital personnel could not measure a temperature \(<\text{26.7°C}\). The authors provide no references demonstrating a protective effect for \(\text{26.7°C}\) water on children following prolonged submersion. Two reports that studied drowning outcome in larger groups found that immersion exceeding 10 min was not tolerated even by victims of cold temperatures.\(^\text{2,3}\) Core temperatures measured at hospital admission were higher in intact survivors than in those who died or recovered incompletely. Interestingly, one source\(^\text{4}\) cited by Modell et al\(^\text{1}\) demonstrated that careful testing could detect defects after apparent recovery from prolonged submersion in very cold water.

Did the victim have a cardiac arrest? If so, when did it occur? Pulse detection in cold, wet children may be difficult. The first EMTs “felt a weak pulse” after the rescuers found the child “pulseless.” The second EMT team found no pulse. After field treatment, a pulse was felt. Upon admission to the emergency department, “a femoral pulse was palpable.”

Was ECG monitoring never performed? One would expect it to have been performed at least during transport in an ambulance and upon admission to the hospital. The evaluation of treatment requires the accurate identification of the condition being treated.

The authors claim that bystander cardiopulmonary resuscitation, advanced life support during transport, and skilled hospital care were “key to the outcome as well.” They fail to prove that any of these treatment modalities extend submersion survival even in victims of cold temperatures.

Advance not in the understanding of drowning and its treatment will require careful data collection and analysis from large groups of patients. Unfortunately, the “Recommended Guidelines for Uniform Reporting of Data From Drowning”\(^\text{5}\) formed by an international task force do not meet the goals. The guidelines do not require data on duration of submersion, water temperature, victim’s temperature at the accident scene, evaluation of bystander resuscitation, and ECG monitoring at the scene or in the hospital. Acceptance of these guidelines would promote anecdotes rather than scientific knowledge.

\text{Christopher W. Dueker, MD}  
\text{Atherton, CA}

To the Editor:

Thank you for permitting us to respond to the letter to the editor authored by Christopher W. Dueker, MD, regarding our article entitled “Survival After Prolonged Submersion in Freshwater in Florida,” which was published in CHEST (May 2004). We are pleased that our article was of sufficient interest to Dr. Dueker that it provoked such an extensive response. Unfortunately, we believe that Dr. Dueker misunderstood the nature of the report and the reason for publishing it. Dr. Dueker stated that our article “lacks predictive value (and) it demonstrates that one victim survived a serious submersion accident.” Further, he suggested that this article should have been vastly expanded to include an entire review of the literature on drowning survival.

Our intent was to publish this article as a single case report of a child who survived prolonged submersion in freshwater in Florida. It is well known, and has been known for at least the past 3 decades, that survival after prolonged submersion in cold water is possible, but those previous articles reported on cases from the northern-most reaches of the United States, from Canada, and from Scandinavia. As a result, several clinicians in our part of the country do not believe that hypothermia, to a degree sufficient to permit normal survival after prolonged submersion, is possible; therefore, many are reluctant to aggressively treat such victims. Indeed, when this victim first arrived in our emergency department, some of our colleagues were concerned that, at best, we would end up with a severely brain-damaged child if we continued aggressive resuscitative measures. Obviously, that was not true.

The fact that this child not only survived that episode, but subsequently had normal results of psychological testing and now, 18 months later, has continued to undergo normal development for his age with no signs of adverse effects proves that our judgment was appropriate. We thought, therefore, that it would be beneficial to share this experience with others and to let them know that, although, obviously, all such victims will not survive with normal cerebral function, that outcome is a real possibility. This case report was never meant to have predictive value but, rather, to demonstrate success.

Dr. Dueker claims that we do not know the immersion time because the majority of the episode was not witnessed. I would point out that this is not uncommon. Collectively, the authors have had the opportunity to treat or consult in the treatment of > 120 drowning victims and have had the opportunity to review the records of > 200 others. From our experience, it is rare that the exact submersion time is known. In the majority of episodes that we have reviewed, it is not known when the victim became submerged or, for that matter, how long they spent under the water. In this particular case, we do have a timeline, which we described in the article, that begins at the time that the victim’s brother went to the neighbor to report that his brother was missing in the water. Adding to that the time spent by the neighbor going to the grandparents’ house, then going to the mother’s house, and then returning to the creek substantiates the immersion time. Thus, while it was not timed by a stopwatch, this, perhaps, is one of the more reliable time estimates that we have seen of the hundreds of cases that we have reviewed.

Further, Dr. Dueker makes the statement, “It is quite possible that the child was not always submerged.” It is unlikely that a 2-year-old child who could not swim, becomes submerged, and is found at the bottom of a relatively deep creek had periods of time when he was not witnessed that he rose above the surface of the water in order to take a breath.

Dr. Dueker goes on to state, “Claims for benefits of cold water should be supported by temperature measurements.” We have done this in reporting that the child’s temperature on admission to the emergency department was < 26.7°C. Dr. Dueker is critical of the emergency medical technicians (EMTs) for failing to measure the victim’s temperature and the temperature of the water at the scene of the accident. Because the EMTs and the child’s mother and aunt were consumed with attempting to resuscitate an apparently dead child, we certainly cannot fault them for not stopping their efforts to obtain the temperature of the water. Furthermore, where does Dr. Dueker propose that they should have measured temperature: at the surface or at the bottom where the victim was found? After all, it is not the temperature of the water that is important but, rather, the temperature of the victim that determines whether the brain will be protected from hypoxia by hypothermia. It also is our observation that in the several hundred other cases that we have reviewed over the years, the reporting of water temperature is the exception rather than the rule.

Additionally, we are aware of instances in which children fell into very cold water in northern climates but had on sufficiently insulated clothing that their body temperature never dropped significantly. In our case, the victim’s temperature did drop significantly. Thus, it is the child’s body temperature, not the water temperature, that is most important. It is well known that hypothermia to the level exhibited in this child does provide a protective effect from cerebral hypoxia. This is true not only in drowning victims that have been reported over the past 3 decades, but also is the major basis for the use of deliberate hypothermia to permit circulatory arrest during surgery to correct congenital heart defects.

Dr. Dueker states that there are two reports that found that immersion exceeding 10 min was not tolerated even by cold variants. This is not surprising. The message of our article is not that all hypothermic victims will survive but, rather, that hypothermia, when the immersion episode is followed by effective cardiopulmonary resuscitation, makes normal survival a possibility. Dr. Dueker refers to two articles that stated that core temperatures measured at hospital admission were higher in intact survivors than in those who died or recovered incompletely. Yet, in reading the article that Dueker refers to, by Bierenen et al., it states, “Predictors for better survival potentials were a young age, submersion of less than 10 minutes, no signs of aspiration, and a central body temperature of less than 35°C at admission.” Thus, contrary to Dr. Dueker’s statement, Bierenen et al. suggest that a decreased body temperature may improve survival rate, thus providing protection.

We are aware that publications exist demonstrating that careful testing can detect defects after apparent recovery from prolonged submersion. Our victim has had extensive testing, as reported in our article, and these tests did not reveal any long-term defects. Furthermore, he is now 18 months postimmersion and is totally normal.

We realize that pulse detection in cold, wet children may be difficult. However, this child was treated by two separate teams of EMTs and also a nursing team that was certified in cardiopulmonary resuscitation. A possible weak pulse was felt briefly by one EMT but could never be redocumented by any other of the EMTs or nurses in the field. A sustained palpable pulse did not return until shortly after the child was admitted to the emergency department of the hospital. The likelihood that none of the people who treated this victim in the field could feel a pulse and yet an effective pulse was still present is extremely low. An ECG
tracing was not reported by the EMTs during transport but was established shortly after arrival in the emergency department. Further, we should point out that it is possible to obtain an electrical impulse on an ECG and still not have an effective cardiac output (ie, pulseless electrical activity). Therefore, even if there were an ECG trace and the patient did not have a pulse despite multiple efforts to find one by multiple trained observers, cardiac output would be so low that one would not expect that adequate brain perfusion was present.

Dr. Dueker goes on to state, “They fail to prove that any of these treatment modalities extend submersion survival even in cold victims.” It is difficult for us to understand his point. Is he saying that our victim could have survived normally without resuscitation efforts by the EMTs and nurses, and without intensive care being administered in the hospital? We find that extremely difficult to believe and do not believe that there is any literature that would support that premise. Dr. Dueker goes on to state, “Advancement in the understanding of drowning and its treatment will require careful data collection and analysis from large groups of patients.” We do not disagree with this statement. However, case reports of this catastrophe have added substantially to our knowledge over the years. We also would point out to Dr. Dueker that the authors of this article collectively have published scores of articles on the subject of drowning based on data collected in large groups of human patients, both survivors and deceased, as well as in controlled animal studies.

The purpose of our case report was not to do a controlled study regarding the effects of various treatments on drowning victims, to establish tolerable lengths of time of submersion, or to be a definitive review article on the subject of drowning. Rather, as pointed out before, this was a single case report designed to bring to the attention of the medical community the fact that, with appropriate treatment beginning at the site of the accident and continuing through hospital management, it is possible for children who become significantly hypothermic during a submersion episode to survive after prolonged periods of submersion and to have normal brain function even in normally temperate climates. We do not believe that anything in Dr. Dueker’s letter challenges that conclusion.

Dr. Dueker also comments on the article entitled “Recommended Guidelines for Uniform Reporting of Data From Drowning” by Idris et al.3 We do not understand why he is critiquing that article in commenting on ours because they are two totally separate publications in different journals.

Jerome H. Modell, MD, DSc (Hon)
Ahamed Idris, MD
Jose Pineda, MD
Janel Silverstein, MD
University of Florida College of Medicine
Gainesville, FL

Reproduction of this article is prohibited without written permission from the American College of Chest Physicians (e-mail: permissions@chestnet.org).

Correspondence to: Jerome H. Modell, MD, DSc (Hon), University of Florida College of Medicine, Department of Anesthesiology, PO Box 100254, Gainesville, FL 32610-0254; e-mail: modeljh@shands.ufl.edu

REFERENCES