

COVER PAGE FOR the PDF file of

A Comprehensive Review of Frequently Misinterpreted and Misrepresented Restraint Research PARTS ONE, TWO, THREE – and the REFERENCES for ALL PARTS.

The following 39 PDF file pages were created and posted in **December of 2005**.
However, in **January of 2006**, PART THREE of this review was altered.
LINKS to the “WEIGHT STUDY” Collection of papers and report were added.

HERE are those January 2006-added LINKS:

The first file in this collection is a REFERENCE FILE randomly obtained from the Internet.
It contains relatively simple explanations of the pulmonary function testing terminology
used in all the Weight Force papers and report:

<http://www.charlydmiller.com/LIB07/PulmonaryFunctionTesting.pdf>

Eisele JW, Chan TC, Vilke GM, Clausen J.

Comparison of respiratory function in the prone maximal restraint position with and without
additional weight force on the back. The unpublished paper presented at the annual meeting of the
American Academy of Forensic Science, in Reno, Nevada: February 21-26, 2000.

<http://www.charlydmiller.com/LIB07/2000FebWeightForcePaper.pdf>

Chan TC, Clausen J, Neuman T, Eisele JW, Vilke GM.

Does weight force during physical restraint cause respiratory compromise?

Ann Emerg Med, October 2003;42(4). The unpublished paper presented at the 2003 ACEP
meeting, and posted in the **ACEP Research Forum Supplement**: pS17.

<http://www.charlydmiller.com/LIB07/2003OctWeightForcePaper.pdf>

Chan TC; Neuman T; Clausen J; Eisele J; Vilke GM.

Weight force during prone restraint and respiratory function.

Am J Forensic Med Pathol 25(3):185-189, September 2004.

**[CHAN's published report of the 1999 study previously represented by
paper presentations at two other medical professional society's meetings.](http://www.charlydmiller.com/LIB04/2004ajfmpweightstudy.pdf)**

<http://www.charlydmiller.com/LIB04/2004ajfmpweightstudy.pdf>

Vilke GM, Michalewicz B, Kolkhorst FW, Neuman T, Chan TC;

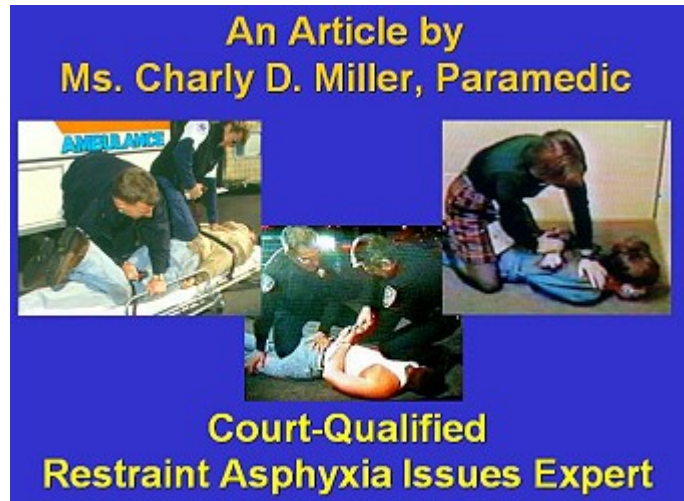
Does weight force during physical restraint cause respiratory compromise?

Acad Emerg Med May 2005;12(5 Supplement 1): page 16. The unpublished paper presented at the
May, 2005, Annual Meeting of the **Society for Academic Emergency Medicine**.

<http://www.charlydmiller.com/LIB07/2005MayWeightForcePaper.pdf>

YOURS, CHAS

(Ms. Charly D. Miller)



A Comprehensive Review of Frequently Misinterpreted and Misrepresented Restraint Research

CITATION:

Miller CD. A comprehensive review of frequently misinterpreted and misrepresented restraint research. Three parts, posted in February, March, & August, 2005.

<http://www.charlydmiller.com/LIB05/2005chasresearchreview.html>

A PDF file created and posted in December, 2005.

<http://www.charlydmiller.com/LIB05/2005ComprehensiveReview.pdf>

ABSTRACT:

Misinterpretation and misrepresentation of research and review articles can have deadly consequences. At this writing, the three most-frequently misinterpreted and misrepresented restraint-asphyxia-related research and review articles are:

1. Chan TC, Vilke GM, Neuman T, Clausen JL.
Restraint position and positional asphyxia
Ann Emerg Med, November 1997;30:578-586.
2. Chan TC, Vilke GM, Neuman T.
Reexamination of custody restraint position and positional asphyxia
Am J Forensic Med Pathol, September 1998;19(3):201-205.
3. Chan TC; Neuman T; Clausen J; Eisele J; Vilke GM.
Weight force during prone restraint and respiratory function
Am J Forensic Med Pathol, September 2004;25(3):185-189.

The primary purpose of this THREE-PART review is to thwart those who persist in misinterpreting and misrepresenting these articles by assisting others to understand their **TRUE content**. This is accomplished by providing accurate explanations of each article's content; and by providing substantiated **BACKGROUND INFORMATION** relevant to the authors' "motivation" for performing

the studies and reviews that generated these articles: information not available to the majority of readers – information not even available to many professionals who subscribe to the journals that published these articles.

This review provides concrete support for the following FACTS;

1. No one has ever performed a "clinical study" of the physical effects experienced by individuals who are subjected to forceful-prone-restraint (or hogtie restraint) during real-life situations.
2. No one has ever performed a "clinical study" proving that no ill effects will occur when an individual is subjected to forceful-prone-restraint (or hogtie restraint) during real-life situations.
3. Unbiased medical and forensic professionals universally agree that application of forceful-prone-restraint during real-life situations (with or without hogtie) is extremely dangerous, is accompanied by a very high risk of causing "wrongful death," and should not be performed by emergency responders (or others).
4. Those who persist in promoting misinterpretation or misrepresentation of restraint-asphyxia-related research and review articles are acting in a "morally and ethically indefensible" manner, and are demonstrating the strong likelihood that they have "personal agendas" inconsistent with a concern for preventing death.
5. In fact, **Theodore Chan** et al have demonstrated incidents wherein he (they) LIED about information they published, in addition to having significantly MISREPRESENTED research regarding the subject of restraint asphyxia.

A Comprehensive Review of Frequently Misinterpreted and Misrepresented Restraint Research; PART ONE

CITATION:

Miller CD. A comprehensive review of frequently misinterpreted and misrepresented restraint research; Part one. February, 2005.
<http://www.charlydmiller.com/LIB05/2005chasresearchreviewpart1.html>

ARTICLE #1:

Chan TC, Vilke GM, Neuman T, Clausen JL.

RESTRAINT POSITION AND POSITIONAL ASPHYXIA

Ann Emerg Med, November 1997;30:578-586.(1)
<http://www.charlydmiller.com/LIB/1997chan01.html>

The "Restraint Position and Positional Asphyxia" (RPPA) clinical study that generated this article was developed and conducted some time in 1996 by Dr.s Theodore C. Chan, Gary M. Vilke, Tom Neuman, and Jack L. Clausen. Dr.s Chan, Vilke, and Neuman were of the Emergency Medicine Department, University of California San Diego Medical Center (UCSD), San Diego, California. Dr. Clausen was of the UCSD Pulmonary Medicine Department.(1)

RELATED BACKGROUND INFORMATION

The UCSD RPPA study was initiated by Neuman at the personal request of San Diego County Deputy Counsel, Ricky R. Sanchez, specifically for the purpose of developing evidence to assist Sanchez in defending two San Diego County Sheriff Deputies against a "wrongful-death lawsuit" filed by the survivors of Daniel Price, a man who died while being hogtied and kept in a forceful-prone-restraint position by them in 1994.(2) In fact, Sanchez arranged for San Diego County to pay for the study.

I first learned something of what "motivated" this study's generation when I was sent a copy of a January 14, 1998 San Diego Union-Tribune Newspaper Article: **"Police Hogtie Restraint Doesn't Kill, Evidence Now Shows"**
<http://www.charlydmiller.com/LIB/sandiegohr.html>

According to the newspaper article, the decision to dismiss a wrongful-death lawsuit against sheriff's deputies who were forceful-prone-restraining a man (with hogtie) when he died in 1994, **"was based largely on new evidence developed at the request of a lawyer for San Diego County."**

Annals of Emergency Medicine

Chan TC, Vilke GM, Neuman T, Clausen JL: Restraint position and positional asphyxia. Ann Emerg Med, November 1997;30:578-586.

The San Diego Union-Tribune.
Police hogtie restraint doesn't kill, evidence now shows
Case here may have impact nationwide
 J. Harry Jones
 STAFF WRITER
 14-Jan-1998 Wednesday

"A San Diego federal judge has dismissed a wrongful-death lawsuit filed by the survivors of a Poway man who died after being hogtied by sheriff's deputies in 1994. The decision last week was based largely on new evidence developed at the request of a lawyer for San Diego County."

Obviously, one cannot trust "facts" presented in a newspaper article. Since then, however, I've obtained **LEGAL DOCUMENTS** wherein both Dr.s Neuman and Chan corroborate the study-generation information reported in the 1998 San Diego Union-Tribune article.(3-6)

Furthermore, the official **Price v San Diego** report also corroborates the 1998 San Diego Union-Tribune article study-generation information.(2)

Neuman has testified that he was **entirely unacquainted with the subject of positional asphyxia or restraint asphyxia** prior to being approached by Sanchez to be an "expert" witness regarding restraint-related asphyxia issues related to the PRICE case.(3) [At this writing, it remains unknown whether or not Chan, Vilke, or Clausen – the other study authors – had any knowledge of positional asphyxia or restraint asphyxia issues prior to the study's development and performance.]

Neuman has also testified that – in spite of his lack of knowledge regarding this subject – he was asked by Sanchez to be an "expert" witness specifically for the purpose of discrediting decades of restraint-related research published by reputable forensic pathologists (Reay et al), in order to assist Sanchez's defense of two San Diego County Sheriff Deputies.(3)

After evaluating some restraint asphyxia materials provided him by Sanchez, Neuman advised Sanchez that a new study would need to be performed in order for Neuman to discredit Reay et al's materials, and that Sanchez would have to provide the funds to accomplish such a study.(3) Sanchez agreed to do so, and soon thereafter the County of San Diego provided UCSD with \$33,900.00 to fund Neuman's study.(6,7) [In deposition, Chan refers to this funding as a "**GRANT**" from the County of San Diego.(8)]

Chan has testified that, because Neuman (a UCSD "senior faculty" member) was too busy to do so, Neuman asked him to develop, organize, and direct the study.(8) That is why Chan's name appears first in the study report's list of authors.

In deposition, Chan described being personally introduced to Sanchez at a restaurant luncheon "get-together" hosted by Neuman.(9) Vilke -- another RPPA study co-author -- was among those who attended Neuman's "get-together" luncheon. However, Chan denies remembering any other attendees' names, and denies remembering (for a fact) whether Neuman's "get-together" luncheon occurred before the study was performed – or after the study was performed.(9)

Yet, according to Neuman, the **average body mass index (BMI)** of the "**general**" population was **not researched** for the purpose of UCSD study subject selection because, "**Mr. Sanchez paid for this study, [so] we were trying to pick a [study] population that looked more towards the type of person that Mr. Price was [the San Diego case victim] as far as size, habitus [BMI], gender.**"(10) Thus, whether or not Chan was personally introduced to Sanchez prior to the study's design and performance, the fact that only study subjects with a BMI similar to Mr. Price were selected for the UCSD RPPA study indicates that Chan knew of Sanchez's specific needs prior to designing the UCSD RPPA study to accommodate them.

BACKGROUND INFORMATION SUMMARY:

- The UCSD "Restraint Position and Positional Asphyxia" study was requested and funded by someone with a **very specific personal agenda**; San Diego County Deputy Counsel, Ricky R. Sanchez.
- The UCSD RPPA study was developed, performed, and evaluated under the guidance of a "senior faculty" member, Dr. Tom Neuman; an individual who was entirely unacquainted with restraint asphyxia-related issues prior to being personally asked by Sanchez to act as an "expert witness" regarding restraint asphyxia-related issues; an individual who hosted a "get-together" luncheon to introduce other study "researchers" to Sanchez.
- The UCSD RPPA study subjects were selected based upon at least one of the specific needs of San Diego County Deputy Counsel, Ricky R. Sanchez. Thus, it is entirely possible that other aspects of the study were developed (or evaluated) in order to meet Sanchez's specific needs.

BACKGROUND INFORMATION CONCLUSION:

- **The UCSD RPPA study clearly was *contaminated* by an entirely inappropriate "BIAS" well prior to its development and performance.** Therefore, it is more-likely-than-not that the authors' evaluation of the study data was equally contaminated by an inappropriate "BIAS."

Thankfully, within the peer-reviewed and published report of the UCSD RPPA study, the authors clearly identified the fact that their study's findings **cannot be applied to real life situations**. Thus, as long as their report is read very carefully, it is unnecessary for readers to have an understanding of its development and performance **background information** in order to recognize that the study's findings are entirely unhelpful to those concerned about the dangers associated with real life restraint situations.

UNFORTUNATELY, not everyone who reads this study reads it carefully. In fact, many who cite it have never actually read it. Instead, their citation relied entirely upon a misrepresentation of the study's data provided to them by those who claim to be "experts" at interpreting it.

Even more unfortunately, when not faced by their peers (when providing expert reports to attorneys, or when in deposition and trial testimony situations), **the authors of this study grossly misrepresent the findings they obtained.** [Support for that statement is provided in a later section of this review.]

THE UCSD RPPA STUDY's PUBLISHED INFORMATION(1) STUDY METHODS

This was a "controlled trial" performed at the University of California San Diego Medical Center's Pulmonary Function Laboratory. The study's 15 subjects were healthy men ages 18 – 40 y/o. Subjects were excluded from the study if they had abnormal lung functions, or if their body mass index (BMI) was greater than 30 kg/m².

"Two potential subjects were excluded for abnormal screening PFT [Pulmonary Function Test] measurements, and another individual was excluded for BMI greater than 30 kg/m²."

I am 5 feet 9 inches tall, and weigh 200 pounds.

My BMI is 30 kg/m².

To calculate YOUR BMI, go to any of these
(or similar) Internet websites:

The National Heart, Lung, and Blood Institute BMI Table

<http://www.nhlbisupport.com/bmi/bmicalc.htm>

The National Institutes of Health BMI Table

http://www.nhlbi.nih.gov/guidelines/obesity/bmi_tbl.htm

CRESTOR BMI Index Table

<http://www.crestor.com/c/tools/bmi.asp>

"PHASE 1" of the study consisted of performing "Pulmonary Function Tests" (PFT) on each healthy, rested study subject, while he was "randomly" placed in four different positions:
A sitting position. A supine position. A prone position. A "restraint position."



a "TUNTURI" cycle ergometer

<http://www.supps.net/Scripts/prodView.asp?idproduct=738>

Next, after another 4-minutes of stationary bicycle exercise, each study subject was placed in a "restraint position" for a 15 minute "rest period."

During this second PHASE 2 "rest phase":

- "Blood samples for ABG [Arterial Blood Gas] analysis were obtained at 1.5 minutes and 15 minutes."
- One "PFT was performed at 3 minutes into the first rest period."
- "Oxygen saturation by both ear and finger probes and pulse rate by ECG tracing were recorded every 3 minutes during the first rest phase."

"The results of this study demonstrated significant changes in both static and dynamic pulmonary function testing with position and exercise."

STUDY METHODS IDENTIFICATION, DISCUSSION & REVIEW

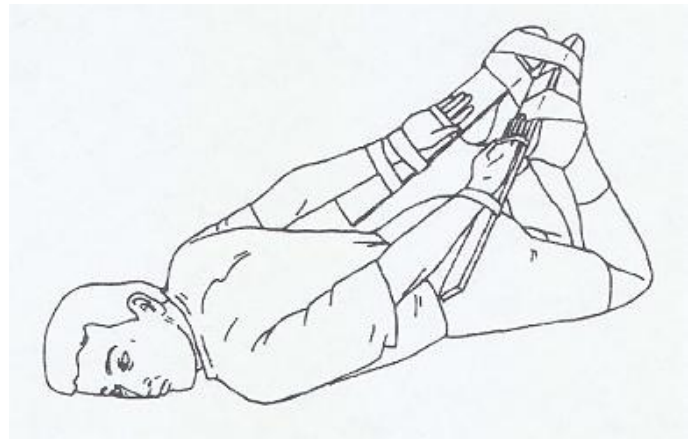
Within the abstract (introduction) of their study, the authors concretely identified their "**Study Objective**" as being,

"To determine whether the '**hobble**' or '**hogtie**' restraint position results in clinically relevant respiratory dysfunction."

However, within their article's body, the authors admitted that the restraint position they used to conduct their '**hobble**' or '**hogtie**' restraint study only "**closely approximates the restraint position noted in previous studies and case reports in the prehospital setting**" – only "closely" approximated the position employed during REAL-LIFE field-application of forceful-prone-restraint with hobble and hogtie restraints.

At right is the "**Figure 1**" drawing, commissioned and published by the authors to demonstrate the "**RESTRAINT POSITION**" employed for their study.

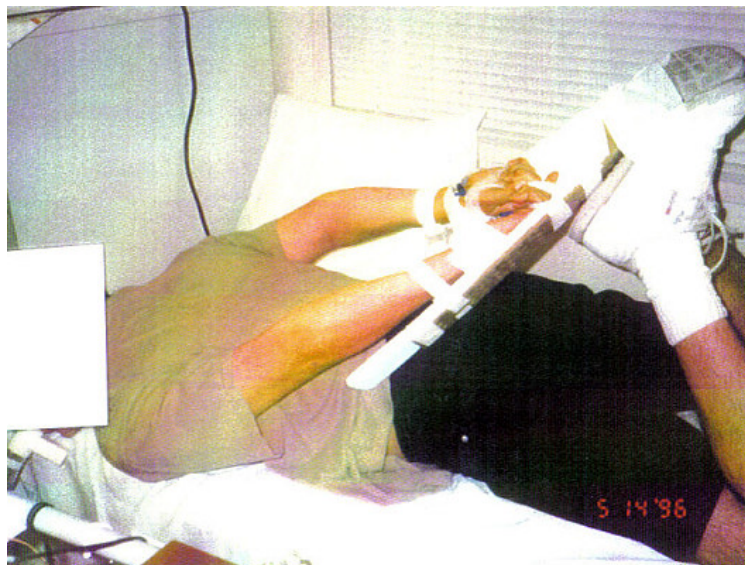
**Clearly, this is
NOT
a real-life
"hogtie" position.**



In 2004 I obtained the following two PHOTOGRAPHS of the study position that generated the **DIAGRAM** Chan et al commissioned for their November 1997 article's Figure 1. (I have no idea why they didn't just use the PHOTOS.)



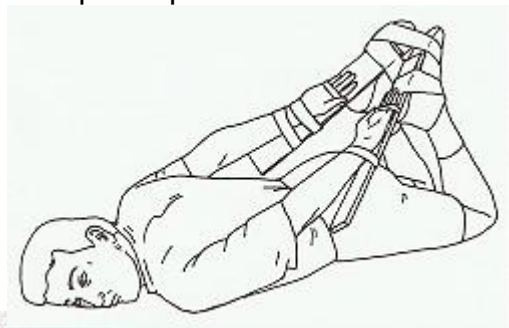
**Again, CLEARLY,
this position is
NOT a real-life
"hogtie" position.**



Below are four photos of what "REAL-LIFE" hogtie restraint looks like:



Note that the **SHOULDERS** of all individuals shown in "real" hogtie restraint are pulled **UP** and **OFF** of the ground. Note that their **KNEES** are also somewhat pulled **UP** and **OFF** of the ground. As you can see; a "real" hogtie position causes someone's **ENTIRE BODY WEIGHT** to rest upon their ribcage and **ABDOMEN**. Thus, a "real" hogtie restraint position interferes with the ability of the **DIAPHRAGM** (the largest muscle of respiration) to easily contract and descend into the abdomen, because it interferes with the room available for the abdominal contents to get out of the way of the descending diaphragm. Consequently, a "real" hogtie restraint position seriously increases the amount of effort required in order for hogtied individuals to breathe – even without "force" being actively applied to their torsos while they are kept in a prone position.



The **SIDE-BY-SIDE COMPARISON** above

clearly demonstrates that the "restraint position" employed for the UCSD RPPA study did **NOT** duplicate a true "hogtie" restraint position.

The differences between the "real" hogtie position and the UCSD RPPA's "restraint position" were ignored (apparently considered inconsequential) by the study authors, but these differences are physiologically significant.

The UCSD RPPA study "restraint position" clearly allowed the healthy study subjects' shoulders and knees to comfortably reach and rest upon the floor. Thus, the subjects' shoulders and

knees had the ability to assist in supporting (distributing) their body weight, diminishing the amount of weight that had to be supported by the subject's ribcage and abdomen.

Since no **force** was being applied to his prone body, if any UCSD RPPA study subject experienced any difficulty breathing while in the study's restraint position, he easily could have slightly flexed his shoulders or his knees (or both), compressing them against the ground so as to elevate his ABDOMEN and allow more room for his abdominal contents to move out of the way of his descending diaphragm. Thus, **unlike truly hogtied individuals**, each study subject had the ability to afford his diaphragm better room to contract and descend into the abdomen – allowing him to breathe better and with much less effort. Additionally, Chan admits that it is unlikely that he or the other study researchers would have noticed (or REPORTED) such a minor – but significant – amount of shoulder- and/or knee-flexion.(11)

To justify their study's failure to use a real hogtie restraint position, Chan et al cited their need **"to allow secure placement of a radial artery [IV] line."** It is true that the radial artery (the wrist artery) is the safest artery to use in order to obtain arterial blood gas samples. However, in a controlled environment such as the University of California San Diego Medical Center's Pulmonary Function Laboratory, the BRACHIAL artery (the one on the "inside" of the elbow) could also have **securely** and **safely** been used. In fact, because the brachial artery is easy to access even when someone is placed in a real hogtie restraint (especially when the person is **cooperative** with restraint – such as the UCSD RPPA study subjects were), using the brachial artery would easily have allowed Chan et al to use a true form of hogtie restraint for their study.

But, because the UCSD RPPA study authors ELECTED to use an artery that allegedly interfered with them being able to study a real hogtie restraint position, the study's findings clearly are NOT RELATED to the authors' purported **"STUDY OBJECTIVE"**: that of determining **"whether the 'HOBBLE' or 'HOGTIE' restraint position results in clinically relevant respiratory dysfunction."**

Interestingly enough, although they elected to ignore the significant physiological differences between real hogtie restraint and the "restraint position" they used, within their peer-reviewed and published article, **the UCSD RPPA study authors identified several OTHER REASONS why their study's findings cannot be applied to real-life situations.**

The following are QUOTES from the study's published article:

"There are limitations to this study. First, we restricted subjects to healthy men between the ages of 18 and 40 years with a BMI less than 30 kg/m²; ... **It is not known what effect positional restraint may have on women, the young, the elderly, or other individuals with underlying cardiopulmonary disease or disability.** It is possible that extremely obese individuals with large abdominal girths and BMIs greater than 30 kg/m² **may be at greater risk for development of restrictive pulmonary function pattern as a result of abdominal compression from body position."**

"We specifically excluded potential subjects who had a positive result on urine toxicology screening for recreational drug use. As noted previously, many of the deaths of restrained individuals involved subjects who were intoxicated or under the influence of recreational drugs. Stimulants, such as cocaine and amphetamines, may increase oxygen demand and muscle fatigue, affecting overall respiratory function."

"This study did not attempt to duplicate exact field conditions under which restraint position deaths have occurred. Although many such deaths have occurred on gurney mattresses or cushioned car seats in the field, some deaths have occurred

while persons were in the restraint position on the ground. Deaths have also occurred on the floors of police cars, where the contoured surface may have increased abdominal compression. **In addition, these individuals may have been subject to forceful apprehension, during which pressure may have been exerted on their backs while they were in the restraint position.** What effects these differences may have **remain to be determined.**"

"We attempted to reproduce the physiologic effects of struggle by requiring our subjects to exercise for 4 minutes before being placed in the restraint position. **It is unlikely that this period of exercise would simulate all the physiologic alterations that may occur with struggle and agitation. In addition, we did not reproduce the effects of trauma and psychological stress that often occur with apprehended individuals.**"

"It is possible that a combination of factors, including underlying medical condition, intoxication, agitation, delirium, and struggle as well as body position, may result in respiratory compromise that would not be detected by our study."

"... further research is needed on the role of these other factors in the deaths of individuals placed in the restraint position."

EXAMINATION OF THE STUDY'S "CONCLUSION":

Even though they neglected to employ a "restraint position" that reproduced the effects suffered by individuals subjected to a true form of hogtie restraint, and in spite of all the other study limitations identified by the authors, **"[Chan et al.] measured significant differences in pulmonary function test results between control and restrained individuals."**(12)

Yet, in the **CONCLUSION** of their study's report, the authors wrote:

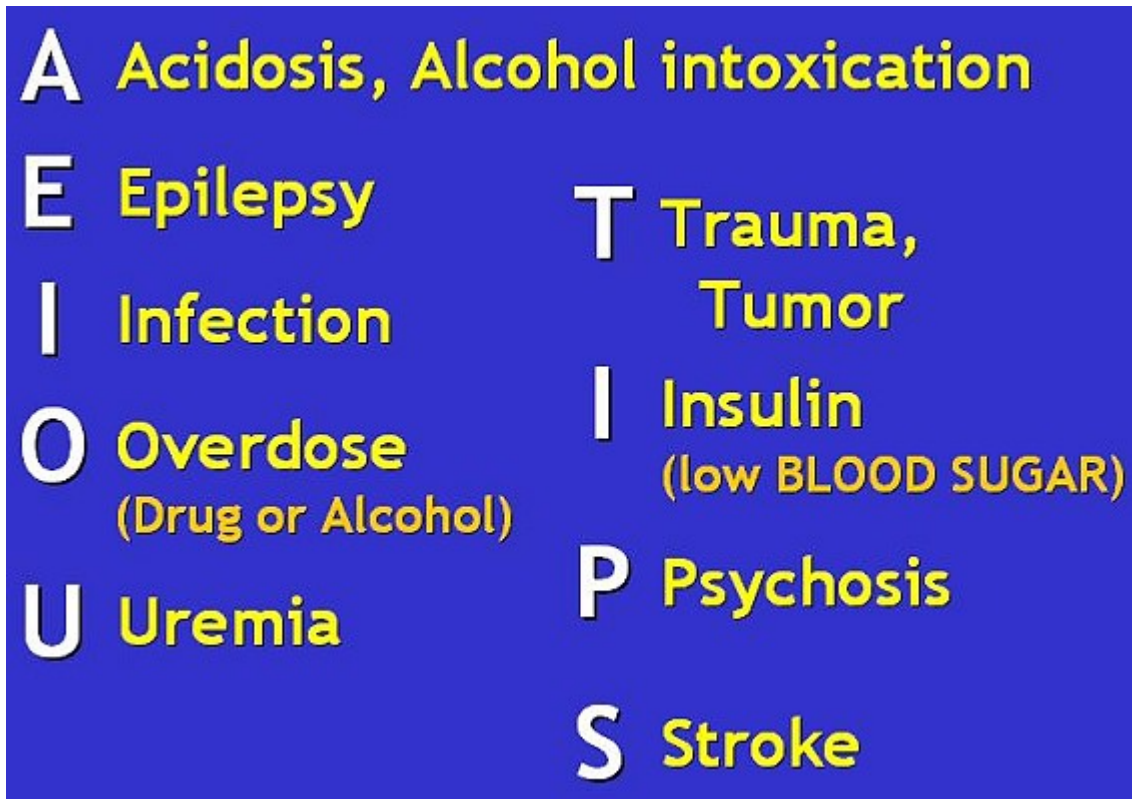
"By itself, the restraint position was not associated with any clinically relevant changes in respiratory or ventilatory function in our study population of healthy individuals with preserved ventilatory reflexes and normal pulmonary physiology. There is no evidence to suggest that hypoventilatory respiratory failure or asphyxiation occurs as a direct result of body restraint position in healthy, awake, nonintoxicated individuals with normal cardiopulmonary function at baseline."

How many **ENTIRELY HEALTHY**, "awake, nonintoxicated individuals with normal cardiopulmonary function ... and normal pulmonary physiology" have required the application of **maximum forms of RESTRAINT** (forceful-prone-restraint, with or without hogtying) in **real-life ("field")** situations?

NONE. That's how many.

ALL individuals who EVER have required application of "maximum" forms of restraint in the field were the victims of SOME sort of

"ALTERED LEVEL OF CONSCIOUSNESS."



The above graphic of the "AEIOU TIPS" mnemonic for **Altered LOC Causes** represents only a VERY SMALL SAMPLE of the MULTITUDE of conditions or emergencies that can cause someone to suffer an Altered LOC.

CLEARLY; if ANY of these multiple "**other factors**" accompany incidents wherein respiratory arrest and/or death occurs during restraint application, since **the UCSD RPPA study did NOT study them**, the UCSD RPPA study's results **cannot be applied when evaluating such incidents**.

MORE "BACKGROUND" INFORMATION: RELATED LETTERS PUBLISHED AFTER the STUDY's PUBLICATION

Excerpts from (Forensic Pathologists) Howard JD and Reay DT.'s
Letter to the editor re. Restraint position and positional asphyxia
Annals Of Emergency Medicine July 1998; 32(1):116-117.(12)
<http://www.charlydmiller.com/LIB/reayannals.html>:

[Chan et al.] measured significant differences in pulmonary function test results between control and restrained individuals, but describe the changes as "not clinically relevant."

...

Restrictive pulmonary function alteration that may not be "clinically relevant" might well be one of several contributing factors in some deaths where restraint is used. ...

Applying only "clinically relevant" values to a measurement may lead to misinterpretation of findings in deaths that occur outside the clinical setting.

Excerpts from a statement sent to me by Dr. Reay in June, 1998(13);
 Reay DT. **Hog-Tied Revisited**
<http://www.charlydmiller.com/LIB/reaysdiego.html>:

[Chan et al's] study showed that our study in 1988 (Am J Forensic Med Pathol 1988;9:16-18), which measured only cutaneous (skin) oxygen saturation, was incorrect and flawed and our interpretation that hog tying produces physiological consequences of recovery times as measured by pulse and oxygen saturation was contradicted by their work.

I readily acknowledged the value of [Chan et al's study] ... This has since been presented in law enforcement publications as my retraction of positional asphyxia as a cause of death, with particular reference to hog-tying.

.....Such is not the case! I still maintain that there are risks and hazards to restraint maneuvers including hog-tying and each case must be evaluated to assess the presence or absence of respiratory restriction in the light of the method of restraint.

A 280-pound man with a large [belly] is at risk in the face down position as well as a person with obstructive pulmonary disease. And there are many shades in between.

The point is that **street deaths are much different than controlled investigations.** If 14% respiratory restriction by hog-tying is not viewed as clinically significant in normal people, it has to be evaluated in the context of the event where it may be significant.

Excerpts from Dr.s Vilke, Chan, and Neuman's
Letter to the Editor Re: Patient restraint in EMS
and the "Patient restraint in EMS" AUTHORS' REPLY;
Prehosp Emerg Care July/September 2003;7(3):417-419.(14)
<http://www.charlydmiller.com/LIB02/2003naemspchanletter.html>:

Kupas and Wydro state that "patients should never be transported while hobbled, 'hog-tied,' or restrained in a prone position (which) has been associated with asphyxia." ... **Reay has retracted his conclusions about the physiologic effects of the hobble position based on "more comprehensive" clinical research.** [Vilke et al cite their UCSD RPPA study here.] This new research included a study by Chan et al. in which subjects were placed in the sitting, supine, prone, and hobbled positions with spirometric, arterial blood gas, pulse oximetry, and cardiovascular monitoring, in which no significant change to suggest increased risk for asphyxiation was demonstrated."

Excerpts from Kupas and Wydro's Reply to Vilke et al.:

Vilke and colleagues express a concern that there is not firm evidence associating deaths in restrained individuals to the prone or hobble positions. ... Despite their comments, **Vilke et al. do not make any statement that advocates for the general use of these positions during restraint of violent patients.**

... [Note that] **none of the physiologic studies related to the prone or hobble positions** [cited by Vilke et al] **were performed on individuals who were violently struggling against the restraints or were under the influence of adrenergic or sedating drugs.** We do not know what effect other medical conditions, psychiatric conditions, violent agitation, or drug intoxication would have on the physiologic effects of prone or hobble restraint positions.

Vilke and colleagues also suggest that "Given the numbers of cases of sudden death that occur to restrained individuals in all positions, we would recommend rhythm monitoring, when possible, in all restrained patients with frequent cardiopulmonary assessments as well." This statement is further reason to avoid prone or hobble restraint positions, because cardiopulmonary assessments are more easily accomplished when the patient is in the supine position.

We still believe that patients should never be transported while hobbled, "hog-tied," or restrained in a prone position with hands and feet behind the back.

MORE "BACKGROUND" INFORMATION: TESTIMONIES PROVIDED by STUDY AUTHORS AFTER THE UCSD RPPA STUDY WAS PUBLISHED

When writing an article for submission to a professional journal, researchers must pen their statements very carefully, because they know their article will be "peer-reviewed" prior to being accepted (or rejected) for publication. In fact, according to Chan, the *Annals of Emergency Medicine* peer-reviewers provided UCSD RPPA study article authors with critical suggestions causing them to rewrite sections of their article (better identifying the limitations of their study's results), so as to win the approval of the peer-reviewers and gain their study's *Annals of Emergency Medicine* publication.(15)

However, when NOT faced by their "peers" – when providing expert reports to attorneys, or when providing deposition or trial testimony – UCSD RPPA authors have grossly misrepresented their study's findings.

All of the following excerpts are from legal documents associated with cases involving deaths due to restraint asphyxia. In each of these cases, the UCSD RPPA study authors who are quoted had been hired by the attorney(s) charged with defending persons who had restrained an individual in a manner that led to his death.

As you read these excerpts, keep in mind that; within the peer-reviewed 1997 publication of their 1996 UCSD RPPA study's report, **Chan et al freely admitted that their study's findings CANNOT be applied to real life situations.**

UNITED STATES DISTRICT COURT, San Diego, California:
Ann PRICE et al., v. County of San Diego et al. January 8, 1998.
"FINDINGS OF FACT AND CONCLUSIONS OF LAW"(2)
<http://www.charlydmiller.com/LIB05/1998priceVsandiego.html>:

Quotes from the Final Report of PRICE v SAN DIEGO:

On June 30, 1994 Price died. A county medical examiner, John W. Eisele, M.D., conducted the autopsy. Dr. Eisele found low levels of methamphetamine in Price's system. He also found petichiae (pinpoint) hemorrhaging in Price's left eye, which suggests that Price's torso had been compressed. Dr. Eisele listed the cause of death as "hypoxic encephalopathy due to restrictive asphyxia with cardiopulmonary arrest due to maximum restraint in a prone position by law enforcement."

Dr. Eisele testified that Price experienced lactic acidosis. ... Dr. Eisele testified that because the hogtie restraint impairs the mechanical process of exhaling, it prevents the body from "blowing off" excess carbon dioxide. In other words, Dr. Eisele opined that Price suffered from asphyxia (an increase in carbon dioxide levels) that, because of the hogtie, Price's body could not correct.

Quotes from the Final Report Attributed To NEUMAN's Trial Testimony Provision:

After Price's death, at the request of defense counsel, Thomas Neuman, M.D., of the University of California at San Diego Medical Center ("UCSD") conducted a sophisticated study of positional asphyxia and the hogtie restraint. Dr. Neuman found ... that although the hogtie restraint impairs the mechanical process of inhaling and exhaling to an extent, the hogtie does not affect blood oxygen or carbon dioxide levels. In other words, the impairment is so minor that it does not lead to asphyxia, and in fact has no practical significance.

The UCSD study also refutes Dr. Eisele's opinion that the hogtie prevents the lungs from "blowing off" excess carbon dioxide.

As Dr. Neuman testified, it is wild speculation to say that a person lying prone with a potbelly will asphyxiate to death ...

Dr. Neuman perfectly captured the cause of death when he made the following statement: **We have clear data that there is no respiratory component to the hogtie position.**

Romero vs. Rural Metro et al. Cause No. 335185

in the Superior Court of the State of Arizona, in and for the County of Pima.

Quotes from a sworn affidavit written by Dr. Theodore Chan (in lieu of submitting an "Expert Report"),

and signed under oath on April 27, 2001:

4. I was directly involved in the research study performed at UCSD, funded by a grant from the County of San Diego for the specific purpose of examining the issue of positional restraint as it relates to pulmonary function and specifically the effect of the hobble restraint on respiratory function.

...

7. In the matter of Romero v City of Tucson; Rural Metro, et al., I was requested ... for my opinion concerning the impact of hobble or partial hobbled restraint of Mr. Romero and what role, if any, it played as a cause of his death.

...

8. It is my opinion, within a reasonable degree of medical probability [based upon the UCSD RPPA study], that the position in which Mr. Romero was restrained and then transported to the Emergency Department of Kino Hospital **did not compromise Mr. Romero's respiration and, therefore, played no role in causing death from respiratory failure, arrest or asphyxiation.**

Moore vs. Guardian Protective Services, INC, and Rural Metro of North Texas, L.P. (MEDSTAR Ambulance); Cause No. 17-187577-01;

in the District Court of Tarrant County, Texas, 17th Judicial District;

Transcript of testimony provided by Dr. Tom Neuman,

during a deposition conducted on Sunday, June 23, 2002: page 46; lines 7-19:

- Question: Do you agree that asphyxial deaths still occur when suspects are held prone with their arms and legs restrained and weight applied to their backs for minutes?

- NEUMAN: Yes, I disagree with that.

...

- Question: And why do you disagree with that generalization?

- NEUMAN: Because the prone restraint position does not have any significant effect upon gas exchange.

Kapanak vs. City of Phoenix; Superior Court of the State of Arizona; County of Maricopa; No. CV 2001-012136;

Transcript of testimony provided by Dr. Theodore C. Chan,

during a deposition conducted on Tuesday, August 26, 2003: page 56; lines 2-8:

- Question: But over time, if the body position is decreasing the ability or the mechanics of respiration, can that not eventually lead to respiratory arrest?
- CHAN: The restraint position?
- Question: Yes.
- CHAN: No. I believe our studies have refuted that theory.

Same Deposition Transcript as above: page 88, lines 6-13:

- CHAN: So the assumption is he's hobbled now.
- ...
- Question: Is that something you would agree is okay medically?
- CHAN: Yes.

Same Deposition Transcript as above: page 89; lines 14-25:

- Question: You wouldn't be concerned with him being in the prone, facedown position based on your work in the field of restraint activity, because based on your studying and testing, you concluded that being in the prone position on a gurney, say, for a five-minute trip to the hospital in the company of medical personnel would not be medically dangerous to a person in Brian's condition?
- CHAN: There is no evidence to suggest that prone positioning itself would place him at risk. The only issues with placing him in a prone position would be, number one, I'd probably not have him face down, you know. If he's prone, we'd want his face turned to the side.

Same Deposition Transcript as above: page 101; lines 16-24:

- Question: ... what would you call the Figure 1 position that you had in your 1997 article where you show a diagram of the restraint position – that's intended to simulate the classic hog tie, is it not?
- CHAN: It is.
- Question: And so would you then refer to that as a classic hog-tie position, Figure 1 of your study?
- CHAN: I would say it does simulate a classic hog-tie position.

Same Deposition Transcript as above: pages 182-183; lines 19-25, 1-8:

- Question: Would you agree that if somebody is in excited delirium, that they should not be put in a hog-tie position?
 - CHAN: No.
 - Question: What possible ethical or moral or legal reason would you have to put somebody who is in excited delirium in a hog-tie position?
 - CHAN: I'm not a law enforcement expert in terms of what is the most effective way and safest way to restrain somebody from the security standpoint. If you're asking me do I know of any medical literature that supports not placing somebody in a hog-tie restraint versus other restraints, I know of no compelling medical literature to support that.
-

The Only TRUE CONCLUSIONS that can LEGITIMATELY be derived from ARTICLE #1 ["Restraint position and positional asphyxia"(1)]:

Based upon a review of this study's methods, its content, its pre- and post-publication background information, and the post-publication testimonies provided by the study's authors; the following facts are indisputable:

1. This study was seriously contaminated by a grossly inappropriate "BIAS" long before it was developed and performed.
 The UCSD RPPA study's "OBJECTIVE" was NOT the one stated by its authors; **"To determine whether the 'hobble' or 'hogtie' restraint position results in clinically relevant respiratory dysfunction."**
 This study's TRUE OBJECTIVE was
"To afford Dr. Tom Neuman the means to provide testimony that could be *INFERRED* as discrediting decades of restraint asphyxia research performed by unbiased forensic medical professionals; testimony that could be *INFERRED* as suggesting that hogtie restraint 'doesn't kill; so as to assist San Diego County Deputy Counsel Ricky Sanchez in his defense of two San Diego County Sheriff Deputies accused of causing the "wrongful-death" of a man who died while being hogtied and kept in a forceful-prone-restraint position by them in 1994."
2. The UCSD RPPA study's un-weighted, pseudo-hogtied, "Restraint Position" – by itself – caused HEALTHY individuals to suffer diminished lung function.
3. The UCSD RPPA study was NOT a "clinical study" of the physical effects experienced by individuals who are subjected to forceful-prone-restraint (or true hogtie restraint) during real-life situations.
4. The UCSD RPPA study provided absolutely NO information regarding the effects that prone (or hogtied) restraint may have upon unhealthy individuals, especially when agitated and involved in violent struggle with others.
5. The UCSD RPPA study did NOT prove that forceful-prone-restraint (or hogtie restraint) is **"safe"** to use during real-life restraint situations.
6. The UCSD RPPA study provided absolutely NO information that can be legitimately considered when someone is evaluating any death that is associated with real-life restraint situations.
7. Those who persist in suggesting that the UCSD RPPA study provided information that can legitimately be considered when evaluating any death that is associated with real-life restraint situations are acting in a "morally and ethically indefensible" manner(16), and are demonstrating the strong likelihood that they have **"personal agendas" inconsistent with an interest in promoting THE TRUTH or preventing death.**

[The REFERENCE LIST FOR ALL PARTS FOLLOW PART 3]

A Comprehensive Review of Frequently Misinterpreted and Misrepresented Restraint Research; PART TWO

CITATION:

Miller CD. A comprehensive review of frequently misinterpreted and misrepresented restraint research; Part two. March, 2005.
<http://www.charlydmiller.com/LIB05/2005chasresearchreviewpart2.html>

ARTICLE #2:

Chan TC, Vilke GM, Neuman T.

Reexamination of custody restraint position and positional asphyxia

Am J Forensic Med Pathol, September 1998;19(3):201-205.(17)

<http://www.charlydmiller.com/LIB/1998chan.html>

ABSTRACT:

"... We review case reports of custody deaths in subjects placed in the hogtie position, as well as related medical literature regarding positional asphyxia. We also review the current research findings from human physiology studies that have investigated the effects of the hogtie position on respiratory and pulmonary function. We conclude that the hogtie restraint position by itself does not cause respiratory compromise to the point of asphyxiation and that other factors are responsible for the sudden deaths of individuals placed in this position."

... CONCLUSIONS

"Based on these findings, **factors other than body positioning appear to be more important determinants for sudden, unexpected deaths in individuals in the hogtie custody restraint position.** Illicit drug use (including sympathomimetic, hallucinogenic, and psychomotor stimulant drugs), physiologic stress, hyperactivity, hyperthermia, catechol[amine] hyperstimulation, and trauma from struggle **may be more important factors** in the deaths of these individuals. Although restraints in general increase the psychological and physiologic stress on the individual, no evidence suggests that body position alone causes hypoventilation, respiratory compromise, or positional asphyxia in the hogtie custody restraint position."

In this 1998 article, Chan et al "reviewed" several previously-published case studies of deaths occurring between 1985 and 1995, wherein the victim died while being subjected to an asphyxial form of restraint (forceful-prone-restraint, with or without hogtie).(17) Chan et al painstakingly re-reported the myriad of different circumstances ("factors") additionally associated with each of these restraint-related deaths. Then, they did their best to cast doubt upon the cause-of-death conclusions drawn by

forensic pathologists (individuals *infinitely more qualified* than Chan et al to draw conclusions regarding the cause of someone's death).

What was Chan et al's **primary support** for their argument that *they* knew better than forensic pathologists? Their November 1997 Annals-published report generated by the controlled clinical study they performed evaluating the effect of a pseudo-hogtie position on perfectly healthy volunteers.(1)

BASICALLY, the BIGGEST problem with Chan et al's September 1998 "Reexamination" article is this:

The way they worded their review article's conclusions **erroneously** infers that "factors other than body positioning" were the REAL cause of these deaths (all of which occurred during the application of an asphyxial form of restraint). Chan et al neglected to discuss the fact that ALL the other factors they identified were **ENTIRELY SURVIVABLE – IF** the person suffering from one or more of them was NOT restrained in a manner that interfered with breathing!

In other words, NONE of these "factors other than body positioning" – all by themselves – were responsible for any of the restraint asphyxia deaths reviewed by Chan et al. Had any ONE of these "other factors" been evidenced at autopsy to have been a **MORE-LIKELY-than-restraint-asphyxia** "cause" of someone's death, that person would never have been considered a restraint asphyxia victim in the first place. That victim's death would immediately have been conclusively attributed to one or more of these "other factors."

And, although any of these other factors may have contributed to **hastening** death AFTER the person was restrained, since none of them were solely-responsible for any of the reviewed deaths, NONE of them can possibly be considered "**more important**" factors than the form of restraint that ultimately resulted in each death. They were simply "associated" factors.

RELATED BACKGROUND INFORMATION

The structure and wording of this review article strongly indicates that

1. Chan et al remain primarily preoccupied with promoting the entirely FALSE idea that hogtie or forceful-prone-restraint "DOESN'T KILL"
2. Chan et al remain primarily preoccupied with providing people like San Diego County Deputy Counsel, Ricky R. Sanchez a means to defend those who restrain someone to death.

But, WHY would seemingly respectable and learned physicians persist in promoting a FALSE interpretation of their research?!

What is Chan et al's **motivation** for doing this?

At this writing, I do not – yet – know the precise answer to those questions. However, I am investigating several potential reasons.

Potential Reason #1; FOR THE MONEY:

Since their November 1997 Annals article was published, Chan, Neuman, and Vilke have been making a rather substantial amount of money working as "Expert Witnesses" for attorneys seeking to defend those who restrain someone to death.

Although I've worked at least one restraint asphyxia case wherein CHAN has testified in support of the individuals who restrained someone to death (maybe two or three cases – I forget), I've not yet managed to obtain **CHAN's "FEE SCHEDULE."** (A "Fee Schedule" is the document that identifies how much an "expert" charges to review case material and provide testimony.)

Either Chan has managed to protect his Fee Schedule from "discovery," or the attorney(s) I worked for didn't think to send it to me.

HOWEVER; on page 12 (lines 14-22) of the August, 2003 deposition testimony he provided in the case of **Kapanak vs. City of Phoenix; Superior Court of the State of Arizona; County of Maricopa; No. CV 2001-012136**; Chan made it very clear that **1998 was the first year that he ever began earning income by providing "Expert" testimony for legal cases.** Consequently, Chan's "Expert Witness" career was non-existent prior to the November 1997 Annals article's publication.

I **have** obtained Neuman and Vilke's 2001 Fee Schedules. They were included within the materials I received to review for **Stetter vs. Village of Hanover Park, et al. US District Court, Northern District of Illinois Eastern Division; CAUSE NO. 99 C 7084** in February, 2003.

If you'd like to see Neuman and Vilke's 2001 Fee Schedules, **CLICK HERE** [link = <http://www.charlydmiller.com/LIB05/feeschedules.html>], and then come right back!

HOWEVER. Even though Chan et al make a lot of money when they perform case material reviews and provide testimony for attorneys seeking to defend those who restrain someone to death, I cannot believe that their motivation for misrepresenting their work is purely monetary. After all ... they're Doctors! They already make a ton of money doing what they regularly do. Plus, "to make money" is not supposed to be a "motivation" for doctors doing the things they do!

Perhaps I'm naïve – perhaps I'm wrong – but, until I obtain other information strongly identifying MONEY being their motivation, I elect to believe that Chan et al's desire to make more MONEY is NOT the motivation for their persistent promotion of a false interpretation of their research.

Potential Reason #2; TO "SAVE FACE":

Since their November 1997 Annals article's publication, Chan et al have received STRONG criticism from a number of Forensic Pathologists (including JD Howard and DT Reay), and a number of their "peers" (including DF Kupas and GC Wydro). Forensic and medical professionals who are **unbiased** and **knowledgeable** in this subject have strongly admonished Chan et al for citing the values they obtained (from studying healthy volunteers in a controlled setting) in a manner that encourages "misinterpretation of findings in deaths that occur outside the clinical setting."**(12,13,14,16)**

To re-read the small collection of published criticism regarding Chan et al's November 1997 Annals article that I provided in PART 1 of this Review, [Revisit page 13 of this PDF file] and then come RIGHT BACK!

When you read them, keep in mind that these are only the **published** letters and comments they received. Clearly, Chan et al probably received numerous other, unpublished, criticisms for the manner in which they misrepresented their study's findings.

Obviously, Chan et al would not want to admit that they misinterpreted (or ever misrepresented) their clinical study's findings. SO: When faced with all the criticism they've received, Chan et al may have decided to write their September 1998 "Reexamination" article in an attempt to **suggest** that there is

further "support" for the conclusions they misrepresent as having been identified by their November 1997 Annals article.

Currently, THIS is the motivation I most-strongly believe led to Chan et al writing a review article that superficially seems to provide additional support for the entirely unsubstantiated and FALSE idea that they've "proven" that hogtie or forceful-prone-restraint "DOESN'T KILL."

Fortunately, anyone who is *unbiased* and *knowledgeable* in this subject easily recognizes that Chan et al's September 1998 "Reexamination" article's CONCLUSIONS are equally as flawed as their November 1997 Annals study article's CONCLUSIONS.

Unfortunately, anyone who is NOT *unbiased* and *knowledgeable* in this subject easily may adopt the erroneous belief that Chan et al's September 1998 "Reexamination" article provides additional support for the misinterpretations and misrepresentations of Chan et al's November 1997 Annals study article.

Potential Reason #3; TO AVOID LITIGATION???:

If more articles and/or review papers are published, containing suggestions that Chan et al's November 1997 Annals article provided "proof" that forceful-prone-restraint (with or without hogtie) "DOESN'T KILL" – **even though that's *not true*** – does that make Chan et al less susceptible to accusations of having "contributed" to the restraint asphyxia deaths that have occurred since their 1997 article's publication and their subsequent *promotion* of its misrepresentation?

I have no idea! I am not an attorney. (I don't play one on TV – I didn't stay in a Holiday Inn Express last night.) But, I'm working as hard as I can to imagine *other* potential motivations – apart from "for the MONEY" and "to SAVE FACE" – for Chan et al to act as they have. And, this is the ONLY other potential motivation that I can come up with.

However, if this IS their motivation, it's entirely INEFFECTIVE.

It has been my experience that; the more Chan et al (and others of their ilk) "muddy" the waters with misrepresentation of the November 1997 Annals article's study findings, the more **time-consuming** it becomes for unbiased and knowledgeable individuals to explain and expose such misrepresentation. But, no matter how muddy the water becomes, unbiased and knowledgeable individuals are NOT PREVENTED from providing explanation and exposure of these individuals' misrepresentation of the facts.

Thus, no matter how muddy the water becomes, IT **IS A FACT THAT:**

ANYONE who has argued that Chan et al's November 1997 Annals article provided "PROOF" that asphyxial forms of restraint "DON'T KILL" has CONTRIBUTED to EVERY RESTRAINT ASPHYXIA DEATH that has occurred SINCE THEN.

In the future, if I can contribute to a "Class Action" law suit being successfully brought against Chan et al (and others of their ilk), I will happily do so. It is my opinion that their persistent misrepresentation of the November 1997 Annals article's study findings has *significantly contributed* to ALL of the restraint asphyxia deaths that have occurred since it was published.

After all; had Chan et al (and others of their ilk) honestly represented the study's findings, **NO ONE would have been able to use their study (or their subsequently-provided legal testimony) as an EXCUSE to CONTINUE USING the ASPHYXIAL FORMS OF RESTRAINT that are STILL KILLING PEOPLE TODAY!**

MORE "BACKGROUND" INFORMATION: A RELATED LETTER Published After Chan et al's REVIEW ARTICLE's Publication

An Excerpt From: Reay DT, Howard JD.

(Letter to the editor regarding) Restraint position and positional asphyxia.

Am J Forensic Med Pathol September 1999; 20(3):300-301.(18)

<http://www.charlydmiller.com/LIB02/1999and2000letters.html>

We still have concern regarding deaths that occur during restraint. From the work of Chan et al., we now know that the hog-tied position should not produce serious physiologic consequences. However, during street restraint maneuvers, the totality of events must be considered. In the process of rendering a person helpless to handcuff him or her in a prone position, the involved officers may be required to "pile on" the suspect, pinning the person to the ground with the partial or full weight of the officers and thus compressing and restricting ventilatory function. The physical condition of the person and the circumstances of restraint can make a difference, and each case must be evaluated with a careful reconstruction of events to identify respiratory interference during and after the "takedown" and before the person is restrained. One of us (D.T.R.) was witness to a take-down of an obese man whom the police were trying to control. While compressed on the ground, the man repeatedly complained that he was having difficulty breathing. He was handcuffed and sat upright and stopped complaining. We wonder what the outcome would have been if he had been held face down on the ground until he became quiet.

'Nuff said, I think.

IN CONCLUSION: Chan et al's September 1998 "Reexamination" article

- FAILED to present ANY "new" or "important" information that has any legitimate relationship to the evaluation of the "real life" cases it was alleged to have "Reexamined."
- FAILED to present ANY "new" or "important" information that – in ANY WAY – legitimately provided additional "support" for the misinterpretation that Chan et al persist in promoting related to their November 1997 Annals article's study findings.
- Is ENTIRELY WITHOUT MERIT, and is – unfortunately

[The REFERENCE LIST FOR ALL PARTS FOLLOW PART 3]

A Comprehensive Review of Frequently Misinterpreted and Misrepresented Restraint Research; PART THREE

CITATION:

Miller CD. A comprehensive review of frequently misinterpreted and misrepresented restraint research; Part three. April, 2005.
<http://www.charlydmiller.com/LIB05/2005chasresearchreviewpart3.html>

ARTICLE # "3":

Chan TC; Neuman T; Clausen J; Eisele J; Vilke GM.
Weight force during prone restraint and respiratory function.
Am J Forensic Med Pathol, September 2004;25(3):185-189.
<http://www.charlydmiller.com/LIB04/2004weightstudyajfmp.html>

RELATED BACKGROUND INFORMATION for ARTICLE # "3": This study and its published report has a very ... interesting ... history.

This article is the THIRD incarnation of the study's report, and is the only version that has ever been published as a journal article.

A paper-presentation of this study's findings was FIRST presented at the annual meeting of the American Academy of Forensic Science, in Reno, Nevada; February 21-26, 2000.

Eisele JW, Chan TC, Vilke GM, Clausen J:
Comparison of Respiratory Function in the Prone Maximal Restraint Position With and Without Additional Weight Force on the Back
<http://www.charlydmiller.com/LIB/2000eiselechan.html>

This means that the actual "Weight Force" STUDY had to have been performed prior to February, 2000. Likely, the study was performed some time in 1999 – more than 4 years before Chan finally found someone to "publish" it!

The original "lead" study author, and the AAFS 2000 conference Presenter, was **Dr. Eisele**.

RUMOR has it [I've honestly forgotten when or from whom I heard this! But, it surely was some time in 2002, and surely from a California physician who **knows** Dr. Eisele and the Chan et al gang.] ... **RUMOR** has it that Dr. Eisele didn't feel the study had yielded information that was in any way worth bothering to publish. Consequently, he wasn't interested in pursuing the study's publication.

[When I first posted the above rumor (September, 2004), I included the following note: Dr. Eisele! If you wish to "refute" this rumor, PLZ contact me and testify to same in writing! I'll

happily retract the "rumor" if you give me a reason to! I have yet to hear a peep from Dr. Eisele. However, the offer still stands.]

Whether or not Dr. Eisele felt that the study yielded information that was in any way worth bothering to publish, the American Academy of Forensic Science (the conference host) clearly did NOT. Had the AAFS felt that the study's report was in any way worthy of publishing, the paper would have progressed to become an article in the AAFS' publication, the ***Journal of Forensic Sciences***. It did not.

When the study's information surfaced again – over three years later, as another conference paper presentation – I found it terrifically interesting (especially considering the rumor about Eisele's opinion of its findings) that **CHAN** was suddenly cited as the "lead" author and paper Presenter. Additionally, Neuman's name was added to the "author" list, and Dr. Eisele's name was bumped back to just before Vilke's name:

Chan TC, Clausen J, Neuman T, Eisele JW, Vilke GM. **Does weight force during physical restraint cause respiratory compromise?** *Ann Emerg Med*, October 2003;42(4), **ACEP Research Forum** Supplement: pS17.

<http://www.charlydmiller.com/LIB03/2003chanweight.html>:

Chan's paper presentation paper was a slightly-altered version of Eisele's 2000 AAFS paper presentation, and was presented by Chan in **October of 2003**, at the ***American College of Emergency Physicians*** conference in Boston.

I'll identify the significant alterations Chan made in an upcoming section of this review.

What is important to note HERE; had the ACEP peer-reviewers considered Chan's representation of this study's findings to be in any way worth bothering to publish, Chan's paper presentation would have progressed to become an article published in ACEP's publication, the ***Annals of Emergency Medicine***.

It did not.

Consequently, not only is the ***Am J Forensic Med Pathol*** September 2004, "Weight force during prone restraint and respiratory function" article the THIRD-version report of a study that was performed prior to February of 2000, it took Chan more than four years to find someone willing to bother publishing it.

Additionally, within his 2004 version of its report, Chan entirely failed to identify the DATE that the study was performed. Thus, only the few people who attended the 2000 AAFS and/or 2003 ACEP conferences – or the very few people who carefully monitor Chan's track record for misinterpretation and misrepresentation – could possibly have known that Chan's report is that of study information obtained back in 1999.

EVIDENCE of CHAN'S MISREPRESENTATION &/or MANIPULATION of the STUDY'S INFORMATION:

Chan's misrepresentation and manipulation of the study's information is most clearly demonstrated by his descriptions of how the study subjects were selected for, or excluded from, study participation.

From Eisele's 2000 AAFS paper presentation:

"Ten healthy volunteers were recruited and informed of the procedure, and gave verbal and written consent. They were screened for pulmonary function in the sitting position and rejected if baseline forced vital capacity (FVC) or forced expiratory volume in one second (FEV1) were outside of acceptable limits.

...

Although body size and habitus are matters of concern in evaluating the effects of restraint, the subjects in this study were intentionally kept within an average range;" [range undefined]

More than three years later, in his 2003 ACEP paper presentation, Chan striped this study subject selection description down to the following:

"Ten volunteers completed a randomized crossover, controlled trial in a pulmonary function laboratory."

[That's it! That is the ENTIRE study subject selection description offered by Chan in his 2003 ACEP presentation paper!]

More than four years after the study was performed, here is the study subject selection description that Chan managed to have published:

"We performed a randomized, cross-over, controlled trial on 10 subjects placed in 4 positions for 5 minutes each ..."

"Ten volunteer male subjects between the ages of 18 and 45 years were recruited to participate in the study. Potential subjects were excluded if they were unable to be placed in PMRP. No exclusion was made on the basis of pulmonary or cardiovascular disease or function, or based on body size and weight."

... "Subjects ranged in age from 21 to 40 years, and body mass index ranged from 21.3 to 35.3 kg/m². There were no exclusions of any participant or subject data."

Considering the above facts, here are the CONCLUSIONS that can legitimately be drawn regarding Chan's REPRESENTATION of the 1999 "Weight Force" study's subject exclusion descriptions, and the questions prompted by same:

1. In his 2004-published report, Chan clearly and purposefully avoided identifying the fact that ALL of the 1999 "Weight Force" study subjects were entirely "HEALTHY" individuals.
What possible reason could Chan have for leaving this terrifically significant factor OUT of his 2004 study report? Is it – perhaps – that Chan is motivated to "down-play" the fact that all the study subjects were perfectly HEALTHY individuals?
2. Chan DID publish his admission that, if a volunteer was "unable" to simply be placed in a prone and pseudo-hogtied position (what Chan calls the "prone maximal restraint position," or "PMRP"), the study subject candidate was EXCLUDED from the study. But, what does that "unable" exclusion MEAN, and why does Chan fail to explain what it means?
Does it mean that some study subject candidates were excluded merely because they **felt uncomfortable** when placed in the PMRP position?
If a study subject candidate's "**inability**" to be placed in a prone and pseudo-hogtied position was not related to his body size and weight – if it was not related to his simple complaint of **discomfort** when placed in the study position before weight addition – then, WHAT was the genesis of this exclusionary "inability"?

And, again, WHY didn't Chan bother to explain this when he was finally given the opportunity to publish the study's report?

3. In his 2004-published report of the study subject candidates' inclusion / exclusion parameters, Chan insisted that **"No exclusion was ... based on body size and weight."**

Yet, in Dr. Eisele's February 2000 paper presentation, Eisele very clearly stated that **"the subjects in this study were intentionally kept within an average range."**

So? Who was lying ... er ... misrepresenting study subject inclusion / exclusion parameters – Eisele or Chan?

Considering the fact that Dr. Eisele has never (to my knowledge) been evidenced to have the same BIAS that Chan has historically been evidenced to have [See Part One of this Comprehensive Review], I am entirely confident that CHAN is the person who misrepresented study subject body size and weight considerations related to study subject inclusion / exclusion parameters – **not Eisele.**

4. In 2000, Dr. Eisele clearly identified that study subject candidates **"were screened for pulmonary function in the sitting position and rejected if baseline forced vital capacity (FVC) or forced expiratory volume in one second (FEV1) were outside of acceptable limits."**

Dr. Eisele had absolutely no conceivable motivation to FABRICATE such very specific screening parameters when creating his February 2000 paper presentation of this 1999 study's information.

Thus, within his 2004-published report of this same 1999 study's information, it is abundantly clear that **Theodore Chan point-blank LIED** when he reported that **"No exclusion was made on the basis of pulmonary or cardiovascular disease or function ..."**

5. Chan is easily demonstrated as having LIED and/or MISREPRESENTED test study subject exclusion information ONLY because Dr. Eisele's very brief February 2000 paper presentation provides a clear and definitive **baseline** for investigating this particular portion of the study's information in comparison to Chan's 2004-published version of it.

But, because Eisele's 2000 paper is so brief, it remains entirely unknown how many OTHER study information misrepresentations (or outright lies) Chan may have perpetrated in his 2004 report. After all, whatever motivated Chan to lie about and misrepresent test study subject exclusion parameters, may also have motivated him to lie about or misrepresent any number of other study finding aspects.

Basically, the lies and misrepresentations so clearly demonstrated by Chan's "version" of the 1999 weight study test study subject exclusion information, entirely destroy any single shred of CREDIBILITY that Chan may have retained prior to this article's publication.

Unfortunately for future victims of forceful-prone-restraint asphyxia, only those who read this review will likely ever learn of Chan's lack of credibility.

ANOTHER Example of Chan's Affinity for Gross Misrepresentation of Information is Demonstrated by the Following Text from his 2004-Published Report:

Chan writes:

"Some have argued that the PMRP [Prone Maximal Restraint Position] prevents adequate chest wall, abdominal, and diaphragmatic movement, leading to hypoventilatory respiratory compromise and risk for death from so-called positional asphyxia.¹² However, case reports and case series of the sudden deaths of restrained individuals do not clearly indicate a specific mechanism.⁴⁻⁷ Historical as well as autopsy evidence is often unrevealing as to a clear cause of death. Importantly, similar sudden deaths have been reported in patients who

were not restraint[sic] in the PMRP, but simply in the prone, supine, lateral side, and even sitting positions.13,14"

Clearly, Chan is dramatically alleging that case reports have been published identifying individuals DYING when **simply** (without force) being positioned "in the prone, supine, lateral side, and even sitting positions." Chan even infers that these case reports identifying "simple" – without force – restraint deaths are more "important" than all other published evidence, when considering the legitimacy of case studies identifying deaths being caused by [what Chan styles as] "so-called positional asphyxia"!

WELL. Let's look at the two references containing the published "case reports" that Chan cites in order to apparently provide SUPPORT his profoundly dramatic allegation – shall we?

Chan's 2004 Reference "13" is Park KS, Korn CS, Henderson SO.

Agitated delirium and sudden death: two case reports.

Prehosp Emerg Care. 2001;5:214-216.

<http://www.charlydmiller.com/LIB02/2001naemsp2cases.html>

[MY **boldface** treatment of text within the following quotes.]

Park/Korn/Henderson Case #1:

"45-year-old African American man with a history of schizophrenia was found standing at the corner of a motel office, shaking his head violently, hallucinating, and resisting aid. Police and EMS were summoned and, after an initial struggle, the patient was **manually restrained and strapped to the gurney in a supine position. Paramedics were unable to obtain vital signs due to the combative nature of the patient**, but noted no overt signs of trauma. The skin was warm and dry, and the pupils were midrange and reactive. Although the patient entered the ambulance awake and alert, he became markedly less responsive during transport, with a rapid decrease in mental status. Within 15 minutes he progressed to cardiopulmonary arrest with an **asystolic rhythm.**"

Park/Korn/Henderson's Case #1 did NOT indicate a "simple" supine restraint position. In fact, they didn't indicate the MANNER in which the patient was supinely restrained – **at ALL!**

They simply described the patient as being "strapped to the gurney in a supine position." Did the paramedics employ their gurney's standard harness safety belt system to restrain this "combative" individual's supine torso? If they did, tightening the harness to a point where it would become a torso "RESTRAINT" would cause significant impedance of both the chest and abdomen.



Standard Safety Harness System

In fact, an ambulance wheeled stretcher's SAFETY BELT harness system is NOT designed to be used for the purpose of RESTRAINING a violent individual.

Furthermore, even after the initial (unknown) manner of forceful supine strapping to the gurney was accomplished, the paramedics still were "unable to obtain vital signs due to the combative nature of the patient." Thus, one can realistically and reasonably presume that the paramedics employed more and more forceful methods of "strapped" or manual forms of restraint (to more and more unknown portions of the patient's body) in continued efforts to obtain vital signs. During this activity, the patient entered "cardiopulmonary arrest with an **asystolic rhythm**."

[BTW: Those who do not recognize the significance of a patient entering cardiac arrest with an initial ECG of "pulseless electrical activity" or "asystolic rhythm" should review their ACLS texts ... specifically refreshing their memory of the most frequently-occurring dysrhythmia following a "simple" or "common cardiac arrest" **vs.** the most frequently-occurring dysrhythmias following an ASPHYXIAL cause of death.]

BOTTOM LINE: Park/Korn/Henderson's Case #1

did NOT indicate a death occurring during "simple" supine restraint position.

Park/Korn/Henderson Case #2:

"A 41-year-old African American woman with a history of polysubstance abuse was brought to the ED by police and paramedics for bizarre behavior including slamming her head against a brick wall. On arrival, she was combative, speaking in incoherent sentences, and refusing to stay on a gurney. ... **The safety personnel placed her in a sitting position on the gurney with her wrists handcuffed to her ankles.** The patient suddenly collapsed with no spontaneous respirations or pulse. Cardiopulmonary resuscitation was initiated. **Electrocardiographic monitoring revealed pulseless electrical activity, ...**"

"Computed tomography (CT) of the patient's head showed signs of **anoxic encephalopathy**. Electroencephalography (EEG) showed slow diffuse encephalopathy. **The patient remained acidotic and was treated for rhabdomyolysis while in the intensive care unit. ...**

"Safety personnel had placed the patient in a sitting position with her **wrists handcuffed to her ankles, so that she was bent forward, placing pressure on her chest, a risk factor for positional asphyxia.** In addition, she showed evidence of rhabdomyolysis, and was hyperthermic and acidotic[sic]."

[BTW: For those who recognize its significance, this woman's pH upon arrival at the hospital was "**6.93**"! Those who do not recognize the significance of this woman's pH should quickly visit my **RESTRAINT ASPHYXIA DEATHS vs. "COMMON CARDIOPULMONARY ARREST" DEATHS: ACIDOSIS LEVELS DISCUSSION** page – and then come right back!

<http://www.charlydmiller.com/RA/restrasphyxacid.html>]

Park/Korn/Henderson's Case #2 did NOT indicate a "simple" incident of restraint while "even sitting" causing death! In fact, the authors clearly indicated within their report that the victim was NOT in a "simple" seated position when she died.

"Safety personnel had placed the patient in a sitting position with her wrists handcuffed to her ankles, so that she was bent forward, placing pressure on her chest, a risk factor for positional asphyxia."



NO. The above two graphics do not show a patient with her "wrists handcuffed to her ankles" while in a seated position on a wheeled stretcher. That kind of restraint is SO entirely inappropriate to medical care and transportation, that no care provider in their right mind would admit to doing it – no educator in their right mind would recreate such an inappropriate form of restraint for photography or graphic art representation purposes.

Still, the above two graphics give you an idea of what aspects of Case #2's restraint position would have looked like.

Simply visualize the victim seated on an ambulance wheeled stretcher. Being on a wheeled stretcher, her knees could not have been flexed much (if at ALL, depending upon how her legs were restrained to the stretcher). So, her legs were probably straight out in front of her body.

Thus, handcuffing her wrists to her ankles while seated on an ambulance wheeled stretcher would cause her to be SIGNIFICANTLY bent-over (forward) at the waist, and would seriously interfere with her abdominal excursion – seriously interfere with her diaphragm's ability to generate adequate breathing efforts.

Granted, Park/Korn/Henderson's 2001 case discussion's evaluation of why this victim suffered respiratory interference while restrained in the manner they reported was WRONG.

It wasn't "pressure on her CHEST" that impeded her mechanical ability to breathe – the structure of her ribcage would have prevented such impedance! It was BELLY MOVEMENT (abdominal excursion) interference caused by her bent-forward "sitting" restraint that interfered with her DIAPHRAGM (the largest and most important respiratory muscle) from functioning so as to generate adequate breathing efforts. Thus (as with forceful-prone-restraint), ABDOMINAL RESTRICTION is what interfered with her mechanical ability to breathe.

But, even given this inaccurate cause of respiratory arrest discussion, Park/Korn/Henderson's case study #2 CERTAINLY offered **no indication** that a **"simple" incident of restraint while "even sitting" had caused this victim's death!** If Chan actually read this case study, he could not help but know that.

Consequently, it appears that Chan cited a case study that he KNEW did NOT represent an incident of "simple" seated restraint causing death, **so as to provide support for a statement he knew to be erroneous!**

In other words, **Chan lied.**

Well, gosh! Perhaps Theodore Chan was merely ... **MISTAKEN** ... when citing reference **13** to support his incredibly dramatic statement that case reports have been published identifying individuals DYING when **simply** (without force) being positioned "in the prone, supine, lateral side, and even sitting positions."

So, let's look at the SECOND reference Chan cited in support of his statement:

Chan's 2004 Reference "14" is Hick JL, Smith SW, Lynch MT.

Metabolic acidosis in restraint-associated cardiac arrest: a case series.

Acad Emerg Med. 1999;6:239-243.

<http://www.charlydmiller.com/LIB/1999acidosis.html>

This is a relatively wonderful case report series! It's one of the first to identify and call attention to the **profound acidosis** that accompanies restraint asphyxia deaths, vs. the very much LESS acidotic pH that accompanies "simple" or "common" cardiac arrest victims.

And, this case report series discusses 5 different incidents of restraint asphyxia.

[Again, MY **boldface** treatment of text within the following case report quotes.]

Hick/Smith/Lynch Case #1:

"A 36-year-old man was acting extremely agitated and belligerent on a downtown sidewalk. ... attacked a police officer and ran. ... subdued by several officers. ... transported to the ED, where he continued to fight vigorously while lying **prone** with his hands cuffed behind him. ... Shortly thereafter, the patient had a witnessed respiratory arrest. ... Shortly after intubation, a 15-second episode of asystole was noted;"

The phrase, "he continued to fight vigorously while lying **prone** with his hands cuffed behind him" suggests that there was something for him to "fight" against, during his efforts to get out of the prone position. It suggests that manual and/or mechanical forms of restraint were maintaining him in a prone position. That's not someone "simply" dying while in a prone position.

No help for Chan's statement here.

Hick/Smith/Lynch Case #2:

"A 39-year-old-man with a history of unspecified psychiatric illness was brought to the emergency psychiatric area for evaluation of agitation and psychosis. The patient became violent and was restrained by several security guards. He was placed **prone** with his arms behind him. During the restraint process he became apneic and pulseless."

"During the [prone] restraint process" is not an indication of someone dying while "simply" in a prone position. It indicates that forceful-prone-restraint was actively being applied at the time the victim "became apneic and pulseless."

No help for Chan's statement here.

Hick/Smith/Lynch Case #3:

"30-year-old man ... after a long foot chase was apprehended by two witnesses **who sat on the patient to restrain him**. He lost consciousness, and when the paramedics arrived, he was in cardiac arrest with an idioventricular rhythm. ... pH 6.8 ..."

This discussion fails to identify whether the two witnesses "sat on the patient" while he was prone or supine. However, it is entirely unlikely that even "lay" individuals (those without a "medical" education) would sit on top of someone's chest or belly while they were SUPINE. Thus, it is far more likely that the victim was PRONE while being sat upon.

Please also note the extremely acidotic pH and the initial pulseless idioventricular rhythm documented. Those things do NOT accompany "acute" or "common" cardiopulmonary arrest. But, they DO accompany deaths caused by forceful-prone-restraint asphyxia.

So, NO HELP for Chan's statement here.

Hick/Smith/Lynch Case #4:

"After firing a gun in an apartment, a 39-year-old-man was apprehended and restrained by several police officers. He continued to struggle during transport in a **prone position with his hands cuffed behind his back**. Upon entering the ED, he violently kicked a door, and then had a sudden cardiopulmonary arrest. ... presenting rhythm was idioventricular ... died."

Since "He continued to struggle during transport in a **prone position**" he had to have been FORCEFULLY – by manual and/or mechanical means – maintained in the prone position while on the wheeled stretcher. That is not an indication of someone "simply" being in a prone position.

Furthermore, how-in-hell could someone who was prone-restrained to an ambulance wheeled stretcher "violently [kick] a door" while "entering the ED"? That's physically impossible. Consequently, there CLEARLY (even to non-medically-educated individuals) are several activity descriptions MISSING from this case report!

How did he get loose enough to violently kick a door? What manner of restraint was used to "subdue" him after he violently kicked the door? What manner of restraint was being employed at the moment he suffered a so-called "sudden cardiopulmonary arrest"?

Oh, surprize! After arriving in an exam/treatment room of this ED, his "presenting rhythm was idioventricular" ... and his pH was "less than 6.8." Obviously, both his "presenting rhythm" and his ABG blood draw were obtained after the missing bits of information that RESULTED in his "sudden cardiopulmonary arrest" occurred.

This was not a case of someone dying while "simply" restrained in a prone position.

STILL no help for Chan's statement here.

Hick/Smith/Lynch Case #5:

"A 38-year-old-man ... The patient was wrestled to the ground, maced, and then carried to the median and **placed on his side. He continued to struggle, then had a sudden cardiopulmonary arrest.**" ... he was chemically "resuscitated" in the emergency department ... admitted to ICU ... "Refractory hypotension and disseminated intravascular coagulation led to an eventual bradysystolic arrest ten hours after admission."

Oh, so CLOSE, Ted!

But, there is absolutely nothing in this Hick/Smith/Lynch case scenario suggesting that the patient REMAINED "on his side" after the time that he "continued to struggle."

In fact, after being dumped on the median "on his side," when the subject resumed his "struggle" it is entirely probable that the **police officers resumed their manual restraint application.** To do so,

it is far more likely that the **police officers resumed their manual restraint application by first returning him to a PRONE position**. It is entirely unlikely that police officers kept him on his SIDE when resuming their manual restraint of this individual.

After all, if this individual had remained on his side while simply struggling against his restraint on the median, or had he remained on his side during the police officers' resumption of manual restraint, there would be no reason for him to have died.

So, again, NO HELP HERE, TED.

SUMMARY of EXAMINATION of the TWO REFERENCES OFFERED BY CHAN to SUPPORT his argument that "similar sudden deaths have been reported in patients who were not restraint[sic] in the PMRP, but simply in the prone, supine, lateral side, and even sitting positions."

- Some may argue that Chan **accidentally** misinterpreted Case #1 of the two Park/Korn/Henderson case studies (Chan's **2004 Reference "13"**) as being a "simple" supine restraint asphyxia case. After all, Chan doesn't work the streets and may not have known about the physical impact of ambulance wheeled stretcher safety belt systems when tightened enough to be used for "restraint" of violent individuals. (Something such systems were NOT designed for!)
Hey! I'm happy to be generous when I can be. So, I'll agree to give Ted the benefit of a doubt here.
- As to the five case studies offered by Hick/Smith/Lynch (Chan's **2004 Reference "14"**), ONE of them (Case #5) **sounds** like death occurred simply during "**lateral side**" restraint application. Again, since he doesn't actually work the streets, Ted may be unaware that police officers do NOT commonly "continue struggle" with someone while the subject remains on his SIDE. So, I'll give Ted the benefit of a doubt here, as well.
However, this is the last benefit of a doubt that I can offer Chan, no matter how generous my mood.
- NONE of the case studies presented in these two collections provide any manner of support for Chan's statement about asphyxial deaths occurring while someone was "simply in the prone" position. In fact, I cannot imagine anything other than "wishful thinking" causing Chan to consider ANY of them as evidencing such a thing. "Wishful thinking" is NOT something that legitimate researchers are supposed to base conclusions on. "Wishful thinking" is NOT something that Chan can be given the "benefit of a doubt" for.
- Park/Korn/Henderson's Case #2 (the only case in these two collections involving a "seated" position) was identified by the case study authors as NOT being a "simple" seated position. Thus, it is inconceivable that Chan could have accidentally misunderstood the circumstances of this case study. Consequently, when he included "and even sitting positions" in the statement he used references number 13 and 14 to support, **Chan LIED**.

SUMMARY of the ABOVE TWO POINTS:

- Theodore Chan LIED ABOUT and/or MISREPRESENTED significantly important study subject inclusion/exclusion information in his published "version" of it's report.
Each of his "co-authors" allowed him to do so.

2. Theodore Chan LIED ABOUT and/or MISREPRESENTED the references he offered to support at least one dramatically important statement that he indicated as being a "fact" (by virtue of the references he cited in its support) within his published discussion of the study's findings and its subject.

Each of his "co-authors" allowed him to do so.

3. I here have PUBLICLY posted my accusations that

Theodore Chan (et al) lied and/or misrepresented information that he (they) caused to be published in a professional medical journal.

Were he able to prove me WRONG, Ted likely would launch a civil suit against me for "libel" or "defamation of character" or the like.

I have absolutely NO fear of such a suit being successfully brought against me. Because, whereas I can show ample evidence demonstrating that my accusations are true, Chan cannot show ANY evidence supporting a claim that my accusations are false.

The OTHER "PROBLEMS" with Chan's 2004 "weight force during prone restraint and respiratory function" report of this 1999 study's results remain exactly the same as when its findings were presented by Eisele in 2000.

But, now that Chan has finally managed to get HIS VERSION of the study's findings **published** (more than 4 years after the study was performed) – and now that I have at least Chan's version of its **full-report**, I have identified SEVERAL MORE erroneous misrepresentations promoted by Chan et al.

Unfortunately, I don't have the time to identify, discuss, and post them EVERY SINGLE ONE OF THEM **at this writing**. [One of these days, I'll get to it! But, don't hold your breath!]

However, I'm happy to take the time to again identify the most important 4-year-old PROBLEMS with this study – and, I'm happy to discuss at least **one additional**, terrifically SIGNIFICANT, problem related to this study's methods that only occurred to me after reading Chan's 2004 representation of it.

(1) All of the 1999 "weight force during prone restraint and respiratory function" study subjects were entirely HEALTHY individuals of "average" weight range.

As previously discussed in Part One of this Comprehensive Review (my review of Chan et al's November, 1997, "Restraint Position and Positional Asphyxia" article), multiple experts agree that information derived from such a clinically-controlled restraint study of healthy individuals' responses has absolutely no realistic relationship to the effects that a victim of ANY of the multiple causes of altered level of consciousness (an "unhealthy" individual) might suffer during forceful-prone-restraint application.

HEALTHY individuals resisting arrest do not generate enough strength of combativeness to prompt application of forceful-prone-restraint in the manner (or for the length of time) that has repeatedly resulted in death of individuals who were suffering from an altered level of consciousness productive of prolonged violent and agitated behavior ("excited delirium" victims).

Yet, even when a study of prone-restrained HEALTHY individuals demonstrates "that weight placed on the back during maximal restraint does cause a decrease in spirometry parameters," Chan et al persist in pooh-poohing this kind of decrement (and others like it).

(2) Chan et al were forced (in order to pass a "peer-review" of their article, and finally get it published)

to ADMIT IN PRINT that:

"Our study has limitations. First, as this was a laboratory physiology study, we could not reproduce all conditions encountered in the field setting with such cases. In particular, we did not simulate trauma, struggle, drug intoxication, and other physiologic and psychologic stresses that commonly occur with individuals who are being restrained in the field setting."

AND

"Second, the amount of weights selected for this study may not reproduce the actual amount of weight force used on individuals during the restraint process. It is possible that heavier amounts of weights would have impacted respiratory function to a greater degree. Similar to traumatic or mechanical asphyxia cases, extreme amounts of weights could have resulted in significant chest wall trauma and marked elevations in intrathoracic pressure that could have impacted cardiovascular function."

Yet, Chan et al elected to publish a "conclusion" that ENTIRELY NEGLECTED to mention the NEGATIVE findings determined by their study. In fact, Chan et al purposefully published a "conclusion" that semantically down-played the importance of the "negative" information derived from their (albeit inadequately-constructed) study.:

"We conducted a study on the impact of weight force placed on the back of individuals in the PMRP on pulmonary and respiratory function. We found that weight force of 25 and 50 lbs did not result in evidence of hypoxia or hypoventilatory respiratory compromise in our study subjects."

Basically, as they did in the two previous articles I've comprehensively reviewed, Chan et al elected to present study information CONCLUSIONS in a manner that **suggests and infers** their study having proven that real-life application of forceful-prone-restraint with "weight force placed on the back" DOESN'T HARM ANYONE!

Given my knowledge of Chan et al's motivation for BIAS regarding the subject manner of restraint asphyxia, their wording of the "conclusion" they manufactured based upon their inadequately-constructed 1999 "weight force" study comes as NO surprize to me. It simply continues to make me angry. And, continues to cost me a ton of time having to explain why seemingly-"professional" medical personnel would so grossly and negligently misrepresent information derived from their studies.

[Again, see Part One of this Comprehensive Review (my review of Chan et al's November, 1997, "Restraint Position and Positional Asphyxia" article).]

(3) The amount of WEIGHT employed for this "Weight Force During Prone Restraint and Respiratory Function" study was ludicrously LIGHT, and it was applied to the study subjects in a LOCATION that was GUARANTEED to effect the LEAST amount of interference in respiratory function!

Way back in 2002, when I first was made aware of Dr. Eisele's 2000 *American Academy of Forensic Science* conference paper presentation regarding "Comparison of Respiratory Function in the Prone Maximal Restraint Position With and Without Additional Weight Force on the Back," I was actually EXCITED to learn of the study, and even HOPEFUL that it would provide important (relevant) information regarding the causes of restraint asphyxia deaths.

However, upon reading Eisele's paper, I was immediately suspicious of the amount of weight employed for the study. So, the first thing I did after reading it, was to accomplish a **personal experience** of the study's methods of weight application while in the PMRP, based upon the descriptions supplied by the study's authors!

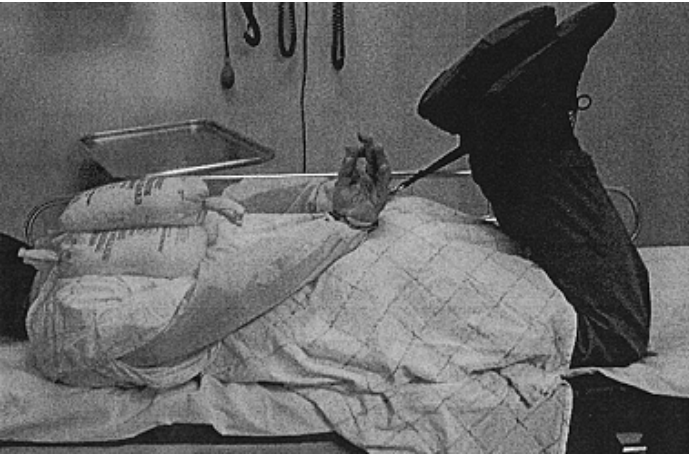


FIGURE 1. "Subject placed in PMRP with weight force on back."

The above study position photography was published by Chan in his 2004-version of the study's report.

I scurried around my home looking for common household items that weighed 25 pounds and/or 50 pounds. I couldn't find a 25-pound household item. So, I decided that I'd rather save time by experiencing the "maximum" weight-force employed for the study, anyway: that of 50 pounds.

DARN! I couldn't find a 50-pound household item, either. The best I could come up with was a 40-pound, rectangular plastic tub of cat litter (CLEAN cat litter, thank you very much!), and two 5-pound packages of granulated sugar.

After gathering these items, I summoned my mother to assist me. I laid down prone on the cement floor of my basement, reached behind my back, and grabbed my own ankles. Then, I directed my mother to place the 40-pound plastic tub of cat litter on the back of my ribcage, subsequently directing her to place the two 5-pound packages of granulated sugar on top of the litter tub.

This 50 pounds of weight was not at all "comfortable" to support. (The hard edges of the plastic litter tub somewhat painfully dug into the flesh of my back.) But, my BREATHING was not AT ALL perceivably "bothered" by this **50 pounds of weight** having been placed on my back, atop my ribcage.

THAT was when I realized that the LOCATION of the WEIGHT-PLACEMENT for this study was ENTIRELY STUPID!

The RIBCAGE is specifically designed to PREVENT compression of the vital organs dwelling within it: the lungs, the heart, and the great blood vessels. Thus, when considering the weight-placement location that Eisele (Chan et al) employed for this study, Eisele (Chan et al)'s study finding "that weight placed on the back during maximal restraint does cause a decrease in spirometry parameters," **was even MORE significant than I had previously recognized.**

How much MORE of a "decrease in spirometry parameters" – or **OTHER DIMINISHED FUNCTION** findings – would have been measured had Eisele (Chan et al) placed even this ludicrously-light amount of weight atop the study subjects' **LOWER BACK**; an area NOT supported and protected by a strong bony structure such as the ribcage?!

(4) NEXT; I considered the REAL LIFE "field" situations consistently associated with restraint asphyxia deaths, in relationship to the WEIGHT AMOUNT and WEIGHT-PLACEMENT-LOCATION employed by Eisele (Chan et al) for this study:

In REAL LIFE, if someone is subjected to manually-applied forceful-prone-restraint by one or more adult individuals, at least HALF of one restraining adult individual's weight is applied to the restrained individual's back. (And, that's entirely ignoring the additional "weight" of the "physically-exerted" FORCE applied by the restrainer.)

SO! For "50 pounds" to be considered a "realistic" amount of weight-force application in relationship to FIELD employment of weight to the BACK of a forcefully-prone-restrained person, the restraining individual would have to weigh **NO MORE THAN 100 pounds!!!**

I don't think I've EVER, in the 15+ years I worked the streets, seen an emergency responder who weighed only 100 pounds. And, certainly, should such a light-weight emergency responder exist somewhere on this planet, she/he would NOT endeavor to restrain an incredibly agitated and violent excited delirium victim ALL BY HER/HIMSELF! Thus, MORE than half the weight of a single 100-pound provider would be applied to the back of any forcefully-prone-restrained individual, because – in a REAL-LIFE forceful-prone-restraint situation – 1 or 2 or 3 or more OTHER (heavier) individuals would be ASSISTING the 100-pounder to restrain the excited delirium victim.

ADDITIONALLY, in REAL LIFE restraint asphyxia cases, there usually is at least ONE person ALSO placing weight on top of the subject's "**lower**" back. The lower back is not protected by a rigid, strong, bony structure, such as the ribcage. And, opposite to the lower back is the ABDOMEN.

This means that, weight applied to the area of an individual's lower back – where there is NO structural protection of a something like a ribcage – immediately causes COMPRESSION of the ABDOMEN ... immediately interferes with the DIAPHRAGM's ability to function ... and, thus it IMMEDIATELY INTERFERES with the victim's ability to BREATHE.

All of the UPCOMING photos are "posed" pictures (staged for commercial or educational purposes), EXCEPT for the lower left photo of a school teacher forceful-prone-restraining a child. That one is "REAL!"

Furthermore: Were any of the ADULTS being restrained in the posed photos truly a victim of violently-exertive excited delirium, IN REAL LIFE it would take so many restrainers to restrain them, that you wouldn't be able to SEE the person being restrained!

Consequently, there are at least one or two "invisible" restraint participants represented in all but one of these photos.



It would be a whole helluvalot more HELPFUL if Chan et al would perform a prone restraint "weight study" with their silly little bit of weight (50 pounds) placed on the study subjects' LOWER BACK.

Unfortunately, I don't believe that Chan et al have ever been honestly motivated to perform a REALISTIC study related to the subject of forceful-prone-restraint and restraint asphyxia. Consequently, I don't believe that Chan et al would ever be interested in repeating their ridiculous weight study utilizing more realistic study parameters.

(5) LASTLY: NONE of the "weight force during prone restraint and respiratory function" study subjects were EVER required to perform ANY amount of EXERCISE prior to being placed in ANY of the study's positions, with or without EITHER of the weights applied to their posterior ribcage!

"Each subject was placed into 4 different positions: sitting, PMRP with no weight force, PMRP with 25 lbs of weight force on the back (PMRP_25), and PMRP with 50 lbs of weight force on the back (PMRP_50). Subjects were placed in these positions in random order.

... Subjects remained in each position for 5 minutes. After each 5-minute period, the subject rested in the sitting position for 10 minutes before starting the next trial."

Thus, not only were the study subjects entirely HEALTHY and average-weight individuals – not only were they NOT subjected to SOME form of EXERCISE (some form of even MINOR exertion-simulation) prior to assuming a study position – each of them were purposefully given the opportunity to become **entirely RESTED** prior to being subjected to Chan et al's 1999 "PMRP" with ridiculously light weights applied to the posterior of their very strong and supportive ribcages.

It is my opinion that, the failure of this study's designers to at least subject these individuals to SOME form of EXERCISE (some form of even MINOR exertion-simulation) prior to measuring several of their pulmonary functions while placed in a so-called "Prone Maximal Restraint Position" – with or without silly amounts of weight placed over their RIBCAGE – **CLEARLY DEMONSTRATES** the fact that these allegedly-professional medical researchers had **absolutely NO legitimate interest in performing a study that could yield anything even remotely-resembling MEANINGFUL information.**

FINAL SUMMARY of this COMPREHENSIVE REVIEW of:

Chan TC; Neuman T; Clausen J; Eisele J; Vilke GM.

Weight force during prone restraint and respiratory function.

Am J Forensic Med Pathol, September 2004;25(3):185-189:

- Theodore Chan LIED ABOUT and/or MISREPRESENTED significantly important study subject inclusion/exclusion information in his published "version" of it's report.
Each of his "co-authors" allowed him to do so.
- Theodore Chan LIED ABOUT and/or MISREPRESENTED the references he offered to support at least one dramatically important statement that he indicated as being a "fact" (by virtue of the references he cited in its support) within his published discussion of the study's findings and its subject.
Each of his "co-authors" allowed him to do so.

- Chan et al may or may not have LIED ABOUT and/or MISREPRESENTED other information reported in the 2004-published "Weight force during prone restraint and respiratory function" article.

I have not taken the time to determine how many other of Chan's apparent statements of "fact" made within his spurious discussion of the study findings (or the subject's research history) are accurately based upon information derived from the references he cites for their support. However, given his track record, it would not at all surprize me if there were several other instances of Chan lying about or misrepresenting research articles cited to provide apparent support for his statements – particularly when his statements infer that restraint asphyxia doesn't happen.

- All of the 2004-published "Weight force during prone restraint and respiratory function" article study subjects were entirely HEALTHY individuals of "average" weight range; and multiple experts agree that information derived from such a clinically-controlled restraint study of healthy individuals' responses has absolutely no realistic relationship to the effects that a victim of ANY of the multiple causes of altered level of consciousness (an "unhealthy" individual) might suffer during forceful-prone-restraint application.
- Chan et al elected to publish a "conclusion" that ENTIRELY NEGLECTED to mention the NEGATIVE findings determined by their study. In fact, Chan et al purposefully published a "conclusion" that semantically down-played the importance of the "negative" information derived from their (albeit inadequately-constructed) study.
- The amount of WEIGHT employed for this "Weight Force During Prone Restraint and Respiratory Function" study was LUDICROUSLY LIGHT, and it was applied to the study subjects in a LOCATION that was **GUARANTEED** to effect the **LEAST amount** of interference in respiratory function!
- NONE of the 1999 "**weight force during prone restraint and respiratory function**" study subjects were EVER required to perform **ANY amount of EXERCISE** prior to being placed in ANY of the study's positions, with or without EITHER of the ludicrously light weights being applied to their posterior ribcage while in any form of prone position.
- Not only were the 1999 study subjects entirely HEALTHY and average-weight individuals – not only were they NOT subjected to SOME form of EXERCISE (some form of even MINOR exertion-simulation) prior to assuming a study position – each of them were purposefully given the opportunity to become **entirely RESTED** prior to being subjected to Chan et al's 1999 "PMRP" with ridiculously light weights applied to the posterior of their very strong and supportive ribcages.
- **Consequently, NO PART** of this study's design ever came even CLOSE to representing anything remotely-resembling a realistic investigation of the effects that "**weight force during prone restraint**" application might have upon the "**respiratory function**" of an altered level of consciousness or excited delirium victim.

EACH of the study's authors KNEW THIS when they designed, performed, and subsequently "reported" the 1999 study's findings.

Yet, Theodore Chan was especially motivated to do whatever it took to get this report published.

And, each of his "co-authors" allowed him to do so.

If ANYONE can provide evidence that ANY of my reveiw comments or conclusions are in any way WRONG or ERRONEOUS, I implore them to SHARE such evidence with me! Unlike Theodore Chan and his ilk, I welcome criticism, and I am happy to amend my conclusions / opinions when provided with "better" information.

Sincerely Yours,

Ms. Charly D. Miller

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