

## Lethal Injection: Painful Fact or Junk Science?



In the last two issues of Straight Tox we discussed "pitfalls in forensic toxicology" and "pharmacokinetics". In this issue we will look at an example of the distortions that can result from failing to heed or properly understand these issues.

I know that it is typically bad form in writing to begin an article with a disclaimer, but in this case I must. Lethal injection is such a highly polarizing subject, and I expect especially so among the intended audience of this brochure, that I must first state the following: **The author is in no way taking a political, ethical or moral side in the debate over lethal injection. It is simply my intent to point out the fallacies adherent to improperly using science to attempt to further a political agenda.**

In the April 16<sup>th</sup>, 2005 issue of The Lancet, Leonidas Koniaris, Teresa Zimmers, David Lubarsky, and Jonathan Sheldon co-authored a Research Letter entitled, **Inadequate anaesthesia in lethal injection for execution.** In this letter the authors' challenged whether lethal injection meets the requirements of the eighth amendment prohibiting "cruel and unusual" punishment. The conclusions of this letter were quickly picked up by the media and to date have influenced judicial decisions in several states.

While it is unlikely that any of the readers herein are unfamiliar with the process of lethal injection, I will review. Typically an IV line is first established. When the order is given, the condemned is administered sodium thiopental to induce a deep sleep. Doses as high as 5 grams, 50 times what is often used in surgical anesthesia, are not uncommon. After the condemned is unconscious, a saline solution is used to flush the IV line before the administration of pancuronium bromide or another paralyzing agent. The paralyzing agent acts upon the diaphragm to stop the breathing. The IV line is again flushed with saline. Finally, in most states, potassium chloride is administered to stop the heart.

The foundation of the authors' argument is that based on postmortem blood concentrations of thiopental, obtained from executions in Arizona, Georgia, North Carolina, and South Carolina, "most of the

executed inmates had concentrations (of thiopental) that would not be expected to produce a surgical plane of anaesthesia, and 21 (43%) had concentrations consistent with consciousness". The authors go on to argue "that it is possible that some of these inmates were fully aware during their executions".

There are several disturbing aspects to these assertions. First, the article was published as a "Research Letter" rather than a peer-reviewed article. Secondly, one of the authors, Jonathan Sheldon, is an attorney who represents inmates sentenced to death. And third, the fact that The Lancet published a decidedly anti-capital punishment editorial in the same issue as the Research Letter, citing the conclusions as if they were settled fact. Probably the most troubling aspect of this whole episode, however, is the cavalier, and likely flawed, application of post-mortem drug concentrations to antemortem clinical interpretations without regard for factors such as post-mortem redistribution.

*In the words of Alexander Pope: "A little learning is a dangerous thing..."*

At one point the authors mention the possibility of postmortem redistribution: "Extrapolation of antemortem depth of anaesthesia from post-mortem blood thiopental concentrations is admittedly problematic. To estimate concentrations of thiopental in the brain from concentrations in the blood in life, details of the rate and duration of drug administration are needed. Unfortunately, such details are usually not specified in lethal injection protocols. Furthermore, no data about post-mortem distribution of thiopental are available." Then the authors, inexplicably, cite the wide distribution of thiopental concentrations found in the specimens, resulting from "nearly identical (execution) protocols", as simply being due to the lack of expertise of the "relatively unskilled executioners" rather than being increasing evidence of potential post-mortem redistribution. By the very next paragraph, the authors seem to have dismissed the possibility of post-mortem redistribution altogether and proceed with their argument. That paragraph begins with the words, "If post-mortem thiopental concentrations are taken as a surrogate marker of concentrations in the blood during life..."

In September 2005, most likely in response to criticism of the methods and conclusions of Koniaris, et al, The Lancet published three letters with the comments of seven authors. The authors, Jonathan Groner, Mark Heath, Donald Stanski, Derrick Pounder, Robyn Weisman, Jeffrey Bernstein, and Richard Weisman, were all uniformly critical of the work. Space considerations prohibit a full discussion of the criticism herein, but I will detail a few of the observations.

One author pointed out that the specimens were not obtained immediately after death, but that most specimens were obtained 12 or more hours after death. The author also challenged Koniaris, et al's erroneous implication that

the fact that thiopental is reported to be stable in stored human plasma somehow negated any argument of post-mortem redistribution. Several of the reviewers argued that the high lipid solubility of thiopental as well as the lack of equilibrium could provide a mechanism for rapid post-mortem redistribution of thiopental from the blood to the surrounding tissues.

In one of the criticisms the authors argue that "the distribution of thiopental in a dying prisoner is likely to be very different from its distribution in a ventilated and oxygenated patient" due to cellular hypoxia and metabolic acidosis. As evidence, the authors cite an interesting study in a dog model, where due to the inhalation of carbon dioxide, the arterial pH decreased from a mean value of 7.35 to a mean of 6.90, resulting in an average 40% decrease in the plasma thiopental concentration.<sup>1</sup>

As a personal observation, Disposition of Toxic Drugs and Chemicals in Man references four thiopental anesthetic deaths where inadequate ventilation was maintained.<sup>2</sup> By the criteria of Koniaris, et al, none of these thiopental concentrations would be considered to rise to a "surgical plane of anaesthesia" and one individual would have been "fully aware" during her death, a rather ironic conclusion.

Clearly, more research is needed to address the concerns posed by this Research Letter. However, while everyone is free to form their own political opinion about capital punishment. We who hold ourselves out as experts must remember that our scientific opinions must withstand the scrutiny of our science.

1. Brodie BB, Mark LC, Papper EM, et al. The fate of thiopental in man and a method for its estimation in biological material. J Pharmacol Exp Therap 1950; 98: 85-96
2. Campbell JE. Deaths associated with anesthesia. J For Sci; 5:501-549, 1960

*This article aptly demonstrates what can happen when an "expert" is either uninformed or ignores established principles of forensic toxicology in favor of a preconceived bias. I believe that objectivity and candor are the cornerstone of being an effective expert or consultant. If I can help you evaluate your case, please give me a call or drop me an Email.*



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