

mine users have been associated with motor slowing (15), and DAT deficits and motor slowing are seen in Parkinsonism.

Finally, Mithoefer and colleagues contend that clinical MDMA use has taken place without evidence of toxicity and cite three references to support this view. The first is a conference presentation and, to our knowledge, has not yet been published. The second reference refers to an exchange of letters that was primarily focused on the merits and drawbacks of interspecies dose scaling for the estimation of neurotoxic dosages of MDMA in humans. The third citation refers to a Phase I study in which previous MDMA users were administered two different dosages of MDMA (and placebo). No measures of neurotoxicity were obtained during that study. Although we understand that Mithoefer and colleagues feel strongly about the potential therapeutic effects of MDMA, we remain of the opinion that there are not sufficient data to conclude that clinical MDMA research can be conducted without running the risk of monoaminergic brain neural injury.

GEORGE A. RICAURTE,<sup>1\*</sup> JIE YUAN,<sup>1</sup> GEORGE HATZIDIMITRIOU,<sup>1</sup> BRANDEN J. CORD,<sup>2</sup> UNA D. MCCANN<sup>3</sup>

<sup>1</sup>Department of Neurology, <sup>2</sup>Department of Neurosciences, <sup>3</sup>Department of Psychiatry, Johns Hopkins Bayview Medical Center, Johns Hopkins University School of Medicine, Baltimore, MD 21224, USA.

\*To whom correspondence should be addressed. E-mail: Ricaurte@jhmi.edu

#### References

1. J. Mordenti, W. Chappell, in *Toxicokinetics in New Drug Development*, A. Yacobi, J. Kelly, V. Batra, Eds. (Pergamon, New York, 1989), pp. 42–96.
2. J. A. Henry, K. J. Jeffreys, S. Dawling, *Lancet* **15**, 340, 384 (1992).
3. G. R. Srean et al., *Lancet* **339**, 677 (1992).
4. I. S. Chadwick et al., *J. R. Soc. Med.* **84**, 371 (1991).
5. P. D. Mueller, W. S. Korey, *Ann. Emerg. Med.* **32**, 377 (1998).
6. K. T. Finnegan et al., *Brain Res.* **447**, 141 (1988).
7. M. S. Kleven, W. L. Woolverton, L. S. Seiden, *Brain Res.* **488**, 121 (1989).
8. W. Slikker Jr. et al., *Toxicol. Appl. Pharmacol.* **94**, 448 (1988).
9. G. A. Ricaurte, L. E. Delaney, I. Irwin, J. W. Langston,

*Brain Res.* **446**, 165 (1988).

10. E. O'Shea, B. Esteban, J. Camarero, A. R. Green, M. I. Colado, *Neuropharmacology* **40**, 65 (2001).
11. D. A. Loeffler et al., *J. Neural Transm. Park. Dis. Dement. Sect.* **9**, 45 (1995).
12. Parkinson Study Group, *Arch. Neurol.* **52**, 237 (1995).
13. U. D. McCann et al., *J. Neurosci.* **18**, 8417 (1998).
14. N. D. Volkow et al., *Am. J. Psychiatry* **158**, 377 (2001).
15. Y. Sekine et al., *Am. J. Psychiatry* **158**, 1206 (2001).

## Research Fraud, Public Policy, and Gun Control

DONALD KENNEDY'S EDITORIAL "RESEARCH fraud and public policy" (18 April, p. 393) alleges that I made up a computer hard disk crash when challenged about the loss of data on a 1997 survey. Unfortunately, *Science* did not contact me about these allegations. I have provided editors with statements from nine different academics, verifying the hard disk crash. Four of them were coauthors who also lost data with me.

When the disk crashed on 3 July 1997, I lost all my data for virtually all the research projects that I had conducted up to that point in time, including the text and data files for my book *More Guns, Less Crime*. With the help of other academics, primarily David Mustard (University of Georgia), I replaced all the massive crime data sets so that academics at dozens of universities could replicate and reexamine every single regression reported in my book. All the additional data have also been supplied for the book's second edition. The survey data Kennedy mentions involve merely one number in one sentence in my book, and he fails to note that I later redid the survey on a smaller scale and obtained similar results. Those data have also been released ([www.johnlott.org](http://www.johnlott.org)).

Kennedy discusses criticisms that I made of Ian Ayres and John Donohue's work (only Donohue is mentioned in the Editorial), but fails to note that I have provided them with my different city, county, and state level crime data sets both before and after they

refused to provide me with data for their own work. I feel that the comments that I posted about their paper were entirely accurate.

I used a pseudonym in Internet chat rooms because earlier postings under my own name elicited threatening and obnoxious telephone calls.

JOHN R. LOTT JR.

American Enterprise Institute, 1150 17th Street, NW, Washington, DC 20036, USA.

IT IS VERY DISAPPOINTING TO SEE THE FOCUS of the Editorials in *Science* shifting from science to politics and gun control. Although Donald Kennedy assails the work of John Lott ("Research fraud and public policy," 18 April, p. 393), he fails to mention the publications of Gary Kleck (Florida State University) on the same issue (1). By his own declaration, Kleck is a card-carrying member of the American Civil Liberties Union (ACLU) and a registered Democrat who does not own a gun. I am confident that he expected, when he undertook his investigations, to reach the same conclusions as the gun-banning advocates. To the contrary, he came to the same conclusions as Lott, and his work preceded Lott's. An incontrovertible fact is that violent crime in general as well as gun-related shootings have decreased substantially in all states that have liberalized their gun-permit laws (2). It is unfortunate that Kennedy did not consider this information worth including in his Editorial. With more than 20,000 gun laws on the books, enacting more will have the same effect on the gun-violence problem as enacting one more drug law will have on our illegal drug problem.

CHARLES G. SMITH

Post Office Box 9814, Rancho Santa Fe, CA 92067-4814, USA.

#### References

1. J. R. Lott Jr., "Gun control misfires in Europe," *Wall Street Journal*, 30 April 2002.
2. G. Kleck, *Point Blank* (De Gruyter, New York, 1991).

## Response

LOTT'S EXPLANATION OF THE LOSS OF HIS DATA should certainly be accepted, although, of course, it does not restore life to the data—which, far from being "one number in one sentence," were at the center of the controversy between Lott and his critics. And Lott cannot dismiss his use of a fictitious ally as a "pseudonym." What he did was to construct a false identity for a scholar, whom he then deployed in repeated support of his positions and in repeated attacks on his opponents. In most circles, this goes down as fraud.

Smith takes me to task for ignoring Kleck's work. My Editorial focused on two cases of questionable research conduct; it wasn't a review. Interested readers may assess whether Kleck's current position supports Lott's; I believe it does not.

DONALD KENNEDY

### TECHNICAL COMMENT ABSTRACTS

#### COMMENT ON "Climate and Management Contributions to Recent Trends in U.S. Agricultural Yields"

Lianhong Gu

According to Lobell and Asner (Brevia, 14 February 2003, p. 1032), the atypical summer cooling trend from 1982 to 1998 increased U.S. yields of corn and soybeans during the same period. However, three potential problems with their analysis bring their results and conclusion into question.

Full text at [www.sciencemag.org/cgi/content/full/300/5625/1505b](http://www.sciencemag.org/cgi/content/full/300/5625/1505b)

#### RESPONSE TO COMMENT ON "Climate and Management Contributions to Recent Trends in U.S. Agricultural Yields"

David Lobell and Gregory Asner

The comment by Gu reflects a misunderstanding of our study and does not invalidate our conclusions. Instead, it highlights the importance of quantification in assessing climate impacts on yield trends and the need to better understand the relative roles of causal mechanisms.

Full text at [www.sciencemag.org/cgi/content/full/300/5625/1505c](http://www.sciencemag.org/cgi/content/full/300/5625/1505c)