Connecting Casinos and Crime:
More Corrections of Walker

EARL L. GRINOLS[1] AND DAVID B. MUSTARD[2]

A REPLY TO: DOUGLAS M. WALKER, "THE DILUTED ECONOMICS OF CASINOS AND CRIME: A REJOINDER TO GRINOLS AND MUSTARD," *ECON JOURNAL WATCH* 5(2), MAY 2008: 148-155. LINK.

ABSTRACT

LESS THAN ONE MONTH FOLLOWING THE PUBLICATION OF OUR DETAILED response to Professor Walker's first commentary on our peer-reviewed work (Grinols and Mustard 2006), we were presented with a second manuscript by Mr. Walker (Walker 2008b). In his present commentary, Professor Walker again provides no new data or research, articulates comments that are already resolved through a careful reading of Grinols and Mustard (2006), and declines to respond to the failings that we raised about his earlier critique. In his second endeavor he also introduces new issues that were not in his first commentary note (Walker 2008a), expands his attention to the work of other authors, and makes factually incorrect statements about their work. Because Mr. Walker's complaints stem from errors of fact, correcting some of them might benefit future readers. We limit ourselves to four.

1. Mr. Walker says that our paper gives a "flawed example." Mr. Walker is incorrect. The model presented in the original paper is a system of three equations.

$$c = s_1 + s_2 + (\sigma_1 + \sigma_2)V/P$$

---

1 Department of Economics, School of Business, Baylor University. Waco, Texas 76798.
2 Department of Economics, Terry College of Business, University of Georgia. Athens, GA 30602.

$$D = (s_1 + s_2)\,\frac{P}{P + V} + (\sigma_1 + \sigma_2)\,\frac{V}{P + V}$$

$$\pi = s_1 + s_2$$

where c is the crime rate, D defines the diluted crime rate, P is the resident population, V is the visitor population, s_1 and σ_1 are the shares of the resident and visitor population, respectively, victimized by residents, and s_2 and σ_2 are the shares of the resident and visitor population, respectively, victimized by visitors. π is the probability that a resident will be victimized. The purpose of the model is to show that the diluted crime rate (number of crimes divided by local population + visitors) can fall while the probability of a resident being victimized rises (i.e. π and D can move in opposite directions.) For example, assume as in the paper that $\sigma_1 = 0$. Let $s_1 = 0.10$. In the no visitor situation $V = s_2 = \sigma_2 = 0$ and $\pi = D = 0.10$. Now let visitors be present, $V = P = 1000$, where $s_2 = \sigma_2 = 0.04$. As we originally showed, then π rises (from 0.10 to 0.14) while D falls (from 0.10 to 0.04). There is an infinite number of other ways the same result may occur.

2. Mr. Walker also expands his criticisms beyond Grinols and Mustard (2006) to papers written by other authors (Thompson, Gazel, Rickman 1996b; Thompson and Quinn 1999; and Schwer, Thompson, and Nakamuro, 2003). This is consistent with his pattern of writing "rebuttals" in which he provides no original research or no new empirical work.[3] In his present commentary, Mr. Walker also misinterprets or misunderstands the work of other researchers. He writes:

> These are only three examples, but they are sufficient to show just how arbitrary such cost estimates are, both in methodological and empirical terms. Indeed, such studies have long been criticized for their poor quality, as discussed by the National Research Council

---

3 In 1998 he produced a rebuttal of Gross (1998) that provided no original research or empirical work. A few years later he wrote a rebuttal of Kindt (2001), which criticized the gambling industry and originally appeared as part of a symposium of 10 papers dealing with gambling in America. Walker was not a participant in the original symposium and none of the papers in the symposium cited any papers by Mr. Walker. His paper contained no new results or additional empirical work of his own. The same year the Las Vegas Sun reported, "Earlier this month, the Nevada Resort Association—the chief lobbying group for Nevada casinos—commissioned a rebuttal report by Georgia College & State University Assistant Professor of Economics Douglas Walker, who said the results of Thompson's study were 'unreliable because their analysis is seriously flawed' " (Benston 2003). The referenced study is Schwer, Thompson, and Nakamuro, 2003 (later Thompson and Schwer 2003). In 2005 the Casino Association of Indiana hired Mr. Walker to write a rebuttal of the study by Policy Analytics 2006. Again, no original research or empirical work was conducted. In 2006 Mr. Walker was hired by the Taiwan Amazing Technology Co. Ltd, a manufacturer of gambling machines. A paper could not be found on the web to know if it contains original research or is promotional in nature. The following year in 2007 the American Gaming Association commissioned Mr. Walker to write a rebuttal of casino cost-benefit studies. No original empirical research was involved. Two months later Mr. Walker wrote his commentary on Grinols and Mustard (2006).

(1999). (Walker 2008b, 153)

Two of the three studies Mr. Walker cites are, in fact, not cited by the National Research Council (NRC) report. One of these papers could not possibly have been cited in NRC (1999) because it was written four years after the report was published. Furthermore, Thompson, Gazel, and Rickman (1996b) was cited *favorably*, not critically, by the NRC.[4] The NRC refers to the study as "an excellent example" and a study that "makes a significant contribution to the literature on the economic impacts of gambling."

3. In the same section, Walker cites Grinols (2004) in his footnote 14, inserted after the sentences "Most of these studies arrive at their estimations in ways highly arbitrary. Here are a few examples." As shown above, the basis for much of Mr. Walker's perspective is misreading of the original papers. Walker (2007), written for the American Gaming Association, the chief lobbying body of the gambling industry, discusses the need for gambling research to adjust for co-morbidity (multi-causality). He writes that "a mechanism is needed to allocate the harm among coexisting disorders, yet most authors make no such attempt." He states: "Most social cost researchers (e.g., Grinols 2004, Grinols and Mustard 2001, and Thompson et al. 1997) simply attribute all of the costs to gambling." This statement, too, is false. Page 173 of Grinols 2004 states that the reported cost figures were adjusted by the author to correct for multi-causality, as well as for a second issue, sample selection bias (representativeness of sample). Both are explained in detail on pp. 170-71.

4. Mr. Walker devotes nearly four manuscript pages to his view of how

---

4 Thompson, Gazel, Rickman 1996b is cited four times by NRC. They are singled out positively on page 173:

> An excellent example of this type of analysis is a study that looked at the economic effects that casinos have had in Illinois and Wisconsin (Thompson et al., 1996b)….The result was a set of estimates of the positive and negative monetary effects of casino gambling in both Illinois and Wisconsin. This, in turn, provided a good estimate of the positive effects of casinos in the two states….

The three other citations are simple references to earlier results as on page 28:

> Studies primarily of gamblers seeking help suggest that as many as 20 percent will attempt suicide (Moran, 1969; Livingston, 1974; Custer and Custer, 1978; McCormick et al., 1984; Lesieur and Blume, 1991; Thompson et al., 1996), and two out of three help seekers have turned to criminal activities to support their gambling (Lesieur et al., 1986; Brown, 1987; Lesieur, 1989).

NRC also reports favorably on Thompson, Gazel, Rickman 1996a on page 181:

> A second study that makes a significant contribution to the literature on the economic impacts of gambling is one that identifies and quantifies the social costs of gambling in the state of Wisconsin (Thompson et al., 1996a).

NRC recognizes that the research they cite can be improved—for example Thompson, Gazel, Rickman do not consider the full range of social costs—but the sentence that points this out is positive, reading, "The most sophisticated gross impact studies painstakingly attempt to measure the net positive economic effects of casino gambling without considering the full range of costs." Two sentences later appears the "excellent example" statement quoted above.

crime statistics should be measured. As stated in our previous response, we addressed this issue extensively in the original paper and encourage readers to return to our original paper. In Mr. Walker's initial commentary, he objected to our using conventional, federally-reported crime rates in our study. His position has now evolved. In his earlier commentary he said,

> the apparent objective of the Grinols and Mustard paper is to analyze the risk of casino county residents falling victim to crime. (Walker 2008a, 7)

and

> In this case, clearly the 'diluted' crime rate is the appropriate one to use if we are trying to measure the risk to residents and/or visitors of being victimized. (Walker 2008a, 10)

His previous concern was based on an incorrect reading of the paper. Our paper states, "We are therefore interested in the *total effect of casinos on crime*" (Grinols and Mustard, 30, emphasis added) as "Correctly Critiquing Casino-Crime Causality" further confirmed.

His original comment resolved, Mr. Walker now shifts his focus to crime costs. In his new comment he says, "they might elude the crime rate criticism, *but the basis for that criticism simply re-emerges in terms of cost burden per resident*." He says,

> Grinols and Mustard have implicitly attributed the entire cost burden to residents—even if they are not victims of crimes! (Walker 2008b, 150)

This also is false. We have not "implicitly attributed the entire cost burden to residents" as Mr. Walker erroneously claims. In fact we explicitly said the opposite. In the section titled "Visitor Criminality," we said that crime could rise "because casinos attract visitors who are more prone *to commit and be victims of crime*" (Grinols and Mustard 2006, 32, emphasis added). Earlier in the paper we provided theoretical explanations of how casinos might affect crime. We said, "These factors are not mutually exclusive, and our empirical results estimate *the total effect*" (emphasis added). That crime might increase in casino counties because visitors were crime victims was repeated again on page 40 in our section labeled "Evaluation" where we wrote, "The regressions in table 4, of course, cannot decompose the net number of offenses to assign them to each alternative explanation."

While Mr. Walker appears to want to ignore crimes committed against visitors, we believe that crimes committed against visitors are part of total costs. We explicitly stated in the original paper and reiterated in our first response, that in

calculating the estimated costs we deliberately chose results that provided smaller estimates of the casino effects than did some of our other specifications.

Concerning the denominator, the social costs associated with increased crime involves the change in absolute number of crime incidents. The divisor used in the initial reporting of crime (whether population or population + visitors) disappears before the final step, hence is irrelevant. In the portion of the paper detailing costs, the paper reports:

> In 1996 the total costs for the 178 casino counties exceeded $1.24 billion per year. (Grinols and Mustard 2006, 41)

What would the costs be if they were extrapolated to the U.S.? The next sentence reads:

> If the estimated coefficients from table 4 are applied to a representative county of 100,000 population, 71.3% of which are adults (as is representative of the United States as a whole), then the social costs per adult are $75 in 2003 dollars.

The answer is $75 per adult, as reported.

In conclusion, many papers, Grinols and Mustard 2006 included, describe areas where further research could improve on earlier contributions. Indeed, this is the theme of much, if not most, academic research in the social sciences. When we saw the need for better research on the statistical link between casinos and crime we researched, wrote, and published Grinols and Mustard (2006). Unless better research comes along to confirm or deny our peer-reviewed results, we stand by our conclusions.

REFERENCES

Benston, Liz. 2003. Expert: Problem Gambling Study Flawed. *Las Vegas Sun*. 31 March 2003.

Grinols, Earl L. and David B. Mustard. 2006. Casinos, Crime, and Community Costs. *The Review of Economics and Statistics* 88(1): 28-45.

Grinols, E.L., and D.B. Mustard. 2008. Correctly critiquing casino-crime causality. *Econ Journal Watch* 5(1): 21-31. Link.

Grinols, Earl L. 2004. *Gambling in America: Costs and Benefits*. New York: Cambridge University Press.

Gross, Meir. 1998. Legal Gambling as a Strategy of Economic Development.

Economic Development Quarterly 12(3): 203-213.

Kindt, John. 2001. The Costs of Addicted Gamblers: Should the States Initiate Mega-Lawsuits Similar to the Tobacco Case? *Managerial and Decision Economics* 25(4): 197-200.

National Research Council. 1999. *Pathological Gambling: A Critical Review*. Washington, DC: National Academy Press.

Policy Analytics. 2006. A Benefit-Cost Analysis of Indiana's Riverboat Casinos for FY 2005, 17 January. Unpublished.

Schwer, R. Keith, William N. Thompson, Daryl Nakamuro. 2003. Beyond the Limits of Recreations: Social Costs of Gambling in Southern Nevada. 2003 Annual Meeting of the Far West and American Popular Culture Association, Las Vegas, NV, February 1, 2003. Revised: February 18, 2003. Revised May 28, 2003 as Thompson, William N. and R. Keith Schwer. Beyond the Limits of Recreation: Social Costs of Gambling in Southern Nevada. 14th International Conference of Gambling and Risk Taking, Fairmont Waterfront Hotel, Vancouver, British Columbia, May 28, 2003.

Thompson, W.N., R. Gazel, and D. Rickman. 1996a. *Casinos and Crime in Wisconsin: What's the Connection?* Thiensville: Wisconsin Policy Research Institute Inc.

Thompson, W.N., R. Gazel, and D. Rickman. 1996b. *The Social Costs of Gambling in Wisconsin*. Thiensville: Wisconsin Policy Research Institute Report, Vol. 9, No. 6.

Walker, Douglas M. 2007. Challenges that Confront Researchers on Estimating the Social Costs of Gambling. Prepared for the American Gaming Association.

Walker, D.M. 2008a. Do Casinos Really Cause Crime? *Econ Journal Watch* 5(1): 4-20. Link

Walker, D.M. 2008b. The Diluted Economics of Casinos and Crime: Rejoinder to Grinols and Mustard. *Econ Journal Watch* 5(2): 148-155. Link

ABOUT THE AUTHORS
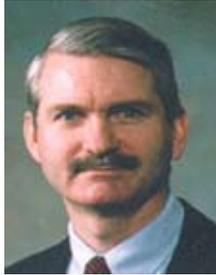
Earl L. Grinols is Distinguished Professor of Economics at the Hankamer School of Business at Baylor University and former Senior Economist for the Council of Economic Advisers. A University of Michigan Angell Scholar and mathematics *summa cum laude* graduate of the University of Minnesota, he earned his PhD at MIT. In addition to Baylor, he has taught at MIT, Cornell University, the University of Chicago, and the University of Illinois. His current research focus, health care, is treated in his book *Health Care for Us All: Getting More for Our Investment (Cambridge University Press,* forthcoming) co-authored with colleague James W. Henderson.

David B. Mustard is an Associate Professor of Economics in the Terry College of Business at the University of Georgia and a research fellow at the Institute for the Study of Labor in Bonn, Germany. Mustard earned a Ph.D. in Economics from the University of Chicago. His research focuses on microeconomic policy-related questions, especially law and economics, crime, casino gambling, lotteries, gun control, sentencing, labor economics, education and merit-based aid. Mustard has won many university- and college-wide teaching awards and regularly teaches in UGA's honors program.

Go to Walker's Original Article

Go to Grinols and Mustard's Reply

Go to Walker's Rejoinder

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Comments** Section

Discuss this article at Jt: http://journaltalk.net/articles/5566