



*Econ Journal Watch,
Volume 5, Number 1,
January 2008, pp 4-20.*

Do Casinos Really Cause Crime?

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A COMMENT ON: EARL L. GRINOLS AND DAVID B. MUSTARD, “CASINOS, CRIME, AND COMMUNITY COSTS,” *THE REVIEW OF ECONOMICS AND STATISTICS* 88(1), FEBRUARY 2006: 28-45.

ABSTRACT

The Review of Economics and Statistics published “Casinos, Crime, and Community Costs” by Earl Grinols and David Mustard in February 2006. The authors claim that their analysis of casinos and crime is “the most exhaustive ever undertaken in terms of the number of regions examined, the years covered, and the control variables used” (43-44). The paper is a noteworthy contribution to the gambling literature. The scope of their analysis is impressive.

Since its publication the Grinols and Mustard paper has generated much discussion in the press, activist websites, policymaking discourse, and the gambling literature.² Because the Grinols and Mustard paper is published in a refereed journal with high academic prestige, it is likely to be influential in subsequent research and political discussions of the casino-crime relationship.

The Grinols and Mustard analysis utilizes county level data on FBI Index I

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I would like to thank—without implication—several people who made helpful comments and suggestions that improved this paper: Jay Albanese, Bill Eadington, David Forrest, Mark Nichols, Don Ross, Richard Thalheimer, and especially John Jackson and Ben Scafidi. Several referees provided important comments and editorial suggestions.

² For example, several newspaper reports have highlighted the Grinols and Mustard study (Morin 2006, Vitagliano 2006, Yarbrough 2006). In recent months the study was discussed in articles in *Parade Magazine* (Flynn 2007) and *The Wall Street Journal* (Whitehouse 2007). Policy reports have utilized the study (Policy Analytics 2006), and recent research has reported the Grinols and Mustard findings (Morse and Goss 2007, 79-82). The paper (or an earlier version, Grinols and Mustard 2001a) has also been posted on activist websites such as the National Coalition Against Legalized Gambling ([link](#)) and CasinoFreePA ([link](#)).

offenses³ for all U.S. counties from 1977 through 1996. Using a series of dummy variables to account for the existence of casino gambling in counties, as well as a number of control variables, the authors model crime rates and find that they have fallen in both casino and non-casino counties during the sample period. However, Grinols and Mustard report the crime rate dropped by 12 more percentage points in non-casino counties than in casino counties (Grinols and Mustard 2006, 30). Their analysis leads them to conclude that the higher crime rates in casino counties are caused by the existence of casinos. Grinols and Mustard find that for the first two or three years following casino openings there is little or no effect of casinos on crime. However, during the fourth and fifth years after casino openings, most forms of crime begin to escalate in the casino counties. The estimated crime effects are used in conjunction with cost of crime estimates to arrive at the estimated cost of crime caused by casinos of \$75 per adult in U.S. casino-hosting counties (28, 41).

Grinols and Mustard provide a detailed discussion of the theoretical connection between casinos and crime (31-32). They discuss two potential factors through which casinos may reduce crime. First, if casinos present better job opportunities for low-skilled workers, crime may fall. Second, there may be economic development effects attributable to casino gambling that could reduce crime.

On the other hand, Grinols and Mustard discuss five ways in which casinos may lead to an increase in crime. First, casinos may harm economic development by draining the local economy of resources. Second, casinos may lead to an increased crime payoff, resulting in more crime. Third, pathological gambling may increase with the spread of casinos, and this can lead to more crime. Fourth, casinos may also attract criminals to a region, leading to more crime. Finally, Grinols and Mustard explain that casinos may induce a change in the local population, toward one more apt to commit crimes. The Grinols and Mustard mechanisms between casinos and crime seem reasonable and largely uncontroversial.

Unfortunately, the Grinols and Mustard empirical analysis has problems, including: (1) a lack of needed data and its effect on measuring the crime rate, (2) potential problems with their crime data, (3) a possible sample self-selection bias, (4) a poor measure of casino gambling activity, and (5) skewed interpretations of the empirical results. Since the Grinols and Mustard paper has been so influential, its shortcomings need to be thoroughly explored.

Gambling is a controversial issue. It may be one of those issues where most conventional sources of support are disinclined to support research that might come to politically incorrect conclusions. Such a situation gives rise to the hazard that politically-incorrect research and interested industry groups tend to make connections, and research with any connection to such groups is then discounted, regardless of its scholarly merits and arguments. In the Appendix to this paper I make disclosures and discuss the general problem of researcher motivations and commitments.

³ These offenses include aggravated assault, rape, robbery, murder, larceny, burglary, and auto theft.

CALCULATING THE CRIME RATE

The crime rate is typically measured as the number of crimes committed divided by the population. This is usually multiplied by 100,000:

$$\text{crime rate} = \frac{\# \text{ of crimes committed} \times 100,000}{\text{population}} = \text{crimes per 100,000 people} \quad (1)$$

If we let C be the number of crime incidents and P be the population, then the crime rate in (1) can be expressed as $C/P \times 100,000$. This rate gives a fair indication of the risk of being victimized by crime.

Relative to the U.S. population, the number of tourists is small. So an adjustment for visitors and the crimes they commit is not likely to affect significantly the U.S. crime rate or the residents' risk of being victimized by crime. However, if one is considering a very small area, such as a county that has a large tourist attraction, then for the crime rate to represent accurately the risk of being victimized, it must be adjusted to account for the crimes committed by visitors *and* for the increase in the population at risk of being victimized by crime.

Several authors have discussed how tourism should be considered when analyzing the crime rate. Nettler (1984, 48) explains, "to increase the accuracy of forecasts, a rate should be 'refined' so that it includes in its denominator *all those persons and only those persons who are at risk* of whatever kind of event is being tallied in the numerator." Nettler describes rates that do not correctly represent the population at risk as "crude" (48). Boggs (1965) considers central business districts, which attract large numbers of visitors. She explains that ignoring the visitors produces a spuriously high crime rate (900). Curran and Scarpitti (1991, 438) explain that the FBI, the source of the Grinols and Mustard crime data, warns against "comparing statistical data...solely on the basis of their population."

To illustrate the effect of visitors (tourists) on the crime rate, let C_R be the crimes committed by residents and C_V be crime committed by visitors. Also let P_R be the resident population and P_V be the population who are visiting. Then the total number of crimes committed will be $C_R + C_V$, and the population at risk is $P_R + P_V$. We can rewrite the crime rate from equation (1) as⁴

$$\text{Crime rate} = \frac{C_R + C_V}{P_R + P_V} \quad (2)$$

Clearly, if we are interested in the crime rate for a single county that is attracting relatively many visitors then it is critical to account for visitors in both the

⁴ For simplicity we hereafter ignore the standard practice of multiplying the rate by 100,000.

numerator (C_v) and the denominator (P_v).

Grinols and Mustard use as the crime rate $C_R + C_v/P_R$, which is greater than $C_R + C_v/P_R + P_v$. Obviously, the difference between the two measures is greater the more tourists there are. Grinols and Mustard explain that county level visitor data are not available (34). As a result, they have no option but to exclude P_v from the denominator of the crime rate. But they do include C_v in the numerator. The result is that Grinols and Mustard overstate the crime rate in casino counties and therefore, overstate the risk to casino county residents of being victimized by crime. This latter observation is particularly important, since the apparent objective of the Grinols and Mustard paper is to analyze the risk of casino county residents falling victim to crime (34, 35). If these risks are overstated then so will be the estimated costs of crime due to casinos.

Grinols and Mustard attempt to justify their crime rate measure by first creating names for two types of crime rate: “undiluted” and “diluted” (34). The “undiluted” or “traditional” rate used in their analysis is what Nettler (1984) refers to as a “crude” rate. It is shown using our notation from above:

$$\text{“undiluted” crime rate} = \text{crude crime rate} = \frac{C_R + C_v}{P_R} \quad (3)$$

When the number of visitors (P_v) is added to the population at risk measure, Grinols and Mustard call the result the “diluted” crime rate. This is what Nettler (1984) refers to as a “refined” rate, and it is the original crime rate from equation (2). The terminology “diluted” and “undiluted” appears to be original with Grinols and Mustard. They explain their “decision” to use the “undiluted” crime rate:

Some have argued for one [rate]...or the other without realizing that the choice is not methodological, but depends on what questions the researcher wants to answer. A common but invalid claim is that the diluted crime rate should be used to determine the change in probability that a resident would be the victim of a crime. However, knowing what happens to the diluted crime rate does not give the needed information and could even move the answer in the wrong direction.⁵ (34)

Grinols and Mustard provide an example to show why the “diluted” crime rate may not provide “the needed information”—and that as a result, P_v should be excluded from the crime rate calculation:

⁵ Note that Grinols and Mustard do not explain *why* the claim that “the diluted rate should be used” is invalid. Nor do they cite work where the claim is invalidly made.

...let s_1 be the share of resident population P victimized by residents, and let s_2 be the share of the resident population victimized by V visitors. Similarly, let σ_1 be the share of visitors victimized by residents and σ_2 the share of visitors victimized by visitors. Then the [undiluted] crime rate is $s_1 + s_2 + (\sigma_1 + \sigma_2)V/P$; the diluted crime rate is $(s_1 + s_2)w_P + (\sigma_1 + \sigma_2)w_V$ where w_P and w_V are the shares of visitors plus residents made up by residents and visitors, respectively; and the probability of a resident's being a crime victim is $s_1 + s_2$. If residents do not victimize visitors ($\sigma_1=0$), then $P=V$, and $s_2 + \sigma_2$ is smaller than s_1 . The probability of a resident being victimized is s_1 without visitors, and it rises to $s_1 + s_2$ with visitors. The diluted crime rate is s_1 without visitors and falls to $(s_1 + s_2 + \sigma_2)/2$ with visitors. Thus in this case the diluted crime rate falls while the probability of a resident being victimized rises. (34-35)

They explain that their interest is in “the costs to the host county associated with a change in crime from whatever source. We are therefore interested in the total effect of casinos on crime, and thus use the undiluted crime rate...” (35).⁶

It appears that their conclusion—the risk to residents rises even though the “diluted” rate falls—occurs only because of their assumptions: “If residents do not victimize visitors ($\sigma_1=0$), then $P=V$, and $s_2 + \sigma_2$ is smaller than s_1 ” (34).⁷ One *can imagine* a situation which provides the conclusion that the risk to residents rises even though the “diluted” crime rate falls. But this is by no means the only possible outcome.

To illustrate, consider Albanese's (1985, 41) simple numerical example:

A city with a population of 100 citizens might experience 10 reported Index crimes in a year. Therefore, the probability that any one citizen will be the victim of one of these crimes is 1 in 10. If the population of this city suddenly doubles [after a casino opens]

6 Presumably, Grinols and Mustard are interested in the costs to the host county because these jurisdictions may be responsible for bearing the costs associated with any casino-related crime. In addition, some residents will be the victims of visiting criminals. Since the decision to adopt casinos is made locally, one could argue that a focus on the local, county-level effects is warranted. On the other hand, one could argue that the casino legalization question begins with the state, so state-level effects are more important to the politicians responsible for the initial legalization. In addition, casinos pay hefty fees and significant taxes that may partially offset any locally-incurred costs of casinos. Even if one agrees with Grinols and Mustard that the local effects are of primary concern, it does not necessarily imply the “undiluted” crime rate is the appropriate one.

7 As the sentence reads, it does not make sense. First, $P=V$ does not follow from the assumption that $\sigma_1=0$; nor does “ $(s_2 + \sigma_2)$ is smaller than s_1 ” follow. Perhaps Grinols and Mustard transposed “then” and “and”. In an earlier version of the Grinols and Mustard paper (2001a, 14), this sentence is worded differently: “For example, assume that residents do not victimize visitors ($\sigma_1=0$), $P=V$, and $(s_2 + \sigma_2)$ is smaller than s_1 .” This wording clearly indicates that all three conditions are assumptions.

to, say, 200 citizens, it is likely that the number of crimes that occur there will also rise—simply because there are more people to be offenders and victims. If the number of crimes also doubled to 20, it would appear as if crime increased 100%. However, this is not the case. If 200 people are now at risk and 20 crimes are committed, the probability of being a victim is *still* 1 in 10 (i.e., 20 in 200). Therefore, the risk of being victimized by crime can remain the same when *both* the population and crime increase together.

One can fabricate an example in which Grinols and Mustard's conclusion obtains, beginning with 100 residents and 10 crimes and the Grinols and Mustard assumption that residents do not victimize visitors. Suppose that now 100 visitors come and commit 8 crimes. Then the "diluted" crime rate will fall to 18 in 200 (9 in 100). If *only one* of the new crimes is committed against a resident, then the risk to residents rises to 11 in 100. It is unlikely that visitors will only victimize visitors, so the Grinols and Mustard assumption that residents do not victimize visitors virtually ensures that the risk to residents will increase, whether the "diluted" rate rises or falls. But the necessary assumptions to ensure that Grinols and Mustard's conclusions obtain are very contrived, so the justification for excluding visitors from the population at risk and using the "undiluted" rate is very weak.

Recall that the crime rate is typically used to measure the likelihood of being victimized by crime for the population at risk. If we exclude visitors from the population at risk, then we are implicitly assuming that only residents are at risk of being victimized. When Grinols and Mustard choose the "undiluted" crime rate, $C_R + C_V/P_R$, they are implicitly forcing the assumption that *all crime is committed against residents*—since visitors are excluded from the denominator. This certainly overstates the crime rates in tourist counties and will overstate the true risk of those counties' residents being victimized.⁸

Clearly there are a number of possibilities for how the "diluted" crime rate will move relative to the residents' risk of being victimized; Grinols and Mustard highlight one scenario. Now let's consider others. Again start with 10 crimes and 100 residents, and the Grinols and Mustard assumption that residents only victimize residents. If 100 visitors come and commit an additional 10 crimes, here are a few of the possibilities: (i) if visitors commit 5 crimes against residents and 5 crimes against visitors, then the risk to residents rises to 15 in 100, while the "diluted" crime rate remains constant (it changes from 10 in 100 to 20 in 200); (ii) if visitors commit all 10 crimes against other visitors, then the risk to residents and the "diluted" crime rate are unchanged; (iii) if visitors commit 5 crimes against visitors and 5 against residents, and the resident criminals also attack residents and visitors equally, then the risk to residents remains constant, and the

⁸ The more tourism in a county, the larger the overstatement of the crime rate and the risk to residents.

“diluted” rate is unchanged; (iv) if all criminals attack only visitors, then the risk to residents falls to zero, while the “diluted” rate is unchanged. Obviously there are other possible scenarios.

The important point is that the relationship between risk to residents and the “diluted” and “undiluted” crime rates depends critically on who the criminals are and who the victims are.⁹ Unfortunately, Grinols and Mustard do not have these data. But a variety of research, as well as common sense and common experience, suggests that tourists are popular targets for criminals (Chesney-Lind and Lind 1986, Harper 2001, Miller and Schwartz 1998, and Fujii and Mak 1980).

What are the odds that all resident and visiting criminals ignore tourists and attack only residents, as Grinols and Mustard implicitly assume? Without evidence to the contrary, it seems more likely that a resident and a visitor are roughly equally likely to be victimized. 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. The Grinols and Mustard “undiluted” crime rate will overstate the crime rate in tourist (casino) counties. This is perhaps the most significant problem in the Grinols and Mustard paper.

ANOMALIES IN THE CRIME DATA

There are two potential problems with the Grinols and Mustard crime data, the *Uniform Crime Reports* (UCR). The UCR data at the county level are based on voluntary crime reporting by a number of agencies within each county. The crimes reported by the various agencies are aggregated to arrive at the county-level UCR data. The problem arises from the fact that *unreported crime data are imputed*. For the 1977-93 data, the UCR explains that the reason for the imputation was to “ensure cross-sectional data comparability and quality.” But it warns, “if there were major changes in the [agencies] reporting in a county across years, artifactual changes in the longitudinal data for a county could be introduced because of potential variation in the type of [agency] used to compute imputed county totals and rates each year” (ii). In order to make the data more useful for longitudinal analyses, for 1994 and later, the UCR changed its method of imputing missing data (i).

There are two problems with the UCR data as they relate to the Grinols and Mustard study. The first is that the imputation for crime by non-reporting agencies may introduce anomalies into the Grinols and Mustard crime data.¹⁰ Maltz

⁹ Knowing where the crimes occur (on casino premises or off) would also provide some insight into the relative probabilities of being victimized. See Curran and Scarpitti (1991).

¹⁰ Grinols and Mustard do note that some of their observations (about 5,300) had missing data and were not included in the model (p. 35). However, they do not explain what the missing data are. Even if this refers to imputed UCR data, the absence of those data could still potentially affect their results. Grinols and Mustard do indicate that they used regressions weighted by county population (35). This could mitigate some of the data problems, to the extent that less populated counties are less likely to

(1999, 26) explains, “Most observers believe that the effect on the estimate of the *overall* crime rate in the United States would be minimal, but that it could be quite problematic when investigating the crime rate for a smaller unit such as a State or county, or when looking at rural crime rates.”¹¹ Maltz and Targonski (2002) believe the problems are so serious that, “until improved methods of imputing county-level crime data are developed, tested, and implemented, they should not be used, especially in policy studies” (297).

The second problem is that, although the Grinols and Mustard sample period is 1977-96, the authors’ model apparently does not account for the 1994 change in UCR data reporting. The UCR data codebook includes a section titled, “Break in Series,” in which it warns, “*data from earlier year files should not be compared to data from 1994 and subsequent years* because changes in procedures...may be expected to have an impact on aggregates for counties in which some [agencies] have not reported for all 12 months” (p. i; emphasis added).

It difficult to speculate on how exactly these data issues might affect the Grinols and Mustard analysis, but the effect could be serious. Much of the U.S. casino expansion occurred in 1991-93.¹² As discussed below, Grinols and Mustard find crime in casino counties starts to rise four or five years after casinos are introduced. For counties that adopted casinos in the early 1990s, this increase in crime rate corresponds to 1994 or later—after the UCR imputation change. It is possible that Grinols and Mustard’s finding of a crime effect results from the UCR data imputation, the 1994 change, or both.

SAMPLE SELF-SELECTION

Grinols and Mustard use a dummy variable to track the first opening of a casino into the county. Variables are also used to account for time relative to the first casino opening in a county, from two lead years to five lag years. The empirical results show no significant changes in casino county crime rates until four or five years after the introduction of casinos. Grinols and Mustard claim that “[by] conducting the most exhaustive investigation and utilizing a comprehensive county-level data set that includes every U.S. county, we eliminate sample selection concerns” (33). The authors do not choose a biased sample, but there is a potential sample self-selection bias in their model.

report crime. This issue is discussed in the debate between Maltz and Targonski (2002, 2003) and Lott and Whitley (2003).

11 It is surprising that Grinols and Mustard used the UCR data at all. In the context of the “right-to-carry” gun law debate, Lott and Whitley (2003) mention that Lott and Mustard were well aware of problems with the UCR data, and that they “had compiled an eight page single-spaced list of problems” (186, note 6). Grinols and Mustard should have at least acknowledged that there are potential problems with the data, even if they are the best data available.

12 Only Nevada, New Jersey, and South Dakota had commercial casinos prior to 1991.

Grinols and Mustard do not account for the fact that counties self-select into the “casino county” category by the decision to permit casinos.¹³ Since casino gambling has often been sold as a potential growth or tax revenue strategy (Walker 2007a), there is good reason to believe that counties with relatively poorly performing economies might be more likely to introduce casinos and to do so more quickly than counties that are better off economically. Indeed, Grinols and Mustard mention the common belief that casinos are more likely to be placed in high-crime areas (36), and that the number of casinos began increasing rapidly in 1991 (38). The time was toward the end of a recession, and corresponds to the 1996 Lag 5 crime estimates, which are the only basis for some of the Grinols and Mustard cost of crime estimates (41). Some states and counties may have legalized casinos in part because of economic hardships caused by the recession of 1990-91, representing factors that may be driving Grinols and Mustard’s results. The importance of state self-selection is shown by Fink, Marco, and Rork (2004) in the case of lottery adoption and the lotteries’ impact on state budgets. A similar consideration should have been incorporated into the Grinols and Mustard analysis.

Grinols and Mustard argue that because they include control variables in the model and find no significant differences between casino and non-casino lead period crime rates, “casinos were not more likely to be placed in areas that had systematically different crime environments than other regions” (40; also see 36). But the lead period crime rates are mostly positive (though statistically insignificant) in casino counties. Perhaps there are observed or unobserved factors that explain casino adoption. Grinols and Mustard do not account for the possibility of sample self-selection bias in their model.¹⁴

CASINO DUMMY VARIABLES

Aside from the potential self-selection problems for casino counties, the variables Grinols and Mustard use to measure casino activity have other problems. They note that the ideal measure of casino activity would be revenues or profits (29), but that such data are not available for Indian casinos.¹⁵ Grinols and Mustard instead use a dummy variable indicating the year in which a casino first opened in the county (35) and lead and lag dummies to account for the existence of casinos for various lengths of time.

The Grinols and Mustard casino dummy may show how sensitive crime rates are to the opening of a casino, but if there is a relationship between casino

¹³ This obviously occurs only after the state has legalized casinos.

¹⁴ A standard procedure for dealing with sample self-selection bias is the Heckman (1979) two-step method. See Fink et al. (2004) for an application of this procedure to lotteries, or Walker and Jackson (2008a) for an application to an analysis of the relationships among gambling industries.

¹⁵ There are available measures of casino volume. For example, Walker and Jackson (2008a) use Indian casino square footage as a proxy for gambling volume.

gambling and crime, one would expect that relationship to be dependent on the volume or size of the casino, the number of casinos, and perhaps even on the types of games offered. But the Grinols and Mustard first-year dummy cannot pick up any such variations in the casino industry in the counties. It essentially treats all the Las Vegas mega-casinos as having the same impact on crime in the county as, say, a single small casino in a Colorado county.

Furthermore, the dummy variable technique used by Grinols and Mustard to denote casino counties will pick up *any* differences in the crime rates between casino and non-casino counties, not just those differences that are due to the presence of casinos. In general, anything that distinguishes the casino counties from national norms will be picked up by the dummy. Even the effects of the included demographic and other normalizing variables, to the extent that their impact on the crime rate differs between casino and non-casino counties, will be picked-up by the dummy. Thus, inferring that a positive and significant dummy coefficient for casino counties implies a higher crime rate in those counties *because of the presence of casinos* is conjectural.¹⁶

For example, it is possible that the crime effect found by Grinols and Mustard in casino counties is due to *tourism in general* rather than to *casino-specific tourism*.¹⁷ If a county had decided to build new attractions along an urban strip and was deciding to authorize *either* a casino or an adventure water park that would attract teens and young adults, it might be misled if it interpreted Grinols and Mustard's results as speaking of casino-specific tourism. Had they compared casino counties with similar non-casino tourism counties,¹⁸ their results would have been more likely to show any existing crime effect attributable to casino-specific tourism.

LAG 5 CRIME RATES

Grinols and Mustard's conclusion that "roughly 8% of crime in casino counties in 1996 was attributable to casinos, costing the average adult in casino

¹⁶ This problem is related to the previous issue, self-selection bias. The Grinols and Mustard dummy variables may be indicative of those variables that would help explain the casino adoption decisions by counties.

¹⁷ Grinols and Mustard anticipate this argument and use available visitor data from Las Vegas and the three largest tourist attractions in the U.S. (Mall of America, Disney World, and Branson, MO) along with National Parks (32, 34; also see Grinols and Mustard note 13). They show that, adjusted for the numbers of tourists, the crime rate in Las Vegas is significantly higher than at the other venues. The implication is that casino tourists are more likely than other tourists to commit crimes. While this may be true, the Grinols and Mustard comparisons do not show it. First, most Las Vegas tourists are adults, while many tourists to the comparison destinations are children. Second, Mall of America and Disney World are destinations principally enclosed in an encompassing private area, quite unlike "the strip" and environs in Las Vegas. Third, National Parks are usually located far outside of urban settings.

¹⁸ Stitt, Nichols, and Giacomassi (2003) perform an analysis of casinos and crime using control communities.

counties \$75 per year” (28; also see 41) is based on a series of questionable assumptions and interpretations, most of which have the effect of increasing the apparent casino effect on crime.

At least some of the of the Grinols and Mustard results and conclusions are based on only the Lag 5 casino crime rate estimates,¹⁹ a technique that calls for two objections. First, the Lag 5 crime rate estimates are the highest of any in the model (37, Table 4).²⁰ Second, the Lag 5 estimates are based on only 49 of the 178 casino counties (or about 28% of them; p. 35).²¹ The truncation raises questions about whether these early adopting casino counties with the highest estimated crime rates are representative of all casino counties. After all, the early-adopting counties represented by Lag 5 crime rates likely attracted more tourism than those counties represented in more recent lag periods, when casinos had become more widespread. This would suggest that the Lag 5 casino county crime rates are probably the most overstated of any period’s, because the “undiluted” crime rate used by Grinols and Mustard excludes visitors from the population at risk.

Finally, one may question whether the Grinols and Mustard results accurately portray the marginal effect of casinos on crime. Their Lag 5 crime rates, for example, show how high the mean crime rates in casino counties (which have had casinos for 5 years) are relative to the mean crime rates of non-casino counties. But this does not take into consideration the fact that the crime rate coefficients in casino counties were often positive (albeit mostly insignificant) relative to non-casino counties prior to the introduction of casinos. As Grinols and Mustard indicate (36), there is a common belief that casinos are more likely to be placed in high-crime areas.

Rather than focusing on Lag 5 casino crime rates relative to non-casino county crime rates, one could argue that a more accurate picture of the effect of casinos on crime could be drawn from, for example, subtracting the average

19 Grinols and Mustard use the fifth year crime rate alone in estimating the number of crimes that would be committed by problem and pathological gamblers if that was the one source of additional crime in casino counties (40-41). They also use only the fifth year period to calculate the average property loss for four of the criminal offenses they study (41). However, when calculating their “implied cost of additional crime” due to casinos (\$75 per adult in casino counties; p. 41), Grinols and Mustard are not clear about how the calculation is made. They write, “Summing the estimated number of crimes attributable to casinos for each county, taking into account how many years the casino was in operation, and dividing by the casino counties’ total population measures the contribution of casinos to observed crime” (41). A reasonable reader could infer from the surrounding discussion that the authors based their results on only the Lag 5 crime rate estimates because they explicitly state that these were the crime rates used in the other calculations, described above. For such a critical issue, one would expect the authors to provide a clear, detailed explanation.

20 Recall that the Lag 5 estimates correspond closely to counties that adopted casinos toward the end of a recession.

21 Each lag period crime coefficient is based on a partially changing sample of casino counties. For example, the Lag 4 sample includes all the Lag 5 counties plus counties that introduced casinos four years ago. Lag 3 includes the counties from Lags 4 and 5, plus counties that adopted casinos three years ago.

lead-period crime rates in casino counties—which are mostly positive—from the average lag period crime rates. This calculation takes into account crime rates both before and after casinos are introduced, and it better accounts for all casino counties. The Grinols and Mustard Lag 5 crime rates are between 1.5 and 5.5 times higher than the average change in crime rates from before to after the introduction of casinos.²² This suggests that Grinols and Mustard may be seriously overstating the true average effects of casinos on crime.

CONCLUSION

Other studies examine crime rates while accounting for visitors in particular casino markets. They find mixed results.²³ It is reasonable to believe that tourist areas might act as “hot spots” for crime, and attract criminals. Casino patrons often carry lots of cash, and many casinos serve free alcohol, so patrons may be less alert than usual. On the other hand, casinos are famous for their security measures. Stitt et al. (2003, 281) conclude that casinos built with the approval of the surrounding community probably do not act as “hot spots.”

Grinols and Mustard confidently present their study as being the “most exhaustive ever undertaken” (43) and their results as being “lower bounds on the true effect [of casinos on crime]” (44). But in this comment I have identified several serious problems with their data, model, analysis, and interpretation of results. Most of the problems identified here will have the effect of overstating the estimated effect of casinos on crime.

My point is not to suggest that casinos do not cause crime. They might.²⁴ Many economists will concede that there are problems in any empirical study. However, the errors in the Grinols and Mustard study deserve attention because of the influence their study seems to be having among researchers, policymakers, the media, and voters.

APPENDIX: COMMITMENTS AND MOTIVATIONS

Gambling research is still fairly young, developing mostly since the spread of casino gambling across the U.S. in the 1990s. Casino gambling is a controversial

22 For each type of crime I took the average lead crime rates and subtracted them from the average lag crime rates. The resulting marginal impacts of casinos on crime were, for the most part, lower than the average lag crime rates, and were much lower than the Grinols and Mustard Lag 5 crime rate estimates used in some of their cost calculations. The only exception is for murder; Grinols and Mustard found a slightly negative coefficient for murder in Lag 5. The difference in means is slightly positive.

23 See Albanese (1985), Curran and Scarpitti (1991), Stitt et al. (2003), and Stokowski (1996).

24 It would be ideal to replicate the Grinols and Mustard analysis using appropriate data and analysis. Unfortunately, the required data (county visitor count) simply do not exist. In addition, county-level crime data are potentially unreliable. Still, it would be interesting to see if the Grinols and Mustard results hold using more recent data, say through 2006.

policy issue, and the controversy has stimulated debate, both public and academic, especially over how to identify and measure the costs and benefits. Readers may wonder what motivated the present comment on the Grinols and Mustard paper. I explain that, as well as some background on gambling research.

My own contributions to this literature and debate have dealt with empirical issues such as the state-level economic growth and tax effects of casino gambling in the U.S., as well as the relationships among gambling industries; and methodological issues surrounding social costs.²⁵ My empirical work has found short-term regional economic growth from the introduction of casino gambling, but there appears to be no longer-run economic growth effect. One of my studies currently under review indicates that casino gambling decreases tax revenues in casino states. My work on social costs has focused on methodological problems in identifying and measuring the social costs of gambling.²⁶ Overall, my research leads me to believe that there is some evidence that casinos may have a positive economic effect in the short-term, but the long-term effects are less certain. This is hardly a warm endorsement of casinos. But at the same time, I do reject the assessment that Grinols and Mustard would have us believe.

In addition to publishing in peer-reviewed journals, I have done a variety of consulting work, primarily on the social costs of gambling. This work has been aimed at identifying potential problems for researchers attempting to measure the costs and benefits of gambling, as well as the refutation of specific cost-benefit analyses which appeared to me to be seriously flawed. Sponsors of my consulting work have included the casino industry (e.g., American Gaming Association, Nevada Resort Association, Casino Association of Indiana) as well as government/research organizations (Alberta Gaming Research Institute and the Canadian Centre on Substance Abuse). I assume that the industry has hired me as a consultant because my social cost methodology (welfare economics) leads to significantly lower social cost estimates than the methodologies used by other researchers, including Grinols and Mustard.²⁷

Much has been made of financial ties that researchers sometimes have to industry. For example, Grinols and Mustard have questioned the validity of casino-crime research that was conducted or funded by pro- or anti-casino groups (28). In other work, Grinols has cited a paper of mine (Walker 2003) as being an example of “shadow research,” or work that is “funded in the hope or expectation that it will contradict research unfavorable to the sponsoring industry” (Grinols 2007,

25 See Walker and Jackson (1998, 2007, 2008a, 2008b), Walker and Barnett (1999), and Walker (2007a, 2007b).

26 I have been critical of a variety of researchers who have attempted to measure social costs without first giving a clear explanation of what they are trying to measure. See Walker (2007a, chapters 6-8).

27 Grinols and Mustard (2001b) and Grinols (2004) provide social cost estimates based on previous research, most of which was not peer-reviewed (Grinols and Mustard 2001b, 152). Such social cost studies have been criticized as being somewhat arbitrary (National Research Council 1999, 185). For a detailed discussion, see Walker (2007a).

517).²⁸ At the same time, Grinols claims to believe that “research can be evaluated on its own merit, regardless of its sponsor. It is certainly not improper for an industry to sponsor research or for a researcher to accept industry money” (516).

In order to address any perceived conflict of interest, I should emphasize that my consulting work has always been an application of my un-funded, peer-reviewed published work. Furthermore, my current comment on Grinols and Mustard’s crime paper was not funded by, nor even discussed with, any industry representative or organization. My motivation for writing this comment was simply to question the Grinols and Mustard analysis and results because they were published in such a prestigious journal and have been influential, despite with what I see as flagrant errors. But even my being paid to write the comment would not, in itself, invalidate the arguments.

Every researcher has sensibilities related to the subjects he studies. To claim otherwise would be disingenuous. The Nobel laureate economist Gunnar Myrdal propounded the view that whenever personal commitments, financial, intellectual, or otherwise, might color one’s formulation or analysis, science and ethics demand that such commitments be made known to readers (Myrdal 1969). I generally take a libertarian perspective on consumer issues such as gambling.²⁹ However, I try to keep these sensibilities from distorting my research, and I attempt to be as transparent as possible in explaining my methods and reasoning.

I do not believe either Grinols or Mustard does paid consulting work on gambling. However, Grinols recently co-authored an op-ed piece with the co-chair of Citizens Against Casino Gambling in Erie County (Grinols and Rose 2007). In fact, he has consistently argued that the costs of casinos are greater than the benefits, at least as early as 1992, prior to there being much of any data on the effects of casinos outside of Las Vegas and Atlantic City.³⁰ And Grinols and Mustard’s work is posted or cited on a variety of anti-casino activist websites. Do these things indicate that Grinols and Mustard are biased, or view casino gambling as a negative “merit good”? No more than being an industry consultant is indicative of a pro-casino bias. Regardless of how controversy, personal or religious beliefs, funding sources, and other factors may affect a researcher’s work, the best way to assess a dispute among researchers is on the basis of the research itself.

28 But as Grinols and Mustard’s paper demonstrates, some gambling research is flawed. I see no good reason that researchers should shy away from debating flawed research simply because there are interested parties.

29 I note that Grinols and Mustard have been, respectively, President and Vice President of the Association of Christian Economists ([link](#)), so their personal views of gambling may well be different from mine. I am not suggesting, however, that these views distorted their research findings.

30 An anti-gambling op-ed by Grinols was entered into the Congressional Record by Senator Simon on January 22, 1992 (p. S187). In the article, Grinols refers to gambling as a “delusion.”

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