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Correctly Critiquing Casino-Crime Causality

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Abstract

A Reply To: Douglas M. Walker, "Do Casinos Really Cause Crime?" *Econ Journal Watch* 5(1), January 2008: 5-20. Link.

We thank Professor Walker for his attention to our paper on casinos and crime, published in the *Review of Economics and Statistics* (Grinols and Mustard 2006). Professor Walker raises five concerns that are standard in empirical research. We addressed these concerns in the working and published versions of the paper and discussed them with the referees and editor during the review process. Some are well-known statistical issues, some are data limitations, and some are methodology issues. All of his concerns speak of *potential* problems. He includes no new research or statistical results to provide evidence that the potential problems are actual problems or that they are important. Nevertheless, we respond by taking in turn each of the issues he mentions, explaining how we treated them in our previous work, providing the references to our previous work, and, where appropriate, elaborating on the concerns. Because he presents no new data, no new research, and his criticisms are largely addressed in the working and published versions of our paper, we have no reasons to alter the conclusions of our existing research.

Calculating the Crime Rate

Professor Walker's first concern is that our use of the standard crime rate as defined by the F.B.I. (the number of crimes divided by 100,000 of the popula-

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tion) is incorrect. Walker believes the correct crime rate measure is the number of crime incidents divided by the sum of population *plus* some estimate of the number of visitors or visitor-days associated with the area in question. In our paper we call this the "diluted" crime rate to distinguish it from the standard (undiluted) crime rate because it divides the number of crimes by a larger denominator. This is an important distinction that we addressed in our original paper (Grinols and Mustard 2006, 33-35) where we provided a framework for addressing the appropriate crime rate. What are the central conclusions about the use of various crime rates?

First, there is no theoretical reason why one crime statistic should be the only object of study. We repeat the conclusions of our original paper, "Some have argued for one [statistic] or another without realizing that the choice is not methodological, but depends on what questions the researcher wants to answer." Those who prefer using the diluted crime rate support their view by arguing that the diluted crime rate is a better indicator of the probability that a resident will be the victim of crime. However, this reasoning is incorrect. Grinols and Mustard (2006) states: "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" (34). We then proceed to provide a hypothetical example and conclude that: "Thus in this case the diluted crime rate *falls* while the probability of a resident being victimized *rises*" (emphasis in original). Professor Walker devotes over 2,000 words in his paper to our single paragraph on this issue and reaches the following points of agreement with us, "One *can imagine* a situation which provides the conclusion that the risk to residents rises even though the 'diluted' crime rate falls (emphasis in original)," and "The important point is that the relationship between risk to residents and the 'undiluted' and 'diluted' crime rates depends critically on who the criminals are and who the victims are" (10).

If there are many crime-related statistics of interest, and we and Professor Walker agree that diluted crime rates and the probability of a resident being victimized can move in opposite directions, then why does Professor Walker believe there is a problem with our paper? The answer, we believe, is twofold. First, Professor Walker misreads our paper. Walker writes "the apparent objective of the Grinols and Mustard paper is to analyze the risk of casino county residents falling victim to crime." Walker's statement is false. Grinols and Mustard (2006, 35) clearly states, "In this study we are interested 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 based on equation (3)." In other words, because crime perpetrated in a given geographical area can impose costs that fall on local taxpayers, it is appropriate to consider the total number of crime incidents relative to the local population and tax base. Second, Professor Walker misreads the literature. Walker says "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 numerator and the denominator." Why is this true? We demonstrated that there is no theoretical reason that only one object of study is correct. A careful reading shows that scholars, including those cited by Walker, prefer one statistic or another *conditional on what the researcher wants to do*.

On the matter of calculating the crime rate, our second central point is empirical—we tried to obtain systematic data sets that record annual visitors at the county level, but found nothing of sufficient quality. We believe that a careful examination of the effect of casinos on diluted crime rates would be a contribution to the literature. The best data that we were able to find on visitors was county-level data on the number of visitors to national parks and monuments. We (2006, 34) gave some preliminary evidence that increased national park visitors are not associated with higher crime rates and suggested that future research investigate the extent to which the type of visitor matters to crime levels. We followed up this line of inquiry by more thoroughly examining how the number of visitors to national parks affects crime rates. We (2007) conclude that the number of visitors to these national landmarks generally has no effect on crime. Therefore, if visitors are a determinant of crime, then the type of visitor that is attracted to casinos is very different from the type of visitor that is attracted to national landmarks.

The last main point on this issue concerns our empirical results on neighbor counties (Grinols and Mustard 2006, sect. VI). One reason that we examined how casinos in one county affect the crime rates in border counties is that if people from surrounding areas substituted committing a crime in a casino area for committing a crime in their home area, then we expected crime to decrease in the neighbor counties, because a substantial share of visitors to casinos that we used to identify the effect of casinos on crime come from nearby (Grinols and Mustard 2006, 42). However, the data clearly do not show such a pattern. The effect of a casino on the crime rate of neighbor counties is very similar to the pattern in the home counties, but the magnitude is not as large. The data clearly reject the hypothesis that casinos reduce crime in border counties. If anything, casinos lead crime to either stay the same or increase in the surrounding areas (depending on the type of crime and how many years after it opened). This pattern is inconsistent with the notion that criminals in neighboring areas are substituting the location of their crime from border counties to casino counties.

To summarize, the choice of the dependent variable in part depends on the question you want to ask. Also, there are empirical limitations to doing exactly what Professor Walker proposes because there are no systematic data that record the annual number of visitors by county. Lastly, to the extent that we can obtain quality data on a subset of visitors, the data show that visitors to national landmarks do not raise crime rates.

Measurement errors in the crime data

Next, Professor Walker is concerned that the crime data may contain measurement error. He focuses on one way that may have occurred—that during the 1990s there was a change in the way that the crime data were put together. However, the measurement errors in crime data go beyond this specific concern and are well known to individuals who work in this area. Other examples are that cities may report crime differently, that the degree of underreporting differs across geographic region, etc. The F.B.I. Uniform Crime Report, which provides the crime data, regularly cautions against comparing crime rates across cities, counties, metropolitan areas, and states because different jurisdictions report crime rates in different ways.[3] Clearly there is the *potential* for bias, but Walker's contentions do not go much beyond that. He does not reference the way the literature addresses potential bias, does not provide any frame of reference for understanding how or when the estimates could be biased, and offers no evidence that the estimates actually are biased. Nevertheless, we examine the possibility of measurement error in more detail and consider the implications they have for our estimated casino effects.

First, if the measurement error is uncorrelated with the error term, then the coefficient estimates are unbiased and the standard errors are larger than they should really be. That means that our reported estimated effects are less precise than the real effects. To the extent that such measurement error exists, our published results are biased *against* our finding an effect.

Second, while it is important to acknowledge the possibility of measurement error in the data it is also important to realize that there is an extensive literature using crime data that has developed ways to deal with the measurement error. For example, many studies, including ours, use county-level, metropolitan-area, or state-level fixed effect regressions. Fixed effect regressions are very powerful because they control for differences in reporting and administration across jurisdictions that are unobservable to the analyst.

Third, if the measurement error in crime rates varies over time and across counties in a way that is correlated with casino openings and that fixed effects do not completely control for, then there is a possibility for the coefficient estimates to be biased. If true, then our published estimated effects could be biased down. So the concern that Professor Walker articulates on this point could make our results even stronger. His concern works against our results only in a relatively narrow set of possible situations, and he provides no statistical evidence that such is case. When we first started to write the paper we ran some simple correlations between counties where casinos opened and counties that changed the way that crime rates were estimated in the mid 1990s, and found that there was little cor-

3 For examples of cautions about the data, see http://www.fbi.gov/ucr/cius2006/rankingmessage.htm and http://www.fbi.gov/ucr/cius2006/about/variables_affecting_crime.html.

relation between the two.

In sum, the question is not whether there is measurement error in the data—virtually all data sets have some measurement error. Instead, we want to know whether the measurement error that exists can be addressed using various research methods, and if not, whether the measurement error is sufficiently large and correlated with the variables of interest in such a way as to lead to an over-estimate of the true effect. Neither our inquiry nor Professor Walker's criticism provided evidence of such systematic correlation. Therefore, we have no reason to believe that the measurement error is large enough or systematically correlated enough with the variables of interest so as to lead to the estimated effects being overstated.

Simultaneity—counties may self-select into the "casino county" category

Professor Walker's third concern is that our results may suffer from simultaneity bias caused by counties self-selecting into the casino-county category. He says, "counties with relatively poorly performing economies might be more likely to introduce casinos and to do so more quickly," and "counties may have legalized casinos in part because of economic hardships…representing factors that may be driving Grinols and Mustard's results" (12). Once again, no theoretical details, supporting statistical evidence, or further explanation is given as to precisely how these facts would bias the results of our paper. The only empirical evidence cited is that of Fink, Marco, and Rork (2004), but that paper deals with states self-selecting into lotteries, not counties into casinos.

Simultaneity is a consideration that could apply to any multiple regression. If y is the dependent variable, variables x and y could be correlated either because x is an exogenous variable that influences y, or x and y are endogenous. In the present context, if "crime causes casinos," then the estimates of the effect of a casino on crime may be biased. We were keenly aware of the possibility of endogeneity[4], and we spent considerable time on this issue.

We addressed the question, first, in a theoretical way. Our paper was the first paper to provide a clear theoretical treatment of how casinos could affect crime differently over time (Grinols and Mustard 2006, sect. III C, 31). We showed that casinos may be more likely to be placed in high crime areas, a concern Professor Walker reiterates. However, we also stated that crime may be lower in counties with casinos before they open because of the crime-reducing effects of better labor market opportunities through the construction and building phase of the

4 Professor Walker acknowledges, "Indeed, Grinols and Mustard mention the common belief that casinos are more likely to be placed in high-crime areas and that the number of casinos began increasing rapidly in 1991" (12).

casino (a potential source of bias that could lead us to understate the effects of casinos on crime that Professor Walker does not mention). Therefore, there are theoretical reasons why crime in casino counties would be higher or lower before opening, and we must examine the data to learn more.

Second, our paper was the first one in this literature to empirically test the degree to which casinos have different effects over time (Grinols and Mustard 2006, sect. V and VI). We did this by including a series of leads and lag variables. Section IV C (35) shows the empirical specification that we used to estimate intertemporal effects. The baseline regression results include two lead and five lag indicator variables, but we estimated many alternative specifications that included up to five lead and seven lags. The lead variables allow us to explicitly test concerns about endogeneity.

We first regressed the crime rate on the casino leads and lags and county and year fixed effects with no other control variables. These results generally showed that the lead variables are positive and frequently statistically significant—providing some evidence that crime is higher in casino counties prior to casino openings (Grinols and Mustard 2006, Table 3, 35-36). Next, we re-ran the same regressions and included a large set of control variables (Table 4).[5] This time the results generally showed that the coefficient estimates on the lead variables, which were statistically significant prior to inclusion of our large set of control variables, were not statistically different from zero.[6] Once control variables were used, casino counties were not statistically different from non-casino counties before casinos were introduced. We emphasized these results, because this specification provides a better fit and accounts for endogeneity.

The third piece of evidence on simultaneity comes from Grinols and Mustard (1999), a working paper version of the published paper, in which we devoted considerably more attention to this problem. There we identified a set of instrumental variables and ran sets of two-stage regressions to control for potential endogeneity. In this earlier version of the paper we wrote:

> The earlier empirical results and Figures 7 and 8 [shown in that paper] show that casino and non-casino counties have very similar crime patterns prior to casino opening. We do not believe that simultaneity is a significant problem. Nevertheless, to investigate

5 Grinols and Mustard (2006, sect. II) lists and defines the basic control variables in detail. They include population density, population distributions by race, age, and sex, and an assortment of income and unemployment variables. This list of control variables is the most exhaustive of studies in this literature. In robustness checks later in the paper we also included control variables on law enforcement variables like arrest rates and four capital punishment variables.

6 We studied seven crimes—murder, rape, robbery, aggravated assault, robbery, burglary, larceny, and auto theft. The leads were not statistically different from zero for the first six offense types. Only for auto theft was there some evidence that, after controlling for an array of variables, crime rates were higher in casino counties before the casino opened. We talk about this case in more detail in the original paper.

> whether our conclusions might be biased by simultaneity, we conducted Hausman tests on each of the crime regressions.[7] The instruments we used were indicator variables for counties with major rivers,[8] counties that bordered states, counties that bordered states and bordered metropolitan statistical areas with population of 50,000 or greater, counties with Indian reservations, and counties with Indian gambling compacts for the years after 1990. Casinos are often located on major rivers (there are riverboat casinos in Indiana, Illinois, Iowa and Louisiana), are deliberately placed on the borders of states to attract clients from neighboring areas (Tunica, Mississippi borders Memphis, Tennessee and East St. Louis, Illinois borders St. Louis, Missouri) and are often located on Indian reservation trust land.
>
> All of the regressions passed the Hausman test except robbery.[9] However, even in this case the casino coefficients for the OLS and 2SLS regressions were very similar. Both methods showed virtually the same pattern over time and very similar coefficient estimates.[10] Because the Hausman test rejects simultaneity and the results for OLS and 2SLS are nearly the same, we use the OLS estimates for the rest of the paper. (Grinols and Mustard 1999)

We presented working paper versions of the published paper at annual meetings of the American Law and Economics and of the American Economic Association, and at scholarly workshops at the Universities of Buffalo, Georgia, Illinois, and Rochester. Workshop and conference participants consistently indicated that we spent too much time examining this problem of simultaneity because there was little evidence that it existed or was problematic. Referee and editorial comments reiterated this theme. Therefore, we dropped the instrumental variable approach with two-stage least squares from the published version of the paper.

To conclude, we believe that it is very important to control for endogeneity in doing quality empirical work. The published and working paper versions of this paper spent considerable time investigating whether endogeneity was a significant concern, and the data clearly show that it is not. Until there is evidence to the contrary, we see no reason to deviate from the original conclusion (Grinols

7 The Hausman (1978) specification test compares an efficient and consistent estimator under the null hypothesis with another consistent estimator. The test evaluates whether there is sufficient difference between the coefficient sets to reject the null hypothesis that the original estimator is consistent.

8 To be included a county had to be in the top 10 in length, volume of flow, and watershed area. This screen resulted in the Mississippi, Ohio, and Missouri Rivers, all of which have riverboat casinos.

9 The null hypothesis failed to be rejected at the 100, 100, 99, 84, 48 and 26 percent levels for murder, larceny, auto theft, rape, burglary, and aggravated assault, respectively. The P value for the robbery x^2 statistic was marginally significant at .05.

10 The simple correlation between the lag coefficients of the robbery OLS and 2SLS regressions was $p = .99$.

and Mustard 2006): "the casino-opening lead variables suggest that after controlling for other variables casinos were not more likely to be placed in areas that had systematically different crime environments than other regions" (40).

CAN WE IDENTIFY THE EFFECT OF A CASINO? IDENTIFICATION—THE DUMMY VARIABLES USED TO ACCOUNT FOR CASINOS DO NOT ALLOW THE AUTHORS TO ISOLATE THE CRIME EFFECT CAUSED BY CASINOS

Professor Walker's fourth point is that indicator variables do not lead us to isolate the effect of crime caused by casinos. In this section he again proposes a variety of *potential* problems, but provides no data to show that they are, in fact, problems. He reiterates a comment that we brought up in the original paper—that we would very much like to use a more dynamic measure of casino growth like casino revenues. However, such data were not available in any systematic and reliable manner. So we took an alternative approach. As we said in our original paper, if good data on casino revenues are obtained in the future we believe that would make a significant contribution to the literature.

Beyond this concern, Walker follows up on his earlier claim that "the crime effect found by Grinols and Mustard in casino counties is due to *tourism in general* rather than to *casino-specific tourism*" (13, emphasis in original). We certainly acknowledge that this is a possibility, but to the extent that data exist on this issue, again, we found no evidence for the contention, and Walker does not provide any evidence. Similarly, he claims, "In general, anything that distinguishes the casino counties from national norms will be picked up by the dummy" (13). It appears as though he is referring to the potential for omitted variables to bias our coefficient estimates, but once again offers no evidence of what the omitted variables might be or how they are correlated with casino openings. Grinols and Mustard (2006, sect. I) provides an overview of the literature on this topic, shows that omitted variable bias is a problem because most studies include very few control variables. In contrast, our study included by far the largest set of control variables and we included county and year fixed effects that control even for unobserved differences across counties and time.

Interestingly, in his discussion of omitted variables, Professor Walker does not refer to variables that were omitted from our base regressions, that do affect crime, and that are correlated with casino openings. We (2006, sect. V C) examine how the casino estimates are affected when law enforcement control variables like arrest rates and measures of capital punishment are included. When these variables are included, the estimated casino effects were larger than the ones we emphasized in the paper. In this case, as in others in the paper, we deliberately chose to emphasize results that provided smaller estimates of the casino effects, something that Professor Walker neglects to mention.

As a matter of logic, it is impossible for us to prove that some unknown variable that affects crime and is correlated with casinos in such a way as to lead our results to be biased upward does not exist. No matter how exhaustive our examination, there is always the logical possibility that some, as yet unknown, mechanism might be found. However, in such cases, the burden is on scholars like Professor Walker to produce an actual example of what they claim, and provide the evidence that proves that it operates in the manner they say it does.

Lag 5 crime rates

Professor Walker is concerned that other calculations might give different numbers than the statistics we report in our paper for the effect after five years from opening of casinos on crime rates. For example, he says,

> 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. (14)

However, Professor Walker provides no evidence for this conjecture. In fact, later-adopting casinos are likely to have had higher tourism in the period we study because novelty initially works in favor of tourism but fades with time.

Other conjectures are simply false. For example, Walker says, "the Lag 5 crime rate estimates are the highest of any in the model" (14). We wrote:

> We checked whether the rising patterns of coefficient estimates in the last three years with the lag 5 estimated coefficients positive and significant persisted or disappeared after the fifth year. Estimates of the sixth- and seventh-year lags were 745 and 1,069 for larceny and 201 and 229 for burglary, respectively. Moreover, lags 5 through 7 pass a 5% F-test for significance for both offenses. (Grinols and Mustard 2006, 38)

The lag 5 estimate for larceny was 614.695, which is *lower* than the reported estimates for lags 6 and 7. However, even if the fifth year lag is larger, why is this a concern? Research should not pre-judge outcomes. Estimates of the lag 5 crime rates are the best econometric estimate of the impact of a casino on crime rates in the fifth year after casino opening. They remain, therefore, the best statistic we have to calculate the effect of casinos on crime rates after five years.

CONCLUSION

It is important to keep a balanced perspective. We undertook our comprehensive study of casinos, crime, and community costs because we believed that existing research could be improved on. We (2006, sect. I) provide a detailed account of weaknesses of the prior casino-crime literature. First, no previous study examined the intertemporal effects of casinos on crime. Second, many studies used very small and sometimes selected samples. Third, some studies made conclusions about crime rates without studying actual crime rates. Fourth, most studies used very few control variables and suffered substantial omitted variable bias. Fifth, few studies examined the theoretical links between casinos and crime. Lastly, many studies were agenda driven and funded by organizations with a vested interest in the outcome of the research.

What are the facts? We know that casinos cause crime, because we have many cases, even documented in the press, where individuals engaged in crime for reasons that trace to their casino gambling. A few examples were cited in our original paper on pages 32 and 33 to document this fact. The research question, therefore, is not whether casinos cause crime, but whether one is able to show the magnitude of this connection statistically and separate it from the many other causal sources. Those who engage in statistical work know that it can be hard, painstaking, and tedious, especially in situations where many factors contribute to the object of study. A failure to show a link does not prove there is none, only that the researchers may have used a sample too small or methodology too weak to make the connection.

Our paper makes clear and substantial improvements to the existing body of literature in at least these six dimensions. Is our paper perfect or without blemish? Absolutely not. For research to be moved forward we can use new methodologies, formulate better theories, and examine different data to better understand how mechanisms work and in what context they work. We welcome criticism and new research along this dimension. Unfortunately, the Walker criticisms provide nothing in this vein. He raises *potential* criticisms that are well-known in the literature and are largely dealt with in our working paper and published versions of our paper. Although he reiterates our discussion that these concerns may bias our results, he provides no new evidence that they in fact occur or are important if they do occur. In light of this absence of new information we have no reasons to alter the conclusions of our initial research on this topic.

REFERENCES

Fink, S.C., A.C. Marco, and JC Rork. 2004. Lotto nothing? The budgetary impact of state lotteries. *Applied Economics* 36: 2357-2367.

Grinols, Earl L., David B. Mustard, and Cynthia Hunt Dilley. 1999. Casinos and Crime. University of Georgia. Working draft.

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

Grinols, Earl L. and David B. Mustard. 2007. Visitors and Crime. University of Georgia. Working draft.

Hausman, J. 1978. Specification Tests in Econometrics. *Econometrics* 46(6): 1251-1271.

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

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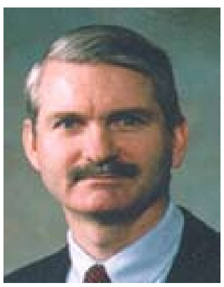

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