Final Comments on the Casinos and Crime *Econ Journal Watch* Exchange

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Notes to Readers

- Citations to earlier papers in this exchange use numbers in brackets. References [1] through [5] are the papers in the casino crime exchange, in chronological order of publication. I also use "GM" to refer to Grinols and Mustard. These formats are chosen for brevity.

- This paper was posted on my College of Charleston website, rather than being published in *Econ Journal Watch*, because prior to providing their latest reply, Grinols and Mustard sought assurance from the *EJW* editor that I would not be allowed to rejoin again in *EJW*. Their request was granted, as indicated in the editor's note at the top of my latest paper [4]. In my view this was a reasonable editorial decision.

- Because much of GM's latest reply uses selective quotations, statements made out-of-context, and straw men arguments, I strongly recommend a careful reading of all the previous papers in this exchange, if readers are truly interested in understanding the issues under debate.

I. Introduction

In my first comment [2] on GM's crime paper [1], I discussed five issues that I believe raise serious questions about the legitimacy of their strong conclusions regarding the casino-crime relationship. My point was not to deny that casinos cause crime, but rather, to show that the empirical estimates provided by GM are less than reliable [4, p. 148]. In their first reply [3], GM address each of the issues I raised, but to varying degrees of relevance and detail. In some cases, they simply make straw man arguments. Seeing this, I decided in my rejoinder [4] to focus on what I see as the primary issue – the crime rate and cost estimates based on it. Readers who are interested in the other issues raised in my first comment should carefully re-read the first exchange [2 and 3].

GM had another chance to clarify the crime rate and cost of crime issues in their second reply [5]. In their latest, astonishing reply, GM choose to ignore the substantive issues and instead provide a diatribe, with the apparent purpose of discrediting me. Although most of their latest reply is utterly irrelevant to the debate over their original paper, I will address some parts of their reply.

GM criticize me because my work "provides no new data or research," and that my "complaints stem from errors of fact" [5, p. 156]. They also blur the substantive issues by making irrelevant points and quoting out of context, rather than addressing the arguments I have made. In the interest of scholarship I address those issues, but relegate the discussion to the appendix. Because GM's first three points [5, pp. 156-158] are mostly irrelevant, the discussion of those points also appears in the appendix. GM's fourth point [5, pp. 158-160] is partly substantive and relevant, and I address it in this paper.

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II. The Crime Rate, the Costs of Crime, and now…the "Total Effect"

My argument remains (unchallenged by GM) that if one calculates a crime rate by including all crimes committed by residents and visitors in the numerator, but dividing by only the county residents – excluding county visitors from the denominator – then the resulting crime rate overestimates the risk of being a crime victim in casino counties. This crime rate also overstates the marginal impact of casinos on crime. Furthermore, any cost of crime estimate that uses this "undiluted" crime rate will overstate the costs of crime attributable to casinos. Crime or cost data based on a crime rate that ignores county visitors in the population measure are likely to be meaningless or deceptive. Rather than addressing these fundamental issues, GM further cloud the debate by again suggesting that their interest in their original paper was something other than what I interpret it to be.

Just to review, in my original comment I explained why I focused on the risk to casino county residents of falling victim to crime [2, pp. 7-8]. Then GM emphasized in their reply [3, p. 22] that they were, in fact, "interested in the costs to the host county associated with a change in crime, from whatever source." So in my rejoinder, I focused on the problem with GM's cost of crime estimate [4, pp. 148-151]. I showed why their cost estimate ($75 per casino county resident per year) is baseless. Now, in their latest reply, GM state that their interest all along was in the "total effect of casinos on crime" [5, p. 159].

It finally occurs to me that it will be impossible to pin-down GM on exactly what they are trying to measure in their original paper [1]. Their strategy in this debate appears to be something like, "be as elusive as possible in explaining our work." After I have shown that the "undiluted" crime rate will overstate the risk of falling victim to crime [2], and that use of that crime rate results in an overestimate of the costs of crime caused by casinos [4], GM now write about me [5, p. 159]:

> His previous concern [the risk to residents of being victimized] 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.

Readers should re-read the context surrounding this issue [3, p. 22; 4, pp. 149-150; 5, p. 159]. Note that GM do not bother to specify what exactly they mean by "total effect of casinos on crime." Furthermore, their quotation of their own previous work excludes important context.[1] In their original study, GM explained

> *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). [1, p. 35, emphasis added]

They repeated this quotation in their first reply [3, p. 22]. This is why I focused my rejoinder [4] on the costs of crime attributable to casinos. Aside from this, the choice of which crime rate to use is completely irrelevant to the "risk to residents." The point remains, GM have no basis for choosing the "undiluted" crime rate, other than that they simply don't have visitor data.

[1] GM have also apparently mis-cited their own paper, as the statement they quote appears in [1], p. 35, not p. 30.

But now GM state that they are really interested in the "total effect of casinos on crime," whatever that means. There are a few possible interpretations.[2] First, if it refers to the crime rates in casino counties relative to non-casino counties, then I have already addressed that issue [2]. Second, if it refers to the absolute number of crimes committed in casino counties relative to non-casino counties, then the GM results are still meaningless. Showing that the absolute number of crimes committed in casino counties relative to non-casino counties tells us nothing about whether casinos cause crime, since this measure ignores the fact that there is a large number of tourists in casino counties.[3] But this appears to be the course GM are now taking, as they later discuss the "social costs of gambling" in the context of the "change in the absolute number of crime incidents" [5, p. 160]. As I have stated previously, such estimated costs will be overstated if the absolute change in crime incidents is not adjusted by the change in population due to casino visitors.

A second point on cost estimates that I made in my rejoinder [4, pp. 150-151] was that GM used cost per crime estimates from Miller, Cohen, and Wiersema (1996), who specified that most of the estimated costs fall on *crime victims*, not on the residents of the counties in which the crimes were perpetrated. Since GM cannot distinguish between resident and visitor victims, and because their crime rate is calculated relative to casino county residents only, they have implicitly assumed all of the costs in their estimate fall on casino county residents [1, p. 28, 41].

This point is not addressed by GM in their latest reply [5]. Instead, they attack a straw man argument [5, pp. 159-160]. They explain that they did not implicitly attribute the cost burden to residents because, as they said in their original paper, "crime could rise 'because casinos attract visitors who are more prone *to commit and be victims of crime*'" [5, p. 159]. They continue, "Earlier in the paper we provided theoretical explanations for how casinos might affect crime…" Both of these statements are true, and completely irrelevant to my point.

III. Public Consumption of Gambling Research…Again

The other major point of my rejoinder [4, pp. 151-154] was that this debate – whether we focus on the crime rate or the costs of crime – is important because voters and policymakers are influenced by research such as GM's [1]. Yet, many public statements regarding the "social costs of casinos" made by GM, and others, are based on highly questionable methodologies and results. The users of such research are unlikely to be cognizant of problems with the research and its conclusions. I suspect few people would be convinced by GM's crime results if they understood that the results are based on the exclusion of visitors from the denominator of the crime rate. GM [5] ignore the issue of whether their public statements on the effects of casinos are supported by valid research.

IV. Conclusion

In this paper I have taken a "minimalist" approach, and have given very brief reactions to GM's latest reply [5]. I took this approach for several reasons. First, I believe the substance of my arguments regarding the crime rate and costs of crime have gone mostly unchallenged by

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[2] Seeing how GM change their story each time they are confronted, the reader may be best advised to re-read the previous exchange and decide for himself whether or not GM interpreted their results correctly.

[3] This would be analogous to arguing that, since more crimes are committed in Orlando than in other parts of Florida, Disney World causes crime.

GM. They have made straw man arguments, discussed tangential and irrelevant issues, and have misrepresented some of my arguments, but have done little to address the basic issues of (i) which crime rate is appropriate to use and (ii) whether their cost estimate really means what they say it means. I encourage interested readers to carefully review the previous papers in this debate [1-5], and judge for themselves how meaningful GM's original results are.

Appendix

Most of the material in GM's first three points of their latest reply is a shallow attempt to convince the reader that I have misstated arguments, made factually incorrect statements, or that I am otherwise trying to dupe readers into doubting GM's work. Indeed, their first three points do not address any substantive issues.

It is readily obvious that GM, unable to defend their original casino-crime paper, have no other option but to turn the focus away from their study. It is unfortunate that I now must stoop to GM's level and waste so much time, just to defend my scholarship. In fact, all of their attacks on me are invalid or irrelevant to the main issues under debate. In the appendix, I will show that it is GM who are engaging in sloppy scholarship, relying on out-of-context quotations, citing the wrong papers, and outright false claims, all in an apparent attempt to discredit my criticisms of their casino crime study.

GM Point 1

In their first point, GM write, "Mr. Walker says that our paper gives a 'flawed example.' Mr. Walker is incorrect…" [5, p. 156]. They then go on to explain that the "purpose of the model is to show that the diluted crime rate…can fall while the probability of a resident being victimized rises" [5, p. 157]. It is as if they never read my first comment [2].

Their point is irrelevant. I showed in my first comment [2, p. 8, note 7] that their model is flawed. (They suggest that two conditions follow from an assumption, when they do not.) The larger point I made was that their result is quite unlikely to occur in reality, so their example does not justify using the "undiluted" crime rate. They have yet to address that issue, and instead make an empty claim that I am incorrect.[4]

GM Point 2

In their second point, GM completely ignore the point I was making in citing the three examples of social cost studies [4, p. 153]. The point was that these types of cost estimates are mostly arbitrary, and researchers who make public statements should qualify their statements, otherwise media, politicians, and the public can be deceived about the true effects of casinos. As we are all social scientists, one would think that GM would share some of my concerns. Instead,

[4] If GM's purpose in their empirical analysis is not to assess the risk to residents of being victimized, as they state [3, p. 22; 5, p. 159], then why is this risk used in their example to justify using the "diluted" crime rate? GM do not explain.

their interest is in showing that casinos have a negative impact on society, and they appear to be willing to go to great lengths to forward that position.

Rather than addressing the validity or quality of the available social cost estimates, or whether it is appropriate to make strong public statements on the cost estimate without qualification, GM instead try to suggest that I have misread or misunderstood the literature [5, p. 157].

The evidence they cite to try to show that my scholarship is weak is that the National Research Council (NRC 1999) did not, in fact, cite two of the three studies that I used as examples of arbitrarily determined social cost estimates [5, p. 158]. Indeed, GM are correct that the NRC did not cite two of the studies, noting that one of the studies I cited was published four years after the NRC report was published. But whether or not the NRC specifically attacked those studies is irrelevant to the point I was making. This is yet another straw man by GM.

I was arguing that many social cost studies arrive at their cost estimates in ways "highly arbitrary" [4, p. 153]. I then gave three examples from the literature. Then I wrote,

> 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 (1999) [4, p. 153].

GM focus on the last sentence quoted above. When I wrote that, I meant that the NRC was critical of studies that arrive at cost estimates arbitrarily – in general. I was not meaning to suggest that the NRC had cited those specific examples in their criticism.

It is beyond me how GM could infer that I was suggesting that the NRC cited those particular studies. I suppose the word, "such" was confusing to GM. But clearly, I was not trying to suggest that the NRC cited those specific studies. See note 15 in my rejoinder [4, p. 153], in which I clarify that a 2003 paper was cited in a 2004 source, but "obviously not" by a 2001 source.[5] As demonstrated in my rejoinder, I am well aware of the problems with citing a paper that has not yet been written.

In referencing the NRC to support my contention perhaps I should have provided of the quotations that I had in mind when I said that the NRC was critical of arbitrary social cost estimates. I had in mind comments including:

> Not surprisingly, most reported economic analysis in the literature is methodologically weak. In their most rudimentary form, such studies are little more than a crude accounting, bringing together readily available numbers from a variety of disparate sources. Among studies of the overall effects of gambling, such rough-and-ready analyses are common. In the area of gambling, pathological gambling, and problem gambling, systematic data are rarely to be found, despite considerable pressure for information. The consequence has been a plethora of studies with implicit but untested assumptions underlying the analysis that often are either unacknowledged by those performing the analysis, or likely to be misunderstood by those relying on the results. Not

[5] Had I intended to imply that the NRC had cited those particular studies, I would have introduced the examples by saying something to the effect that "the NRC has been critical of studies that arrive at their results arbitrarily. Here are some examples *they cite*…" Instead, I referred to the NRC after I had already discussed the examples from the literature, to show that that source generally supported my assessment.

> surprisingly, the findings of rudimentary economic impact analyses can be misused by those who are not aware of their limitations. (NRC 1999, p. 162)

and

> In most of the impact analyses of gambling and of pathological and problem gambling, the methods used are so inadequate as to invalidate the conclusions. Researchers in this area have struggled with the absence of systematic data that could inform their analysis and consequently have substituted assumptions for the missing data. (NRC 1999, p. 185)

and

> Finally, few of the studies on the economic impact of gambling to date have appeared in peer-reviewed publications. Most have appeared as reports, chapters in books, or proceedings at conferences, and those few that have been subject to peer review have, for the most part, been descriptive pieces. (NRC 1999, p. 186)

All of these criticisms by the NRC (1999) apply to all of the studies I quoted as examples of arbitrary cost estimates [4, p. 153]. Furthermore, the criticisms also apply to most of the studies Grinols uses in his book to derive a social cost of gambling estimate. These are the criticisms I had in mind when I cited the NRC on p. 153 [4]. I was not suggesting that my quotations were taken from the NRC, or that the NRC cited those particular studies.

Overall, the point GM are trying to make is a trivial attempt to show that I am somehow trying to deceive readers. Yet their attempt will fail quickly if readers simply read the NRC report (chapter 5).

Three other attempts by GM to question my scholarship in their second point deserve a brief response. One wonders whether GM actually read the NRC report or the cited papers in their footnote 4 [5, p. 158], or the papers cited by NRC.

In their text (5, p. 158) and in the first part of their note 4, GM suggest that the Thompson, Gazel, and Rickman paper that I cited in my rejoinder ("The Social Costs of Gambling in Wisconsin") was praised by the NRC. This is false. The NRC was actually referring to a different paper by Thompson et al.

Toward the bottom of GM's footnote 4, they claim that the NRC favorably cites another paper by Thompson et al. Here, GM are again wrong, as the NRC was actually citing the paper that I quoted in my rejoinder. While the NRC does applaud the Wisconsin social cost paper, they also write, "Even this study, however, is not without serious flaws and often counts as benefits things that would properly have been considered transfers. Nevertheless, this study is an improvement over many previous ones" (NRC 1999, p. 184). NRC write that the study has "serious flaws," but GM claim this is a "favorable citation."

Overall, a reading of the NRC (1999, chapter 5) shows that it was largely critical of social cost of gambling studies, including some of those that were used in Grinols' book (2004) and as the foundation for his public statements on the social costs of gambling. GM's claim that I misstated the NRC's evaluation of social cost studies is misleading. They apparently did not even read the papers that they were citing. Otherwise they would have easily realized that the NRC actually listed the wrong papers on its bibliography (NRC 1999, p. 191).[6]

The last comment to be made with regard to GM's second point relates to their footnote 3 [5, p. 157]. GM write, "In 2006 Mr. Walker was hired by the Taiwan Amazing Technology Co. Ltd,

[6] A detailed elaboration is available to interested readers. Please send an email request to WalkerD@cofc.edu.

a manufacturer of gambling machines." This is false. I was hired by Amazing Taiwan Co., Ltd., which is a land development firm, not a manufacturer of gambling machines. Again, this is a case where GM were sloppy.

GM Point 3

In yet another irrelevant point, GM argue that Grinols' social cost of casino estimates from his 2004 book were adjusted for comorbidity. In this discussion they refer to a short report I wrote for the American Gaming Association. In footnote 3, GM list the variety of consulting work I have done, as if this is relevant to the issue of whether or not GM's crime estimates are legitimate.

Yes, I wrote that Grinols (2004) did not account for comorbidity. GM reply that

> 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.

On p. 173, Grinols (2004) writes, "WI, CT, SC, and NV, figures were adjusted by the author to correct for multi-causality according to Schwer et al. (2003) findings." However, his social cost estimate is also based on studies of MD, FL, SD, and LA, which he does not indicate were (or how they were) adjusted for multi-causality. Again here, Grinols appears to take the strategy of giving as few details about the analysis as possible.

On pp. 170-71, Grinols (2004) writes, "Schwer et al. (2003), for example, reported per individual average annual social costs of $19,324 for their sample of 93 individuals with gambling problems, whereas the thirty-nine saying that they did not have other addictions had costs only eighty-eight percent as high, at $17,056." This statement is false. The quoted cost figures appear no where in the Schwer et al. (February 1, 2003) paper cited by Grinols. The estimated social cost in that paper is $19,085.

In fact, the paper cited by Grinols (2004, p. 222) has absolutely no mention of adjusting for comorbidity. The paper does acknowledge the issue of comorbidity (section XI, "Treatment Costs") but the authors make no attempt to adjust for comorbidity in deriving their cost estimate.

Here we can give Grinols the benefit of the doubt and say that he cited the wrong paper. In fact, the cost figures he cites do appear in a 2005 published version of the Schwer et al. paper. Perhaps the adjustment for comorbidity appeared in an earlier draft of the paper that Grinols failed to cite. But the adjustment does not appear in the Feb. 1 or Feb. 18 versions of the paper that I have. How can GM blame me, when Grinols listed the wrong paper in his bibliography?

Other Issues

- Grinols and Mustard indicate in both of their replies that my comments include no new data or research [3, p. 21; 5, p. 156]. It is unclear how this is relevant, as my comments were written for *Econ Journal Watch*. The editorial page on the *EJW* website explains, "*EJW* watches the journals for inappropriate assumptions, weak chains of argument, phony claims of relevance, and omissions of pertinent truths. Pointed, constructive criticism requires an independent forum and an accessible and timely medium." Although GM may not see criticism of their work as being a

useful endeavor, my comments clearly fit in with the mission of *EJW* and can be helpful to gambling researchers and users of gambling research.

- The critical issue that remains is whether they have justified their exclusion of the visitors from the population at risk (denominator) of the crime rate. I argue that they have not. The use of the "undiluted" crime rate simply does not follow from a desire to measure the costs of crime, or the "total effect" of casinos on crime. Hence, they have overstated the crime effect of casinos.

- GM quote me out of context in order to change the meaning of my arguments. For example, after quoting me explaining why, in my first comment, I focused on the risk to casino county residents of being victimized, GM write,

> 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*."

In this statement GM have not provided the appropriate context to their quotation. They do this, apparently, to convince readers that I am shifting focus because I have conceded GM's point. However, the context of that quotation is an explanation that GM have explicitly stated that they are, in fact, interested in the "costs to the host county." I wrote [4, p. 150]:

> So they want us to focus on the costs of crime to casino hosting counties, rather than the risk to residents of falling victim to crime. Yet, the estimated cost of casino crime on casino hosting counties will be overstated if the crime rate attributed to county residents is overstated, as it is using their "undiluted" rate. If Grinols and Mustard want us to think in terms of cost burden per resident, they might elude the crime rate criticism, *but the basis for that criticism simply re-emerges in terms of cost burden per resident*.

This quotation out of context is typical for GM. Readers can find other examples if they view that as a worthwhile use of time.

- Finally, GM have misrepresented a number of my arguments and, instead, attacked straw men. Indeed, much of their latest reply falls into this category, as they shift the focus from real issues to simply trying to attack my scholarship.

  - In their first reply [3, p. 29], GM claim that "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 casino on crime rates." That was not my concern, as is clear from reading my comment [2]

  - GM's entire discussion at the bottom of [5, p. 159] is a straw man argument.

  - Later on the same page, GM write, "While Mr. Walker appears to want to ignore crimes committed against visitors, we believe that crimes committed against visitors are part of total costs" [5, p. 159]. Again, this is a straw man. Note that GM do not provide a page number or quotation for this claim.

In conclusion, the latest GM reply [5] contains so many straw men, arguments taken out of context, and selective quotations that it would take weeks to write a reply to each point. Instead, I leave the debate here and hope that interested readers will consider all of the information from all of the papers in this debate. To me, it is still clear that GM's casino crime results are invalid.

References

[1] Grinols, E., and D. Mustard. 2006. Casinos, crime and community costs. *The Review of Economics and Statistics* 88(1): 28-48.

[2] Walker, D. 2008a. Do casinos really cause crime? *Econ Journal Watch* 5(1): 4-20.

[3] Grinols, E., and D. Mustard. 2008a. Correctly critiquing casino-crime causality. *Econ Journal Watch* 5(1): 21-31.

[4] Walker, D. 2008b. The diluted economics of casinos and crime: A rejoinder to Grinols and Mustard's reply. *Econ Journal Watch* 5(2): 148-155.

[5] Grinols, E., and D. Mustard. 2008b. Connecting casinos and crime: More corrections of Walker. *Econ Journal Watch* 5(2) 156-162.

Bibliographical information on other papers cited herein can be found in the references lists from the other papers in the *EJW* debate. Or readers can request a reference list from me by sending an email to WalkerD@cofc.edu.