Measurement Error in County-Level UCR Data

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Maltz and Targonski (2002) have provided an important service by disaggregating the county level data to help researchers examine measurement errors in the county level data, but their conclusion ''that county-level crime data, as they are currently constituted, should not be used, especially in policy studies'' is not justified. All data has measurement error, presumably even their measures of this error. Unfortunately, however, Maltz and Targonski provide no systematic test for how bad the data are. Their graphs obscure both the small number of counties affected, that these are rural counties, and that just because some of the population in a county is not represented in calculating the crime rate, that is not the same thing as showing that the reported number is in error. Nor do they provide evidence for the more important issue of whether there is a systematic bias in the data. The evidence provided here indicates right-to-carry laws continue to produce substantial reductions in violent crime rates when states with the greatest measurement error are excluded. In fact, restricting the sample results in somewhat larger reductions in murders and robberies, but smaller reductions in aggravated assaults.

KEY WORDS: measurement error; county level UCR crime data; systematic biases.

1. INTRODUCTION

Virtually all data have measurement error.[4] Such problems usually bias results against finding relationships, but the issue is not simply whether measurement error exists but whether it is systematic. The paper by Maltz and Targonski (2002) concentrates on the county data obtained from the Uniform Crime Reports and notes that not all police jurisdictions in a county report their crime data, producing measurement error in the county

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[4]See Klepper and Leamer (1984) and Leamer (1978) for detailed discussions of measurement error in data. An interesting recent discussion of measurement error in crime data can be found in Miron (2001).

level data set derived from these reports. Despite somewhat better impu-
tation methods used for state level data, some of the measurement problems
apparent in the county level data will also be present in the state level data if
only because the state level data are created using these missing individual
police departments.[5] Maltz and Targonski rightly point out that this mea-
surement error may be a potentially serious issue for both the county level
and the state level.

After discussing how the data are compiled and what potential pro-
blems may exist, Maltz and Targonski investigate this measurement error
problem by quantifying under-reporting. In a series of graphs, they show the
variation in percent of the county population unrepresented in the county
crime data for a few selected counties, Georgia, and the average rates of
missing information within counties in different states. The implication,
though it is only briefly discussed for one county, is that crime rates exhibit
similar variability and are unusable.

Despite Maltz and Targonski arguing that measurement error brings
existing research on gun laws that use this data into question, they do not
directly test if it effects the results. In fact, the particular measurement
problems focused on by Maltz and Targonski are not present in the city
level data, and it is important to note that the city, county and state level
UCR data all produce similar results with respect to right-to-carry laws,
waiting periods, one-gun-a-month rules, safe storage laws, and other gun
control laws.[6] This paper focuses on the impact that these data problems
have on right-to-carry laws (the law Maltz and Targonski discuss).[7] While
providing an important service in putting this data together and focusing

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[5]For example, for county level data prior to 1994, when a city misses reporting more than six
months of data no crime and no city population is reported for that city in that year. When six
or more months are available, the available data is used to calculate the annual rate. After
1993, this method to calculate the annual rate is used even if data is available for only three
months. This too creates measurement error problems because the months for which the data
are available may not be representative of the data for the entire year.

[6]While Maltz and Targonski criticize the work of David Mustard and one of the current
authors, John Lott, Mustard and Lott were familiar with the problem raised by Maltz and
Targonski. Indeed, they brought these problems to Michael Maltz's attention. But Maltz and
Targonski have done an important service by actually obtaining the data to help us see whether
these problems are large or small. Lott and Mustard did not have the data available to do more
than crudely try to account for this problem despite literally hundreds of hours on the telephone
with the FBI and the ICPSR. When David Mustard approached Michael Maltz to see if he
knew of any data errors that we had missed we had already compiled an eight page single-
spaced list of problems. It is also part of the reason why Lott and Mustard used county and
state level data and why later work also used city data (Lott, 2000). A debate has arisen over
the county level data (e.g., Lott, 2001; Moody, 2001; Plassmann and Tideman, 2001).

[7]For a survey of the debate over concealed handgun laws see Lott (2000 and 2001).

attention on its shortcomings, Maltz and Targonski draw conclusions much stronger than their evidence supports.

2. A NOTE ON THE GRAPHS

As Maltz and Targonski state in section five of their paper, "small counties are more likely to have extensive reporting deficiencies than larger counties." But the figures they use tend to mask or hide this important fact and they apparently fail to realize that the gun control work they're criticizing uses regressions weighted by population.

Their Fig. 5 examines the 159 counties of Georgia and purports to show how wide spread under-reporting is.[8] The figure dramatically draws attention to the counties with a high level of under-reporting. For example, there are 377 county/years in the figure with under-reporting over 30% (18.2% of the total 2067 county/years). However, the figure tends to obscure the 1474 counties/years with under-reporting less than 10% (71.3% of the total). Even more important, however, the figure does not account for the fact that most of the counties with high rates of under-reporting have very small populations (and thus received very little weight in the gun control work cited since all the estimates were weighted least squares). In fact, the 377 county/years with over 30% under-reporting only account for 6.3% of the total population covered in the 2067 county/years. In contrast, the 1474 county/years with less than 10% under-reporting account for 89.9% of the total population.

Examining the Georgia counties by population further illustrates the fact that low population counties under-report at a higher rate. The 16 least populated counties in 1992 (10% of Georgia's total counties) contain ~1% of Georgia's population. The next 16 least populated counties contain another 1.8% of Georgia's population. Figure 1 illustrates the under-reporting rates for these two groups of counties and the under-reporting rate for the other 127 Georgia counties (80% of total) that account for 97.2% of Georgia's population. The average (weighted across the 13 years) under-reported rate for the bottom decile of counties is 37.3% and the next decile is 28.5%. The 127 most populated counties averaged an under-reporting rate of 5.6% over these 13 years.

The proper way to deal with the disparity in size (and, thus, importance in estimation) of counties is to weight the analysis by population size. This concentrates the attention on high population counties without totally eliminating the information that may be contained in low population observations. Failing to weight by population is the primary reason why

[8]In section five, they first argue "that there is no underlying pattern to the non-reporting behavior." If true, then the measurement error's only effect is to inflate standard errors—no bias is imparted.
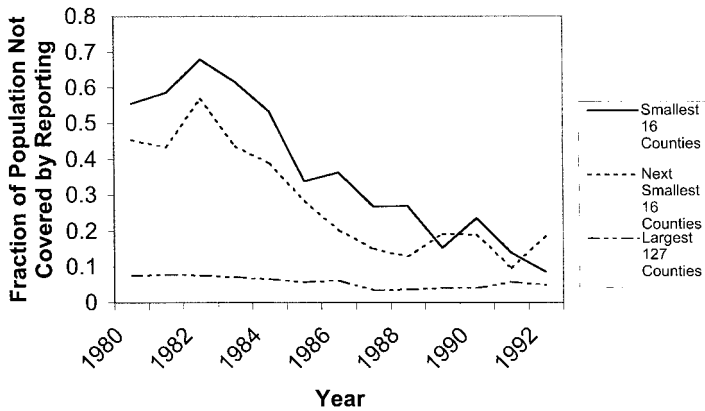
Fig. 1. Under-reporting by small counties.

Maltz and Targonski's Fig. 6 appears to be so dramatic. To understand the problem with not weighting by population, begin with the Georgia example. From above, the smallest counties in Georgia averaged 37% under-reporting. Over those same 13 years, the 10 Georgia counties with populations over 100,000 in 1992 (which constituted 46.7% of Georgia's population) averaged 2.0% under-reporting. If the average Georgia under-reporting rate were computed as a simple average across all 159 counties, those small counties with high under-reporting rates are given equal weight as Fulton, DeKalb, Cobb, and the other 100,000 + counties with virtually no under-reporting. The correct way to construct an average Georgia under-reporting rate is to weight each county by their population. Figure 2 illustrates the Georgia average under-reporting rate computed with and without population weights.

Returning to Fig. 6 in Malatz and Targonski, they show a large number of observations with at least 30% under-reporting. What their figure does not indicate is how large (important in a weighted regression) these observations are. Figure 3 illustrates the fraction of the state's population contained in those observations (county/years) with at least 30% under-reporting.[9] The fraction of county/years with 30% or greater under-reporting is illustrated with solid bars while the fraction of the total possible population in these counties is illustrated with empty bars. While our analysis reveals six states with over 40% of their county/years under-reporting at 30% or greater and four states with between 30 and 40% of their county/years at that level, only in Mississippi do these under-reporting counties include more than 30% of the total state population. Over all with

[9]We had some trouble recreating the Maltz and Targonski Fig. 6. The figure included here uses all available data (county/years) from 1980 to 1992 for the 48 contiguous states.
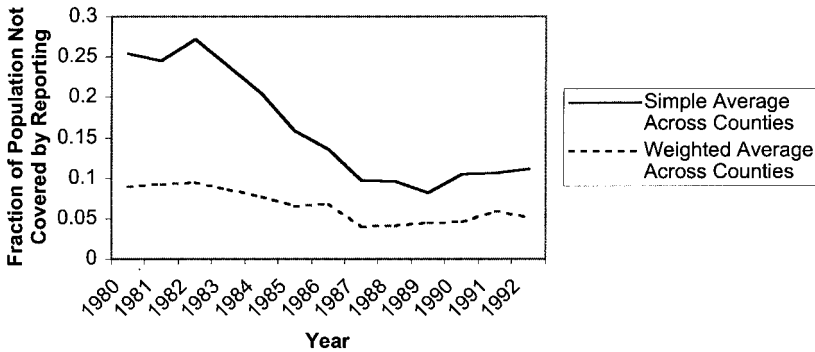
Fig. 2. Georgia state-wide average under-reporting.

48 states included, only 6.8% of the total possible population came from counties with 30% under-reporting or greater.

A final point should be made. Just because a portion of a county's population is going unrepresented in calculating the county's crime rate is not the same thing as implying that an error is occurring. A county's rate will be the same whether 70 or 100% of the jurisdictions in the county are reporting if the missing jurisdictions are similar to those that are reporting. In the more rural counties where crime is relatively unlikely it is quite likely that in most years the murder rate will be zero whether 70 or 100% of reporting agencies in a county are reporting.

3. SYSTEMATIC BIASES

Take a simple example of measurement error. Academics use survey data all the time. Yet, few would probably be surprised to find that 5% of those being surveyed were not paying close attention to the questions that they answered.[10] Even if 10% of those surveyed randomly answered 50% of the questions that they were given, being told that 5% of all the questions were answered randomly would not seem like a particularly high number, but that is the order of magnitude of error that is implied in the county level data shown by Maltz and Targonski.

Even in Maltz and Targonski's careful paper there are errors even in their evaluation of the data. For example, their Fig. 6 is a mishmash of incorrect labels showing when different right-to-carry concealed handgun laws have been adopted. They list states such as Tennessee, Kentucky, Louisiana, Arizona, Nevada, Texas, South Carolina, North Carolina,

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[10]Most academics would probably be shocked if the percentage of students in their classes who did not pay attention was as low as 10 or 20%.
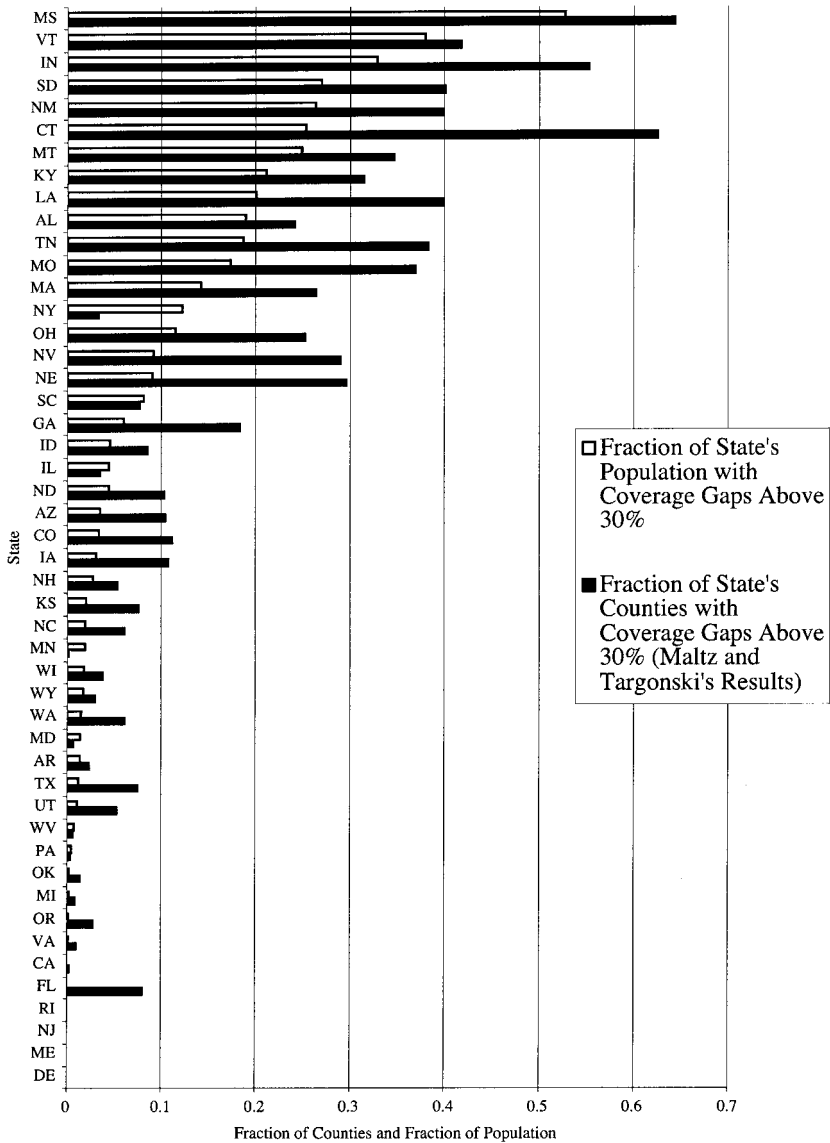
Fig. 3. Coverage gaps of 30% or greater.

Oklahoma, Arkansas, and Wyoming as having their laws before 1977, when
in fact all their laws were adopted after 1992.

What is more important than the existence of measurement error is
whether it is systematically biased. For example, Maltz and Targonski note that

there are fewer police departments that are failing to report their crime data over time. If the rate of increased reporting over time is greater for the non-right-to-carry states and if those newly reporting police departments had a higher crime rate than already reporting departments, the bias would work to exaggerate the benefits of right-to-carry laws. Alternatively, if increased reporting over time is greater for right-to-carry states and those newly reporting police departments had a higher crime rate than already reporting departments, an reported bene-fits of reduced crime from right-to-carry laws would be an underestimate.

According to the Maltz and Targonski reporting data, however, under-reporting was getting worse from 1980 to 1992. More importantly, it was getting worse at a faster rate for the "restrictive" states than it was for the states changing from being "restrictive" to "permissive" between 1977 and 1992.[11] Figure 4 illustrates the under-reporting rates for the three categories of states (restrictive, change, and permissive) from 1980 to 1992. Over the thir-teen years covered, the average under-reporting rate for the states that didn't change their law was 7.0% while the under-reporting of the change states was 5.1%. More importantly, Maltz and Targonski did not notice how the under-reporting rate for the states with permissive laws over the entire period increased from 13% in 1980 to 27.5% in 1992, the rate for states with restric-tions over the entire period rose from 3.8 to 8.5%, while under-reporting in the change states only rose from 4.0 to 5.9%. If Maltz and Targonski are correct that under-reporting biases down measured county crime rates, the much faster rise in under-reporting rates for states that do not change their laws will lower the measured crime rate in states that are not changing their laws relatively to those that adopt right-to-carry laws—biasing the results *against* the hypothesis that concealed carry laws reduce violent crime.

Fortunately, the measurement error regarding crime rates is on the left-hand side of any of the regressions examining crime rates, where it is not normally viewed as that much of a concern and does not require more sophisticated techniques to bound the maximum likelihood estimates. Maltz and Targonski's Fig. 6 however provides an interesting way of testing how sensitive earlier results were to errors in the variables. Figures 5(a) through 5(d) breaks down the original 1977 to 1992 data by whether the county level data in particular states have different levels of error.[12] The estimates repeat

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[11]This discussion is related to Fig. 7 in Maltz and Targonski. Unfortunately, as noted earlier, they have eleven states misclassified in their analysis rendering their Fig. 7 irrelevant. A correct version of Maltz and Targonski Fig. 7 is available from the authors.

[12]Recent empirical work by Plassmann and Tideman (2001) indicates that weighted least squares greatly biases downward the estimated impact of right-to-carry laws on murder and rape rates. They argue quite convincingly that the proper way to estimate these regressions is to treat the crime data as count data and to use a Poisson regression. In the case of murder, they estimate that the drop is twice as large as that found for weighted least squares. See Lott (2001) for a graphical discussion of the Plassmann and Tideman results.
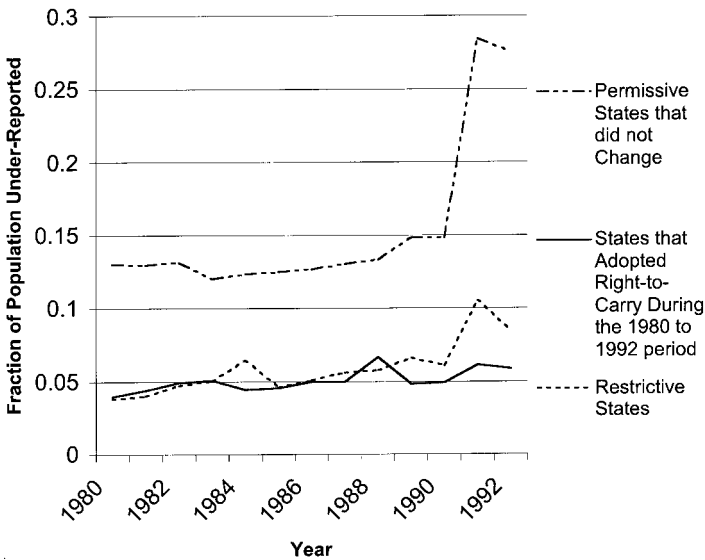
Fig. 4. Under-reporting by type of state.

the nonlinear before-and-after trends that were first reported in Lott and Mustard (1997) and then in both editions of Lott's book. We use Maltz and Targonski's Table 6 to exclude the 21 states that they list with at least 10% of their county observations missing at least 30% of the county populations. Yet, even excluding all these states generally produces results that are similar to those reported previously. The one difference from previous results involves rape where crimes decline at a fairly constant rate both before-and-after the adoption of the right-to-carry laws and the law seems to have produced no real impact on crime.

Finally, Table I analyzes the effect of excluding the states with the greatest measurement error using the more simplistic and sometimes misleading before-and-after averages and before-and-after trends. Section A re-estimates the regressions deleting the 16 states where at least 20% of their county observations missing at least 30% of the county populations. Sections B and C then repeat this by excluding the 21 states with at least 10% of their county observations missing at least 30% of the county populations and the 30 states with at least 5% of their county observations missing at least 30% of the county populations.

The results in Table I imply consistently larger drops in murder and robbery rates than using the full sample. With the before-and-after averages, dropping out those states whose county crime rates are measured with the most error implies larger drops in murder, rape, and robbery rates and
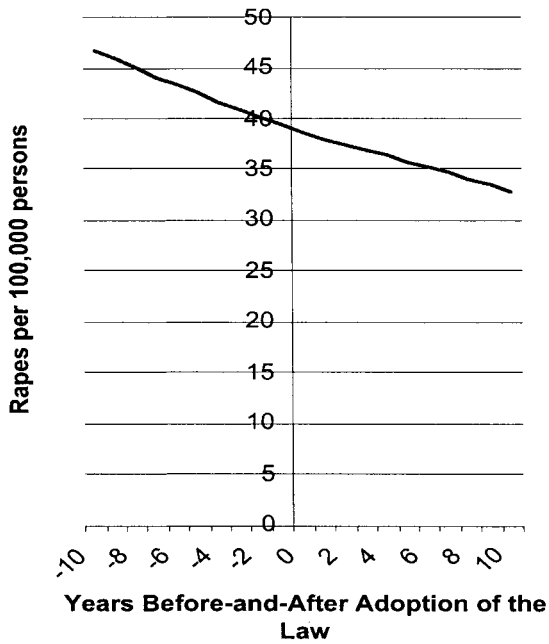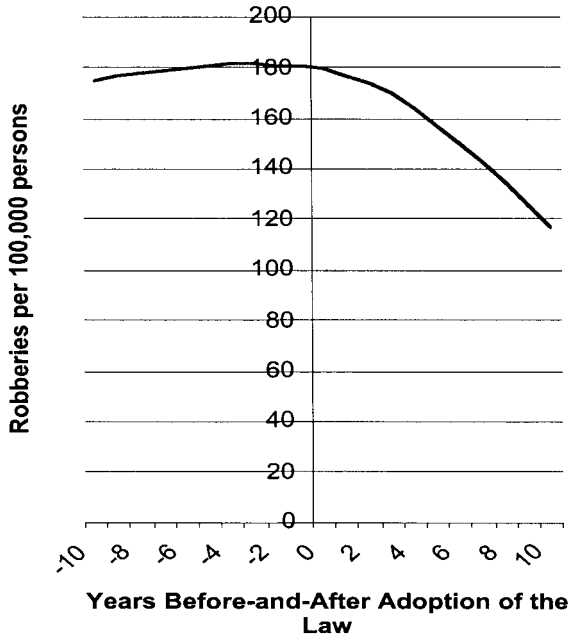
(a)

Years Before-and-After Adoption of the Law

(b)

Years Before-and-After Adoption of the Law
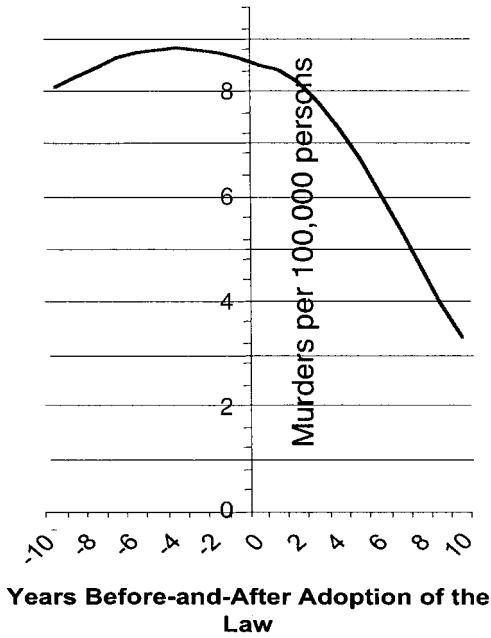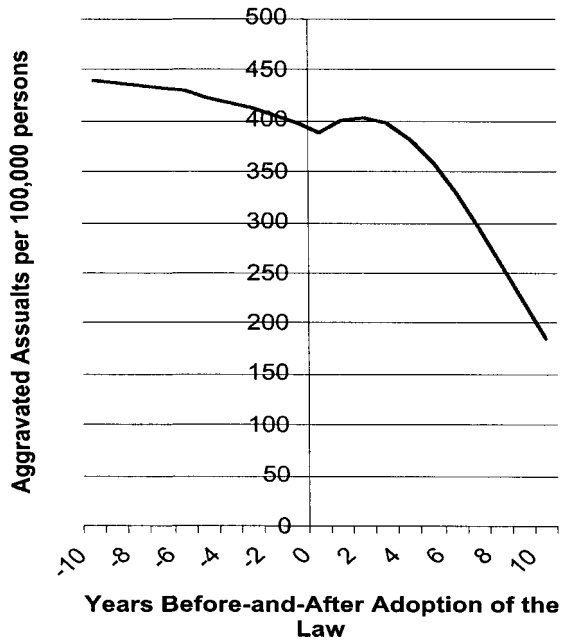
Fig. 5. Please supply caption.

(c)

(d)

Fig. 5. Continued.

Table I. The Impact of Errors in County-Level Data Using Weighted Least Squares (Robust *t*-statistics Reported in Parentheses for Dummy Variables and *F*-statistics in Parentheses for the Difference in Before and After Trends)

	Violent crime	Murder	Rape	Robbery	Aggravated assault
(A) Eliminating States when at least 20% of their counties have at least 30% of their county populations unrepresented in calculating the crime rates (13 states removed from data including the right-to-carry states of Mississippi and Montana)					
(1) Regression estimates examining simple dummy variable for measuring impact of right-to-carry law	−0.033 (2.897)***	−0.093 (3.411)***	−0.088 (5.85)***	−0.055 2.72)***	−0.026 (1.446)
(2) Change in before and after trends	−0.013 (6.56)**	−0.048 (18.63)***	−0.009 (1.86)	−0.035 (19.6)***	−0.005 (0.49)
(B) Eliminating right-to-carry States when at least 10% of the counties have at least 30% of their county populations unrepresented in calculating the crime rates (19 states removed from data including the right-to-carry states of Georgia, Mississippi, and Montana)					
(3) Regression estimates examining simple dummy variable for measuring impact of right-to-carry law	−0.076 (6.296)***	−0.113 (3.41)***	−0.093 (5.79)***	−0.0855 (3.42)***	−0.074 (3.522)***
(4) Change in before and after trends	−0.01 (0.88)	−0.057 (22.9)***	−0.002 (0.09)	−0.03 (13.96)***	−0.0011 (0.02)
(C) Eliminating right-to-carry States when at least 5% of the counties have at least 30% of their county populations unrepresented in calculating the crime rates (26 states removed from data including the right-to-carry states of Georgia, Idaho, Mississippi, and Montana)					
(5) Regression estimates examining simple dummy variable for measuring impact of right-to-carry law	−0.060 (4.538)***	−0.1356 (3.64)***	−0.089 (5.14)***	−0.076 (2.60)***	−0.059 (2.39)**

Table I. Continued.

	Violent crime	Murder	Rape	Robbery	Aggravated assault
(6) Change in before and after trends	−0.001 (0.03)	−0.064 (25.8)***	−0.005 (0.41)	−0.035 (14.6)***	−0.005 (0.43)

The regressions account for year and state fixed effects; county population; per capita income; per capita welfare payment; per capita unemployment insurance; average income support payments to those over 65 years of age; the thirty-six different demographic categories by age, sex, and race; different gun control laws (safe storage, right-to-carry, one-gun-a-month rules, waiting period, penalties for using guns in the commission of crimes); and the state unemployment and poverty rates.

either comparable or smaller drops in aggravated assaults. Compared with the full sample, the impact of right-to-carry laws on reducing murders is 26 to 85% larger, on rapes 66 to 75% larger, and on robbery 150 to 290% larger, but the impact either remains the same or falls by 66% for aggravated assaults.[13] With respect to the before-and-after trends, only the impacts on murder and robbery are statistically significant and extremely large, implying up to an additional 6.4% drop in murder rates for each additional year that the law is in effect. Reducing the sample size further with stricter and stricter criteria for measurement error actually produces larger and larger reductions in murder.[14] The results for rape should provide a cautionary example of how misleading before-and-after averages can be. While the before-and-after averages show a large drop, once one examines the results shown earlier in Fig. 5(b) it is clear that the decline was occurring for this set of states long before the adoption of the law.

4. CONCLUSION

Maltz and Targonski have provided an important service by disaggregating the county level data to help researchers examine measurement errors in the county level data, but their conclusion "that county-level crime data, as they are currently constituted, should not be used, especially in policy studies" is not justified. All data has measurement error, presumably

[13]The results for rape are no longer always statistically significant compared to past work because these estimates report robust standard errors.

[14]These results also show why using simple before-and-after averages can be problematic. The before-and-after averages for rape show a large significance in rapes, but Fig. 5(c) makes it obvious that this is because rapes are falling continuously over the entire period and not because there was any change that occurred for the set of states examined here when the laws went into effect (see also Lott *et al.*, 2003).

even their measures of this error. Unfortunately, however, Maltz and Targonski provide no systematic test for how bad the data are. Their graphs obscure both the small number of counties affected, that these are rural counties, and that just because some of the population in a county is not represented in calculating the crime rate, that is not the same thing as showing that the reported number is in error. Nor do they provide evidence for the more important issue of whether there is a systematic bias in the data. The evidence provided here indicates right-to-carry laws continue to produce substantial reductions in violent crime rates when states with the greatest measurement error are excluded. In fact, restricting the sample results in somewhat larger reductions in murders and robberies, but smaller reductions in aggravated assaults.

There are trade-offs with all different types of crime data. State level data has some of the same measurement problems found with county data and in addition has severe aggregation problems (Lott, 2000). City level data may avoid the measurement problems discussed by Maltz and Targonski, but it doesn't cover large areas of the country. County level data shares the differing problems to differing degrees. There are measurement error issues, but county data do not face the aggregation problems of state data and do not miss the large portions of the country missed by city level data. Previous research on guns and crime by Lott has used all these different types of UCR data and more (such as the Supplement Homicide Report and data on multiple victim public shootings collected from Nexis searches) precisely to test whether the results were sensitive to the type of data used. The consistent results indicated that there was not a systematic problem with the county data.

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