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## WHICH HOMICIDES DECREASED? WHY?

MICHAEL D. MALTZ\*

In a sense, criminologists are luckier than economists. Economists are asked to forecast what *will* happen to the economy, and a lot of them get it wrong. Criminologists are asked to *back-cast*, to explain what happened after it happens, and probably with about the same success as economists have at forecasting: even though we know what the outcome is, we often don't know why. But this is not for lack of trying.

After reading the papers and hearing the presentations, I am struck by the care with which the authors have analyzed the homicide data at three levels: national, multi-jurisdictional, and single-city. My comments should in no way be seen as criticisms of their efforts; rather, they are suggestions as to additional steps that might be taken. My comments focus on two areas: disaggregation of homicide data, and considering whether "regression to the mean" might explain the recent decline in New York City homicides.

### I. DISAGGREGATION.

As Frank Zimring pointed out in his remarks, these three papers go from the general to the specific. The first<sup>1</sup> discusses time trends for the whole country, the second<sup>2</sup> focuses on eight cities, and the third<sup>3</sup> is limited to one city. But I think that even

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<sup>1</sup> Alfred Blumstein & Richard Rosenfeld, *Explaining Recent Trends in U.S. Homicide Rates*, 88 J. CRIM. L. & CRIMINOLOGY 1175 (1998).

<sup>2</sup> PAMELA K. LATTIMORE ET AL., U.S. DEP'T JUSTICE, REPORT NCJ 167262, HOMICIDE IN EIGHT U.S. CITIES: TRENDS, CONTEXT, AND POLICY IMPLICATIONS (1997).

<sup>3</sup> Jeffrey Fagan et al, *Declining Homicide in New York City: A Tale of Two Trends*, 88 J. CRIM. L. & CRIMINOLOGY 1277 (1998).

more disaggregation is necessary in trying to understand homicide trends.

Homicide is a crime in and of itself; but in another sense it is the fatal outcome of a many different crimes.<sup>4</sup> *Child abuse* is a crime, and when it turns fatal it becomes infanticide. *Domestic abuse* is a crime, and when it turns fatal it is homicide among intimates.<sup>5</sup> *Armed robbery* is a crime, and when it turns fatal it becomes a felony homicide.<sup>6</sup> I would like to see studies that look at trends in child abuse, domestic violence, and armed robbery—as well as other offenses—and see the extent to which they are related to the trends in homicide: for example, since most infanticides are not committed with guns, perhaps a long-term decrease in child abuse is the cause for the long-term decrease in non-gun homicides that Fagan, Zimring, and Kim noted.<sup>7</sup> To understand a phenomenon, it is usually necessary to go to the next level, to understand the context in which the phenomenon is found. Looking only at homicide statistics begs the question of the many different motivations that may be driving the statistics. Knowing the authors of these papers, I am sure that they are well aware of this, but the readers should be cautioned that additional steps need to be taken to provide a greater understanding of homicide.

## II. REGRESSION TO THE MEAN.

The three homicide studies to which I am responding<sup>8</sup> all used homicide data from the FBI's Uniform Crime Reports (UCR)<sup>9</sup> and Supplementary Homicide Reports (SHR),<sup>10</sup> or ar-

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<sup>4</sup> See generally, Richard Block & Carolyn Rebecca Block, *Homicide Syndromes and Vulnerability: Violence in Chicago Community Areas Over 25 Years*, in 1 STUDIES ON CRIME & CRIME PREVENTION 61 (1992).

<sup>5</sup> LAWRENCE A. GREENFELD ET AL, VIOLENCE BY INTIMATES, REPORT NCJ-167237, BUREAU OF JUSTICE STATISTICS, U.S. DEP'T JUSTICE (1998).

<sup>6</sup> Michael D. Maltz, *Secondary Analysis of the UCR: An Index of the Risk of Death Due to Robbery*, 4 J. CRIM. JUSTICE 153, 153-56 (1976).

<sup>7</sup> See Fagan et al., *supra* note 3. I doubt that this is the case, but it is exemplary of the type of analysis that should be done.

<sup>8</sup> See *supra* notes 2-4.

<sup>9</sup> FEDERAL BUREAU OF INVESTIGATION, CRIME IN THE UNITED STATES, 1996. The FBI publishes this annual report based on data sent by police departments to the FBI.

rest data from the UCR; that is, all used data that were obtained from the police. It turns out that victimization data—data about crime obtained from victims—can also be used to explore at least one of the reasons given for the declining homicide rate in New York City, “regression to the mean.”<sup>11</sup>

A well-known example of this phenomenon is found in “The Connecticut Crackdown on Speeding.”<sup>12</sup> In 1956, following a year in which Connecticut experienced a very high rate of traffic fatalities, then-Governor Abraham Ribicoff instituted a new policy for the State Police: zero tolerance of speeding. The next year there were 40 fewer traffic fatalities (a 12.3% decrease), as shown in Figure 1, which the governor attributed to this policy.<sup>13</sup>

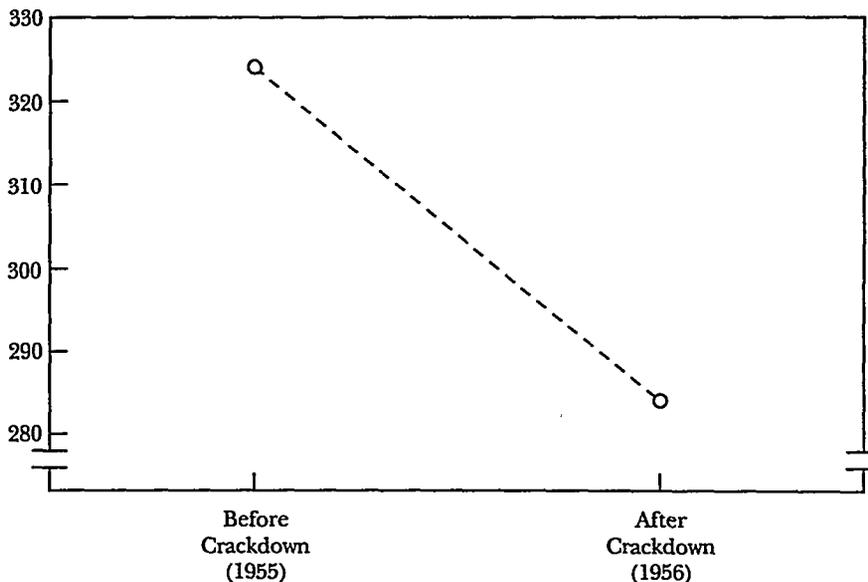


Figure 1. Connecticut Traffic Fatalities, 1955-1956

<sup>10</sup> The SHR data set can be downloaded from the National Archive of Criminal Justice Data; located at <http://www.icpsr.umich.edu/nacjd>, it is Study Number 6754. It is based on data sent by police departments to the FBI.

<sup>11</sup> See Fagan et al., *supra* note 3 at 1277.

<sup>12</sup> Donald T. Campbell & H. Laurence Ross, *The Connecticut Crackdown on Speeding: Time Series Data in Quasi-Experimental Analysis*, 3 L. & SOC'Y REV. 33 (1968). The figures from that paper have been redrawn.

<sup>13</sup> *Id.*

But it's not always that simple; a before-after comparison can often be very misleading.<sup>14</sup> When Campbell and Ross added to the picture prior and subsequent years (Figure 2), the attribution of the decrease to the policy seemed much more debatable: note that the fatalities in the first two years after the treatment were higher than five of the six years prior to the treatment.

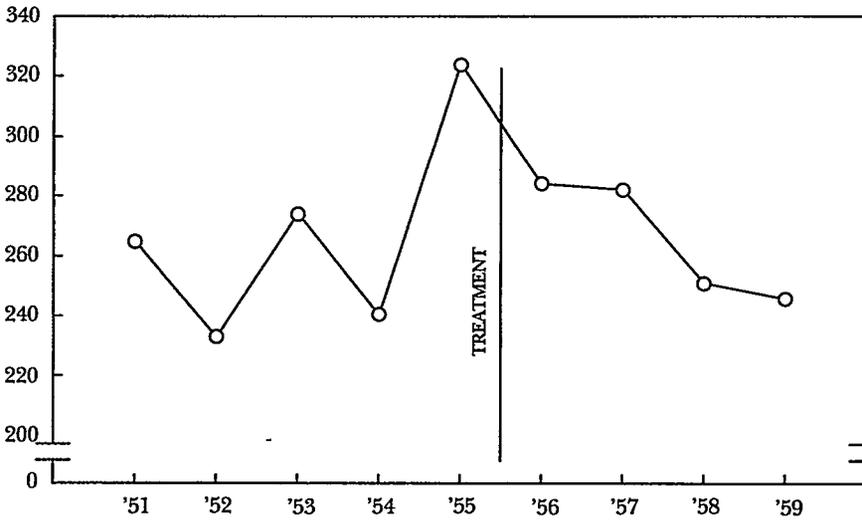


Figure 2. Connecticut Traffic Fatalities, 1951-1959

It appears that two conditions made the policy makers mistakenly conclude that the new policy had caused the reduction. First, there was a great deal of volatility, or year-to-year variation, in the traffic fatality rate, as seen in Figure 2. This means that it would be difficult to determine the extent to which any change (up or down) is due to the policy and the extent to which it is due to the inherent variation in the data.

Second, the policy was implemented in a year following one that had a rather extreme number of traffic fatalities. One

<sup>14</sup> Michael D. Maltz et al., *An Artifact in Pretest-Posttest Designs: How It Can Mistakenly Make Delinquency Programs Look Effective*, in 4 EVALUATION REV. 225, 225-40 (1980); Michael D. Maltz & Stephen M. Pollock, *Artificial Inflation of a Delinquency Rate by a Selection Artifact*, 28 OPERATIONS RES. 547, 547-59 (1980).

would expect that, *regardless* of any change in policy, the next year's rate would be more moderate (i.e., closer to the historical mean rate): it is harder to go *up* from an all-time high than it is to go down. In fact, a better name for the phenomenon might be "selection of the extreme," because it was the selection of an extremely high rate as the "before" year that foreordained a more moderate rate—a decrease—in the "after" year.

To what extent can this phenomenon explain the New York City experience? Is there evidence of volatility in the data, and is there evidence that the homicide rate in the late 80's and early 90's was in some sense extreme?

Let us first look at whether the homicide rates for New York were in some sense extreme. Evidence from a different source of data, the National Crime Survey (NCS), suggests that they were.<sup>15</sup> Since 1995 I have been a Visiting Fellow at the Bureau of Justice Statistics (BJS); one of my first projects there was to look at the relationship between aggravated assault as reported by police to the FBI, and as reported by victims on the NCS, for various states. Although the NCS is a national survey, at that time the sample size was sufficiently large that one could obtain estimates of victimization rates for the eleven largest states.<sup>16</sup> This permitted us to investigate the relationship between aggravated assault, as reported on the NCS, and homicide. While most criminologists have felt that the relationship between the two is quite strong (because many homicides, had they not been fatal, would have been classified as aggravated assaults), it could not be investigated using police-collected data because so many

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<sup>15</sup> Begun in 1973, the NCS was a longitudinal sample survey of 60,000 households in the United States; all household members aged 12 and over are interviewed about incidents in which they have been victims of a crime. That has been used to estimate the nation's victimization experience. It was redesigned in 1992 and is now known as the National Crime Victimization Survey (NCVS). The Census Bureau collects the survey data for the Bureau of Justice Statistics. Further information on this program can be obtained at the BJS website, <http://cjp.usdoj.gov/bjs>.

<sup>16</sup> The Census Bureau, which collected the NCS data, provided data disaggregated by state, but only for the eleven most populous states (as defined by the 1990 Census). Moreover, for budgetary reasons the sample size has since been reduced, so the state-specific data available even for these eleven states may now be of more limited utility.

victims of aggravated assault do not report them to the police.<sup>17</sup> Since homicide data are considered to be very accurate and reliable, we were able to compare two reliable data sets, NCS-collected aggravated assault data and police-collected homicide data, for these eleven states.<sup>18</sup> As seen in Figure 3, the relationship between the two is fairly strong for most states, but New York State's homicide rate is considerably above what one would expect from the other states' data.

My conclusion from using a different data set than that used by Fagan et al is that it appears that New York State (and therefore New York City) did have a higher than expected number of homicides during this period.<sup>19</sup> Thus we have one of the conditions necessary for regression to the mean—an extreme number of homicides, considerably greater than “normal.”

However, another condition is necessary before one can attribute the finding to a statistical anomaly like regression to the mean—volatility in the data so that the year-to-year variation is of the same order of magnitude as the presumed decrease. From the data that Fagan, et al present, this is not the case.<sup>20</sup> Homicide rates are consistently high for a number of years, and the year-to-year variation does not exhibit the volatility that would make it a candidate for regression to the mean.

What I suggest is a friendly amendment to their paper, that they replace the term “regression to the mean” with “compensatory feedback” as a possible explanation of the increase and decrease, in some ways equivalent to the homeostasis that Blumstein and his colleagues noted in corrections data before

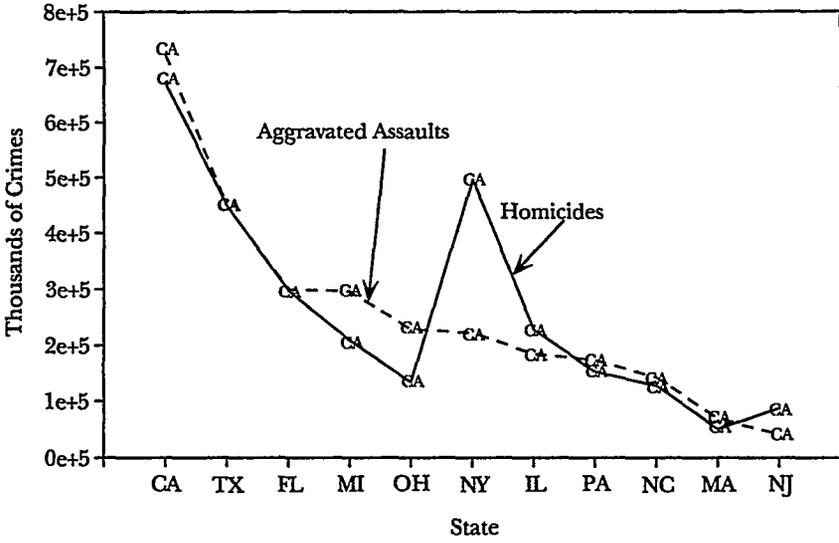
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<sup>17</sup> According to the NCVS, 45% of aggravated assault victimizations are not reported to the police. CHERYL RINGEL, BUREAU OF JUSTICE STATISTICS, REPORT NO. NCJ-165812, CRIMINAL VICTIMIZATION 1996, at 8 (1997).

<sup>18</sup> We chose to look at the years 1988-1990, due primarily to the availability of NCS state-level data for those years. More recent data were not available at BJS at the time of the study.

<sup>19</sup> An analysis of the SHR data, *supra* note 10, shows that from 1988-90 New York City accounted for 84.8% of the 6202 homicides that occurred in New York State, so an extreme number in New York State is attributable for the most part to New York City.

<sup>20</sup> See Fagan et al., *supra* note 3.



Note: Homicides are multiplied by 70 to provide a meaningful comparison

Figure 3: A Comparison of NCS-Collected Aggravated Assault Data with Homicide Data, for the Eleven Largest States, for the period 1988-90

the 1980s.<sup>21</sup> By “compensatory feedback” I mean that when extreme homicide rates began to accrue year after year, various steps were taken, perhaps by the police, perhaps by others, to mitigate this problem.

Some observers may feel that the steps produced a “sea change” in the practices of the New York City Police Department, while others may feel that the decline was bound to occur regardless of the new police tactics, because, as Fagan et al note, similar reductions were experienced in other cities and at other times. On whether it was due to the police or an inevitable downswing, the jury is still out.

In a subsequent conversation with Jeffrey Fagan, he suggested that the rise and fall of homicides in New York City might have been attributable to an epidemic that grew and was sustained due to the combined effect of the ready availability of drugs and guns, and that there might have been some self-limiting aspects to the epidemic: a finite number of “susceptibles” that limited growth, and various factors that inhibited its “intergenerational” transmission.<sup>22</sup> This explanation, too, would fit under the rubric of “compensatory feedback,” without having to invoke a statistical “regression to the mean” to explain the phenomenon.

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<sup>21</sup> Alfred Blumstein & Jacqueline Cohen, *A Theory of the Stability of Punishment*, 64 J. CRIM. L. & CRIMINOLOGY 198 (1973); Alfred Blumstein et al., *The Dynamics of a Homeostatic Punishment Process*, 67 J. CRIM. L. & CRIMINOLOGY 317 (1976); Alfred Blumstein et al, *On Testing the Stability of Punishment Process: A Reply*, 72 J. CRIM. L. & CRIMINOLOGY 1799 (1981).

<sup>22</sup> Email conversation with Jeffrey Fagan (April 13-14, 1998).