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Executions, Deterrence and Homicide: A Tale of Two Cities

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EXECUTIONS, DETERRENCE AND HOMICIDE:
A TALE OF TWO CITIES

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EXECUTIONS, DETERRENCE AND HOMICIDE: A TALE OF TWO CITIES

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Franklin E. Zimring, Jeffrey Fagan, David T. Johnson

ABSTRACT

We compare homicide rates in two quite similar cities with vastly different execution risks. Singapore had an execution rate close to 1 per million per year until an explosive twentyfold increase in 1994-95 and 96 to a level that we show was probably the highest in the world. Then over the next 11 years, Singapore executions dropped by about 95%. Hong Kong, by contrast, has no executions all during the last generation and abolished capital punishment in 1993. Homicide levels and trends are remarkably similar in these two cities over the 35 years after 1973, with neither the surge in Singapore executions nor the more recent steep drop producing any differential impact. By comparing two closely matched places with huge contrasts in actual execution but no differences in homicide trends, we have generated a unique test of the exuberant claims of deterrence that have been produced over the past decade in the U.S.

EXECUTIONS, DETERRENCE AND HOMICIDE: A TALE OF TWO CITIES

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EXECUTIONS, DETERRENCE AND HOMICIDE: A TALE OF TWO CITIES

Franklin E. Zimring[*]
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INTRODUCTION

A hearty perennial in debates about the death penalty concerns whether or not the threat of execution deters homicide better than the next most severe criminal sanction. The salience of this debate waxes and wanes over time, but in recent years a dispute has emerged in the United States around claims by economists studying American data that each execution prevents 3 to 18 or even 74 murders (Liptak 2007; Tanner 2007; Adler and Summers 2007). Reacting to these claims, two prominent Harvard professors have concluded that since executions do seem to deter, the death penalty is not only reasonable, it is "morally required" (Sunstein and Vermuele 2005). Many scholars have contested this assertion and the studies on which it depends, for a wide variety of reasons (Donohue and Wolfers 2005; Steiker 2005; Fagan 2006; Fagan, Zimring, and Geller 2006; Zimring 2008; Cohen-Cole et al., in press; Hmarlsson, in press).

The central problem with U.S.-based claims that the death penalty deters is that the available evidence is weak, largely because there are so few executions in the country – the United States – that has been the focus of the vast majority of deterrence studies. As two analysts recently put it, "It seems unlikely that any study based only on recent U.S. data can find a reliable link between homicide and execution rates" (Donohue and

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Wolfers 2005; see also, Sunstein and Wolfers 2008). The ambiguity in the American experience is not being able to tell whether executions do not work as a medicine for preventing homicide, or whether the dosage of this lethal medicine in the United States is simply too low to detect any effect (Cohen-Cole et al. 2009).

This article considers evidence from a country that may well be the best case possible for pro-deterrence arguments – Singapore, which has often been called the "world execution capital" (Johnson and Zimring, 2009:408). In the mid-1990s, Singapore executed at a rate that was almost double the execution rate during China's notorious "Strike Hard" campaign (Johnson and Zimring, 2009:264). At its peak, we will show that the execution risk for murder was 24 times higher in Singapore than in Texas. The comparison of Singapore to Houston (the largest city in Texas and the locale that most often obtains capital convictions) further reveals the Asian city-state's zeal for capital punishment. Houston is the execution capital of the most aggressive executing state in the most aggressive executing democracy in the world, and in 2000, its homicide rate was about nine times higher than the homicide rate in Singapore. Nonetheless, from the time the U.S. Supreme Court reinstated capital punishment in 1976 until 2004, Harris County – the Houston metropolitan area, with a present population of almost 4 million – accounted for 73 executions (Johnson 2006a:104). Singapore, by comparison, with a population only a little larger than Houston, had 76 executions in 1994 and 73 more in 1995. And for the most recent three decades, Singapore's peak execution rates are 20 to 25 times higher than Houston's annual rates. In 1994, Singapore executed as many people (76) as did the nation of Japan (with a population 30 times larger) in the 30 years between 1978 and 2007.

So, Singapore is a best case for deterrence because its death penalty policy fits the theory of deterrence well. That theory holds that three main features of punishment predict its deterrent power. The first is severity. In Singapore, as in other retentionist jurisdictions, the death penalty is the most severe sanction. But compared to most other retentionist nations, Singapore stands out with respect to two other dimensions of the deterrence model: certainty of punishment, and celerity (speed of administration). In

Singapore, a death sentence is mandatory for murder (and for many other offenses, including possession of drugs with intent to traffic; see Johnson and Zimring 2009:411). Thus, persons convicted of murder will be sentenced to death. In the United States and Japan, by contrast, only one to two percent of all known murder offenders are sentenced to death, despite broad eligibility (Johnson 2006a:105). As for speed of administration, homicide trials in Singapore seldom take more than a few months, and death sentence appeals are typically disposed of within 18 months of conviction (New Straits Times, 25 February 2009). In the United States and Japan, by contrast, the appeals process routinely takes 10 years or more, and waits on death row often take another 10 to 20 years, even after appeals have been exhausted (Liebman, Fagan and West, 2000; King and Sherry, 2008; Zimring 2003:78; Johnson 2006a:70).

The final advantage of Singapore as a setting for deterrence research is the existence of a similar city – the Special Administrative Region of Hong Kong – with a strikingly different death penalty policy. These two jurisdictions are similar in scale, geography, demographics, culture, historical and economic development, and recent growth in population. **Table 1** compares Hong Kong and Singapore across ten dimensions. The first two columns in the table show two dimensions of significant difference: Hong Kong is a larger city, and it is 95 percent Chinese compared to 75 percent in Singapore. The rest of the columns reveal that these two cities are quite similar in economy, demography, literacy, and population growth and density.

Table 1 Here

Singapore established self-government as part of the British empire in 1959, declared independence from Britain (which had governed the island since 1858) and merged with Malaya, Sabah, and Sarawak to form the nation of Malaysia in 1963, and became an independent nation-state in 1965 when it was expelled from Malaysia following a bloody period of terrorism and rioting.

Hong Kong was under British rule from 1842 until 1997, when it returned to the Chinese mainland under the PRC's "one country, two systems" policy. As a "Special

Administrative Region" of China, Hong Kong has been allowed to retain its capitalist system and some degree of political autonomy until 2047. Under this scheme, the central government of the PRC is responsible for the territory's defense and foreign affairs while Hong Kong maintains its own legal system, police force, customs policy, monetary system, and delegates to international organizations and events (Johnson and Zimring 2009:365, 407).

But they couldn't be more different in the practice of capital punishment. In the last third of the twentieth century, these two cities adopted policies toward the threat and use of capital punishment that were in the mid-1990s as divergent as any two political entities on earth. Singapore went from no more than four or five executions a year in the late 1980s to the world's highest execution rate in 1994 and 1995 before reducing executions by more than 90 percent from 1996 to 2007. Thus, Singapore conducted two of the most dramatic natural experiments in execution on record: a huge increase, followed by an extraordinary decline. Its closely similar sister city of Hong Kong was execution-free throughout this period. Did these huge differences and extreme Singapore swings make a visible impact on homicide rates in these two cities? As we show in the sections that follow, evidently not.

This article traces trends in officially noted rates of intentional homicide in Hong Kong since 1967 and in Singapore since 1973 and then compares the pattern in these two places. The first section of this article traces homicide rates in Hong Kong through the change in death penalty policy and practice over the past half-century. Part II tells the contrasting story of capital punishment in Singapore and provides a profile of homicide rates in that city-state. Part III compares homicide in zero-execution Hong Kong with homicide in Singapore before and after the spike in executions and the return to near zero in recent years. Part IV presents a statistical analysis of the two cities that assesses the impact of variations in execution in Singapore on homicide rates. Part V discusses the limits and implications of this study as one chapter in the evolving knowledge of execution threats on the behavior of potential offenders.

I. CAPITAL PUNISHMENT AND HOMICIDE IN HONG KONG

The conventional method that sociologists use to test the impact of changes in death penalty policy is to compare rates of homicide before and after a formal legal change – usually the abolition of capital punishment (Sellin 1959; Zeisel, 1976). **Figure 1** uses Hong Kong police data on homicides to produce a rate per 100,000 persons for intentional homicide for the period 1967 to 2007. The figure punctuates this time line with two landmarks of death penalty policy in Hong Kong: the last executions in the Crown colony (in 1966), and the formal abolition of the death penalty (in 1993). Homicide data for years prior to 1967 were not available.

Figure 1 Here

The rate of homicide per one-hundred thousand in Hong Kong cycles to some extent in the first two and a half decades after 1967, then drops steadily through the 1990s and the first eight years of the twenty-first century. In 2007, Hong Kong records its lowest homicide total of the period – eighteen deaths, or a rate per one-hundred thousand of just 0.26, which is only half as high as the lowest rate (0.5, in Israel, in 1997) in a recent cross-national comparison of homicide rates in 37 nations (Johnson 2006b:77).

Neither of the two death penalty landmarks in recent history have any visible trend-changing impact on Hong Kong homicides. There is a modest increase in homicide five years after the last execution, but that is followed by a drop to previous low rates in 1977. The 1993 abolition is the usual landmark for deterrence analysis over time (Sellin 1959; Archer and Gartner 1984), and there is no apparent shift in Hong Kong in the immediate wake of the death penalty's demise. Over the longer run, homicide rates decrease after abolition, but most of the decline happens much later on, and there is little reason to suppose that the shift from a death penalty regime without executions to no death penalty threat at all would generate a major decline in homicides.

The time series on homicide in Hong Kong underscores two of the traditional weaknesses of focusing on short-term measurements in killing before and after formal

death penalty abolition. In the first place, formal abolition normally comes sometime after executions stop (Zimring and Hawkins 1986). **Figure 1** shows a full quarter-century lag between the last execution and the formal end of the death penalty. With such a long break, the immediate wake of abolition does not signal a major shift in actual execution risk to would-be killers. This is compounded by the fact that nations on their way to abolishing death as a criminal sanction usually do not have high rates of executions at the onset of cessation. There were only two executions at the beginning of the Hong Kong time series, another reason why the changes in risk generated by the 1993 abolition are not large. So the Hong Kong data are consistent with no marginal deterrent impact from the end of either executions or the threat of execution in law, but this lack of evident impact happens where the risk of execution had been low for a long time. Would this lack of impact also occur if the variation in execution risk were more substantial?

II. EXECUTION AND HOMICIDE IN SINGAPORE

The island nation of Singapore is one place where the threat of executions became in recent years anything but attenuated. With a population that did not cross four million until 2000, there is reason to believe that Singapore in the mid-1990s has experienced execution clusters that on a per capita basis make the PRC's infamous "strike hard" campaigns look like small potatoes (Johnson and Zimring, 2009:231-43). The government of Singapore does not publish statistics on executions or on its crime rates but Amnesty International has been generating estimates on Singapore executions since 1981. **Table 2** shows Amnesty's estimated executions for 1981 through 1990 and for 2006 and 2007 in Singapore and the rate per million each estimate produces for the year it was reported, while we use data from government sources from 1991 through 2005.

Table 2 Here

Because Amnesty will not include an execution that does not come to its attention, the estimates it produces can be conservatively low in many instances (see Johnson and Zimring 2009:231). This makes the Amnesty totals quite reliable as trustworthy

minimum estimates, but can in some environments produce large errors of underestimation, a situation that Johnson and Zimring (2009:231) document for the PRC in recent writing. The data in Table 2 were all reported by Amnesty International, but the numbers for 1981 to 1993 and for 2006 and 2007 rely exclusively on the Amnesty estimates. For 1994 to 2003, we were able to get data released by the government of Singapore in responding to Amnesty criticisms, data from local newspaper accounts published in 2006, and data provided to Professor Zimring by the Ministry of Home Affairs with no reservations. So, we have used the reports from Singapore's *The Straits Times* as the equivalent of government data. The fact that Amnesty had published its own estimates for the years in the mid-1990s prior to the government release of data provides us with a basis for also estimating the accuracy of Amnesty's estimates for total executions in Singapore for other years. For 1993 to 1997, we thus can compare the original Amnesty estimates to actual volumes, a unique window into the value and limits of Amnesty estimates. By triangulating these data sources, we have developed a reliable minimum estimate of Singapore executions since 1981.

A. Estimated versus Actual Executions

In Singapore, there are strong indications that the Amnesty estimates capture a majority of actual executions and do a good job of reflecting trends in the larger number of actual state executions. The data that inform this conclusion come from the peak years of Singapore executions – when the execution numbers jumped and one would expect maximum danger of undercount. For those four years, the estimates published by Amnesty were 134 executions, 63% of the 214 executions that were announced later. And the Amnesty batting average improved after 1994 when only 42%, "at least 32" of the 76 actual executions, were in their estimates. For 1995-1997, the original Amnesty estimates were 74% of the total executions. Further, when execution volume dropped to lower and more typical levels in 1997, the Amnesty system captured 14 of the 15 actual executions eventually reported, 93%.

Even for 1994, when the execution surge was undercounted, the Amnesty numbers were consistent with the trend – the "at least 32" Amnesty reported was four and half times their estimate for the previous year and by far the highest total for any earlier year in Singapore. At its weakest then, the Amnesty system seems a good measure of trends but with problems of undercounting particularly during sudden upward shifts in executions. In more typical settings, the Amnesty estimates tend to be lower than actual but about three-quarters of actual volume.

This limited information on execution volume suggests a rather stable execution rate at around two per million population per year in the 1980s, on the high end of reported rates for other executing states in that era, but far lower than the estimates compiled for the 1990s. It is followed by a huge increase in 1992 and 1994-1996, which is followed by historically high but reduced rates through 2002 and then drops off by 90% by 2007.

There are three striking patterns worthy of note in Table 2's history of execution estimates. The first is the substantial variance in different years in rates of executions. The gap between the highest execution rate in the mid-1990s and the lowest rate (in 2007) is greater than 35 to one, a huge variance over short time periods. Using the last officially reported rate in Singapore (six in 2005), the drop in ten years is 92%.

Second, there are clear temporal patterns to the data – estimated execution rates are low until the early 1990s, escalate sharply to a peak in 1994 through 1996, then trend downward for most of the next decade. The downward trend in volume in recent years is very significant – the four years from 1994 through 1997 generate an average estimated execution rate each year of 12.3 per million while the years 2004 through 2007 have an average annual rate of 1.9 per million; a drop of eighty-four percent from the peak rates a decade before.

Because all execution estimates after 2005 are based on Amnesty information rather than official statistics, the annual totals may be less than the actual number of executions – this is one implication of our finding that Amnesty's estimates were lower than the later announced total by the government. So we have no great confidence that

the actual number of executions in Singapore was only two in 2007. The critical question, however, is how much confidence can be placed on a downward trend in executions that is anchored by that two-execution total.

We think the evidence strongly supports a downtrend of at least 90% in Singapore executions from their mid-1990s peak rates. The 2007 total of estimated executions is more than 90% below Amnesty's own estimates for the five years that begin in 1998 which share the same tendency to undercounting. There is no reason to believe that Amnesty information is getting less reliable over time; indeed, we suspect that the most recent estimates are much better than the information available for the 1980s.

So the relative drop in execution risk is 90% or more in Singapore, but how close is the real number in 2007 to the estimated total of two? If the average undercount of 37% during the 1990s applies, the estimated total for 2007 would expand to a true count of three executions, a 96% drop from 1994. If Amnesty only heard of half the actual executions in 2007, that would produce a true rate of four executions and a drop of 95%. There is a significant margin of error in this calculus, but no doubt of a very large drop in executions.

The third clear pattern is that modern Singapore's peak rates of execution in the mid-1990s are very high indeed. **Figure 2** compares estimated peak rates of execution in the PRC during the strike hard campaigns with the Amnesty estimates for Singapore and official statistics for Texas from 1995 through 2000 (the peak year of United States executions since reintroduction was 1999).

Figure 2 Here

By using the Communist Party estimate of 15,000 executions per year for the PRC, we get an annual peak rate of 11.5 per million, slightly more than half the rates for Singapore (see Johnson and Zimring, 2009). If we had used Amensty's much lower estimates for the same years in China, the Singapore rate would be more than ten times the peak Chinese rate (Johnson and Zimring 2009:236). The execution rate for Singapore

is sixteen times as great as the annual rate in Texas, the execution capital of the United States.

While Singapore does not publish any regular reports of crime, it does report intentional homicide as part of the death reporting system of the World Health Organization ("WHO"). And an analysis of homicide prosecution in 1985 produced police murder statistics for the ten years 1973-1982. Two other published reports provide police statistics for intentional homicide for 2002 through 2007. The solid line in **Figure 3** combines the police reports for 1973-1982 with WHO reports for 1983-2001 and police reported killings for 2002-2007. The dotted line shows Amnesty's execution estimates for the years after 1990.

Figure 3 Here

It should be remembered that in Singapore executions cover not only homicides but a wide variety of other serious offences; no detail is available from the government on the offence-specific execution rates. But of course, no such data are available to the citizens of Singapore either, so the gross execution rate may be the appropriate measure of perceived risk for homicide to the extent that potential homicide offenders are aware of executions. Yet even though the volume of murder executions is not announced, the economist's preferred measure of risk for a deterrence study is the actual number of executions for murder as a fraction of the rate of murder.

B. Executions in Singapore for Murder

Table 3 provides estimated execution volume for criminal homicide in Singapore. Data on executions for the ten years beginning in 1991 were obtained from Amnesty; for the three years beginning in 2003, data were obtained from newspaper reports (which, evidently, relied on government data); execution data for 2006 and 2007 again came from Amnesty.

The Amnesty estimates of murder-specific executions for 1991-1993 and 2006-7 are reliable minimum estimates but may understate murder-specific totals. The *Straits*

Times reported detailed breakdowns by crime and the nationality of the person executed for 2003-2005 that could only come from a government source. We requested official statistics or comments on the 2002 and 2006-2007 estimates from the Ministry of Home Security but were repeatedly told that no information could be provided. The secret nature of both individual executions and aggregate murder statistics must be a deliberate choice of the highly centralized and statistically meticulous Singapore government. All executions are conducted by the same agency. Public notice of the fact of an execution is quite rare (the government justifies this by saying that criminal trials are matters of public record). No periodic statistics on executions are reported.[1]

Table 3 Here

The run up in murder executions starts in 1992, peaks in 1994 and 1995 then drops back quickly (Table 3). Every year after 1996 except 1998 has less than one-quarter the murder executions of the 1994-1995 levels, and the rate drops by 90% in the semi-official Singapore Straits Times report for 2005.

The limited data on executions has no clear relationship with variations in homicide rates. There is a downward trend in homicides that starts in the mid-1980s. Over the

[1] On two occasions, the government of Singapore has released statistical data on executions. The first and largest release was in a government report replying to a 2004 study published by Amnesty International. The second set of detailed, statistics of executions by crime and year was published in *The Singapore Straits Times* in January, 2006 and could only have come from the government. We asked for annual crime-specific data on three occasions. The Ministry of Home Affairs provided us a copy of the 2006 newspaper report and the reply to Amnesty and said this was the only information available. See emails to Franklin Zimring from MHA Feedback (MHA_Feedback@mha.gov.sg); the final installment of this poignant exchange is reproduced below in its entirety:

February 23, 2009

Dear Professor Zimring,

1. I refer to your third letter to the Ministry of Home Affairs requesting for execution statistics.
2. As indicated in our last reply, we are unable to provide further information that you require.

Yours sincerely,

Annie Lim Hong Gek
for Permanent Secretary (Home Affairs)

eighteen years after 1983, the rate drops in steady fashion. The four years between 1983 and 1986 average 2.4 per hundred thousand, compared to an average of 1.25 for the four years beginning in 1990, eight-tenths in the four peak execution years 1994 through 1997, and 0.8 also for the last four years in the homicide series. The largest declines in Singapore homicides take place well before the 1994 rate explosion. Annual homicides continue to drop during the four peak execution years and stay low thereafter as execution rates fall after 1996. By 1993, when Amnesty estimated only seven Singapore executions, the homicide rate had already fallen by half to 1.15 per hundred thousand, so any marginal deterrent impact that the tenfold expansion of executions starting in the next year would have contributed would have to be found in the gap between 1.15 per hundred thousand and eight-tenths per hundred thousand. But it is also quite possible that this further decline, only twenty-five percent of the drop that had already occurred, was a continuation of the long-term drop independent of execution effects.

Figure 4 presents data from on homicides compared to the volume of executions for murder. The pattern is similar to total executions but the volume of "murder-only executions" peaks at 5 per million per year and then drops by 90% to two executions in 2005 in semi-official Singapore data. Amnesty reports a further drop to zero in murder executions for 2007. The information gathered only extends from 1991 to 2000, so the last seven years of the execution decline are not covered, but all of the peak execution years are in the pattern.

Figure 4 Here

The pattern of estimated murder-only execution peaks in the same two years as total executions but then falls off faster in 1996 than the total execution aggregate. Since the only basis for yearly estimates of murder executions is Amnesty, this might reflect some missing data. There is, again, no clear linkage between increases and drops in executions for murder and homicide rates in Singapore.

When comparing the Singapore execution rate and crime risks with other death penalty jurisdictions, it is important to consider differences in crime rates as well as in

executions rates. **Figure 2** showed an execution rate difference between Singapore and Texas of sixteen to one, but this would both overstate and understate the risk that an intentional homicide would result in execution. In 1995, when Singapore executed seventy-three times, it recorded twenty-seven homicides, a ratio of more than two executions to every one intentional homicide. In 1999, the peak year for executions in the United States, the State of Texas conducted thirty-five executions (all for murder) and reported 1,080 intentional homicides to the FBI Uniform Crime Reports - a ratio of one execution for every thirty-one intentional homicides. In 1994 and 1995, the ratio of estimated executions for murder to total homicide in Singapore were 41 executions for murder to 53 homicides, or .77. So the execution risk for murder in Singapore was 24 times higher in Singapore at its peak than in Texas at its peak, and this should be close to the best case imaginable for a death threat producing high marginal deterrent effects when compared with non-execution sanctions.

Yet the case for any executions effects on Singapore homicide over time in **Figures 3** and **4** is far from clear on the visual evidence. The explosive increase in executions comes after the bulk of Singapore's homicide decline, but the drop does continue for the first years of the high rate execution era. The decline in executions after 1996 is then associated with a continuation of homicide rates at their lowest rates six years after the two-thirds drop. There is no apparent impact on homicides when the city resurfaced from Guinness Book of World Records levels of execution. The gross execution rate drops 97% between the 1994 peak and 2007 while the homicide rate is stable to slightly down. The roller-coaster ascent and fall in executions over the 18 years after 1990 produce two of the sharpest temporal variations in execution level ever recorded and no apparent impact on homicide.

One way to resolve some of the difficulties of imagining homicide trends in Singapore without its large and rapidly fluctuating execution policy would be to find a similar city with no variations in capital punishment, which brings us back to Hong Kong - this time as a comparison city for Singapore.

III. HOMICIDE AND TIME IN SINGAPORE AND HONG KONG

Hong Kong is not a perfect match for Singapore – Table 1 showed it is a larger city and lacks some of the cultural ties to Malaysia and the presence of Malay ethnic minorities found in Singapore. By the usual standards of such comparisons, however, Hong Kong and Singapore are a very good fit: none of the known demographic, socio-economic and geographic differences between these two cities should render them non-comparable in temporal homicide trends.

In this first part of the analysis, we use the demonstrated similarity in rate and trend of Hong Kong and Singapore as a statistically established pattern, instead of attempting to create or validate any model to estimate expected murder frequencies over time in these cities. **Figure 5** reproduces the Hong Kong and Singapore homicide data in rates per one hundred thousand.

Figure 5 Here

One function of this comparison is to see whether there are uncharacteristic movements in homicide rates in Singapore during its peak execution years or thereafter. The expectation would be for decreases in Singapore in the 1994 through 1997 period that are distinct from patterns in Hong Kong due to deterrent effects of high execution rates in Singapore. For similar reasons, we would check for increases in homicide in the period around 2000 after Singapore's drop in execution rates. Neither of these potential deterrence markers is apparent in Figure 4.

What the limited data available indicates are two rather similar cities with similar low rates of intentional homicide and temporal trends downward that are close in both magnitude and timing. The huge differences in capital punishment policy between Singapore and Hong Kong don't produce clear cleavages in the mapping of their homicide experience. From 1990 to 2000, the two cities experience similar downtrends in homicide, with a slight downtrend continuing after 2000. The end point in the series in

2007 matches the historic low for each city. There is no sign of residual deterrence in Singapore in the current decade that may have sustained from its acute execution activity in the mid-1990s.

Next, we conducted a series of regressions to assess whether the huge increase in Singapore executions in the mid-1990s and their dramatic drop thereafter produced changes in homicide over time in that city that are statistically and probabilistically distinct from temporal patterns in zero-execution-risk Hong Kong. Our measure of homicide is the police and WHO data presented in Tables 1 and 3. For execution risks, we use the 27-year series of total executions rather than the 15 annual estimates available to us for executions for murder.

There are two reasons (other than the obvious advantage of more observations) to use total execution numbers as the leading deterrence variable communicated to potential killers in Singapore. The first is that frequently there has been no public identification of what crime led to executions in Singapore, in which case there is no specific homicide cue to separate murder executions from those for other crimes. Also, there seem to be no clear differences in the salience to potential killers of non-homicide and homicide executions as deterrence cues.

The second reason why annual variations in total executions are the best predictor of the risks of execution for murder is the degree to which variations in total executions mirror variations in executions for murder. For the 15 years in which we had data for both total and murder executions, the rank order correlation between a year's standing in relative rate of murder-only executions was .875, suggesting that variations in the larger category were highly-correlated with variations in the murder execution category over time. Thus, the total execution rate is a good indication over time of trends in the murder-only rate of execution, and it is available for a much longer and more continuous time span.

We did not attempt to create or test a complex theoretical model to predict the non-penal social and economic determinants of variations in homicide for Singapore, for Hong Kong, or for both. There are no available conceptual homicide models for these

places, and no obvious or even plausible homicide models from other settings to adapt to these specific and unique contexts. Moreover, once such adventures begin, the loss in transparency and the opportunities for both conceptual and statistical manipulation frequently hamstring the research (Zimring, 2008). What we have tried to do here is to use simple multivariate analytic methods to examine whether two cities with similar social and economic structures have visible differences in homicide when only one of them is increasing execution risk for murder by a large multiple and then decreasing it by 90 percent.

A. Model Specification

We estimated a series of regressions to identify the effect of executions in Singapore on the Singapore murder rate, controlling for the contemporaneous trend in Hong Kong homicide rates and a linear time trend to incorporate the broader secular pattern of decline in homicides in both cities. We use two time series, one that begins in 1973 when comparable homicide data were available, and the second beginning in 1981 when detailed execution data were systematically available in Singapore. Each time series is scaled to zero at its initial year. Effects of Singapore executions were estimated both as the number of executions (lagged one year) and the rates of executions per 1,000 homicides (lagged one year).

First, a series of regressions were completed using Prais-Winsten estimators to correct for autoregression in the homicide trends over time (Cochran and Orcutt, 1949; Prais and Winsten, 1954; Green, 2006). The Prais-Winsten transformation estimates time series regressions in the presence of autocorrelated standard errors in Y_i and corrects for the distorting effects of serial correlation (Wooldridge, 2002).[2] **Figure 5** shows that in both Singapore and Hong Kong, homicide rates over time are classically correlated from one year to the next. Under these conditions, the best predictor of what the murder rate will be in the next year is what it was last year. This is an empirical constraint in

[2] In general, to produce accurate standard errors when there is serial correlation or autoregression in the outcome variables, the Prais-Winsten regression estimates standard errors that are robust to autocorrelation as well as heteroskedasticity. *See* Wooldridge, 2002.

identifying the relationship between execution and murder. Failure to correct for this temporal dependence will bias the standard errors in estimates of execution effects, and this distortion remains even when fixed effects are used to control for temporal trends (Duflo, Bertrand and Mullainathan, 2004; Baltagi, 2001; Baltagi and Li, 1995; Jung and Tremayne, 2003). Statistically and conceptually, it is unlikely that effects of extremely rare events, such as executions, can impact trends that are so heavily influenced by their own history.

The Prais-Winsten specification corrects for this potential distortion by including a first order autoregression term, AR(1), in the estimations. Under this assumption, the linear regression model is

$$y_t = x'B + u_t$$

where the errors u_t satisfy the conditions of a first-order autoregressive process

$$u_t = \rho u_{t-1} + e_t$$

and they are independent and identically distributed as $N(0, \sigma^2)$. This error term is used to generate the Prais-Winsten coefficients as a generalized least squares estimator (Judge, 1985). Regressions were estimated first to examine the effects of executions in Singapore (lagged by one year) on Singapore homicide rates, controlling for the contemporaneous trend in Hong Kong. In this design, Hong Kong functions as a comparison city that was not exposed to the 'treatment' effect of execution. Both the number and rate of executions were tested, and each tests address different dimensions of deterrence. The volume of executions serves as a proxy for the announcement effects of execution that can provide information efficiently to networks of socially connected potential offenders through saturation of information markets (Sunstein, 2005). The rate of execution is a measure of the contingency or risk of execution, part of the rationality calculus central to deterrence (Posner, 1973, 2000; Becker, 2006; Polinsky and Shavell, 2006; Bar-Gill and Harel, 2001; Robinson and Darley, 2004; but see Tversky and Kahneman, 1986, and Anderson, 2002).

We then examined regressions controlling for specific time periods when executions in Singapore spiked to more precisely identify the effects of executions on homicides in Singapore. For each model, we estimated a series of alternate specifications, including a model with fixed effects for time, a quadratic time function that allows for non-linearity in the distribution of homicides, log-log estimates where homicide and execution rates were log transformed to cabin outliers and better approximate linearity in the distributions, and Poisson distributions that use a functional form based on counts of homicide events and executions instead of rates. Looking ahead for the moment, all results in Tables 4-6 were robust to these specifications.

Next, we estimated a series of difference-in-difference models, or DD models, as an alternate method to identify potential effects of executions on homicides (Abadie, 2005; Newey and West, 1987; Stock and Watson, 2003). DD models are commonly used to organize data to mimic experimental designs under conditions when randomization is unavailable. Following Imbens and Wooldrige (2009), we use the time series for the difference in Singapore and Hong Kong homicide rates to estimate whether any differences in its homicide rates from one year to the next were sensitive to simultaneous changes in both homicides and executions in Singapore. First, we estimate an ordinary least squares (OLS) regression to determine whether the presence of the death penalty in Singapore influences its homicide rate. Assume

$$Y_{it} = \beta_0 + \beta_{1*}t_{+} \beta_2 S_{it} + \beta_3 {*}t{*}\ S_{it} + \varepsilon$$

where Y_t = Singapore homicides (or homicide rate) in time t, with t = 1973 to 2007. β_1 estimates the linear time trend, β_2 estimates the effects of the difference in average homicides between Singapore and Hong Kong, and β_3 is the interaction of time with the difference estimator. In this model, β_2 is the difference, or DD, estimator in the homicides rates in Singapore associated the presence of the death penalty. Again, models were estimated using both generalized least squares and Prais-Winsten regressions to adjust standard errors for autoregression and heteroskedacity (Bertrand et al., 2004).

Next, we expand the model to include the effects of Singapore executions on Singapore homicide rates. We assume

$$Y_t = \beta_0 + \beta_1 * t + \beta_2 S_{it} + \beta_3 EXEC_t + \beta_4 * t * S_{it} + \varepsilon$$

where β_2 again is the difference, or DD estimator, which identifies the difference in homicide rates associated each marginal Singapore execution for each year *t*=1981-2007, the years when execution data were available for Singapore. We estimate this model with β_3 to include – in alternate specifications - the lagged Singapore execution rate and the lagged count of Singapore executions. Following Imbens and Wooldridge (2009), we estimate these models using ordinary least squares regressions. Tests for autoregression in the differences estimators S*it* and $EXEC_t$ showed this not to be a feature of these distributions.

Finally, given the small sample size, we assessed the risk of Type II error by conducting a simple power analysis. For the sample over 27 years, we estimate the power in these tests at .950, based on an effect size of 0.5 and two-tailed α=.05 (Erfelder, Faul, Lang, and Buchner, 2007).[3]

B. Results

We begin with a series of simple regressions and progress to more complicated analyses that test the specific effects of the uneven patterns of rising and falling executions in Singapore. **Table 4** shows results from regressions that compare Singapore and Hong Kong homicide rates under three conditions.

Table 4 Here

First, Model 1 shows simply the difference in homicide rates in the two places over time, a simple test of the parallelism of the two cities. In this analysis, we compare whether the presence of a death penalty has any effect on year-to-year differences in the

[3] Power estimates were computed using G*Power 3, available at: http://www.psycho.uni-duesseldorf.de/abteilungen/aap/gpower3 .

homicide rates. The dependent variable is the Singapore (SP) murder rate. The time interval here is 1973-2007. In this specification, any difference in the two well-matched cities could be attributed to the announcement effects of the death penalty in Singapore.[4]

As expected, the HK murder rate is not a significant predictor of the SP murder rate: murder rates between the two cities are uncorrelated. So, this first simple test shows that the presence of the death penalty in Singapore has no significant effect on homicide rates between the two places, and that variation in the year-to-year differences appears to be random. The large standard error shows that there are large variations in the differences in murder rates between the two cities. The model statistics suggest that this estimates are reasonably strong and have good fit.

Models 2 and 3 test two different measures of executions. In Model 2, we include a predictor for the number of SP executions (lagged by one year), and in Model 3 a predictor for SP executions per 1000 murders (also lagged by one year). In each case, the coefficient for the execution measure is very small and negative. Neither execution predictor is statistically significant. The large standard errors relative to the size of the regression coefficient suggests that there is too much year to year variability in effects of execution to report a stable trend over time. These are strong models with good fit to the actual distribution of the data.

Table 5 shows results of analyses that address the question of time or period-specific effects that may detect deterrence from executions during the period in the 1990s when Singapore executions rose sharply. In this set of regressions, we exclude the interaction of year with the Hong Kong murder rate because we are isolating specific time intervals to determine whether there may have been discrete moments of deterrence associated with these spikes in execution. Put it another way, we try here to determine if there are breaks in the comparative homicide trends between the two cities that would provide some evidence of a significant shift toward deterrence. Since we use specific

[4] We first included the interaction of time with the Hong Kong murder rate to more precisely specify the relationship between the homicide trends in the two cities (results not shown). The interaction term was not significant in any of the models and the parameter estimates were very small. Accordingly, we excluded it from the results shown in Table 4.

annual rates of Singapore executions, the time series for these models is limited to 1981-2007 when such data were available. Recall from **Figure 1** that executions in Singapore rose sharply in 1993 and then fell equally sharply after 1996. Accordingly, the analyses here attempt to identify specific breaks in the homicide rate relative to Honk Kong during that volatile period of executions. We include both those specific periods as predictors as well as the actual execution rates over the entire 27 year interval.

Table 5 Here

Model 4 tests whether Singapore homicide rates were different before and after 1993 when the current era of executions began. Model 5 includes dummy variables for two specific intervals: the peak execution era of 1993-1996, and the subsequent period of declining executions from 1997-2007. Cost-based deterrence theory would suggest that homicide rates would be lowest during the high execution period, and then perhaps rise again in the subsequent years when executions declined. The results suggest that this was not the case. There were no significant breaks in the Singapore homicide trends for any of the periods of volatility in Singapore executions. The execution rate itself also was not a significant predictor of homicides in Singapore. Both models have excellent fit statistics.

In Model 2, we further divide this time period into two eras: 1993-96, when executions were rising, and 1997-2007, when they stabilized and then fell. Only the linear time trend was significant, illustrating the overall secular decline in homicides in both Singapore and Hong Kong for the entire period. So, we again find no evidence of deterrence when examining this important interval, either over the entire interval or when disaggregating the interval into periods of rising or falling execution rates.

Table 6 further disaggregates the 1993-1996 period to determine whether there are specific effects that could be identified for any of the years during this period when executions in Singapore were spiking. Model 6 shows the period for all years in the entire time series prior to 1992, two years before the 1993 increase. Model 7 repeats the analysis for the period through 1992, one year before the 1993 increase. Model 8 and 9

each move the time period forward by one year to determine if there was a phase shift in the effects of the increments in executions for those years.

Table 6 Here

In all four models, the HK murder rate is not significant, again showing that the trends between the two cities are uncorrelated and independent across the time period. The linear time trend over the entire 26 year interval also is significant, again reflecting the long-term trend. Only one of the specific period effects is significant, but it is for the period that ends in 1991 (Model 1), two years preceding the runup in SP executions that began in 1993. In fact, the negative coefficient in Model 6 suggests that there was a meaningful decline in Singapore homicides that ends two years in advance of the onset of years of high executions. Models 7-9, which specify break points that are closer to or within the period when executions rose, find no such effect. The absence of year-specific effects close to the point when executions spiked also suggests the absence of any deterrent effect from executions.

Table 7 Here

The results of the DD analyses are shown in **Table 7**. In all four models, the estimate for the difference between Singapore and Hong Kong murder rates is positive and significant. The positive coefficient in each model suggests that Singapore homicide rates tend to increase as a function of widening differences between Singapore and Hong Kong homicide rates. Model 10 estimates the difference in the Singapore execution rate as a function of differences between the homicide rates in the two cities for the full time series from 1973-2007. Controlling for year, wider gaps in the homicide rates between the two cities predict higher homicide rates in Singapore. To the extent that any annual differences in homicide rates are attributable to the presence of the threat of capital punishment in Singapore, that threat does not produce a statistically significant difference in Singapore homicide rates in the direction that a cost-based deterrence theory would suggest. Model 11 repeats Model 10 with the shorter time series from 1981-2007, and

includes the number of Singapore executions in the previous year. We also estimated the same models using a lagged function for the difference estimator, and the result is unchanged.[5] Differences from year to year in the number executions in Singapore in Singapore in the prior year has no effect on Singapore homicide rates, net of the difference in homicide rates between Singapore and the "untreated" comparison city of Hong Kong. From these first two analyses, we can preliminarily conclude that the existence of the death penalty in Singapore has no apparent announcement effect on homicide rates.

Models 12 and 13 repeat the same estimates, this time including the execution rate in Singapore per 1,000 murders in the preceding year. Model 12 includes only the execution rate, and Model 13 includes an interaction with time to control for any year-specific outliers in the Singapore execution rates. In each case, neither the number nor the rate of executions predict the Singapore homicide rates when we again control for differences in the homicide rates between the two cities. The regression diagnostics suggest good fits for these model. Finally, we re-estimated Models 10-13 using quadratic time functions, log-log specifications, and Poisson functions on homicide counts, and obtained similar results.[6]

C. The Specific Deterrent Effects of Murder Executions

Executions for homicides are not easily distinguished from executions for drug offenses or other offenses as they are reported in Singapore. The rank-order correlation between these two categories of executions is .849 ($p < .001$).[7] The strong correlation raises obvious problems in estimating the unique and specific deterrent effects of murder executions on murders. Would-be offenders in Singapore are aware only that someone was executed, but have no knowledge of the crime for which the condemned was

[5] Results not shown but are available from the authors.

[6] Id.

[7] With 2001 and 2002 removed from the time series, the rank order correlation is .875 ($p < .001$). Without 2001-2 and 2006-7, the rank order correlation is .818 ($p < .001$).

executed. Nor do they have any way to update their estimates of the risk of execution for a murder, an essential element of deterrence (Kahneman and Tversky, 1979; Kleck et al., 2005; Robinson and Darley, 2004). Our approach up to now has been to blur the distinction between executions for murders and executions for other crimes. A precise account of the deterrent effects of execution in this natural experiment requires that we take the next step and attempt to disaggregate homicides.

There are reasons to do so and reasons not to. One could argue that the policy of masking the crime that generated the execution may widen the perception of risk and raise the estimates of detection and cost associated with a homicide. Prospective offenders are, in effect, flying blind with respect to the likelihood that a murder will result in the death of the murderer. Even if they may be likely to discount the risks and costs of crime, as behavioral economists suggest (Korobkin and Guthrie, 2000; McAdams and Ulen 2009), this discounting may be offset by the complementarity of murder executions and executions for other crimes, producing a perceptual augmenting of risk that is then generalized across crimes (Niraj, Padmanabhan, and Seetharaman, 2008).[8] In other words, murder executions are carried on the same risk bandwidth as other executions, so the signal is very difficult for offenders to parse and segment into unique compartments of risk.

We also face constraints in the data that argue against formal econometric estimates to develop precise estimates of their specific deterrent effects. **Table 3** and **Figure 5** show that the time series when reliable data are available to disaggregate executions by committing offense is simply too short – 17 years – to develop stable and robust estimates of the unique effects of murder executions on murder.

[8] For example, products such as bacon and eggs tend to be consumed in complements, and the prices of the more commonly consumed product are often manipulated to leverage consumption of the second. So, in this case, making the focal product (drug executions, in this case) more attractive for consumers renders a complementary product (murder executions) to be more attractive since the consumer gains additional satisfaction from consuming the two products jointly, even if the consumer pays a premium for the second product.

IV. Limited Inferences and Implications

The city-states of Singapore and Hong Kong are a change of setting from the contentious American debate for attempts to study the deterrent effect of punishment threats in a number of respects. The variations in rates of execution in this analysis are much larger both over time and in the cross-sectional comparison of the two cities than in other systematic studies. That is the good news. But the lack of complete and reliable data on punishments and on many crime-related phenomena in Singapore also exacerbates the usual problems of interpretation of variations in crime over time.

Three problems keep the current analysis from being a comprehensive assessment of the deterrent effectiveness of executions on crime in a setting like Singapore. The first is the scarcity of information available on the levels of illicit drug sales and use in Singapore and their variation over time as executions peak and then fall dramatically. Drug offenses account for the majority of executions in the peak periods of the 1990s, and drug crimes are a larger concern and priority to the government than garden-variety murders, but there are no reliable indicators of variations of hard drug commerce and usage.

Measuring illegal drug use is difficult in any city. There are inherent problems with measuring the rate of a crime where no victims wish to report to authorities, but some data can be used to estimate drug usage in target populations in other developed countries. In the U.S. for instance, reports of urine tests over time of jailed persons and trends in hospital drug overdose treatments can provide indications of levels of drug use (Zimring 2006:233-234; Wish and Gropper, 1990; *but see* Manski, Pepper, and Petrie, 2001). But no decent indicators of drug use over time are available for Singapore, and if such data exist, they are a government secret.[9] So a criminological study of the impact of

[9] In its response to Amnesty International's highly critical (2004) report about "the hidden toll of executions" in Singapore, the Ministry of Home Affairs did list the number of "drug abusers arrested" from 1993 to 2003, the number of "first-time abusers" for the same period, and the "2-year re-incarceration" rates for persons released from drug rehabilitation centers between 1994 and 2000 (Ministry of Home Affairs 2004). But these data do not allow for a meaningful deterrence analysis. As Singaporean scholar Michael Hor has observed, "One might have expected that if the death penalty is being imposed on drug offences in order to deter or incapacitate, the government would be keenly interested in statistical and other studies to find out if, in fact, the increased penalties are working. But such studies, if they exist, are seldom

executions on drug crime cannot be launched until Singapore's government provides access to the necessary data. Until then, the impact of the threat of executions on drug use is unknown.

A second limit of this study is that the patterns in homicide volume we report cannot eliminate the possibility of a deterrent impact of execution even on criminal homicide. The wild swings in execution rate in Singapore seem to have no direct effect on homicide volume, but even with no obvious or visible impact, marginal deterrence might be possible in a few cases. The proof of a negative is always problematic where there is no direct measure of deterrence, and the size of the differences required to establish highly probable non-chance differences does leave some room for possible deterrence (there is, after all, a small dip in 1996 in Singapore homicide that could be a one year impact). There cannot be certainty of zero homicide deterrence in this (or any other) set of crime statistics.

The third limit on generalizations from the Singapore experience relates to the peculiar information policy that the government of Singapore employs. By government policy, neither the fact of execution nor the crime which provoked it was announced to the public during the period covered in Table 3. So while the pervasive threat of hanging was in the air in Singapore during the mid-1990s, communication of the rate of executions or its causes was not a major element of Singapore punishment policy. For those who believe better communication might enhance the execution threat, Singapore's policy of secrecy is one possible explanation for the lack of a measurable deterrent impact on murder. And the lack of visible impact might not generalize to settings where the hangman's activities receive more attention.

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Notwithstanding such limits on data and inference, the statistics reported here provide three important insights about the impact of executions on homicide in Singapore

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revealed. Statistical data are not provided in any consistent or meaningful way by the government. One can only speculate why" (quoted in Lines 2007:14).

and elsewhere. The first clear message comes from comparing the temporal pattern of execution and homicide in Singapore over time. Homicides have trended downward in Singapore since the mid-1980s, with most of the decline happening prior to the sudden increase in executions in 1992 and the surge in executions from 1994 to 1996. The decline continued during the peak period of execution, and the homicide rate then stayed close to its lowest levels while the execution rate in Singapore dropped by more than 90 percent. The homicide rate per 100,000 had declined to 0.47 per 100,000 by 1996, the endpoint of the surge.

By 2007, after ten years of declining executions, the homicide rate in Singapore was even lower, at 0.39 per 100,000, and the three-year average at the end of the decade-long drop in executions is one third lower (at 0.42 per 100,000) than in the peak execution years of 1994-1996 (0.66). Thus, Singapore is a slightly safer city in an era of 5 executions per year than it was with 60. This does not prove zero deterrence, nor does it disprove the deterrent potential of a different publicity policy or a different homicide environment. But it does provide yet another case history of national homicide trends which are largely unrelated to the extent of execution, and the lack of a clear connection between hangings and homicide in Singapore is especially significant because of the huge dosage of death and the substantial variation in execution risks over time. Over the past two decades, Singapore would seem to be a best case natural experiment for highly visible homicide deterrence through the threat of execution. But that did not happen.

A second lesson on crime and punishment comes from the two city comparison that motivates the title of this analysis. Hong Kong and Singapore are similar in many social and demographic respects, and they also turn out to be quite close in their levels of homicide in the 1970s and 1980s as well as in their homicide trends over the period reported in Figure 5. Between 1972 and 2007, the correlation between Hong Kong homicide rate fluctuations and those in Singapore is .69.

The central question addressed by the statistical analysis in section four was whether Singapore's swings up and down in execution levels had any impact on the relative homicide trends of Singapore versus Hong Kong, and the answer was no. The

statistical inference is that since the trends in homicide over time in Singapore are close to those in Hong Kong, they are therefore the product of other temporal patterns and not of executions. There is one other critical point that these data establish: Hong Kong is just as safe a city from criminal homicide as is Singapore.

The third perspective we gain from the Singapore and Hong Kong data concerns the debate about the possible deterrent effect of executions in the United States. Rates of homicide are high in the U.S. while levels of execution are low. Statistical manipulations have produced a breath-taking variety of estimates of deterrent and counter-deterrent influences of execution on homicides. The economist Johanna Shepherd has published state-level estimates that range from each execution deterring 61 homicides in South Carolina to each execution causing more than 150 additional homicides in Oregon and Utah (Shepherd 2005 at Figure 2; Zimring 2008). Shepherd (2005, at Table 8) identifies six states (South Carolina, Texas, Florida, Georgia, Delaware and Nevada) where executions deter homicides. The execution rate from 1977-1996 in those six states was 1.68 per year.[10] South Carolina, the state with the largest deterrent effect, executed 11 persons during this time, or about one person every other year. Contrast this with the annual execution rate of 17.2 per year in Singapore from 1981-2007, and a rate of 66.3 per year during the peak years of 1994-96. But Singapore also had seven years with no more than two executions.

The extraordinary scale and variability of executions in Singapore provide a different environment for testing the claims and hypotheses that are in play in the U.S. One example of the value of the Singapore experience as a laboratory for U.S. theory derives from a recent paper by two American law professors who endorse an econometric estimate from regression equations that each execution reduces homicides by 18 (Sunstein and Vermuele 2005). After some temporizing on issues of scale, these scholars conclude that they "will speak of each execution saving eighteen lives in the United

[10] These estimates are based on data on homicides and executions from 1977-96. She reports an average of 32 executions for the six deterrence states taken as a whole, or 1.68 executions per year. In a separate analysis using a slightly longer time period, Shepherd (2005: Table 10) reports an average of 45 executions, or 2.05 executions per year across the six states.

States, on average" (Sunstein and Vermuele 2005:706). And they do expect this result to carry over to high execution environments, arguing that "more frequent executions, carried out in closer proximity to convictions, are predicted to amplify the deterrent signal" (Sunstein and Vermuele 2005:715). On such criteria, Singapore is a near perfect setting for highly leveraged deterrent results to shine clearly through the other factors that influence homicide rates.

Figure 6 puts the Sunstein and Vermuele estimate to a laboratory test by using Singapore data on homicide and the estimates of Singapore murder executions from **Table 3**.

Figure 6 Here

The solid line in **Figure 6** is the actual number of homicides reported in Singapore from 1991 to 2007, which varies in a tight range from 35 to 16 (without adjustments for increases in population). The dotted line starts the Sunstein-Vermuele machinery in 1991 with the actual level of homicide in Singapore for that year and then estimates future homicides by multiplying each year's added or subtracted executions by 18 and adding or subtracting that total from the 1991 homicide total. In 1992, for example, the number of homicide executions goes from 1 to 13, which generates a predicted decline of 216 homicides from the base level of 33, or *minus* 183 killings (which for the sake of reality must be rounded up to zero). The following year, 1993, has only 5 murder executions, so the homicide volume should increase by 144, but this is still below *Ground Zero* for homicide in Singapore. In fact, all of the values for this 1991 start remain below zero until 2005, when homicides rise to 15 and then more than triple to 51 in 2007.

The dashed line in Figure 6 uses the same estimate of deterrent elasticity but starts the time series in 1994, when Singapore had 21 murder executions and a total of 26 homicides. By adding 18 homicides for each reduction in execution throughout the remaining years, we expect an average of over 300 homicides each year for the years after 1997 and over 400 killings in 2007, which is more than 22 times the actual homicide total.

Neither hypothetical curve bears the slightest resemblance to the reality of Singapore homicide trends and volumes. In 1995, there were 27 recorded homicides, not the minus 309 that a Sunstein and Vermuele elasticity and a 1991 homicide base generate. And in 2007, there were 18 homicides, not the 404 that result from starting the count in 1994 and relying on econometric illusions.

So **Figure 6** is a silly graph, but why? Because the hypothesis that each execution prevents 18 homicides is demonstrably impossible in Singapore at whatever starting point one chooses to inject the assumption. Are the behavioral dynamics of deterrence different on Singapore's side of the international date line? If not, is a killing elasticity of 18 just as silly elsewhere?

The curves in **Figure 6** are nonsense because Singapore's policies provide a critical test of the exuberant claims produced by statistical extrapolation over time in a low-execution environment like the United States. The Singapore experience magnifies the behavioral impact of American assertions to a patently silly status. In our view, the claims are every bit as silly for Texas or Kansas, just not as visible. The extremities of Figure 6 also result from the size of the execution risk difference between Singapore and Hong Kong. Finding a setting where the peak level of execution risk for homicide was 24 times the high point in Texas and where the risk then drops by 90 percent promised one of two conclusions to the search for marginal general deterrence: either the impact of executions on homicide in Singapore would be large and obvious, or the made-in-the-USA estimates of deterrent elasticity would generate equally obvious science fiction. Figure 5 demonstrates the false hope of the American claims.

V. CONCLUSION

We compared homicide rates in two quite similar cities with vastly different execution risks. Singapore had an execution rate close to 1 per million per year until an explosive twentyfold increase in 1994-95 and 96 to a level that we show was probably the highest in the world. Then over the next 11 years, Singapore executions dropped by

about 95%. Hong Kong, by contrast, has no executions all during the last generation and abolished capital punishment in 1993. Homicide levels and trends are remarkably similar in these two cities over the 35 years after 1973, with neither the surge in Singapore executions nor the more recent steep drop producing any differential impact. Singapore's aggressive capital punishment policies provided a critical test of the exuberant claims among American empiricists produced by statistical extrapolation over time in a low-execution environment like the United States. The Singapore experience magnifies the impact of American assertions to a patently silly status.

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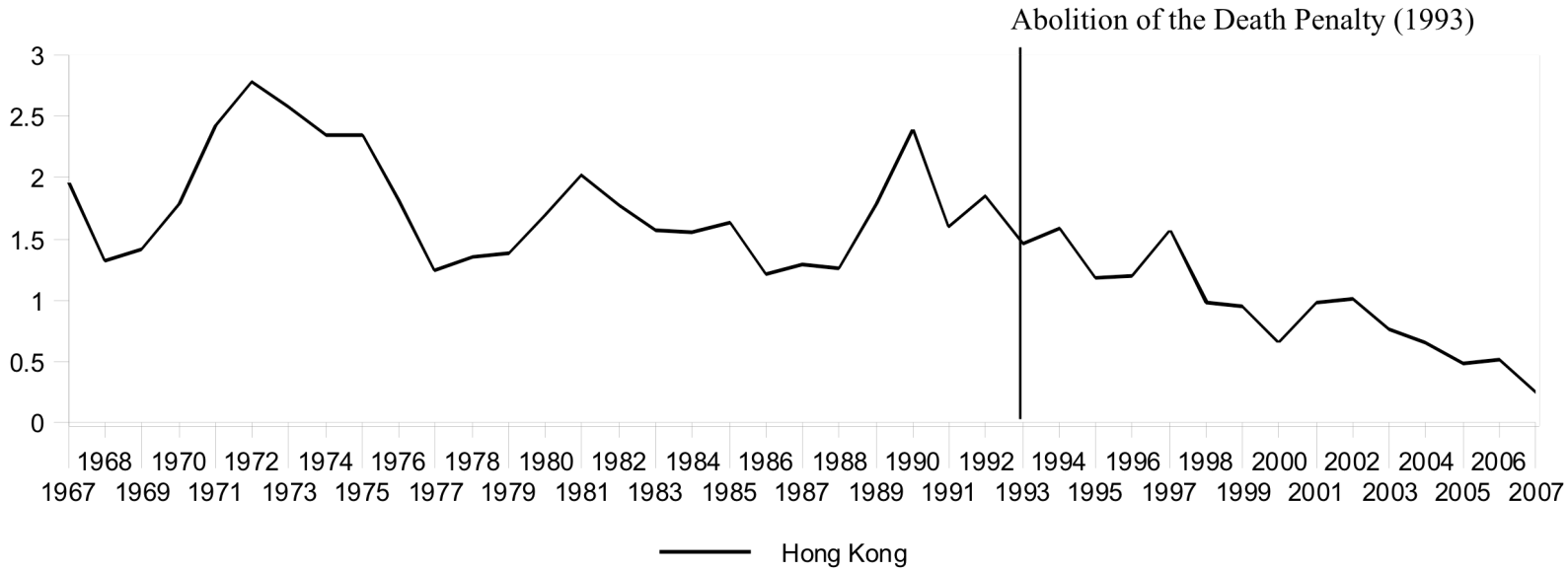
Table 1. Comparative Data on Hong Kong and Singapore.[1]

	Population	Percent Chinese	Population Density	Annual Population Growth	Per Capita GNI	Economic Growth in 2007	Adult Literacy	Birth Rate	Migrants per 1,000	Life Expectancy
Hong Kong	7.0 M	95	6.4 per KM	1.4%	28,460 (21st)	5.8%	92%	11.4 per thousand	8.1	79.5
Singapore	4.6 M	75	6.5 per KM	1.4%	29,320 (20th)	7.4%	93%	9.4 per thousand	9.1	81.7
Source	U.S. Census Bureau	HK Census &Singapore DOS	United Nations	HK Census & Singapore DOS	World Book (2006)	U.S. Central Intelligence Association	HK Census & Singapore DOS	HK Census & Singapore DOS	HK Census & Singapore DOS	HK Census & Singapore DOS

Sources: U.S. Census Bureau; HK Census and Singapore DOS; United Nations, *World Book 2006;* U.S. Central Intelligence Association.

[1] Hong Kong and Singapore are similar across other dimensions as well. Consider five more: (1) The percentage of the population that is age 10 to 24 is 18 in Hong Kong and 21 in Singapore (Population Reference Bureau, 2008). (2) The percentage of legislators, senior officials, and managers that is female is 26 in Hong Kong and 26 in Singapore (United Nations Development Project, 2004). (3) The percentage of professional and technical workers that is female is 40 in Hong Kong and 43 in Singapore (United Nations Development Project, 2004). (4) On the Gender-Related Development Index, Hong Kong ranks 23rd out of 144 political units and Singapore ranks 28th (United Nations Development Project, 2004). In Asia, Hong Kong and Singapore are often regarded as "the most equal [societies] in terms of gender relations" (Bell 2008:66). (5) On infant mortality, Hong Kong ranks 4th out of 206 political units (with 2.93 deaths per 1000 live births) and Singapore ranks 1st (with 2.30) (CIA World Fact Book).

Figure 1. Homicide rates, Hong Kong, 1967-2007.



Sources: Hong Kong police data, 1967-2007, provided by Edna Chan, Office of Commissioner of Police. Populations for Hong Kong, 1967-2005, come from the World Development Indicators database via Nationmaster.com.

Table 2. Executions and Execution Rates in Singapore, 1991–2007.

Year	Executions	Rates per Million
1981	3	1.3
1982	0	---
1983	2	0.7
1984	2	0.7
1985	1	0.4
1986	1	0.4
1987	0	---
1988	4	1.4
1989	5	1.7
1990	3	1
1991	6	1.9
1992	21	6.6
1993	7	2.1
1994	76*	22.4
1995	73**	20.9
1996	50***	13.9
1997	15****	4.1
1998	28	7.4
1999	43	11.0
2000	21	5.2
2001	27	6.6
2002	28	6.7
2003	17	4.0
2004	19	4.4
2005	6	1.4
2006	5	1.1
2007	2	0.5

Source: Amnesty International (1981-1990 and 2006-2007), Singapore Ministry of Home Affairs (1991-2003), and The Straits Times (2003-2005 reporting from government sources).

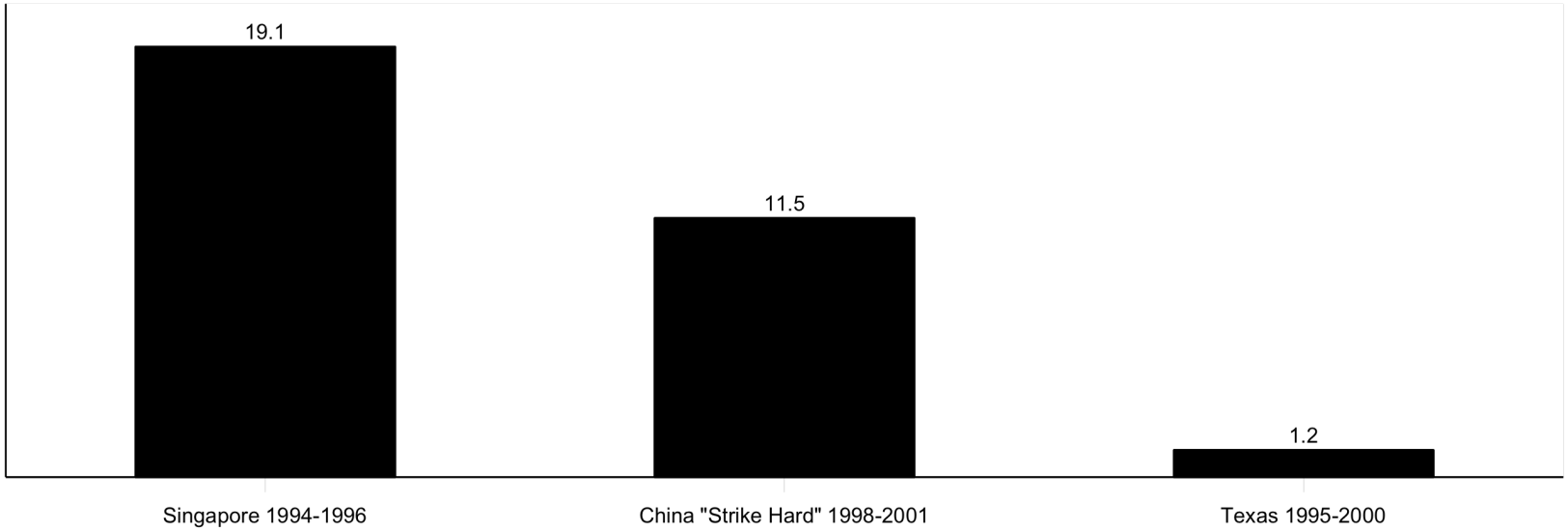
* The original Amnesty report estimated "at least 32" but was changed to 76 in 1996 when a government report became available.

** Originally "at least 50" but later increased.

*** Originally "at least 38."

**** Originally "at least 14."

Figure 2. Comparative Annual Rates of Execution in Three High-Volume Jurisdictions, Rates per Million.



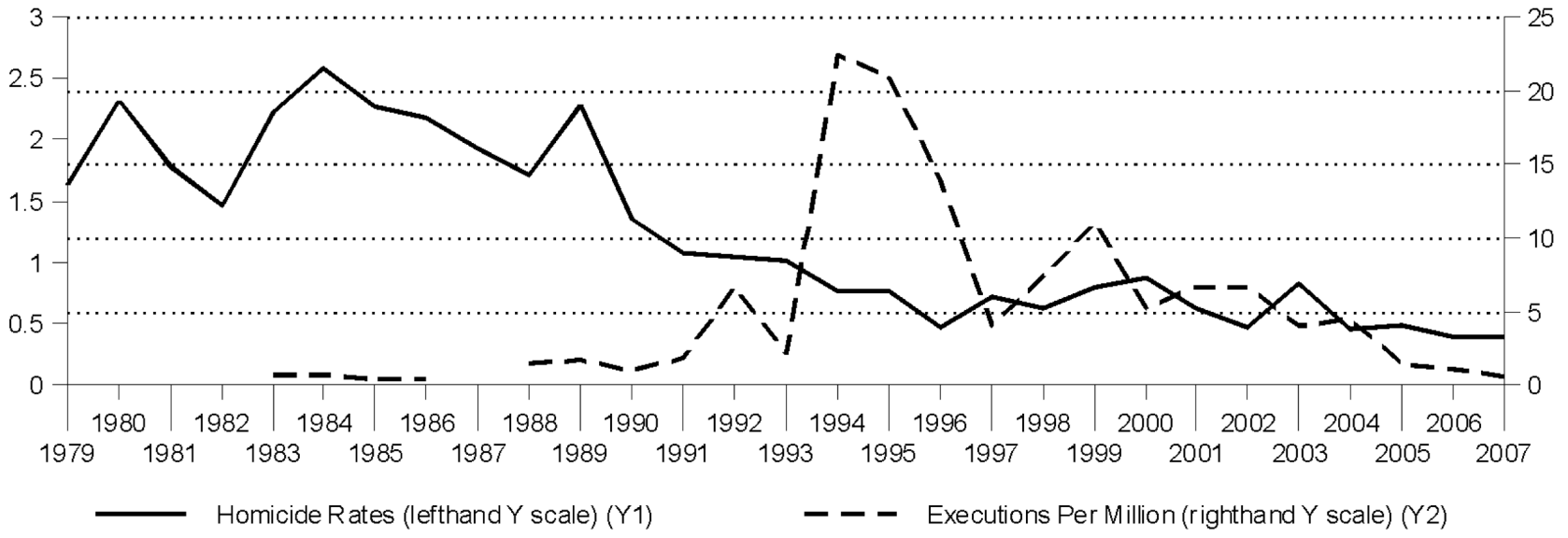
Sources: Johnson and Zimring 2009:241, 410; Bureau of Justice Statistics; Zimring 2003:228.

Table 3. Estimates of Executions for Murder, Singapore, 1991 – 2007

Year	Murder Executions
1991**	1
1992**	13
1993**	5
1994**	22
1995**	20
1996**	10
1997**	3
1998**	4
1999**	8
2000**	4
2001	N/A
2002	N/A
2003*	5
2004*	4
2005*	2
2006**	2
2007**	0

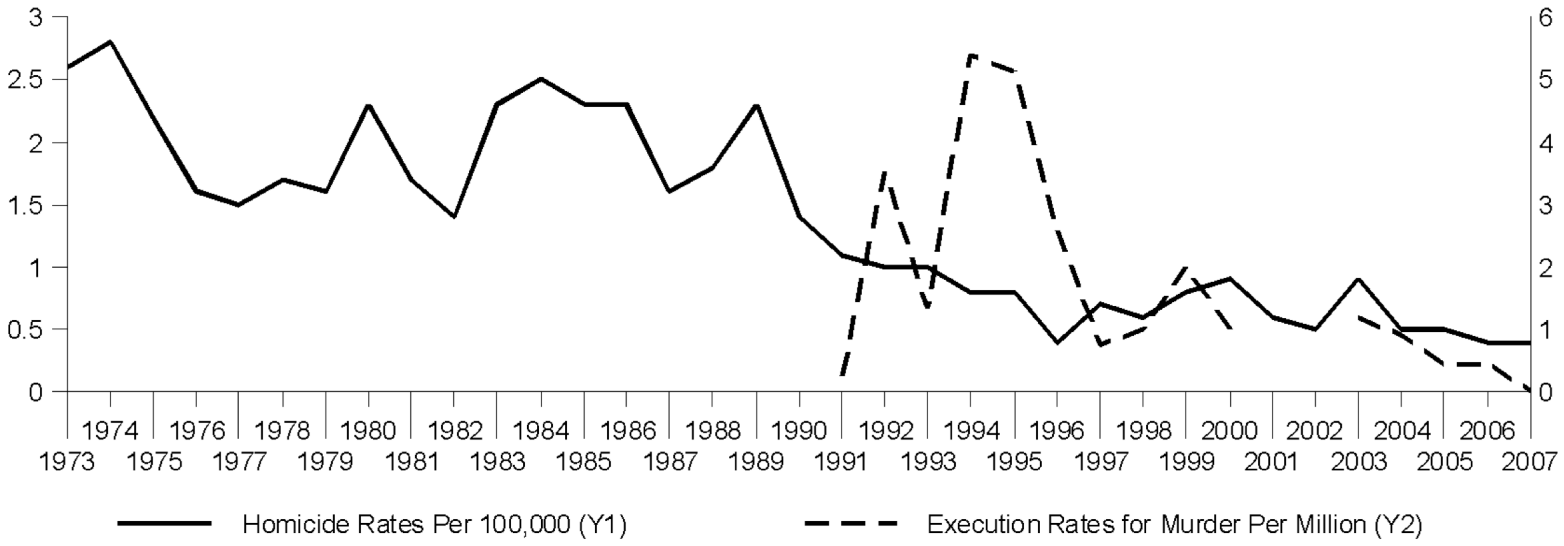
Sources: *Lim and Yong 2006; **Amnesty International.

Figure 3. Homicide and Execution Rates for All Offenses, Singapore, 1979-2007 (Homicide) and 1981-2007 (Total Executions)



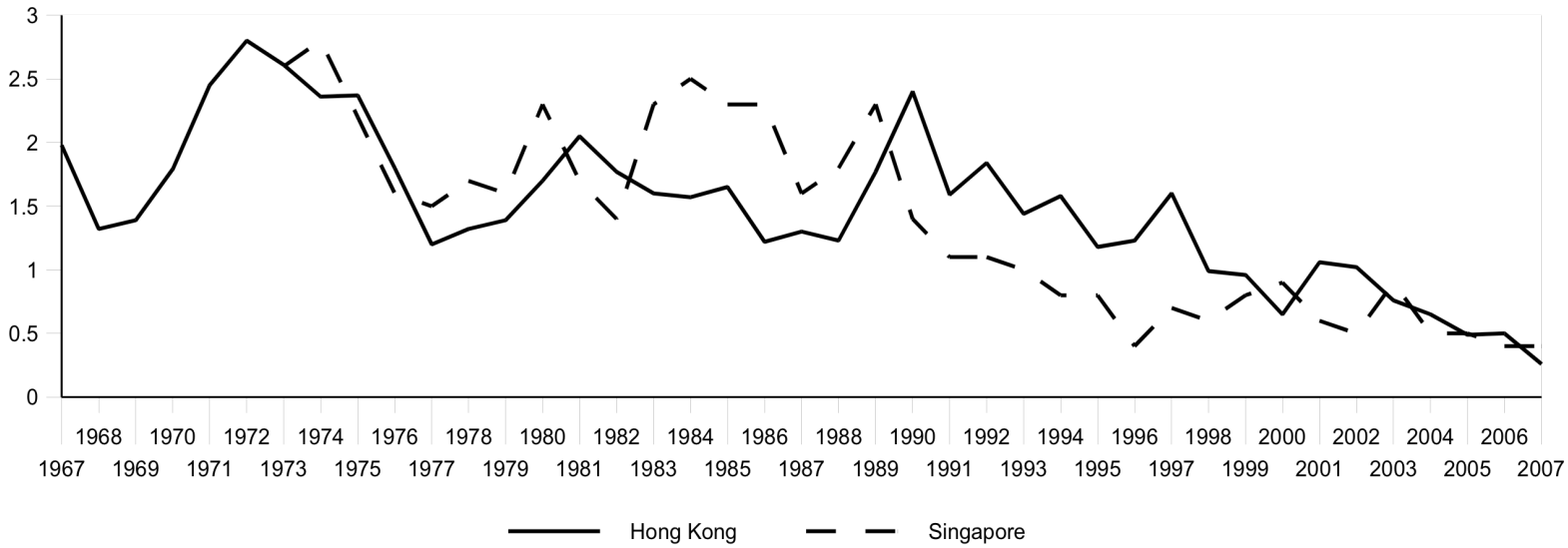
Sources: World Development Indicators database via Nationmaster.com (Singapore population); police data for 1973-82 from 27 Mal.L.R. 113 (1985) at Table 1; W.H.O. homicide data from Singapore E55 for 1984-2004, Statistics Singapore, www.sing.stat.gov.sg; police data for 2005-07 from police homicide reports.

Figure 4. Execution Rates for Murder and Homicide Rates, Singapore, 1973-2007



Sources: World Development Indicators database via Nationmaster.com (Singapore population); police data for 1973-82 from 27 Mal.L.R. 113 (1985) at Table 1; W.H.O. homicide data from Singapore E55 for 1984-2004, Statistics Singapore, www.sing.stat.gov.sg; police data for 2005-07 from police homicide reports.

Figure 5. Homicide Rates, Hong Kong and Singapore, 1967-2007.



Source: Chan, Edna; Hong Kong police data 1967-2007 provided by (Edna Chan) Office of Commissioner of Police; World Development Indicators database via Nationmaster.com (population for Hong Kong); Singapore E55, unstats.un.org/unsd/demographic/products/dyb/dyb2 2004, 2003 and 2002, for Singapore homicides from police data; Singapore police (2005, 2006 and 2007); Statistics Singapore, www.singstat.gov.sg; 27 Mal. L.R. 113, at Table 1(c), p. 118 claiming police origin (1973-1982) (Homicide in Singapore).

Table 4. Prais-Winsten Regressions of Singapore Murder Rate per 100,000 Population by Hong Kong Murder Rate and Year (b, SE), 1973-2007

Predictor	*Model 1 1973-2007*		*Model 2 1981-2007*		*Model 3 1981-2007*	
Hong Kong Murder Rate	.033		-.311		-.286	
	(.193)		(.205)		(.212)	
Year	-.063	***	-.089	***	-.089	***
	(.014)		(.014)		(.016)	
Singapore Executions (lagged)			-.005			
			(.003)			
Singapore Executions per1,000 Murders (lagged)					-.00008	
					(.00008)	
Constant	2.412	***	2.805	***	2.738	***
	(.479)		(.420)		(.438)	
Model Statistics						
Adjusted R^2	.583		.756		.725	
Log Likelihood	-9.130		-3.436		-3.901	
BIC	28.926		19.905		20.833	

Notes:
a. Significance: * = p < .05, ** = p < .01, *** = p < .001
b. Alternate specifications for each model include Time * Time, Poisson distribution, and Log-Log estimates

Table 5. Prais-Winsten Regression of Singapore Murder Rate per 100,000 Population by Hong Kong Murder Rate, Year and Singapore Executions per 1,000 Murders, Controlling for Specific Periods, 1981-2007 (b, SE)

Predictor	***Model 4***		***Model 5***	
Hong Kong Murder Rate	-.351		-.302	
	(.204)		(.227)	
Year	-.070	**	-.077	***
	(.019)		(.023)	
Singapore Executions per 1,000 Murders (lagged)	-.00003		-.00007	
	(.0009)		(.0001)	
Period: 1993-2007	-.460			
	(.299)			
Period: 1993-1996			-.479	
			(.300)	
Period: 1997-2007			-.294	
			(.407)	
Constant	2.792	***	2.760	***
	(.413)		(.422)	
Model Statistics				
Adjusted R^2	.776		.787	
Log Likelihood	-2.615		-2.420	
BIC	21.522		24.390	

Notes:
a. Significance: * = p < .05, ** = p < .01, *** = p < .001
b. Alternate specifications for each model include Time*Time, Poisson specification, and Log-Log estimate

Table 6. Prais-Winsten Regression of Singapore Murder Rate per 1,000,000 Population by Hong Kong Murder Rate, Year and Singapore Executions per 1,000 Murders, Controlling for Specific Periods, 1981-2007 (b, SE)

Predictor	*Model 6*		*Model 7*		*Model 8*		*Model 9*	
Hong Kong Murder Rate	-.171		-.279		-.352		-.304	
	(.165)		(.188)		(.204)		(.212)	
Year	-.049	***	-.064	**	-.070	**	-.071	**
	(.016)		(.019)		(.019)		(.021)	
Singapore Executions per 1,000 Murders (lagged)	-.00006		-.00005		-.00003		-.00003	
	(.00006)		(.00008)		(.00009)		(.00009)	
Period Dummy								
Prior to 1992	-.701	**						
	(.199)							
Prior to 1993			-.516					
			(.251)					
Prior to 1994					-.460			
					(.298)			
Prior to 1995							-.370	
							(.310)	
Constant	3.614	***	3.712	***	3.812	***	3.621	***
	(.393)		(.465)		(.553)		(.575)	
Model Statistics								
Adjusted R^2	.890		.834		.776		.716	
LL	1.291		-1.982		-2.616		-3.019	
BIC	13.707		20.254		21.522		22.328	

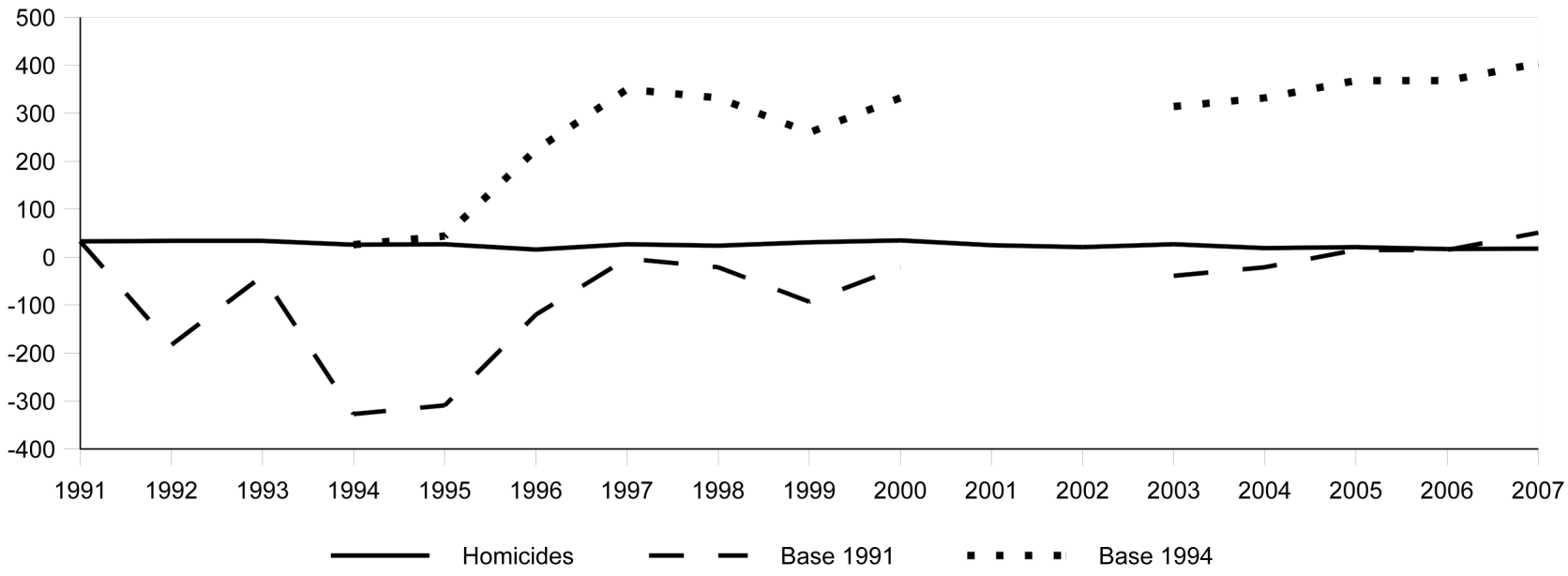
Notes:
a. Significance: * = p < .05, ** = p < .01, *** = p < .001
b. Alternate specifications for each model include Time*Time, Poisson distribution, and Log-Log estimate

Table 7. OLS Regression of Difference-in-Difference Estimates of Singapore Murder Rate per 1,000,000 Population by Hong Kong Murder Rate, Year and Singapore Executions, 1973-2007 (b, SE)

Predictor	**Model 10 1973-2007**		**Model 11 1981-2007**		**Model 12 1981-2007**		**Model 13 1981-2007**	
D.Singapore -Hong Kong Murder Rate	.641	*	.624	**	.635	**	.617	**
	(.283)		(.165)		(.197)		(.177)	
Year	-.054	***	-.062	***	-.062	***	-.066	***
	(.005)		(.006)		(.006)		(.009)	
Year*D.Singapore -Hong Kong Murder Rate	-.006		-.014		-.016		-.015	
	(.017)		(.017)		(.017)		(.017)	
Singapore Executions (lagged)			-.003					
			(.002)					
Singapore Executions per 1,000 Murders (lagged)					-.0006		-.0002	
					(.00005)		(.0003)	
Year*Singapore Executions per 1000 Murders (lagged)							-.00001	
							(.00002)	
Constant	2.284	***	2.047	***	2.037	***	2.065	***
	(.103)		(.088)		(.090)		(.108)	
Model Statistics								
Adjusted R^2	.870		.927		.925		.921	
LL	-2.525		-9.369		8.942		9.093	
BIC	19.272		-2.447		-1.593		1.363	

Notes:
a. Significance: * = p < .05, ** = p < .01, *** = p < .001
Alternate specifications: Prais-Winsten, Lag SP executions, Log-Lag SP executions, Concurrent SP execution rate

Figure 6. Actual and Predicted Homicide Volume for Singapore, using 1991 and 1994 as Prediction Bases and 18 Homicides as Marginal Deterrent Estimates



Sources: Cass R. Sunstein and Adrian Vermeule, Sunstein, "Is Capital Punishment Morally Required? Acts, Omissions, and Life-Life Tradeoffs," 58 *Stanford Law Review* 703 (2005), citing Hashem Dezhbakhsh, Paul Rubin, and Joanna M. Shepherd, "Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data," 5 *American Law and Economics Review* 344 (2003).