Economic Liberalization and Violent Crime

Kislaya Prasad *University of Maryland*

Abstract

I study the effect of economic liberalization on violent crime. The particular emphasis is on the case of India, where, in the years following 1991, there was a virtual dismantling of controls on entry and production in registered manufacturing. This was accompanied by a significant reduction in impediments to foreign trade and access to foreign exchange. Economic controls create an incentive for illegal trade, and a frequent by-product of illegal trade is violent crime. Consequently, violent crimes such as murders would be expected to decline following market-based reforms. Analysis of aggregate all-India data, as well as data at the state level, suggests that economic reforms did indeed lead to a reduction in violent crime. I extend the analysis to a panel of countries and find strong evidence that greater trade openness is negatively related to violent crime.

1. Introduction

Restrictions on economic activity and voluntary trade frequently give rise to attempts to contravene controls. The experience with prohibition of liquor, drugs, and prostitution, or the imposition of rationing and rent controls, is unambiguous. Inevitably, production and consumption go underground, and individuals start to trade in black markets. Trade restrictions—especially when they are extreme—lead to widespread smuggling. An unfortunate by-product of such illegal trade and production is violent crime. A variety of social institutions, courts of law for instance, have been devised to manage conflict between agents engaged in trade. When a transaction becomes illegal, agents lose access to such institutions. An alternative way in which disputes might be resolved, and agree-

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ments might be enforced, is through violence (Miron 1999, 2001).[1] Individuals engaged in legal activities also have access to protections offered by the state, which are unlikely to be available to those involved in illegal activities. For instance, a drug dealer who is muscled out of his or her territory could hardly approach the police for help. This means that turf battles are more likely to turn violent. Such arguments suggest that restrictions on voluntary trade are likely to lead to an increase in violent crime. Conversely, the removal of economic controls should be associated with a decline in violence. In this paper, I test this hypothesis in the context of India's economic liberalization experiment. The analysis is then extended to a panel of countries, and I examine more broadly whether greater trade openness is negatively related to the murder rate.

India's economic reforms of 1991, initiated by the government of Prime Minister Narasimha Rao, were extensive in both scale and scope.[2] Under the Industries Act of 1951, which covered all registered manufacturing, an industrial license was required to set up a factory, with the government maintaining tight controls on its location and size. Delicensing began under the Rajiv Gandhi government in 1985, but the large majority of industries were delicensed in 1991. In addition, import licensing requirements for most manufactured, intermediate, and capital goods were removed in 1991 or shortly thereafter. Tariffs were gradually reduced, and rules for foreign direct investment were relaxed. Foreign exchange controls were relaxed substantially, and starting in August 1994, the Indian rupee became fully convertible as a current account transaction.

Is it plausible that the lifting of controls could have led to a measurable decline in Indian murder rates? An account given by Luce (2007, p. 98) of his conversation with a Mumbai police officer seems to suggest so. According to the officer, "The mafia dons were making most of their money from smuggling gold and electronic goods. Since the 1990s restrictions have been lifted so there is much less money to be made in smuggling. They still have protection rackets and prostitution rings, but they are not as lucrative." The police officer went on to add: "Ten years ago we would have two or three gang killings every day. Now it is a few each month. It has definitely improved." A preliminary look at the national murder data, illustrated in Figure 1, would seem to confirm the officer's assertions. The results are quite striking: there is a significant reversal in the trend for murders after 1991. After rising steadily through the 1980s, the murder rate begins to decline soon after the initiation of reforms. What these data cannot tell us is whether this reversal was caused by reforms or just happened to coincide

[1] This is not to suggest that violence is the only option available. The underground economy may develop its own private set of institutions (Gambetta 1993). In addition, if transactions are repeated, and individuals place sufficient value on the trading relationship, there may be no need for third-party enforcement.

[2] They were also largely unanticipated. The reforms were made necessary by a foreign exchange crisis and the need to approach the International Monetary Fund. Even if there were intimations of such a crisis, what was surprising was the enthusiasm with which the government embraced the opportunity for reform. For an (informal) account of events in the summer of 1991, see Das (2000). For an economist's account of liberalization, see Panagariya (2008).

Figure 1. Murder rates in India, 1953–2005 (Government of India 1980–2006)

with them. The data raise other questions. If the decline was caused by reforms, what aspects of those reforms led to the decline in murders? Also, was the reversal a consequence of the lifting of controls or of the faster income growth of the postreform period? The data identify a second puzzle: What caused the increase in murder rates just preceding the reforms? Can this increase also be explained in terms of economics, or does it have some other explanation? These are some of the questions addressed in this paper.

First, I look at the all-India murder rate and analyze its dependence on the differential between the Mumbai price of gold and the London price of gold (to correct for inflation, the difference is measured in units of gold). The differential is a measure of the attractiveness of gold smuggling.[3] If this differential increases, one would expect murder rates to increase as well. I find that the gold price differential, which was increasing through the 1980s and declined postreform, is a significant predictor of murder rates. An advantage of this approach is that controversies relating to the timing of reforms can be circumvented. In a second, complementary, set of analyses, I use state-level data and run fixed-effects regressions. The response variable is the murder rate, and the set of covariates includes manufacturing output and the strength of the police force. The con-

[3] My data are only for gold, but it is likely that price differentials for other commodities follow a similar pattern.

jecture here is that more industrialized states will be more strongly affected by reforms (in particular, by delicensing and lifting import restrictions). The lifting of controls will have a greater impact there and, by reducing the need to conduct illegal transactions, will lead to a greater reduction in the quantity of violence. Consequently, postreform murder rates of industrialized states will decline relative to murder rates of the less industrialized states. I find support for this hypothesis as well. While the results of the analysis for India strongly support the assertion that a greater incentive for illegal trade leads to more murders, it is natural to ask whether this claim is valid for other countries as well. To this end, I estimate three panel regressions: the effect of the murder rate on trade as a percentage of the gross domestic product (GDP), customs revenue as a percentage of tax revenues, and the black-market exchange rate premium. Restricted to within-country variation, these measures of trade openness are reasonable. The key finding is that greater openness is associated with a lower murder rate.

The paper is organized as follows. Section 2 begins with some background. The data are described, and I proceed to develop the hypotheses to be tested. The empirical results for India are presented in Section 3. Section 4 presents the multicountry analysis. I conclude, in Section 5, with a summary of the main results of the paper.

2. Background: Theory and Data

The Eighteenth Amendment to the U.S. Constitution made the manufacture, sale, or transportation of alcoholic beverages illegal. At different times, other countries have also chosen to impose prohibitions on the sale of alcohol and drugs. A question that has attracted considerable attention concerns the relationship between alcohol or drug prohibition and violence. A claim that is made in support of prohibition is that inebriation causes violence, so prohibition is likely to decrease violence. There is an alternative viewpoint, alluded to previously, based on the fact that prohibitions create black markets. Price differentials between legal and illegal markets make crime attractive. In the economic theory of crime, pioneered by Becker (1968), Stigler (1970), and Ehrlich (1973), rational criminals weigh these rewards against the risk of apprehension and the severity of punishments and make optimal choices. Factors that affect this trade-off have predictable effects on the quantity of illegal trade. Conflicts arise in all manner of trade, but when people engaged in illegal trade need to resolve disputes, there is a greater likelihood that they will resort to violence. As a result, prohibitions will tend to increase violence (Miron and Zweibel 1995; Miron 1999). Increased enforcement of prohibition laws can also increase violence because, for instance, it disrupts the dispute resolution mechanisms put in place by those engaged in illegal trade. Stated another way, prohibitions change the relative costs of different ways of settling disputes and so will have predictable consequences for the quantity of violence. Miron studies the historical behavior of the homicide rate in the United States and finds that "increases in the enforcement of drug and alcohol prohibition have been associated with increases in the homicide rate" (Miron 1999, p. 1).

2.1. Analysis of Data from India

The same logic should apply to the dismantling of controls in India. Consider controls on the import of gold.[4] The controls would be expected to create a differential between the Indian price of gold and the international price of gold (the gold price spread). If this differential is sufficient to compensate for the risks involved in gold smuggling, smuggling would be expected to occur. Commercial disputes among smugglers—arising from the need to enforce agreements or from turf wars—would lead to violence and possibly murder. This suggests that it is reasonable to expect a positive relationship between the gold price spread and the murder rate. The possibility for crime is compounded when one considers the financing of a smuggling operation. The era in question was one of strict foreign exchange controls. The smuggler needed to buy gold in a foreign currency, but final sales were in Indian rupees (which were not convertible). A complementary operation was needed to finance the purchase of gold. This was provided in many instances by the *hawala* system. Illegal currency dealers offered their customers (who might be emigrants sending remittances to relatives in India) attractive exchange rates and greater convenience (relative to the official system). The dealers would take the foreign exchange in, say, Dubai, and an associate would deliver Indian rupees to their proper destination (see Upadhyay 2001; Virmani 2001). This financing system also required an enforcement mechanism. It is possible that mutual trust between a set of individuals interacting repeatedly was sufficient, but one cannot rule out the possibility that compliance was achieved by threats and acts of violence. Smuggling also sometimes required the complicity of customs officials and other members of the government and bureaucracy. The web of illegal exchanges needed to facilitate the smuggling operation created further opportunities for violence. Completing the circle, illegal income that could not be deposited into commercial banks was often stored as gold (as a hedge against rampant inflation).

What was true for gold was likely true for a number of other consumer goods (such as liquor and electronics) as well as industrial inputs (such as polyester filament yarn). The focus here is on gold smuggling for two reasons—first, because of its sheer importance (by some estimates almost 200 metric tons of gold were being smuggled annually into India around 1990)[5] and, second, because

[4] India is the world's largest consumer of gold, accounting currently for approximately 15–20 percent of world demand. Participation in equity markets is still not very widespread, and gold has long been regarded by Indian households as an investment and a store of value.

[5] Gold smugglers such as Haji Mastan and Yusuf Patel were as well known in India as Al Capone was in the United States.

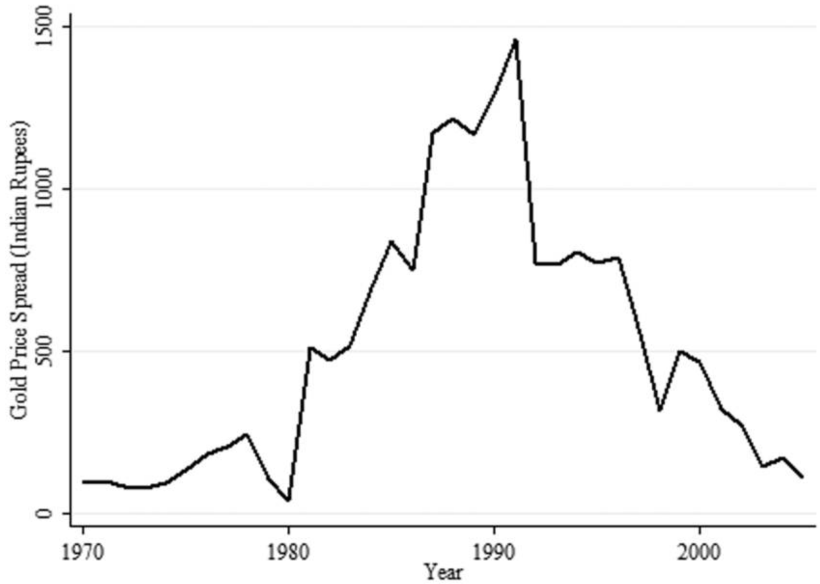

Figure 2. Differentials between the London and Mumbai prices of 10 grams of gold

of the availability of data.[6] The key data series used comprises the differences between the Mumbai and London prices of gold.[7] This series—the differences in the prices of 10 grams of gold—is depicted in Figure 2. There is clear evidence of the effect of the removal of gold controls, which started in 1992, in the decline in the gold price spread. An increase in the price spread over the 1980s is also observed. The first set of analyses examines the nature of the association between the gold price spread and the murder rate. The hypothesis of a positive relationship between these two quantities is confirmed by an analysis of the data.

In a separate, complementary, analysis, I examine state-level differences in murder rates (1980–2006). The focus here is on the delicensing of production that occurred after the 1991 reforms. During the so-called license raj, there was widespread corruption in the allocation of licenses, rampant evasion of controls, and corruption of the inspection and enforcement process. The system of controls gave rise to a nexus of corrupt relationships with its own enforcement problems. The *Vohra Report* of 1992 studied the extent of the problem but has not been

[6] Although tariff data are available, they are more difficult to use because tariffs are only one form of import control. Indeed, there were phases of tariffication in which nontariff barriers were converted to tariffs, so the extent of protection could have been reduced even as tariffs increased. At other times, nontariff barriers were likely increased even as tariffs were decreased. Any use of tariff data, without simultaneously considering the (harder to obtain) data on nontariff restraints, would be highly questionable.

[7] Data are from table 41 of Reserve Bank of India (2008).

made wholly public. Indirect and partial accounts of its contents are available, including the following quotes: "The nexus between the criminal gangs, police, bureaucracy and politicians has come out clearly in various parts of the country" and "some political leaders become the leaders of these gangs/armed *senas* and over the years get themselves elected to local bodies, State assemblies, and national parliament" (Government of India 2010, p. 7).[8] If parties to transactions do not have access to a dispute resolution mechanism, then the possibility of violence would be present.

The state-level analysis is limited to 15 major states.[9] The number of states has increased over time, as three of these states were split in 2000. I aggregate data on the basis of old state boundaries. In an ideal experiment, some random subset of these states would be selected to undergo liberalization (the treatment). One could then compare the differences between the pre- and postreform murder rates across the treatment and control groups to make causal inferences about the effect of reforms. Unfortunately, such a comparison is not feasible. Reforms were enacted at the national level, affecting all states. Hence, separation into treatment and control groups is not possible. However, a significant number of reforms (such as delicensing and lifting import controls on industrial inputs) most directly affected manufacturing, and states do differ with respect to their level of manufacturing activity. Consequently, real manufacturing output can be taken as a measure of the treatment. The idea behind this test is that once reforms were enacted, there would have been a greater decline in illegal activity (stemming from an avoidance of controls) in more heavily industrialized states. Such states, it is hypothesized, would see a greater decline in murder rates (relative to less industrialized states). This hypothesis is tested in the context of a fixed-effects regression model. I use state fixed effects to control for unobserved state-level heterogeneity and time fixed effects to control for macroeconomic shocks and technological improvements (for example, in trauma care).

Descriptive statistics for the key variables used in the analysis are presented in Table 1. The data on murders and police strength are collected by the National Crime Records Bureau (Government of India 1980–2006), and data were input manually from various issues of its publication *Crime in India* (data since 2000 are available in electronic form). Manufacturing and agricultural output figures are part of the State Domestic Product series and were downloaded from the Web site of the Ministry of Statistics and Programme Implementation (Government of India 1980–2006). These are converted to constant 1980 rupees using

[8] I do not intend to suggest that liberalization has caused the problem of criminalization of politics to disappear.

[9] Most studies focus on the 16 major states. Jammu and Kashmir is excluded here since, because of the decades-long unrest, data for some years for some variables were unavailable. Available data are likely unreliable. Results are found to be sensitive to the inclusion of data from Punjab, which was home to an insurgency at approximately the same time. Indeed, the violence peaked around 1991 and then declined. Results both with and without the Punjab data are presented here.

Table 1
Descriptive Statistics

Variable	Prereform Mean	Prereform SD	Postreform Mean	Postreform SD
Murders per 100,000 people (all India)[a]	3.3091	.3713	3.7896	.5276
Gold price spread (units of gold)[a]	.4185	.1617	.1390	.1320
Real manufacturing output (100,000 million rupees)[b]	19.8	15.4	43.6	33.4
Real per capita state gross domestic product (rupees)[b]	23,370	10,585	42,373	21,938
Police officers per 100,000 people[b]	1.44	.65	1.53	.95

[a] The prereform period is 1973–90; the postreform period is 1991–2005.
[b] The prereform period is 1980–90; the postreform period is 1991–2006.

the wholesale price index. The data used to construct the gold price spread are from Reserve Bank of India (2008).

The reliability of data is a valid concern. First, as evidenced by Table 1, India is a thinly policed country and crimes are possibly underreported. The Indian news media is rife with reports of instances in which the police refuse to record crimes.[10] However, because a corpse offers indisputable evidence, it is probably the case that murder data are more reliable than data on other types of crimes (for example, rape, kidnapping, or abduction). Even if the police were to attempt to suppress evidence of a murder, the aggrieved party can approach courts directly. For the analysis here, underreporting is not per se a problem, unless there are hidden systematic trends in the nature of reporting.[11] Another worrisome possibility surrounds the fact that when a body is discovered, the police need to determine whether foul play is involved. This introduces an element of subjectivity in the murder numbers.[12] However, I have found no reports to suggest that there was a systematic change in either reporting or classification trends in the years following 1991. It may be that there have been systematic changes over time in the reporting of certain categories of murders (for example, violence against women) and that these changes differed across states. But it would be curious for the timing, and for a correlation with included variables, to be such as to generate these results as spurious effects.

While there have been numerous amendments, the basic structure of the Indian Penal Code is little changed since the nineteenth century. For the period of this study, one significant change was Section 304B. This 1986 amendment

[10] A comment—only half in jest—I heard from an Indian police officer while discussing this research was that "if crime goes down this has to be because of better policing; if it goes up it must be a result of more conscientious recording of crimes."

[11] It is sometimes suggested that reporting increases with economic development (Soares 2008), but in this case one would see an increase in crime postliberalization, instead of a decrease.

[12] This study focuses on culpable homicide amounting to murder (Section 299 of the Indian Penal Code). There may be an additional element of subjectivity relating to the category of culpable homicide not amounting to murder (homicide mitigated by provocation).

is the Dowry Deaths Law: "Where the death of a woman is caused by any burns or bodily injury or occurs otherwise than under normal circumstances within seven years of her marriage and it is shown that soon before her death she was subjected to cruelty or harassment by her husband or any relative of her husband for, or in connection with, any demand for dowry, such death shall be called 'dowry death' and such husband or relative shall be deemed to have caused her death." The intent of the law was not to create a category that was a substitute for murder, and the deaths classified under this law should not affect the results. However, it is worth observing that deaths classified as dowry deaths under Section 304B (as well as Section 304A, Causing Death by Negligence) and suicides create the scope for playing with categories.

Among variables missing from the analysis, two are potentially significant because of their timing. The data set has no variable to capture the potential rise of religious discord and tensions, and there is nothing on intercaste tensions. The demolition of Babri Masjid (a mosque with disputed Muslim origins) by a Hindu mob occurred in December 1992, making this a period of heightened religious tension. There were well-publicized instances of rioting and bombings in 1992, 1993, and 2002. By most accounts, the period of 1990–2000 was one of increased tension. Hence, one would then expect increasing rather than decreasing trends in violence in this period (as we see from Figure 1). In 1990, the V. P. Singh government implemented the Mandal Commission recommendations increasing the reservation quota for government jobs. This led to student demonstrations in many Indian cities. While the protests may have led to violence, it is also possible that better access to jobs by some previously disadvantaged groups resulted in a decrease in violence. However, by the logic of the latter claim, the greatest decline in violence should occur in the least industrialized states, where access to government jobs is most highly valued. On the contrary, murders declined most in heavily industrialized states. Finally, I do not include any of the (necessarily imprecise) data on corruption in the analysis.[13]

A final shortcoming of the state-level analysis is that, although there are several years of data, there are only 15 states. Since there is the need to cluster data to account for nonindependence of observations within a state, this is a limitation. Murder data for the period in question are available at the district level. Unfortunately, there is no corresponding series of covariates. I compensate for this limitation by conducting extensive robustness checks.

2.2. Analysis of International Data

Data on the homicide rate for a large set of countries are obtained from the United Nations Office on Drugs and Crimes, which has conducted several surveys

[13] It is not obvious that corruption declined postreform. Corruption relating to government procurement could have increased even as corruption relating to production controls decreased. Indeed, Prime Minister Narasimha Rao—the architect of reforms—was under a cloud relating to his alleged involvement in the *Jain Hawala* case.

on crime trends and operations of the criminal justice system. The focus is from the fourth survey until the tenth survey, covering the years 1986 to 2006 (United Nations Office on Drugs and Crime 1986–2006). The variable of interest is the number of recorded intentional (completed) homicides, with the rate per 100,000 persons being the dependent variable in the model. In later years, the homicide rate is reported in the surveys. However, for the fourth and fifth surveys, the rates were computed with population numbers for those years. It is important to note that the panel is unbalanced—rarely did countries report their crime statistics in every year, and most countries report them for only a few years. Overall, there are data from 118 countries, but that number is reduced for some of the regression specifications estimated. It is not possible to construct a panel of crime data in which a major liberalization episode (of the kind that India experienced in 1991) occurs within the sample period. However, since trade statistics are readily available, the empirical strategy is to test whether increased trade openness is associated with lower homicide rates. Trade data are from the World Development Indicators (World Bank 2010). Trade (as a percentage of the GDP), customs and import duties (as a percentage of tax revenues), and tariffs (the weighted mean of the applied tariff rate on all goods) are used as measures of trade openness. The per capita GDP and the inflation rate from the same source are used as controls. A natural extension of the analysis of the gold price spread for India is to use the black-market exchange rate premium as a measure of the distortions in international trade and currency markets. For this, I use the data set from Easterly (2001).[14]

3. Empirical Results: India

I first present, in Section 3.1, the results for the relationship between the murder rate and a direct measure of the incentive for illegal activity (the gold price spread). This is followed, in Section 3.2, by an analysis of state-level data, in which I test whether the level of manufacturing output (a measure of the extent to which a state is affected by reforms) is informative about the decline in postreform murder rates. The two sections deal with different dimensions of reform—Section 3.1 deals with trade restrictions, while Section 3.2 deals additionally with restrictions on production.

3.1. Murder and the Price of Gold

The question of interest is the following: do changes in the gold price spread lead to changes in the murder rate? The spread between the Mumbai and London prices of 10 grams of gold in rupees (graphed in Figure 2) is converted into units of gold by dividing the spread by the London price in rupees. In the rest of the paper, the gold price spread refers specifically to this quantity. The focus

[14] The data are from Macro Time Series, in World Bank, Global Development Network Growth Database (http://go.worldbank.org/ZSQKYFU6J0).

is on the post–Bretton Woods era (1973–2005).[15] This series is plotted in Figure 3 together with the other variable in the analysis—the murder rate (per 100,000 people). Upon visual inspection, the two series appear not to be stationary but do seem to move together. While the comovement postreform is to be expected (since declines in the price spread were a consequence of the loosening of controls), it is reasonable to ask why this should be the case prereform. Since gold production amounts in India are negligible, any increases in demand have to be met from international sources. Absent legal imports, such demand increases would cause the price spread to increase, making smuggling more attractive. With greater smuggling activity, one would expect to see more violent crime.[16]

To check for stationarity of the series, I apply the augmented Dickey-Fuller test for a unit root. At a 1 percent significance level, the null hypothesis of a unit root is accepted when the two series are in levels, but it is rejected when they are in first differences (the MacKinnon P-value is .2418 for the gold price spread in levels and less than .0001 for differences; the corresponding values for murders per 100,000 people are .4367 and .0079, respectively). So both series appear to be $I(1)$. To test for cointegration, I apply the Johansen (1988, 1991) methodology, finding that one cointegrating relationship exists between the two variables.[17] For cointegrated systems, the vector autoregression (VAR) in first differences will be misspecified, whereas the VAR in levels ignores constraints on the coefficient matrices (which are, however, satisfied asymptotically). The appropriate approach, when the time-series system includes integrated variables of order one and cointegrating relations, is to use a vector error-correction model (VECM) rather than a VAR (Engle and Granger 1987). This results in more efficient estimates and more accurate forecasts. Results from both the VAR and the VECM are presented here. In estimating the VAR model, the optimal lag length was determined to be one. Lag length selection was performed using both the Akaike and Bayesian information criteria, and in this instance they both indicate that the optimal model has a single lag. The appropriate VAR(1) model is then

$$\begin{aligned} M_t &= c_1 + \alpha_1 y_{t-1} + \beta_1 M_{t-1} + \varepsilon_{1t}; \\ y_t &= c_2 + \alpha_2 y_{t-1} + \beta_2 M_{t-1} + \varepsilon_{2t}, \end{aligned} \tag{1}$$

[15] The end of U.S. gold-dollar convertibility occurred in August 1971, and the price of gold increased rapidly for some time. There was a period of adjustment to the new regime, which creates some ambiguity about the appropriate start date. I have chosen to use 1973 but have carefully explored the sensitivity of results to this choice. Results are robust, and the choice is not critical.

[16] In principle, it is possible to find the opposite relationship as well. For instance, if risks of smuggling increase, there could be less smuggling, while smaller quantities of gold are traded on Indian markets at higher prices. Since India is thinly policed with a vast coastline, the demand explanation appears to be more plausible. However, which relationship is observed can only be settled empirically.

[17] The trace statistic for the null hypothesis of a maximum rank of 0 is 19.5024, whereas the 5 percent critical value is 15.41; the trace statistic for the null hypothesis of a maximum rank of 1 is 1.0281, whereas the 5 percent critical value is 3.76.

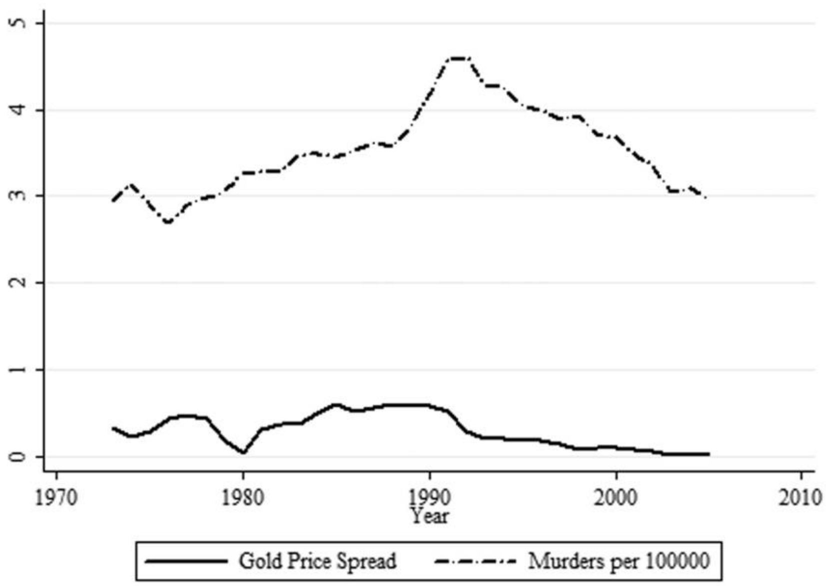

Figure 3. The murder rate and price spread for 10 grams of gold (in units of London gold), 1973–2005.

where M_t denotes the murder rate (per 100,000 people) in year t and y_t is the gold price spread. Estimation results are presented in Table 2. Note that the first lag in the gold price spread is a good predictor of the murder rate, but lagged values of the murder rate do not predict the gold price spread well. Granger causality Wald tests reveal the same thing: the null hypothesis of no Granger causality from the gold price spread to the murder rate can be rejected ($P <$ .001), but the null hypothesis of no Granger causality from the murder rate to the gold price spread cannot be rejected ($P = .630$).[18]

This analysis is not focused just on the 1991 reforms. Of course, there was a sharp decline in the gold price spread (and, hence, the incentive to smuggle) following the reform of gold controls after 1992. But in the prereform period, in the years leading up to 1991, there was an increase in the gold price spread.[19]

[18] I conduct a Lagrange multiplier test to check for autocorrelation and am unable to reject the null hypothesis of no autocorrelation at the chosen lag order. The eigenvalue stability condition is satisfied, as all eigenvalues lie inside the unit circle. The Jarque-Bera, skewness, and Kurtosis tests fail to reject the hypothesis of normality of residuals.

[19] Regulations involving gold were relatively stable in the years between the Gold Control Act of 1962 and the dismantling of controls starting in 1992. There is evidence of an increase in a variety of other controls during Indira Gandhi's term in office (which ended in 1984) and some relaxation after that when Rajiv Gandhi took office. India produces negligible amounts of gold, and increasing demand for gold over time could have contributed to the increase in the price spread. This was an inflationary period, and gold was considered to hold a particularly high store of value.

Table 2
Vector Autoregression for the Murder Rate and the Gold Price Spread, All India, 1973–2005

	Murder Rate (t)	Gold Price Spread (t)
Murder rate ($t-1$)	.9243**	−.0174
	(19.77)	(.48)
Gold price spread ($t-1$)	.5524**	.8362**
	(4.57)	(8.97)
Constant	.0982	.0950
	(.57)	(.72)
R^2	.9252	.7102

Note. Absolute values of z-statistics are in parentheses. $N = 33$.
** $P = .01$.

The murder rate shows a tendency to match both these movements with a lag. Overall, a unit increase in the gold price spread is seen to lead to an increase of .5524 in the murder rate.

The VECM specification estimated is

$$\Delta M_t = c_1 + \alpha_1(M_{t-1} - \theta_0 - \theta_1 y_{t-1}) + \varepsilon_{1t};$$
$$\Delta y_t = c_2 + \alpha_2(M_{t-1} - \theta_0 - \theta_1 y_{t-1}) + \varepsilon_{2t}, \tag{2}$$

where Δ is the difference operator. Note that, corresponding to a lag of one in the VAR, this specification does not contain the lagged differences (ΔM_{t-1} and Δy_{t-1}) as regressors.[20] The implication is that there is only a long-run equilibrium relationship between M_t and y_t, and short-run dynamics of neither series have any effect on the other. The coefficients α_1 and α_2 on the error-correction term give the long-run correction response. The prediction is that $\alpha_1 < 0$ and $\alpha_2 = 0$—as the spread between M_{t-1} and y_{t-1} increases, y_t does not respond at all, but M_t decreases to close this larger-than-usual gap. The estimation results are presented in Table 3. The Lagrange multiplier test results indicate no autocorrelation at lag order, the eigenvalue stability condition is satisfied, and the hypothesis of normality of residuals cannot be rejected (these test results are not reported here).

The signs of the estimated coefficients are consistent with the hypothesis. The gold price spread series is the difference in the price of gold in two markets. In the absence of controls, the values of these numbers should be close to zero. The measured departures are indicative of restrictions in the trade of gold. Increases in the spread are associated with increases in the murder rate, whereas decreases are associated with reductions in the murder rate. In particular, what I find is that an increase in the spread between the two series, caused, for instance, by a reduction in y_t, is followed by a decrease in the murder rate as it adjusts to close this larger-than-usual gap.

[20] When lagged values of ΔM_t and Δy_t are added to the vector error-correction model specification, their coefficients turn out not to be statistically significant.

Table 3

Vector Error-Correction Model for the Murder Rate and the Gold Price Spread, All India, 1973–2005

	Δ Murder Rate (t)	Δ Gold Price Spread (t)
Error correction term ($t-1$)	−.0669**	.0162
	(4.73)	(1.46)
Constant	−.0038	−.0156
	(.15)	(.81)
R^2	.4199	.0893

Note. Absolute values of z-statistics are in parentheses. $N = 33$.
** $P = .01$.

3.2. State-Level Analysis

A second set of analyses examines whether murder rates in more heavily industrialized states declined (postreform) relative to rates in less industrialized states. Such a decline may be expected, since the more industrialized states would have been more greatly affected by delicensing and related reforms. To test this, I focus on the coefficient of the interaction between the log of real manufacturing output (x_{it}) and a dummy variable (d_{it}) for reform (equal to one from the year 1991 onward and zero before then). The regression equation estimated is

$$\ln(M_{it}) = \alpha_i + \beta_t + \gamma[\ln(x_{it}) \times d_{it}] + \theta \boldsymbol{Z}_{it} + \varepsilon_{it}, \tag{3}$$

where $\boldsymbol{Z}_{it}$ is a vector of variables that vary across state and across time. Data are for the years 1980–2006. I include state fixed effects (α_i) to control for (time-invariant) state-specific determinants of the murder rate, time fixed effects (β_t) to control for time trends, and a stochastic error term, ε_{it}. Standard errors are clustered by state. This addresses serial correlation and heteroskedasticity concerns while also allowing for the possibility that observations from the same state are not independent.[21] Of interest here is the sign of the coefficient γ (which is expected to be negative). The main effect, $\ln(x_{it})$, is included in $\boldsymbol{Z}_{it}$, and its coefficient measures how the murder rate varies with real manufacturing output per capita prereform. Coefficient γ measures the difference between the pre- and postreform effect of x_{it} on M_{it}. So, for instance, if the postreform murder rate decreases more for the highly industrialized states, then γ would have a negative sign. The results from an estimation of this model are presented in columns 1 and 2 of Table 4. The Punjab insurgency of the 1980s, and the resulting violence that peaked in around 1991, is a potential problem because of its timing. Consequently, I also estimate the model after excluding data from Punjab. These results are presented in columns 3 and 4.

In columns 1 and 3 of Table 4, $\ln(x_{it})$ and $\ln(x_{it}) \times d_{it}$ are the explanatory variables (in addition to time and state fixed effects). In addition, in columns 2 and 4, police strength is included as a control. A main effect for reforms (d_{it})

[21] After conducting the Wooldridge test for autocorrelation in panel data, I cannot reject the null hypothesis of no first-order autocorrelation.

Table 4
Determinants of the Murder Rate

Variable	Including Punjab (1)	(2)	Excluding Punjab (3)	(4)
Log of real manufacturing output	.183	.226⁺	.194	.201
	(.121)	(.126)	(.113)	(.117)
Log of real manufacturing output × Dummy91	−.100⁺	−.128*	−.124*	−.129*
	(.0548)	(.0468)	(.0487)	(.0442)
Police officers per 100,000 people		−.219*		−.0630
		(.0994)		(.123)
Constant	−1.885	−2.303	−2.078	−2.113
	(1.936)	(1.977)	(1.822)	(1.848)
N	400	400	373	373
R^2	.743	.755	.823	.824
Number of clusters	15	15	14	14

Note. Values are the log of murders per 100,000 people in fixed-effects regressions for a panel of Indian states. Robust standard errors, adjusted for within-cluster correlation, are in parentheses. Country and year fixed effects are included for all regressions.
⁺ $P < .1$.
* $P < .05$.

is made unnecessary by the inclusion of time fixed effects.[22] The coefficient γ of the interaction term is, as expected, negative in all four specifications. The table reports statistical significance for a two-tailed test. The coefficient of manufacturing output tends not to be statistically significant (except for the marginal significance in column 2). This suggests that variations in manufacturing output in the prereform period do not lead to changes in the murder rate. In fact, when data from only the postreform period are used, manufacturing output is again unrelated to the murder rate. Apparently, it is not increases in output that affect murder rates. Rather, it appears that states with high manufacturing output witness a relative decline in postreform murder rates. I have estimated (but do not report results from) a model that additionally includes real per capita state GDP as a regressor. This is not statistically significant (and the key coefficient of the interaction term remains unchanged and is still statistically significant).

I also find that increases in police strength are associated with a decrease in the murder rate. The key result—the negative relationship between the murder rate and $\ln(x_{it}) \times d_{it}$—continues to hold when data for Punjab are excluded. However, the relationship between the murder rate and police strength is almost entirely driven by the Punjab effect. Police strength ceases to be statistically significant when Punjab data are excluded (and the magnitude of the coefficient becomes much smaller). It is worth noting that the Punjab insurgency involved a large number of murders stemming from religious strife between the Hindu

[22] The case for the inclusion of other variables is not as compelling. Using data from Aghion et al. (2008) with data ranging from 1980 to 1997, I replicated the results. The key results continue to hold when there are controls for agricultural output, poverty head count ratio, and population. However, because of the small number of clusters, parsimony in controls is appropriate.

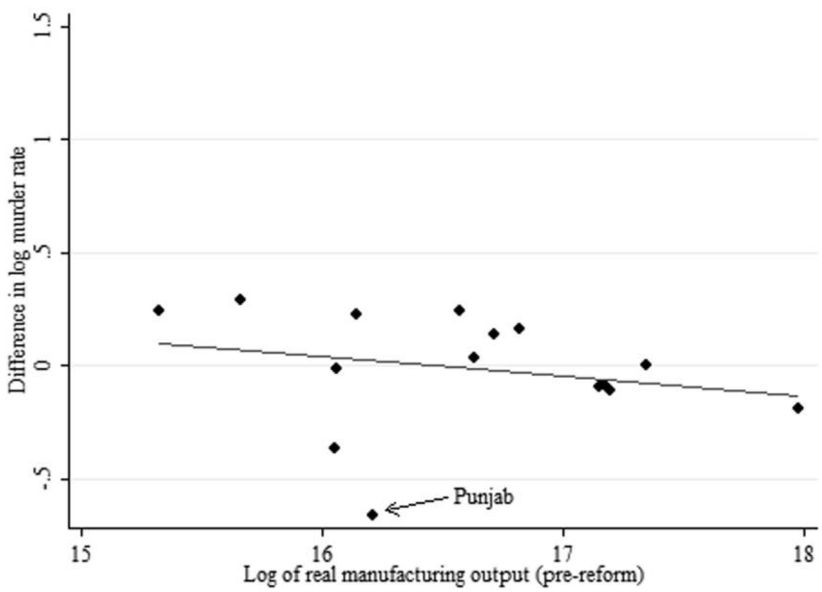

Figure 4. Effect of economic reforms on the murder rate: major Indian states

and Sikh communities and was contained with brutal police action. The prereform murder rate and the postreform police presence are both of a much greater magnitude in Punjab than in other states.

As a variation on the above analysis, I take an approach that has been found to alleviate concerns about serial correlation and small numbers of clusters. This is to average values of variables pre- and postreform (see Bertrand, Duflo, and Mullainathan 2004). The real manufacturing output per capita is averaged separately for the pre- and postreform periods. Similarly, the murder rates over the two eras are averaged. Time fixed effects are added by including a dummy variable equal to one for the postreform period and equal to zero for the prereform period. The corresponding regression specification is

$$\ln(M_{it}) = \alpha_i + \beta_t + \gamma[\ln(x_0) \times d_{it}] + \varepsilon_{it} \quad (t = 0, 1), \tag{4}$$

where x_0 is the prereform average manufacturing output and d_{it} equals zero prereform and equals one postreform. The coefficient of the interaction term is, in essence, the differences-in-differences estimate of the effects of reform. The parameter γ can also be estimated from the regression of $\Delta\ln(M_{it})$ on $\ln(x_0)$ (see Figure 4). Results of the estimated regression equation (including a control for policing) are presented in column 1 of Table 5. Column 2 is the same equation but with Punjab excluded from the sample. Column 3 presents results for a robust regression (using Huber weights to reduce the influence of outliers). This

Table 5
The Effect of Reforms

Variable	(1)	(2)	(3)
Log of real manufacturing output (prereform)	−.153⁺	−.135*	−.187**
	(.0769)	(.0567)	(.0468)
Police officers per 100,000 people	−.345**	−.0703	−.0815
	(.0948)	(.110)	(.119)
Constant	2.987*	2.367*	3.282**
	(1.323)	(1.007)	(.828)
N	15	14	14
R^2	.496	.242	

Note. Values are the difference in the log of murders per 100,000 people averaged over the pre- and postreform periods. Robust standard errors are in parentheses.
⁺ $P < .1$.
* $P < .05$.
** $P < .01$.

is a useful check on the robustness of results (Punjab data are automatically excluded by this procedure). The value of γ (when all 15 states are included in the sample) is $-.153$ (with a P-value of .071 for a two-tailed test). When Punjab is excluded, the coefficient estimate is $-.135$ ($P = .036$). In the postreform period, the murder rates in the high-output manufacturing states declined relative to those in the low-output manufacturing states.[23]

Several checks were conducted to establish the robustness of results. In addition to a robust regression to reduce the effect of outliers, I examined the stability of estimates and standard errors when individual (and groups of) regressors were included and excluded. I also examined the effect of excluding individual states to ensure that results were not driven by an outlier state. I further considered the effects of changing the time horizon under consideration, although for results reported here, as much data were used as were available (the exception being that the focus in Section 3.1 is on only the post–Bretton Woods era). The results hold up very well to these different robustness checks.

4. Empirical Results: International Data

Although striking, the results in Section 3 focus on the experiences of a single country. Any general claim about the existence of an equilibrium relationship between violent crime and the incentive for illegal activity would need to rest on more. However, India appears almost unique in terms of the ready availability

[23] Further support for this interpretation of results is provided by the fact that agricultural output appears not to have an effect on murder rates (in an identical analysis including the Aghion et al. [2008] data). This suggests that it is not output or income that matters but the amount of manufacturing. Also, as noted above, the inclusion of real per capita state gross domestic product has no effect. However, I do find that the poverty head count ratio has a positive effect on the murder rate. This relationship is consistent with the finding of Iyer (2009) that conflict and violence have been increasing in South Asia, especially in economically lagging regions.

of detailed and reliable data over a period during which significant liberalization took place. The change occurred without a revolution or attending political turmoil that would—among other things—affect the quality of data. There are studies (for instance, Sachs and Warner 1995; Wacziarg and Welch 2008) that have tried to date trade liberalization with a view to establishing a relationship with economic performance.[24] And, of course, high-quality macroeconomic data are readily available. What is more difficult to construct is a panel of crime data for which the liberalization episode occurs during the sample period. On the other hand, there are reasonable measures for trade openness and a good direct measure of the incentive for illegal activity (to parallel the analysis with the gold price spread)—the black-market exchange rate premium.

Consequently, I conducted the following three related analyses. In the first and more indirect approach, I use trade (as a percentage of the GDP) as a proxy for trade openness and investigate whether it is related to a country's murder rate. The measure would change as a consequence of broad-based trade liberalization but also in response to falling trade barriers that may not qualify as radical regime changes. In either case, incentives for illegal activity would be reduced, with a concomitant likelihood of reduced violence.[25] The trade percentage has a deservedly bad reputation in cross-sectional analysis (an obvious objection being that factors other than openness affect it—for example, that large countries trade less). However, since panel data are available and attention is restricted to within-country variations in the trade percentage, the objections do not have much force here. I ask, in effect, whether increases in a country's trade as a percentage of its GDP are negatively related to its murder rate. Country fixed effects account for country-specific factors (such as country size) that affect trade, while time fixed effects account for secular effects (such as innovations in transportation).

Previous studies (for instance, Wacziarg and Welch 2008) have found that the trade percentage increases upon liberalization. But it may be affected by other factors as well. In recent decades, there has been a significant increase in trade relative to output across countries. An important puzzle, this phenomenon has been studied by Rose (1991), Baier and Bergstrand (2001), Obstfeld and Rogoff (2000), Dean and Sebastia-Barriel (2004), and others. The explanations for the increase in trade considered in this literature include (1) reductions in tariff and nontariff barriers, (2) decreasing transportation, insurance, and freight costs, (3) increases in per capita income, and (4) productivity gains in the tradable goods sector. There is considerable support for falling trade barriers as an explanation and less agreement about the importance of the other options. Of these others,

[24] Interestingly, Sachs and Warner (1995) and Wacziarg and Welch (2008) disagree on whether India can be said to have liberalized in 1991. The issues are not germane to the analysis in this paper. The important point is that in 1991 and shortly thereafter, the incentive for illegal activity decreased significantly (even if there were, for a few years after, significant trade restrictions in place).

[25] In other words, the theoretical arguments that underpin the relationship being tested here do not require a radical and broad-based policy shift of the kind that occurred in India starting in 1991.

Table 6
Relationship between Trade and Murders for a Panel of Countries, 1986–2006

Variable	(1)	(2)	(3)
Trade (percentage of GDP)	−.00226**	−.00163**	
	(.000485)	(.000404)	
Log of customs revenue (percentage of tax revenue)			.03687**
			(.012563)
Log of GDP per capita		−.333*	−.31396
		(.154)	(.269736)
Log of the inflation rate		.0378*	.08308**
		(.0165)	(.031731)
Constant	1.246**	4.046**	3.68319
	(.0883)	(1.368)	(2.46731)
N	1,010	889	429
R^2	.931	.936	.945
Number of clusters	118	102	88

Note. Values are the log of murders per 100,000 people. Robust standard errors, adjusted for within-cluster correlation, are in parentheses. Country and year fixed effects are included for all regressions. GDP = gross domestic product.
* $P < .05$.
** $P < .01$.

per capita income is plausibly related to the crime rate, and so per capita GDP is included as a control. Note that since falling trade barriers induce relative price changes between tradable and nontradable goods, the relevant trade percentage measure is real, not nominal.[26]

The second analysis focuses directly on measures of trade restrictions. The variable included is customs and other import duties (as a percentage of tax revenues). The analysis was also conducted with the weighted mean of the applied tariff rate for all products, but with less success. Neither variable is a comprehensive measure of trade restrictions, because, for example, countries can substitute tariffs with quantitative restrictions, and customs revenues would be expected to fall with prohibitively high duties. But both seem reasonable as proxies. The bigger problem is that (relative to the trade percentage) their inclusion leads to a much smaller number of complete observations. As seen in Table 6, the regression with the log of customs revenue as an exogenous variable has less than half as many observations as the trade percentage regression.

The third approach employed here is to directly generalize the gold price spread analysis for India. It would be difficult to find, for every country, the price spread for commodities that were significant in the smuggling trade. However, a very good alternative is the black-market exchange rate premium (computed as the percentage premium of the parallel exchange rate over the official

[26] A simple example illustrates: if the relative price of tradable goods decreases, while quantity increases, then one can easily imagine a situation in which the total expenditure on tradable goods remains fixed. If the same happens with nontradable goods, the trade percentage will remain unchanged even though the country is trading more. While this example is extreme, in general, nominal trade percentage changes will understate the effect of falling trade barriers.

Table 7
Analysis of the Black-Market Exchange Rate Premium

Variable	(1)	(2)	(3)
Lag of black-market exchange rate premium	.000462*	.000374*	.0003703**
	(.000221)	(.000147)	(.000123)
GDP per capita	-2.47×10^{-5}	-4.37×10^{-5}**	-3.80×10^{-5}**
	(1.55×10^{-5})	(1.28×10^{-5})	(8.87×10^{-6})
Constant	1.251**	1.625**	3.902**
	(.274)	(.227)	(.175)
N	415	393	414
R^2	.050	.202	

Note. Values are the log of murders per 100,000 people. Robust standard errors are in parentheses. Country and year fixed effects are included for all regressions. GDP = gross domestic product. Columns 1 and 2 contain 87 clusters.
* $P < .05$.
** $P < .01$.

exchange rate).[27] This series has been used to good effect in growth regressions (see Durlauf and Quah 1999). As with the gold price spread, departures of the black-market exchange rate premium from zero would be indicative of distortions in international trade and in currency markets. The prediction would be that this is positively related with the murder rate. As in the analysis of the gold price spread, 1-period lagged values of the spread are included as a predictor of murder rates.

The results are reported in Tables 6 and 7. Results from the fixed-effects regression of $\ln(M_{it})$ on trade (as a percentage of the GDP) are in column 1 of Table 6. The prediction is that increases in the trade percentage will lead to reductions in the murder rate. This is what I find—in columns 1 and 2 the coefficients are statistically significant at the 1 percent level. The magnitudes are not large: when the trade percentage increases by 1, the percentage decrease in the murder rate is about .16. For the specification in column 3, the prediction is that an increase in customs revenue will lead to an increase in the murder rate. The finding is that a 1 percent increase in customs revenue (percentage of tax revenues) leads to approximately a .04 percent increase in the murder rate. This effect is significant at the 1 percent level. There is also a negative coefficient for per capita GDP, although the effect is not statistically significant in specification (3), which could be a result of the smaller data set. The inflation rate is included as a measure of economic distress and has a positive relationship with the murder rate.

Table 7 reports results for the black-market exchange rate premium. I find that a greater black-market exchange rate premium is associated with a higher murder rate. As seen in column 1, the results are significant at the 5 percent level. An issue here is that the data set is fairly small (compared to that for trade

[27] I thank an anonymous reviewer for this suggestion.

as a percentage of the GDP), so a careful robustness check is warranted. There are several outliers in the data (measured by the size of residuals in units of the standard deviation of the error term). With an exclusion of outliers, the coefficient and *P*-value of the black-market exchange rate premium are not much affected. However, the coefficient on per capita GDP now becomes statistically significant (column 2). I exclude outliers by removing observations that were down weighted to zero in robust regression. Robust regression results are presented in column 3. This gives smaller *P*-values (although in this case it is not possible to correct for intracluster correlation).

To summarize, the measures of trade openness have a negative relationship to the murder rate. In particular, as a country's trade percentage increases (an effect plausibly caused by falling trade barriers), its murder rate decreases. Further, as customs revenues (as a percentage of tax revenues) decrease, so does the murder rate. And finally, the higher the black-market exchange rate premium, the higher the murder rate. These results are consistent with the previous findings for India and provide broader support for the existence of an equilibrium relationship between the incidence of violent crime and government control of economic transactions.

5. Conclusions

This paper began by exploring the effects of India's liberalization experiment on its murder rate. Then, for a larger set of countries, it investigated the relationship between trade openness and murder. The theoretical basis for a relationship between the two quantities is the well-established link between economic controls and black markets. Since violence is a frequent by-product of illegal economic activity, one would expect the removal of economic controls on trade and production to be accompanied by a decline in the murder rate. One mechanism for this is via the lifting of import controls. Reducing the gap between domestic and international prices of commodities reduces the incentive for illegal trade. Reductions in illegal trade lead, in turn, to less violent crime. As per the predictions, I found that changes in the gold price spread in India are positively related to its murder rates. Extending this to a large group of countries, I found that the black-market exchange rate premium is positively related to the murder rate. Liberalization in India also involved a dismantling of licensing in manufacturing (in addition to the lifting of controls on imports of inputs). The analysis of Indian state-level data provides further evidence that the removal of controls on manufacturing reduced murder rates. States with high levels of manufacturing activity (where, presumably, incentives to contravene controls are strongest) witnessed a decline in the level of crime relative to that in less industrialized states. The evidence suggests that it is not the increase in manufacturing output or income (postreform) that is behind the decline. Rather, the effect appears to be a consequence of the removal of controls. Finally, in a multicountry setting, using a variety of measures of trade openness, I find a robust negative relationship between trade openness and violent crime.

The results here contribute to the literature on the effects of market-based reforms in India and elsewhere. Much information has now been gathered on the effects of reform, both positive and negative. This paper points toward a neglected dimension of the reform experience. A proper accounting of the effects of reform requires that the underground economy be kept in sight. For obvious reasons, this is not easy to do. Murders and other violent crimes constitute one very visible dimension of the societal costs of illegal transactions. Clearly these costs—in terms of the violence they engender—are immense.

This paper provides a test of the theory linking economic controls to violent crime. This theory has been subjected to tests before, for example, in the context of prohibition. What is novel here is the entirely different context—economic liberalization in India and elsewhere. New insights are obtained on the value of social institutions, such as courts of law, for enforcing agreements and resolving disputes. The violence associated with illegal transactions is one measure of the relative inefficiency of alternative mechanisms for managing conflict among trading partners. The results here enable us to formulate new conjectures about the economic determinants of violence. I provide one illustrative instance. The relative efficiency of the legal system (for example, delays in resolving disputes and corruption levels in law enforcement) should also have an effect on the quantity of violence. When the legal system is relatively inefficient, violence becomes more attractive as a means for resolving disputes. For instance, in places where the legal eviction of tenants is made difficult by pro-tenant laws, landlords have been known to hire thugs to help them circumvent the legal system. In India, there has been some movement in the direction of more pro-landlord laws in recent years (for example, the Maharashtra Rent Act of 1999), and the passage of such laws should have predictable effects on violent crime. This would be an attractive area for further study.

Finally, a note of caution on intercountry comparisons is in order. The results should not be taken to suggest that countries with fewer economic controls will, on average, have lower levels of crime. The multicountry analysis focused on within-country variation in measures of trade openness, and country-specific determinants of crime could possibly swamp the effect of fewer controls. So, for instance, the United States has fewer economic controls but more crime than many countries in Europe. Similarly, totalitarian countries might have strict economic controls together with better law and order, and a dismantling of economic controls may come simultaneously with a breakdown in law and order. The end of communism in the former Soviet Union provides such an example: the 1990–2000 period was one of high and increasing crime in Russia. Some of this is likely attributable to the fact that the weakened security infrastructure made for favorable conditions for illicit trafficking in drugs, arms, and people (among other illegal activities). The fact that Indian reforms were unaccompanied by major political turmoil, or significant changes in the law and order infrastructure, makes it an ideal test case. However, even in the case of India, my important claims are not about the absolute levels of crime pre- and postreform (despite the striking story told by Figure 1) but about the marginal effects of the lifting of economic controls.

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