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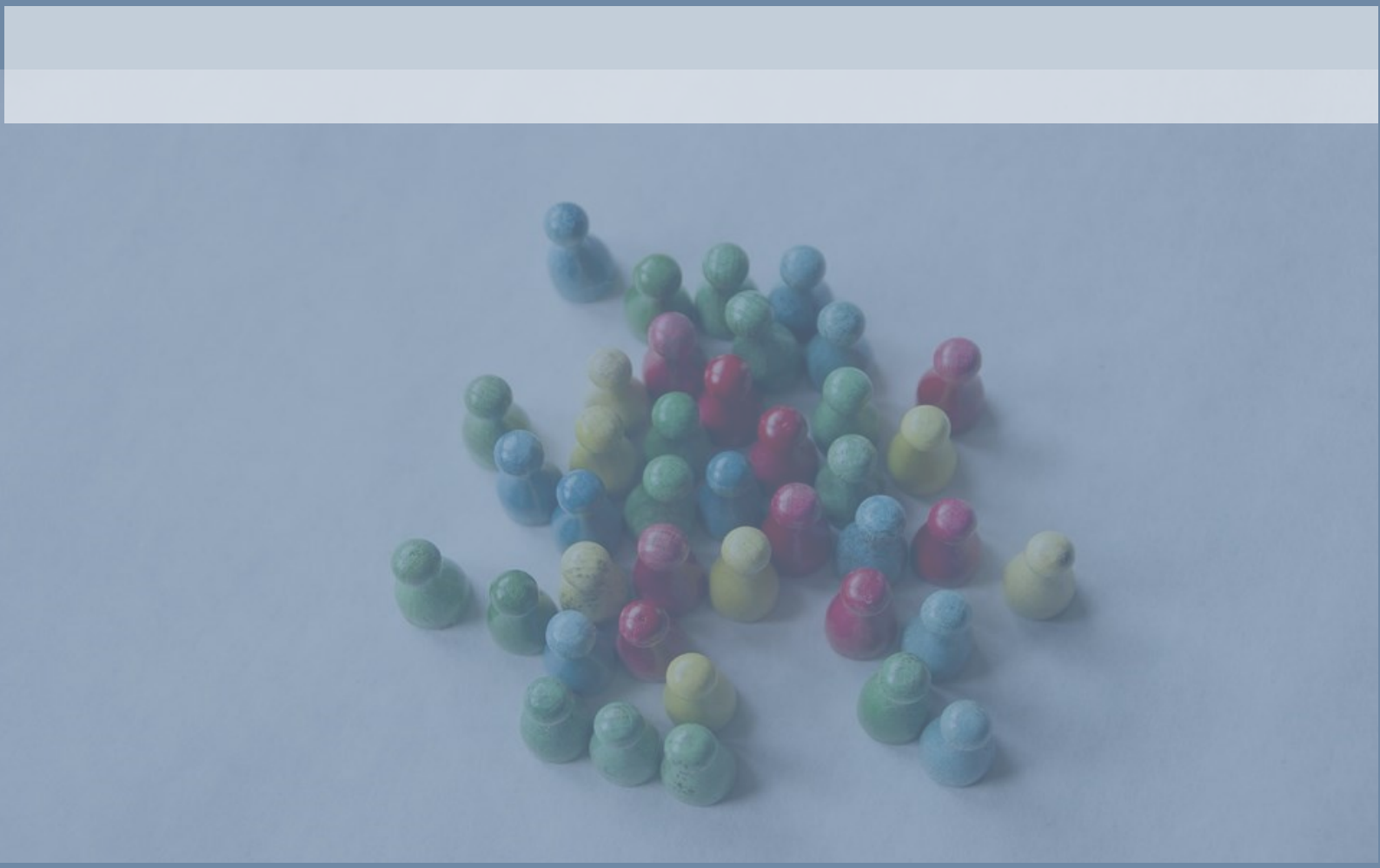

Fear and Loitering in Mexico? The Significance of Age Structure, Education, and Youth Unemployment for Explaining Sub-National Variation in Violent Youth Crime

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Abstract

Violent crime in Mexico occurs at a rate that dwarf the human costs of most contemporary civil wars, and the drug cartels responsible for the violence exercise *de facto* control over significant geographical territories. In this respect, the Mexican 'drug wars' resemble conflicts over the control of rich natural resources in Sub-Saharan Africa and elsewhere, blurring the distinction between 'political' and 'social' or 'criminal' violence. In the civil war literature, a young age structure has been argued to provide inexpensive rebel labor and thus increase opportunities for a rebel group to wage war against a government. Similarly, relatively large groups of 'idle' young men could arguably be a factor that reduces recruitment costs for criminal enterprises through the abundant supply of youth with low opportunity cost. Acknowledging organized crime around drugs trafficking as a major cause of crime and violence in Mexico, we ask whether the availability of large young male cohorts, or male 'youth bulges', low education, and high youth unemployment eases the recruitment to these organizations and may contribute to explain variance in violent crime rates across Mexican states over time. Using panel data covering 32 states in Mexico during the 1997–2010 period, we find that while a coarse measure of regional youth bulges is not associated with patterns of violent youth crime, high youth unemployment in low-education strata is, in particular, in the context of large male youth bulges. These results remain robust against alternative data, sample size, estimation techniques and controls for potential endogeneity concerns.

Keywords

Youth bulge, education, unemployment, violent crime, Mexico.

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1. Introduction

The resemblance between the 'drug wars' in Mexico and many contemporary civil wars over access to natural resources is striking. However, while some scholars have noted the similarities between factors explaining armed conflict and violent crime (e.g. Neumayer 2003: 619) the two phenomena are usually studied separately. This article addresses the issue of violent youth crime in Mexico employing a theoretical framework, the 'opportunity perspective', which has been a dominating narrative in the civil war literature. The framework emphasizes structural factors providing opportunities for potential rebel organizations to launch an insurgency against a state, such as large youth cohorts, or 'youth bulges', as well as other factors that determine economic opportunities for youth like education and unemployment. In the political violence literature it has been noted that 'youth bulges' have historically been associated with times of political crisis and upheaval (Goldstone 1991, 2001) and it has generally been observed that young males are the main protagonists of criminal (Neapolitan 1997: 92, Neumayer 2003: 621) as well as political (Mesquida and Wiener 1996, Elbadawi and Sambanis 2000: 253, Urdal 2006) violence. Generally, the increasing acknowledgement of the role of demographic factors in shaping conflict and international political developments is underscored by recent contributions in the field of political demography (e.g. Goldstone et al. 2012).

Studies of violent crime, particularly studies of homicide rates, have long employed cross-national time-series research designs. Most of these cross-national studies have included few developing countries, however. A much-cited homicide study, Fajnzylber et al. (2002), included only 39 countries, of which the minority were developing countries, citing problems with low data availability for developing countries as well as underreporting. Underreporting, the authors argue, should not be considered random noise, but measurement error that is systematically correlated with factors assumed to affect crime rates (Fajnzylber et al. 2002: 14).

Furthermore, while sub-national panel studies have become prominent in the civil war literature (e.g. Buhaug and Rød 2006, Urdal 2008, Østby et al. 2011, Vadlamannati 2011), similar studies of sub-national violent crime outside the US and a few other developed countries are few and far between. A rare exception is Dreze and Khera's (2000) study of homicide across Indian states. By assessing variation in violent crime within Mexico over time, this study is less prone to measurement error stemming from differences in data collection and reporting procedures across countries, although we acknowledge several potential sources of bias. Furthermore, the subnational focus enables the use of data sources - in particular youth unemployment - that are not available for a large number of countries, and thus may not be used in cross-national studies.

Mexico provides an ideal case for testing propositions about the significance of youth opportunities for violent crime. Demographically, Mexico is a relatively young country with about a third of its current population falling into the age range of 12 to 29 years. The period of study, 1997-2010, covers a time of significant youth population growth in Mexico. According to the Mexican Institute of the Youth, the Mexican population aged 12-29 increased by 40.6% between 1990 and 2000 (Instituto Mexicano de la Juventud 2008). While the overall growth in youth population is slowing down, regional

differences in growth rates still exist due to migration and geographic fertility differentials. Detailed demographic, social, and crime data further allows us to use econometric methods to consider how large youth cohorts in the context of limited education and employment opportunities affect violent crime

This study adds to the existing literature in several ways. The article identifies and discusses youth opportunities and their potential implications for violent youth crime and tests these propositions empirically in one of the first sub-national studies of violent crime in a developing country. It is further the first study to look at youth bulges and violence, either political or criminal, in the context of both education and employment, a unique opportunity arising from the rare availability of such data for Mexican states. Our results suggest that while youth crime and high homicide rates in Mexico are not associated with the ebb-and-flow of the male youth population, both high youth unemployment and low youth education are associated with higher levels of crime and homicide. And in this context, the relative size of the male youth population does matter. We also report further results of notable significance. In particular, there is an increasing concern that rapid urban population growth around the globe could lead to increasing levels of criminal as well as political violence. While this study, generally, finds some support for urban environments being more conducive to violent crime in Mexico, the pace of growth in the urban population does not appear to be associated with crime levels.

2. Theory

The literature on youth bulges and violence has particularly focused on the role of large youth cohorts in facilitating spontaneous and low-intensity political violence. Two different explanatory frameworks have primarily informed the discussion: one focusing on opportunities, and the other on motives for political conflict. The opportunity framework is particularly relevant for explaining criminal violence and has a parallel expression in the literature on violent crime (Neapolitan 1997). Neumayer (2003) notes that 'opportunity theory' "tries to understand variation in violent crime rates in terms of different opportunities or favorable conditions for committing crime". Basing their approach primarily on Gary Becker's (1968) opportunity framework, Fajnzylber et al. (2002: 1-2) contend that, "crime rates depend on the risks and penalties associated with apprehension and also on the difference between the potential gains from crime and the associated opportunity cost".[1] Berman et al. (2011: 499) note that the relevance of the opportunity cost opportunity cost theory for crime is generally supported by sub-national level evidence.

2.1 Youth bulges and violent crime

The opportunity literature, often referred to as the 'greed' perspective (e.g. Collier 2000), has its roots in economic theory and focuses on structural conditions that facilitate an organization's engagement in violent activity: whether that be a rebel group, or a criminal organization. These are conditions that either provide the

[1] Arguably, violent crime may also be driven by feelings of disadvantage or unfairness (Fajnzylber et al. 2002: 2) as emphasized in motive-oriented or relative deprivation studies. However, it is empirically difficult to distinguish between these two types of explanations since they yield largely identical predictions (Urdal 2006).

organization with financial means, such as rents from drug trafficking, or reduce the costs of operation, including costs of recruitment. Relatively large youth cohorts can reduce recruitment costs for insurgent groups through the abundant supply of 'rebel labor' with low opportunity cost, increasing the risk of armed conflict (Collier 2000: 94). Similarly, large youth bulges may depress the cost of recruitment to criminal organizations. Opportunities for violence may be further boosted by a weak government with limited capabilities (Fearon and Laitin 2003, Collier and Hoeffler 2004).

A key assumption is that organizational structures that may be used for illegal purposes, whether political or criminal, exist exogenously, and that recruits join these organizations in order to obtain a private good. Hence, the collective action problem is presumed to be negligible. Organizations are able to recruit successfully only when the potential gain from joining is so high and the expected costs so low that potential recruits will favor joining over alternative income-earning opportunities. Collier (2000: 94) argues that the mere existence of an extraordinarily large pool of youth is a factor that lowers the cost of recruitment since the opportunity cost for a young person is generally low. Hence, our expectation is that:

Hypothesis 1: *In regions with large youth populations relative to the adult population, violent crime rates are higher, everything else being equal*.

However, Hirschi and Travis (1983) argue that age, in and of itself, is an insufficient explanation for violence, and that shifting attention towards the meaning or interpretation of the relationship is required. Hence, in the following we consider two factors that are key determinants of youth opportunities: educational attainment and youth unemployment.

2.2 Educational opportunities and violent crime

Governments can expand educational opportunities in response to youth bulges and hence ease demographic transition problems. Higher levels of education among men arguably act to reduce the risk of political violence, resulting from the higher opportunity cost of rebellion for educated men (Collier 2000). Since educated men generally have better income-earning opportunities than uneducated men, their alternative cost is higher, and they will be less likely to be recruited to criminal organizations.

Hence, higher levels of education are expected to be associated with a reduced risk of violence. While for 'criminal entrepreneurs', a high level of education may in fact lead to higher rewards if it enables more efficient management of illicit activities (Barakat and Urdal 2009), the argument that to the involvement of young people in criminal activity is economically less attractive the more highly educated a person is refers to mass participation. In areas with large potential pools of recruits, increasing education can act to reduce this pool. Although the argument that education increases the opportunity cost for young people takes a broad form, we focus here on secondary education for young males since they are the primary target for recruitment to criminal organizations, and secondary education is typically an entrance requirement to modern-sector employment.

Hypothesis 2: *In regions with low secondary male education levels, violent crime rates are higher, everything else being equal.*

2.3 Youth unemployment and violent crime

Central to the opportunity cost framework is the availability of youth employment opportunities. If the ability in the labor market to absorb a sudden surplus of young job seekers is limited, a large pool of unemployed and frustrated youth with low opportunity cost arises, providing potential recruits for either political or criminal violence (Moller 1968; Choucri 1974; Braungart 1984; Goldstone 1991; Cincotta et al., 2003).

The expectation that exceptionally large youth cohorts increase the supply of cheap recruits for criminal enterprises is further supported by studies in economic demography suggesting that the alternative cost of individuals belonging to larger youth cohorts are generally lower compared to members of smaller cohorts due to higher unemployment and thus increased pressure on male wages (Easterlin 1987, Machunovich 2000: 236). Increases in relative cohort size arguably result in a reduction in male relative income. Such a direct relationship has been found in several studies using wage data for smaller samples of countries (Machunovich 2000: 238, see also Korenman and Neumark 1997). Berman et al. (2011: 500) note that according to opportunity cost theory, recruits to violent crime are drawn not only from among the unemployed, but also from among individuals in low-wage employment.

So not only do youth bulges provide an unusually high supply of individuals with low opportunity cost, as anticipated by Collier (2000), but an individual belonging to a relatively large youth cohort generally also has a lower opportunity cost relative to a young person born into a smaller cohort. While labor markets differ substantially with regard to flexibility, but also within countries, empirical evidence suggests that on average, large youth cohorts are substantially more likely to experience both lower relative wages and higher unemployment rates (Korenman and Neumark 1997). Hence, we expect that:

Hypothesis 3: *In regions with large unemployment among young males, crime rates are higher, everything else being equal.*

Finally, we consider the possible impact of violent crime on the factors leading to low opportunities for young people. Given the expectations that low education and high unemployment among male youth should be associated with increased levels of violent crime, we would further expect that high unemployment in low-education male strata should be particularly strongly-associated with violence, and that the economic opportunities for this group of males may be particularly limited in the context of large male youth bulges.

Hypothesis 4: *The association between large youth cohorts and violent crime is particularly strong in regions where education levels are low and unemployment rates among young males are high, everything else being equal.*

2.4 Existing research

Previous studies have found mixed evidence for a relationship between age structure, or 'youth bulges', and violent crime. Hansmann and Quigley (1982) and Pampel and Gartner (1995) both find a significant impact of age structure on homicide rates in cross-national studies, while Gartner and Parker (1990) find a strong age structure effect on homicide in two (US and Italy) out of five countries, acknowledging that differential patterns within countries may still have affected internal variation in homicide among the remaining three countries. On the other hand, Avison and Loring (1986), Fajnzylber et al. (2002), Neumayer (2003), Cole and Gramajo (2009), and Pridemore (2011) do not find statistically significant effects of age structure on crime among country-level panel data analyses. In a meta-analysis of cross-national homicide studies, Nivette (2011) reports that static population indicators were among the group of variables that exerted the weakest effect on homicide. Fox and Hoelscher (2012) find some initial and strong support for the youth bulge hypothesis, although the relationship washes away once controlling for socioeconomic factors. A possible reservation here is that introducing socioeconomic variables also reduces the sample considerably. However, both Fox and Hoelscher's (2012) results, as well as Neumayer's (2003) finding that economic growth reduces homicides, point to the salience of socioeconomic factors. Hence, what we should be looking for are conditional factors determining youth opportunities.

There appears to be somewhat stronger, albeit by no means unequivocal, evidence for a link between education and violent crime. Cole and Gramajo (2009) find that increasing male education reduces homicide, Fajnzylber et al. (2002) conclude that higher education levels overall are associated with less homicide, while Dreze and Khera (2000) found that higher literacy levels moderated criminal violence levels in India. However, some results appear more puzzling: Cole and Gramajo (2009) found that higher female education was associated with higher homicide levels, while Fajnzylber et al. (2002) unexpectedly found that higher education was associated with higher levels of robbery. Furthermore, Pridemore (2011) reports inconclusive results with regards to education, while Robbins and Pettinicchio (2012) only finds weak support for the assumed beneficial effects of social capital on homicide.

While previous studies have identified a theoretical link between youth unemployment and violence, the lack of reliable youth unemployment data for many developing countries has made cross-national assessment of this relationship difficult. Several studies have rather tested the opportunity cost framework using measures of overall economic performance, assuming that youth unemployment will generally be affected by poor economic performance. Low economic growth has been identified as a robust predictor of both homicide (Neumayer, 2003) and the onset of civil war (Collier et al. 2003, Sambanis 2002: 229). Two cross-national studies including unemployment data for a limited number of developing countries and studying the impact of national-level unemployment rates on crime came to different conclusions. Pampel and Gartner (1995) found no effect of unemployment on homicide rates, while Neumayer (2005) reported that higher unemployment rates were found to increase levels of both robbery and homicide. In a rare meso-level analysis of sub-national level unemployment and violence data spanning Afghanistan, Iraq, and the Philippines, Berman et al. (2011)

found no relationship between local-level unemployment rates and insurgent attacks that kill civilians. While empirically sophisticated and a significant improvement over national-level analyses, the study is limited by the lack of age-specific unemployment rates. Providing more supportive evidence for the opportunity cost framework, Blattman & Annan (2016) found that, among Liberian ex-fighters, illicit and mercenary activities declined as their engagement in ordinary, peaceful work increased.

In the civil war literature there has been a certain discussion about the measurement of age structure (Urdal 2006, Barakat and Urdal 2009). Like two authoritative civil war studies by Fearon and Laitin (2003) and Collier and Hoeffler (2004), most of the studies above employ suboptimal age structure measures. The commonly used operationalization counts 15 to 24 (or 29) year-old cohorts relative to the total population, including all cohorts under the age of 15 years in the denominator. Such definition is highly problematic both theoretically and empirically. First, most theories about youth revolt and crime assume that violence arises because youth cohorts experience institutional 'bottlenecks' in the education system or in the labor market due to their larger size compared to previous cohorts. Second, when using the total population in the denominator, youth bulges in countries with continued high fertility will be underestimated because the large under-15 populations deflate the youth bulge measure. At the same time, countries with declining fertility and relatively smaller under-15 populations – which are in a position to experience economic growth driven by age structural change, a so-called demographic bonus, which may reduce both criminal and political violence – score relatively higher. The issue of measurement appears not to have been discussed in the homicide literature, with the lone exception of Fox and Hoelscher (2012).

3. Data and Methods

In this section, we describe the data covering all 32 Mexican states, including the Federal district, also known as Mexico City, (see Appendix 1 for details) during the 1996–2010 period and the estimation specifications.

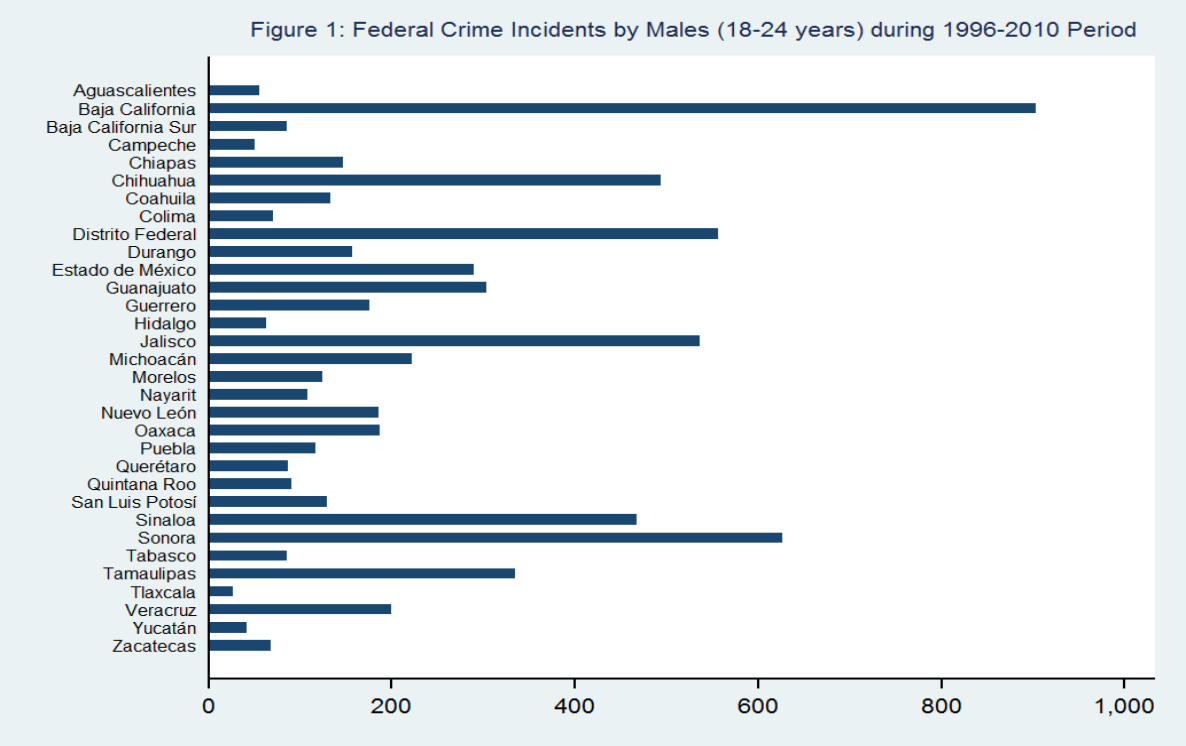
3.1 *Estimation Specification*

The baseline specification estimates the number of crime incidents committed by youth (YC_{it}), in state *i* in year *t* as a function of a set of youth opportunity variables: YE_{it-1}, and control variables Z_{it-1}:

$$YC_{it} = \gamma YE_{it-1} + \beta Z_{it-1} + \nu_i + \lambda_t + \omega_{it} \qquad (1)$$

Where, ν_i denotes state-fixed effects to control for unobserved state-specific heterogeneity in the panel dataset, λ_t is time specific dummies and ω_{it} is the error term. Note that the Hausman (1978) test overwhelmingly favors fixed effect over random effect estimator. For the dependent variable we use the number of federal crimes

committed by Mexican males in the age cohort 18–24[2] in state *i* in Mexico in year *t* measured in per capita logged. This data is reported by the National Institute for Statistics and Geography (INEGI hereafter) for the 32 states (including the Federal district) for the 1997 through 2010 period (INEGI 2012). Federal crimes include all counts of drug-related crime and other violent organized criminal activity, but exclude 'common crime', providing for an appropriate proxy for violent crime to be tested specifically against youth opportunities (see Appendix 4 for details). Figure 1 shows the number of youth federal crime incidents reported across Mexican states during the 1997–2010 period. As seen, the states with the highest number of youth federal crimes are Baja California, Sonora, Jalisco, Federal District, Chihuahua, and Sinaloa, many of which are heavily affected by drug-related violence.



Our main hypotheses variables in the vector of youth opportunity (YE_{it-1}) in equation (1) are: male youth bulge, male youth education attainment rate, and male youth unemployment rate. We define male youth bulge as 18–24 year-old males as a share of all males aged 18 years and above, capturing the dynamics in the younger working-age segments.[3] The demographic data is sourced from Mexican population censuses carried out by INEGI across all the 32 Mexican states (including the Federal District) once every 10 years. Once every five years INEGI also conducts random surveys known as population count. Thus, the data used to construct youth bulge is sourced from the censuses of 1990, 2000, and 2010 (INEGI, 1990; 2000; 2010), and from the population surveys of 1995 and 2005 (INEGI, 1995; 2005). The youth education variable also

[2] A crime is included if at least one of the reported suspects is a male between the ages of 18 and 24. For more details about categories and definitions of federal crimes in Mexico, see Appendix 4 and www.inegi.org.mx (Estadísticas Judiciales en Materia Penal).

[3] We have also used the conventional (Urdal 2006) definition of youth bulges measuring 15–24 year-old males as a share of male population aged 15 years and above. Our results remain unchanged when we use this alternative measure of youth bulge.

originates from the census data, as well as the 2005 survey. This variable measures the proportion of males aged 18-24 years with at least a secondary education attainment normalized by the total male population aged 18-24 years. Youth unemployment is defined as the number of males aged 18-24 years who are reportedly unemployed divided by the total male labor force aged 18-24 years. The unemployment and labor force data are available from the Mexican census files for 1990, 2000 and 2010 only (INEGI, 1990; 2000; 2010). Missing years between the reported census and survey observations for these variables are interpolated. We believe this is defendable given that demographic and education variables normally change relatively slowly. We do acknowledge, however, that unemployment figures are likely to be much more volatile, and that the interpolation between the census observations is likely to miss considerable variation. While this is unfortunate, unemployment data based on census records are clearly preferable to less reliable survey data, given our aim to study age-, gender-, and education-specific unemployment across all Mexican states over time.

We further disaggregate the youth unemployment data by the category of education, which is only possible given the use of census information, constructing data that as far as we know have not previously been used to test the youth opportunity and violence nexus. We specifically use *unemployment rate in low education and high education strata,* respectively, in our subsequent specification (2):

$$YC_{it} = \gamma URlowY_{it-1} + \delta URhighY_{it-1} + \beta Z_{it-1} + \nu_i + \lambda_t + \omega_{it} \qquad (2)$$

Where, $URlowY_{it-1}$ denotes unemployment rate in low education stratum, while $URhighY_{it-1}$ denotes unemployment rate in high education stratum in state *i* and year *t-1* respectively. We first condense the categories for 'no', 'primary' and 'incomplete secondary' education into the *low education stratum*, defined as those males aged 18-24 years with lower education than completed secondary level. We then divide the number of males who are unemployed in this category by the total male population aged 18-24 with low education. Note that data on employment by education is available only from the 1990, 2000 and 2010 population census. Likewise, we categorise male youth in the *high education strata* as those aged 18-24 who have obtained completed secondary schooling or higher (including tertiary education). We then construct a measure for *unemployment rate in high education strata* by dividing unemployed male youth with high education, with total male population with high education, in the age group of 18-24 years. We also control for time- and state-fixed effects in equation (2).

We further examine under what conditions youth bulge can be associated with an increase in youth crimes using the specifications (3) and (4) below:

$$YC_{it} = \zeta (URlowY \times YB)_{it-1} + \gamma URlowY_{it-1} + \phi YB_{it-1} + \beta Z_{it-1} + \nu_i + \lambda_t + \omega_{it} \qquad (3)$$

$$YC_{it} = \xi (URhighY \times YB)_{it-1} + \delta URhighY_{it-1} + \phi YB_{it-1} + \beta Z_{it-1} + \nu_i + \lambda_t + \omega_{it} \qquad (4)$$

Where, $(URlowY \times YB)_{it-1}$ denotes unemployment rate in low education stratum coupled with youth bulge in state *i* and year *t-1* in equation (3). While,

$(URhighY \times YB)_{it-1}$ is the interaction between unemployment rate in the low education stratum and youth bulge in state *i* and year *t-1* in equation (4). These interactions help deduce whether the effect of youth bulges on violent crime are conditional upon unemployment rate in low or high education strata. As before, along with control variables we also include both time- and state-fixed effects.

Finally, the vector of control variables ($\mathbf{Z_{it-1}}$) includes other potential determinants of youth crime incidents per capita (logged) in state *i* during year *t-1* which we obtain from the extant literature on the subject. In selecting the controls, we follow earlier studies by Barakat and Urdal (2009), Demombynes and Ozler (2005), Fajnzylber, Lederman and Loayza (2002), Hashimoto (1987) Miron (2001), and Urdal (2006). We are aware of the potential traps of "garbage can models" (Achen 2005) or "kitchen sink models" (Schrodt 2010), in which all sorts of variables are dumped onto the right hand side of the equation, making interpretation more difficult. Thus, we follow a conservative strategy of accounting only for known factors that affect youth crime. Accordingly, we include *state per capita GDP (logged)* in US$ 2003 constant prices[4] in state *i* during year *t-1* to proxy for the level of economic development. The income data is available from the National Accounts System of INEGI. Likewise, we also use *state population (logged)* which is drawn from the population census data compiled by INEGI. We further include a measure on urban population namely, *urbanization* which captures urban population as share of total population in state *i* during year *t-1*. Urdal and Hoelscher (2012) point out that managing urban development sustainability pose significant challenges for the respective governments and therefore large youth bulge in urban centres could be a source of instability and violence. We then include a measure of state governor elections. We follow Schneider (2011) to generate an indicator for the timing of elections that varies between 0 and 1. For all non-election years, the value is 0. For election years we make use of the following measure: (12 − (Mn − 1))/12, wherein *Mn* is the month in which the state governor election took place. The data on the exact date and month in which the elections are held in each state are obtained from the state elections results and information published by the Institute of Marketing and Opinion (Instituto de Mercadotecnia y Opinión 2012). Accordingly, for election years this indicator takes smaller values the later the election takes place within the year.[5] The details on variable definitions and data sources are reported in Appendix 3. We estimate all our models with the Feasible Generalized Least Squares (FGLS hereafter) controlling for two-way fixed effects. [6] Using FGLS over a simple Ordinary Least Squares (OLS) estimator allows estimations in the presence of AR (1) autocorrelation within panels and cross-sectional heteroscedasticity across the panels.

4. Empirical Results

We begin with the results depicted in the scatter plot presented in Figure 2 which provides stylized facts on the bivariate correlation between youth crime incidents (count measure) and the youth unemployment in low education strata. Notice that states with a higher level of youth unemployment in low education strata are driving

[4] The data of state per capita GDP was available only in Mexican pesos 2003 constant prices. We use the exchange rate to US$ to convert these data into US$.

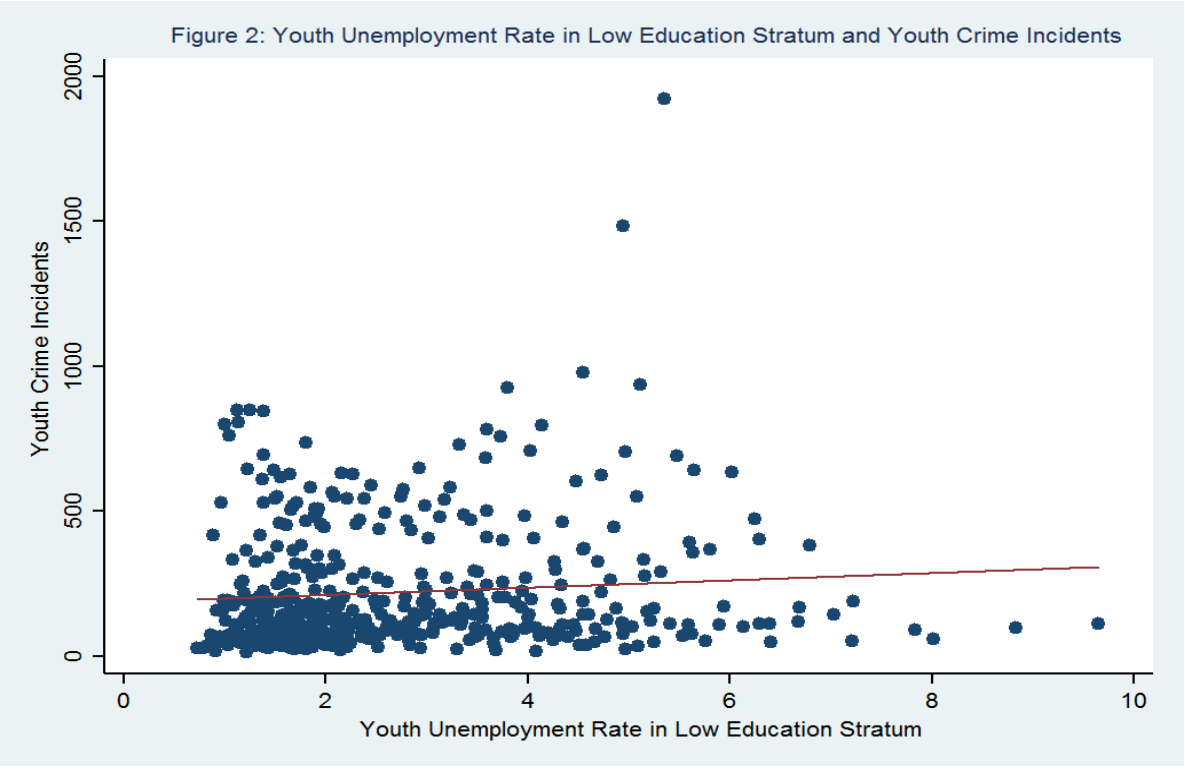
[5] The results remain quantitatively the same if we use a dummy for the Governor Election years.

[6] The fixed-effects estimator captures factors such as geographic location of states, which is also expected to affect the level of criminal violence.

the positive correlation. This provides preliminary support for the relationship between these two variables. These simple bivariate statistics, however, may lead to spurious conclusions without important control variables, such as income, because poverty rather than mere unemployment may explain the differences. We therefore move to examine this relationship in greater detail and precision in multivariate models.

4.1. *Baseline results*

Table 1 presents the baseline results estimated using specification (1) capturing the effects of youth bulge, youth education and youth unemployment rate on youth crime incidents. In Table 2 estimating specification (2), we disaggregate the youth unemployment rate by category of education, i.e. unemployment in the low education strata. In Table 3, we estimate specification (3) by introducing the interaction between unemployment rate amongst low and high education strata and youth bulge. Finally, in Table 4 we replace the two youth unemployment measures for education with a measure, 'Density of Low Opportunity Cost Youth', capturing the overall 'density' of unemployed male youth with low education as a percentage of all male youth. In all the four tables we estimate our models with FGLS fixed effects estimator. Descriptive statistics are presented in Appendix 2.



Beginning with Column 1 in Table 1, the results show that the youth unemployment rate is positive and statistically significant at the 1% level. At the mean value of youth unemployment rate (2%) there is a 0.6% chance of increase in youth crime incidents, independent of a lagged dependent variable (we retain this lagged dependent variable hereafter in all our models). An increase by a standard deviation in youth unemployment rate (roughly 1.37%) above the mean increases the mean impact of youth crime incidents by roughly 25%. However, going from an average unemployment

rate of 2% to the maximum value of 8.3% increases the youth crime incidents by roughly 50%. These effects remain almost similar when we introduce various control variables in a step wise manner in column 2-4. Notice that these results marginally loses statistical significance in column 5 when we include all potential control variables into the model. In Column 2, we also include the youth education attainment ratio, finding, as expected, that higher levels of education have a strong negative effect on youth crime. The finding is significantly different from zero at the 5% level. The substantive effect suggests that at mean value (30.38%) an increase in youth education is associated with 0.2% fewer youth crime incidents. An increase in youth education by a standard deviation (4.29%) lowers the average youth crime incidents by 63%. However, going from an average value of youth education to maximum value in the sample (39.22%) reduces the youth crime incidents by roughly 71%.

Table 1: Effect of youth bulge and youth opportunity on youth crime
Dependent variable: Federal youth crime incidents per-head (logged)

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	FGLS-FE	**FGLS-FE**	**FGLS-FE**	**FGLS-FE**	**FGLS-FE**
Lagged Dependent Variable	0.357***	0.345***	0.341***	0.337***	0.315***
	(0.0428)	(0.0428)	(0.0427)	(0.0427)	(0.0430)
Male Youth Unemployment Rate t-1	**0.0605*** **	**0.0578** **	**0.0452***	**0.0422***	**0.0318**
	(0.0226)	**(0.0225)**	**(0.0235)**	**(0.0236)**	**(0.0234)**
Male Youth Secondary School Enrolment t-1		**-0.0182** **	**-0.0203*** **	**-0.0159***	**-0.0178** **
		(0.00775)	**(0.00782)**	**(0.00845)**	**(0.00895)**
State Per capita GDP (log) t-1			0.457*	0.435*	0.632**
			(0.257)	(0.257)	(0.268)
Male Youth Bulge t-1				-0.0557	-0.0393
				(0.0409)	(0.0404)
State Population (log) t-1					0.765**
					(0.325)
Urbanization t-1					0.0224*
					(0.0116)
Timing of State Governor Elections					-0.114***
					(0.0402)
Constant	-6.351***	-5.823***	-9.648***	-8.431***	-23.03***
	(0.458)	(0.508)	(2.205)	(2.375)	(5.741)
Hausman test (p-value)	0.87	0.00	0.00	0.00	0.00
State specific dummies	YES	YES	YES	YES	YES
Time specific dummies	YES	YES	YES	YES	YES
Number of States	32	32	32	32	32
Observations	448	448	448	448	448

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Results in bold reflect relationships that are central to the theoretical argument (main independent variables).

The youth education variable remains negative and significantly different from zero at the 5% or 10% level respectively throughout all the columns in Table 1. Notice that unemployment rate remains statistically insignificant once controlling for youth education. Interestingly, our crude measure for male youth bulge has a negative association with youth crime, though this relationship is statistically insignificant. These results do not lend support to those who attribute crimes committed by youth in Mexico to a surge in youth bulge. In all tables reported here the lagged dependent variable remains significantly different from zero at the 1% level. While the results for unemployment and education remain similar to Column 4, the youth unemployment rate becomes marginally insignificant in column 5. These results do not provide clear-cut evidence on the effect of youth unemployment on youth crime. We therefore disaggregate the unemployment levels among youth by low and high education in Table 2.

Table 2: Effect of youth unemployment rate by education category on youth crime
Dependent variable: Federal youth crime incidents per-head (logged)

	(1)	**(2)**	**(3)**	**(4)**
	FGLS-FE	**FGLS-FE**	**FGLS-FE**	**FGLS-FE**
Lagged Dependent Variable	0.354***	0.351***	0.340***	0.323***
	(0.0427)	(0.0426)	(0.0426)	(0.0429)
Unemployment Rate in Uneducated Youth (Males) t-1	**0.0652*** **	**0.0574*** **	**0.0542** **	**0.0373***
	(0.0214)	**(0.0221)**	**(0.0220)**	**(0.0225)**
State Per capita GDP (log) t-1		0.346	0.335	0.570**
		(0.252)	(0.251)	(0.270)
Male Youth Bulge t-1			-0.0883**	-0.0740**
			(0.0376)	(0.0371)
State Population (log) t-1				0.847***
				(0.325)
Urbanization t-1				0.0146
				(0.0109)
Timing of State Governor Elections				-0.113***
				(0.0403)
Constant	-6.385***	-9.345***	-7.444***	-22.80***
	(0.450)	(2.204)	(2.335)	(5.896)
Hausman test (p-value)	0.00	0.00	0.00	0.00
State specific dummies	YES	YES	YES	YES
Time specific dummies	YES	YES	YES	YES
Number of States	32	32	32	32
Observations	448	448	448	448

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Results in bold reflect relationships that are central to the theoretical argument (main independent variables).

As seen in Column 1 of Table 2, we find a positive effect of youth unemployment rate in the low education stratum statistically significant at the 1% level in column 1 where only the lagged dependent variable is included. At mean value (2.5%), the youth

unemployment rate in the low education stratum is associated with 0.65% increase in youth crime incidents per capita. However, a standard deviation increase in youth unemployment rate in the low education stratum is associated with a 24% increase in youth crime incidents per head, which is about 4% of the standard deviation of youth crime incidents per head.[7] Note that these results remain robust when we add other control variables in an incremental manner in Columns 2-4. These results broadly support our hypothesis that the opportunity cost of engaging in violent crimes is lower among young unemployed men in the low education stratum.

4.2. *Conditional effects*

In Table 3 we turn our attention to the interactive effects between youth bulge and youth unemployment rate by education category. First, in Columns 1 and 2 we interact youth bulge and unemployment rate in the low education stratum, and in Columns 3 and 4 youth bulge and unemployment rate are coupled in the high education stratum. As seen in Column 1, we find that the interaction between youth bulge and unemployment in the low education stratum has a positive effect on youth crime incidents per head and is significantly different from zero at the 1% level.[8] This means that states with a higher percentage of male youth in their populations are more vulnerable to youth crime incidents if the unemployment rate in the low education stratum increases. In other words, a youth bulge is not a problem in itself, but rather the risk of violence is conditional upon higher levels of youth within the low education stratum and thus scant employment opportunities.

Table 3: Effect of youth unemployment rate - interactions with education category
Dependent variable: Federal youth crime incidents per-head (logged)

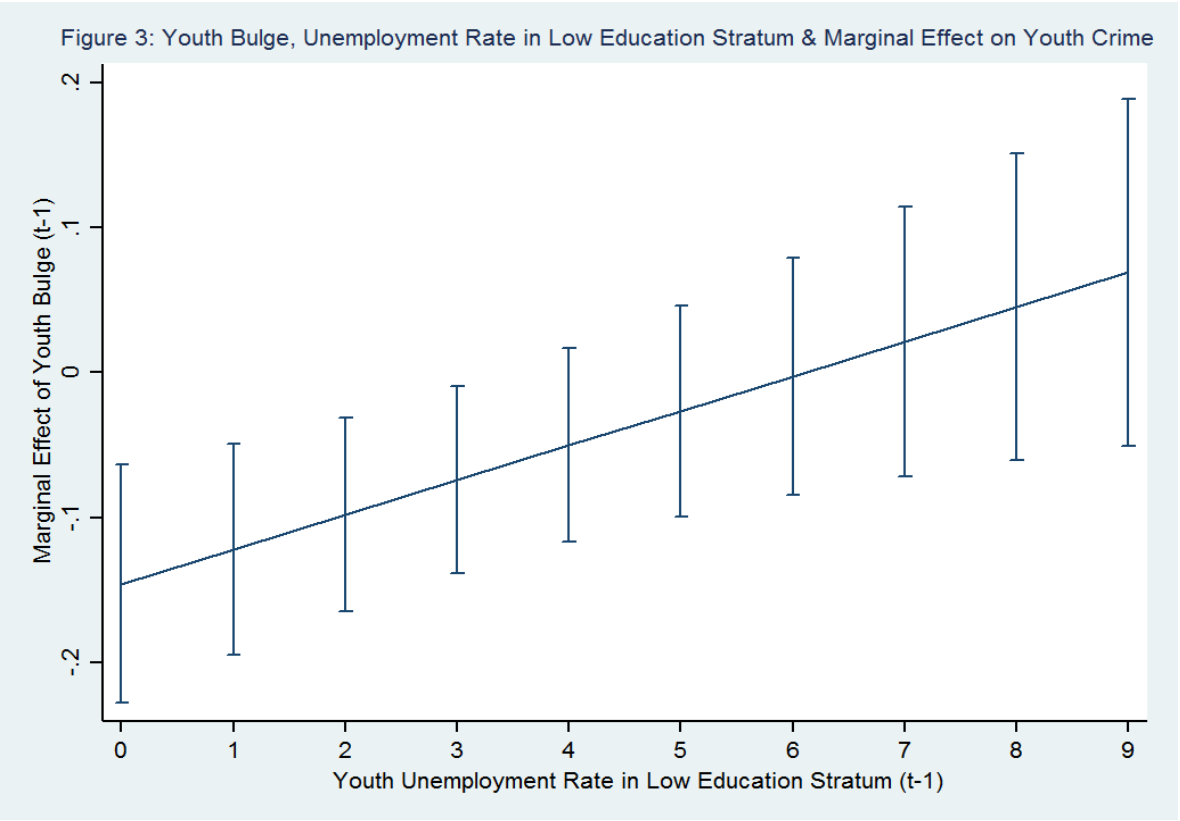
[7] Note that in the robustness check, we also estimated a model where we also control for youth unemployment in the high education stratum. We do not find any statistical significance when we include this measure on youth crime incidents per capita.

[8] The youth bulge variable on its own has a negative effect in explaining youth crime incidents and interestingly, we now find that unemployment rate in the low education stratum also has a negative sign. This is due to a high correlation of these variables (0.96) with the interaction term leading to switching of signs.

	(1)	(2)	(3)	(4)
	FGLS-FE	**FGLS-FE**	**FGLS-FE**	**FGLS-FE**
Lagged Dependent Variable	0.335***	0.325***	0.347***	0.326***
	(0.0423)	(0.0426)	(0.0429)	(0.0430)
Youth Unemployment Rate in Low Education Stratum (Males) t-1 × Youth Bulge t-1	**0.0252*** **	**0.0239** **		
	(0.00822)	**(0.00954)**		
Youth Unemployment Rate in Low Education Stratum (Males) t-1	-0.459***	-0.455**		
	(0.172)	(0.198)		
Youth Unemployment Rate in High Education Stratum (Males) t-1 × Youth Bulge t-1			**0.00921**	**0.000827**
			(0.00718)	**(0.00807)**
Youth Unemployment Rate in High Education Stratum (Males) t-1			-0.146	0.00326
			(0.148)	(0.166)
Male Youth Bulge t-1	-0.163***	-0.146***	-0.122***	-0.0764
	(0.0444)	(0.0467)	(0.0458)	(0.0477)
State Per capita GDP (log) t-1		0.686**		0.649**
		(0.272)		(0.270)
State Population (log) t-1		0.504		0.950***
		(0.351)		(0.340)
Urbanization t-1		0.00930		0.0134
		(0.0110)		(0.0112)
Timing of State Governor Elections		-0.113***		-0.112***
		(0.0400)		(0.0404)
Constant	-3.145***	-17.13***	-3.775***	-24.66***
	(0.995)	(6.279)	(1.026)	(6.164)
Hausman test (p-value)	0.00	0.00	0.00	0.00
State specific dummies	YES	YES	YES	YES
Time specific dummies	YES	YES	YES	YES
Number of States	32	32	32	32
Observations	448	448	448	448

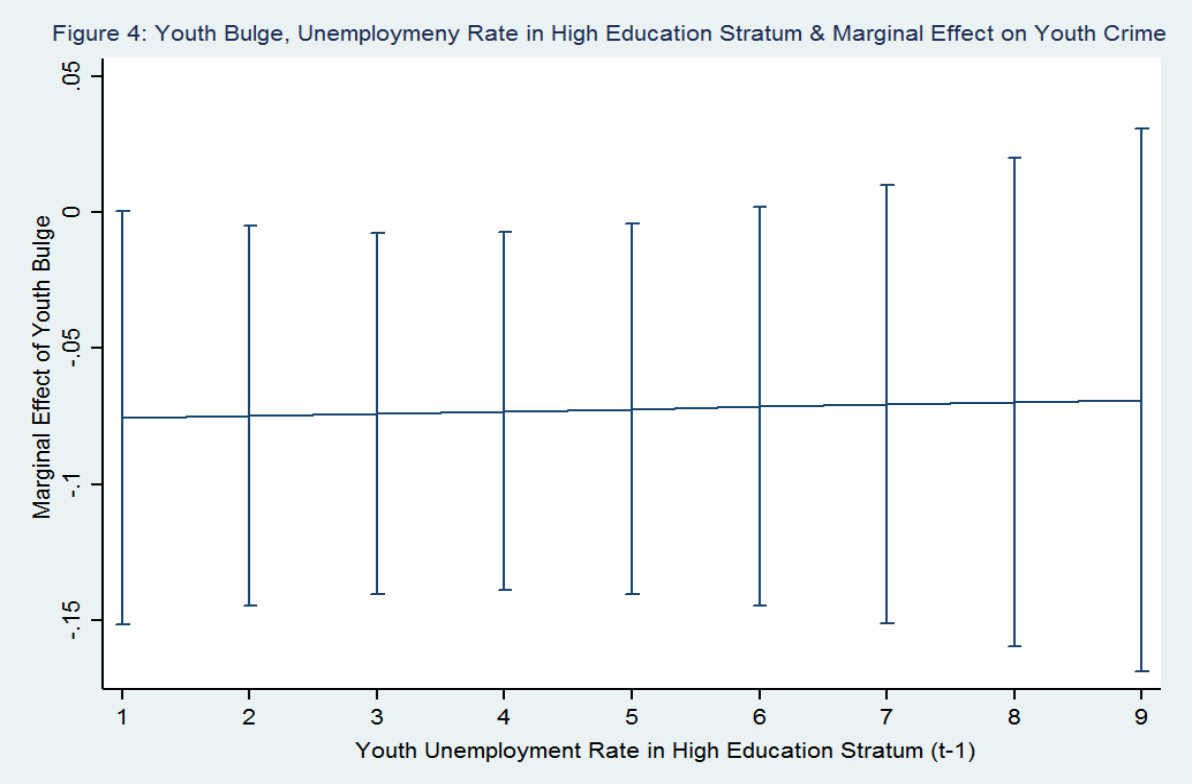
Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Results in bold reflect relationships that are central to the theoretical argument (main independent variables).

To better understand the interaction effect, we rely on margins plot in Figure 3 (Greene 2009). To calculate the marginal effect of an increase in the youth bulge variable, we take into account both the conditioning variable (unemployment rate in the low education stratum) and the interaction outcome and display graphically the total marginal effect conditional on unemployment rate in the low education stratum. The y-axis of Figure 3 displays the marginal effect of a unit increase of the youth bulge variable and this marginal effect is evaluated on the unemployment rate in the low education stratum along the x-axis. Note, that we include the 90% confidence interval.



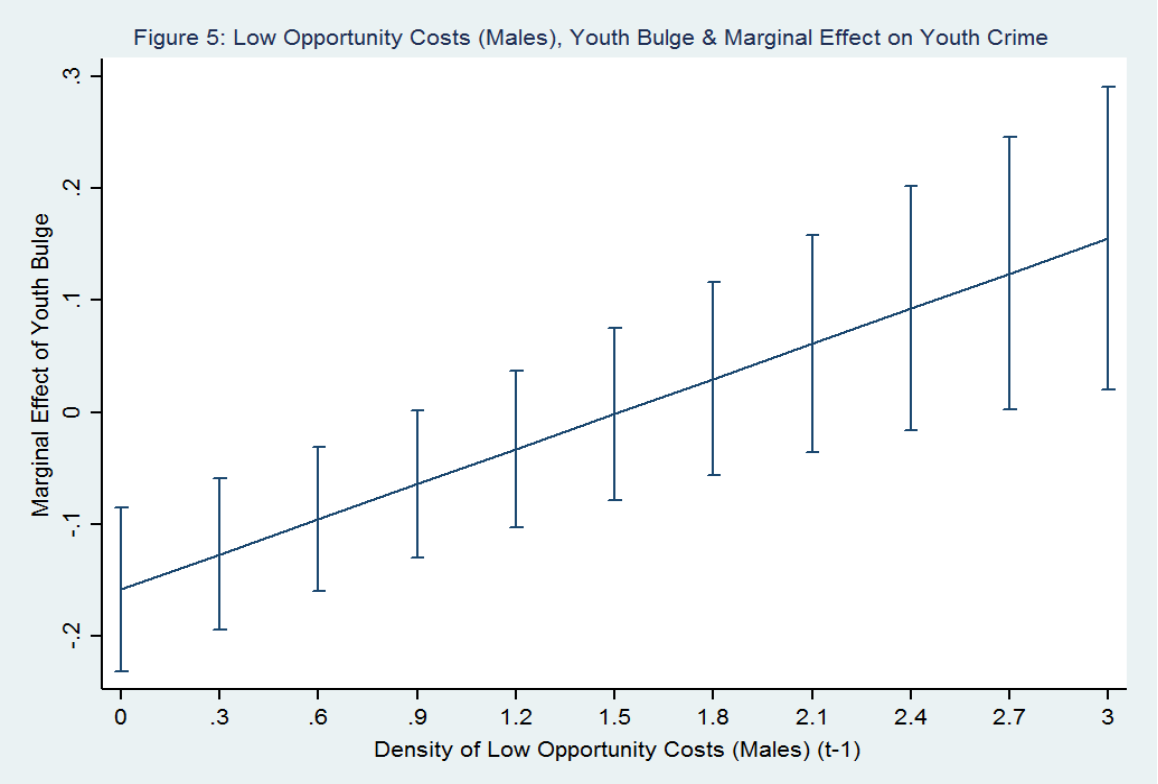
As seen in Figure 3, a unit increase in youth bulge variable decreases youth crime incidence (at the 90% confidence level at least) when the unemployment rate in the low education stratum is lower than 3% (with maximum value being 8.83%). For instance, at 0% of unemployment rate in the low education stratum crime incidents per capita is lowered by 16%, which is significantly different from zero at the 1% level (The marginal effects are significant and negative when the upper bound of the confidence interval is below zero). However, the margin plots also show that the impact of youth bulge on youth crime becomes positive but statistically insignificant once unemployment rate in the low education stratum is over and above 3%, i.e., at moderate to high levels of the unemployment rate. These results suggest that states with low unemployment rate among low educated youth, are far less likely to witness crime by youth independently of variables such as state income per capita, population, urbanization, among other factors. Note that the three terms are all jointly significant ($p< 0.0004$). We do not find much difference in the results on interactions depicted in the margin plot in Figure 3 concerning the results reported in Column 1 where no control variables are included.

We now turn to the interaction between youth bulge and unemployment rate in the high education stratum in Column 3-4, Table 3. As seen, the interactive effect is not significantly different from zero. At the first glance, this suggests that larger youth bulges do not appear to increase the risk of violent youth crime even when the unemployment rate in the high education stratum is increasing. However, as suggested above, the interaction results can be best assessed with a margins plot presented in Figure 4.



The y-axis in Figure 4 displays the marginal effect by a unit increase of the youth bulge variable and along the x-axis with the unemployment rate in the high education stratum at which the marginal effect is evaluated. Again, we include the 90% confidence interval. As seen there, an increase in youth bulge decreases the youth crime incidents (at the 90% confidence level) when the unemployment rate in the high education stratum is lower than 5%. The marginal effects are negative and statistically significant when the upper bound of the confidence interval is below zero. However, the margin plot also shows that the impact of youth bulge on youth crime incidents per head is positive, albeit statistically insignificant, when the unemployment rate in the high education stratum is higher than 5%. These results suggest that the opportunity costs of engaging in crime are markedly higher for unemployed youth in the high education stratum.

Lastly, in Table 4, we use a variable, 'Density of Low Opportunity Cost Youth', capturing the overall 'density' of unemployed male youths aged 18-24 with low education measured as the share of the total male youth population in that age group. We restrict our specification to only include unemployment in the low education stratum since the relative number of unemployed youth with low education is the quantity of greatest theoretical relevance to the opportunity perspective. As seen in Column 1, the density of unemployed youth with low education is positive and significantly different from zero at the 1% level (see column 1). In Columns 2 and 3 we interact youth bulge with the density variable wherein column 2 is a parsimonious model while in column 3 all control variables are included. As seen, the interactive effect is positive and significantly different from zero at the 1% levels in both column 2 and 3. The marginal effects of the interaction variable are shown in Figure 5.



The Figure 5 shows that a unit increase in youth bulge variable would decrease the youth crime incidents per head (at the 90% confidence level) when the density of unemployed male youths with low education is lower than 0.9%. However, when the density of unemployed male youths with low education is high, i.e., at 2.7% and 3%, the impact of youth bulge variable on the youth crime incidents per capita (logged) is positive and statistically significant at the 5% level. For instance, at 3% of unemployed male youth with low education, an additional point increase in youth bulge variable is associated with a roughly 16% increase in youth crime incidence per head. These results highlight that irrespective of whether we use the measure for unemployment rate or density, unemployment in the low education stratum is the best predictor of youth crime incidents in Mexico.

Before moving forward towards robustness checks, we will briefly discuss the results of control variables in Tables 1-4. Interestingly, we find robust evidence for an impact of violent youth crime on per capita state GDP which is positive and statistically significant at the conventional levels. At mean value (9.06), per capita income (logged) is associated with 0.46% increase in youth crime per head (logged). The substantive effects suggest that a standard deviation increase in per capita income increases youth crime incidents per head by 4.5%, while moving to maximum value of income (logged) leads to 5.5% decline in youth crime. Notice that these results are contrary to general expectations that higher levels of income are associated with lower levels of crime. It is noteworthy that urbanization, education and unemployment variables are controlled in the models, while the impact of income on these variables is not accounted for. For instance, as per capita income increases governments have more resources to spend on public services, such as crime prevention. It is then plausible that an increase in income per head is associated with lower levels of actual crime but higher levels of reported

crime as public expenditure on law and order allows for police to enforce the law more effectively.

Table 4: Effect of the density of low-opportunity cost youth on youth crime
Dependent variable: Federal youth crime incidents per-head (logged)

	(1) FGLS-FE	(2) FGLS-FE	(3) FGLS-FE
Lagged Dependent Variable	0.354***	0.342***	0.333***
	(0.0426)	(0.0420)	(0.0424)
Youth Unemployment Density in Low Education Stratum (Males) t-1	**0.199*** **	-1.746***	-2.033***
	(0.0648)	(0.537)	(0.613)
Youth Unemployment Density in Low Education Stratum (Males) t-1 × Youth Bulge t-1		**0.0936*** **	**0.104*** **
		(0.0255)	**(0.0292)**
Male Youth Bulge t-1		-0.169***	-0.158***
		(0.0405)	(0.0418)
State Per capita GDP (log) t-1			0.680***
			(0.263)
State Population t-1			0.297
			(0.349)
Urbanization t-1			0.0180*
			(0.0108)
Timing of State Governor Elections			-0.114***
			(0.0396)
Constant	-6.348***	-2.958***	-14.61**
	(0.443)	(0.913)	(6.110)
Hausman test (p-value)	0.00	0.00	0.00
State specific dummies	YES	YES	YES
Time specific dummies	YES	YES	YES
Number of States	32	32	32
Observations	448	448	448

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Results in bold reflect relationships that are central to the theoretical argument (main independent variables).

Next, after controlling for time- and state-fixed effects we find our population (logged) variable to increase youth crime incidents, which in all the Tables is statistically significant at conventional levels. Naturally, states with higher levels of population tend to witness more incidence of crime than sparsely populated states. Also, like others, we do find a strong positive impact on youth crime of the level of urbanization, which is consistent with the idea that urban environments are more conducive to violent crime (e.g. Urdal and Hoelscher 2012). The variable capturing the timing of elections is associated with fewer number of crime incidents during the run-up towards governor elections. This might be due to two reasons. Firstly, there is every possibility of under reporting of crime incidents during the run-up towards elections by the incumbent government. Second, it is also plausible that the incumbent governor would impose measures aimed at reducing violence during the election period, signalling to voters her/his commitment to control crime and restore law and order. We also cannot rule out the possibility that the result is driven by a combination of the two factors.

4.3. *Endogeneity*

Finally, we address the question of whether causality runs from youth unemployment and education measures to youth crime incidents per head (logged) or the other way around. It is quite possible that our key explanatory variables capturing youth opportunity are endogenous. That is, it might be that criminal activities attract more

youth with low opportunity cost towards areas with high crime rates, and especially towards drug-related activities which might maximize their returns in the short run. This could affect the education and unemployment measures. It could also be that high levels of crime deter local investment, driving up unemployment levels. Although the case for reverse causality is indirect and presumably relatively weak, not taking this endogeneity into account might induce bias in our estimates of the effect of youth opportunity on violent crime. To determine the direction of causality in the first instance, we use a dynamic model of Granger Causality (Granger, 1969). Accordingly, once the past influence of *y* has been accounted for, the variable *x* is said to "Granger cause" the variable *y* if the past values of *x* help explain *y* (Engle and Granger, 1987). Furthermore, we follow Dreher et al. (2012) to account for Granger Causality in a panel setting as:

$$y_{it} = \sum_{j=1}^{\rho} \psi_j \, y_{i,t-j} + \sum_{j=1}^{\rho} \xi_j \, x_{i,t-j} + \delta_i + \zeta_t + \omega_{it} \qquad (5)$$

where the parameters are denoted as: ψ_{it} and ξ_{it} for state *i* during the year *t*, and the maximum lag length is represented by ρ. While δ_i are unobserved individual effects, ζ_t are unobserved time effects. ω_{it} denotes the error term. Under the null hypothesis, the variable *x* is assumed to not Granger cause *y,* while the alternative hypotheses allow for *x* to Granger cause *y* after controlling for past influence of the variable *y*. We use three lags to estimate the models. Note that the joint F-statistic is used to gauge the joint significance of the youth crime incidents per head (logged) on youth unemployment, education measures, and vice-versa. We estimate four sets of Granger causality models in which set 1 captures estimations of youth unemployment Granger cause youth crime and vice-versa. In set 2 we test whether unemployment rate in low education strata Granger cause youth crime and vice-versa. Set 3 estimates whether unemployment rate in high education strata Granger cause youth crime and vice-versa. Lastly, set 4 tests whether 'density' of low opportunity costs of youth Granger cause youth crime and vice-versa. Our findings from all four sets reveal that we do not find any statistically significant effects of youth crime incidents per head (logged) on youth unemployment and education measures. The joint F-statistics show that none of the three lags in the youth crime incidents display correlation with youth unemployment and education measures. Likewise, we do not find youth unemployment and education measures explain increases in youth crime incidents per head (logged) as joint F-statistics is less than the thumb rule of 10 and remains statistically insignificant at all three lags (these results on granger causality are not shown here due to space constraints, but are available in online appendix). Hence, our results reveal no significant reverse causality flowing from youth crime incidents per head (logged) to the youth unemployment and education measures.

4.4. *Robustness checks*

We have examined the robustness of our main findings in the following ways. First, we used alternative measures for the youth bulge, youth unemployment, and education variables. Departing from the measure of 18–24-year-old males, we used 18–30-year-old males as a share of all males aged 18 years and above. We also computed the federal crime incidents registered under the age group of 18–30 years. Likewise, we also used the 18-30 age group to compute unemployment rate by standard of education. Using our alternative measures does not alter our results significantly. We still find that the

unemployment rate in the low education stratum matters the most. The results for the interaction between youth bulge and unemployment rate in the low education stratum remain robust. Second, we re-estimated our FGLS fixed effects models with negative binomial models where we used the dependent variable as an event count of youth federal crime incidents in the male 18-24-year-old category. We also control for time- and state-specific dummies. The results estimated using negative binomial methods remains qualitatively similar to those reported in Tables 1–4 estimated using the FGLS fixed effects approach. Third, in some of our FGLS fixed effects models the Hausman test rejects fixed effects. Thus, we estimate all the FGLS fixed effects using random effects. The results remain robust. Fourth, as an additional test for robustness, we exclude the few observations with extreme values in youth crime incidents reported.[9] Excluding outliers, the baseline results are qualitatively unchanged, suggesting that our results are not driven by extreme values.

Next, we replicate the FGLS fixed effects models using the system-GMM estimator suggested by Arellano and Bond (1991), Arellano and Bover (1995) and Blundell and Bond (1998) to counter endogeneity concerns, if any. The results are based on the two-step estimator implemented by Roodman (2006) in STATA 13. We treat the lagged dependent variable (i.e. youth federal crime per capita logged) and our measures of youth opportunity in all models as endogenous and the rest of the variables as exogenous. We apply the Hansen test to check the validity of the instruments used and the Arellano-Bond test of second order autocorrelation, which must be absent from the data in order for the estimator to be consistent. In all our system-GMM regression models we include time dummies. To minimize the number of instruments in the regressions, we follow Roodman (2006) and collapse the matrix of instruments. Our results using SGMM estimator are largely in line with the baseline results reported in Tables 1-4. Finally, we have also examined the effects of youth opportunities on homicide rates across Mexican states. Unfortunately, reliable age-specific perpetrator data for homicides is not available.[10] We use homicides per 100,000 population logged as the alternative dependent variable. The data is collected by INEGI on an annual basis and available for all the 32 states in Mexico from 1990 to 2010. Compared to the youth crime incidence data, the homicide data may not be as vulnerable to underreporting as it appears to be consistently reported across states. The results for the homicide models generally uphold our baseline results reported in Tables 1 and 2, i.e. the unemployment rate in the low education stratum contributes to explain variation in homicide rates after controlling for relevant socio-economic factors. However, it is noteworthy that we could not replicate the results on the interaction between youth bulge and unemployment rate in the low education stratum as reported in Table 3. The results of these robustness checks are not reported due to brevity but are available in an online appendix.

5. Conclusion

[9] We use 'avplot' to identify the outliers in youth federal crime incidents.

[10] Note that the available age specific data for homicides show several shortcomings. For instance, they do not show variation in some years for some states. Furthermore, there is a sudden drop and jump in several years for most of the states. Therefore, we rather use the data coming directly from the mortality statistics (which doesn't provide homicides by age groups).

This article investigates potential causes for variation in violent youth crime across Mexican states, with a particular focus on the role of youth opportunities. Building on an opportunity framework prominent both in the civil war and criminology literatures, we initially hypothesized that violent crime should vary with demographic age structure, so that Mexican states with large youth bulges should have higher levels of violent crime, everything else being equal. This expectation is not borne out by the empirical models, however, as our measure for male youth bulge is consistently negatively associated with violent crime rates. We further hypothesized that the two factors that arguably most strongly determine the actual opportunity cost for youth, levels of education and employment, should be associated with crime levels, and particularly so when low education levels and high unemployment levels occur in states with large male youth bulges. These much more specific expectations regarding youth opportunities are not easily tested for global cross-national samples due to data limitations. The availability of reliable and comparable census data for Mexico providing age and gender-specific educational attainment and unemployment rates at the state level allow for a detailed sub-national panel study of youth opportunities and violent crime. Our empirical models, also taking into account possible confounding factors and endogeneity, find strong support for the importance of youth opportunities. This pertains in particular to educational attainment as our models consistently find low levels of education to be strong predictors of high levels of violent crime. We further find that high unemployment among males with low education is clearly associated with higher crime rates, and that this effect is amplified by an interaction with large male youth bulges. No similar effect is found for high unemployment among males with higher education levels, suggesting that the higher opportunity cost of youth with at least completed secondary education may inhibit recruitment to criminal organizations. This study provides some crucial insights into the complex root causes for the high levels of violent crime in emerging economies undergoing extensive economic, social, and demographic change. While being a middle-income country with relatively well-developed institutions, Mexico is experiencing a *de facto* lack of territorial control over certain geographical areas to drug cartels, and levels of violence that vastly surpass most contemporary armed conflicts. As such, improving knowledge of structural factors determining violent crime and ultimately building increased capacity to reduce crime has implications for understanding the security situation of the greater region as challenges pertaining to gang violence and drug trafficking extend beyond the Mexican context. Furthermore, the findings reported here may have implications for understanding the drivers of violent crime beyond the Latin American context and should spur more detailed data collection and empirical study of youth opportunities and violence elsewhere. The developmental consequences of political and criminal violence are vast (World Bank, 2011) and to this end, failing to invest in human capital among young people may represent a double-developmental challenge.

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Appendices

Appendix 1: Mexican States included in Study

Aguascalientes	Distrito Federal	Morelos	Sinaloa
Baja California	Durango	Nayarit	Sonora
Baja California Sur	Estado de México	Nuevo León	Tabasco
Campeche	Guanajuato	Oaxaca	Tamaulipas
Chiapas	Guerrero	Puebla	Tlaxcala
Chihuahua	Hidalgo	Querétaro	Veracruz
Coahuila	Jalisco	Quintana Roo	Yucatán
Colima	Michoacán	San Luis Potosí	Zacatecas

Appendix 2: Descriptive Statistics

Variables	Mean	Standard Deviation	Minimum	Maximum	Observations
Youth Crime Incidents (Male)	182.15	216.31	1.00	1921.00	553
Homicides (Number of cases)	407.69	546.50	1.00	6234.00	672
State Per capita GDP (logged)	9.06	0.68	7.76	11.96	640
State Population (logged)	14.62	0.80	12.67	16.54	672
Urbanization	72.53	14.95	39.45	99.76	640
Timing of Governor Elections	0.12	0.29	0.00	1.00	672
Youth Bulge (Male)	23.38	2.41	17.93	29.73	640
Male Youth Unemployment Rate	2.82	1.37	1.04	8.30	640
Male Youth Secondary School Enrolment	30.38	4.29	18.95	39.22	640
Youth Unemp Rate: Low Education Strata (Males)	2.50	1.19	0.72	8.83	640
Youth Unemp Rate: High Education Strata (Males)	3.09	1.42	1.21	8.38	640
Youth Unemp Density: Low Education Strata (Males)	1.02	0.43	0.36	2.84	640

Appendix 3: Data definitions and sources

Variables	Definitions and data sources
Youth Federal Crimes	Total number of federal crimes committed by males in the cohort 18-24 and 18-30 in state *i* in year *t*. The data was obtained from the Penal Judicial Statistics provided by INEGI. The log of this variable is used in the OLS and System-GMM models.
Homicides	Total number of homicides in state *i* in year *t*. The data obtained from the Mortality Statistics provided by INEGI. The variable used is Homicide per 100,000 inhabitants logged.
Youth Bulge (male)	Males in the cohort 18-24 as a share of all males aged 18 years above. The same definition applies when we expand the cohort to 18-30. The data are from the 1990, 2000 and 2010 population censuses, and from the 1995 and 2005 population surveys carried out by INEGI.
Youth Unemployment (male)	Own construction using the number of males under the age group of 18-24 years who are reportedly unemployed divided by the total male labor force under the age group of 18-24 years. The unemployment and labor force data are from the population censuses of INEGI. The same definition applies when we expand the cohort to 18-30.
Youth Education (male)	Own construction using the total number of males under the age group of 18-24 years with completed secondary education normalised by the total male population under the age group of 18-24 years. The data on youth secondary schooling attainment is from the 1990, 2000, and 2010 population censuses, and from the 2005 population survey. All data are from INEGI.
Unemployment rate in low education stratum youth (male)	Own construction using the number of males under the age group 18-24 years who are unemployed and have low or no education (incomplete primary school, primary school only, and incomplete secondary school) divided by the male population under the age group 18-24 years with low education. The data is from the 1990, 2000, and 2010 population censuses carried out by INEGI. The same definition applies when we expand the cohort to 18-30.
Unemployment rate in high education stratum youth (male)	Own construction using the number of males under the age group 18-24 years who are unemployed and have high education (at least completed secondary school) divided by the male population under the age group 18-24 years with high education. The data is from the 1990, 2000 and 2010 population census carried out by INEGI. The same definition applies when we expand the cohort to 18-30.
Urbanization	Share of the total population living in urban areas in state *i* in year *t-1*. The data was own construction based on the information data from the population censuses 1990, 2000, 2010 and population surveys 1995, 2005 provided by INEGI.
Timing of Governor Elections	Timing of state level Governor elections varies between 0 and 1. It takes smaller values the later the election takes place within the calendar year of the election year and is 0 for all other years. We follow Schneider (2011) and make use of the following measure: (12 − (Mn − 1))/12, wherein Mn is the month in which the state Governor election took place. The data on the exact date on which the elections are held in each state are obtained from

	the state elections results and information published by Institute of Marketing and Opinion (IMO) in Jalisco, Mexico.
State per capita GDP (logged)	Own calculation using data on state-level GDP and population. Values are in U.S. dollars, 2003 constant prices. The data is form the National Accounting System and the population data are from the 1990, 2000, 2010 population censuses, and population surveys 1995, 2005. All data is sourced from INEGI.
Population	Population count data is from the 1990, 2000, 2010 population censuses, and the 1995 and 2005 population surveys done by INEGI.

Appendix 4: Collection and categorization of the federal crime data

The criminal procedure system in Mexico classifies crimes to be recorded under two broad categories namely, federal crimes and common crimes. The federal crimes include criminal activities associated with drug violence and other forms of organized crime; homicide; blocking of roads; possession, use and sale of weapons; piracy; illegal migrant and other human trafficking; falsification of documents; and kidnapping. Common crimes on the other hand include such crimes as sexual harassment; stealing of animal livestock; property expropriation; theft; rape; and domestic violence. While federal crimes are prosecuted in Mexico under the Federal Penal Code, the common crimes are adjudicated under the Penal Code of the respective states in which the offence took place.[11] The focus of this study is federal crimes only, which are typically associated with large-scale organized crime.

The criminal procedure system in Mexico specifies that when a crime incident occurs the investigative agencies decide whether the particular crime committed falls under the category of federal or common crime. If the crime is identified as a federal crime, the agents of the Federal Public Ministry together with the judiciary police start a preliminary investigation into the incident. The incident is then and there recorded as a federal crime. The investigative agencies are then required to investigate the crime and maintain detailed records of the progress of the investigation. During such investigation, they may question or arrest any suspects. Based on the preliminary investigation and evidence gathered, the agencies decide to either approach the judiciary court or dismiss the case (typically due to lack of sufficient evidence against the suspect(s)). If the investigative agencies decide to approach the judiciary court, all arrested individuals must be produced before a judiciary court and charged with a specific federal crime within 48 hours of the decision or be released. The investigative agencies must submit a report to the judge which details the results of the investigation. Based on this report, the judge makes a decision about whether there are sufficient grounds for proceeding with a criminal case. If s/he so rules, a formal ruling is announced, detailing the offence with which the accused is charged. If the judge on the other hand concludes that the report from the investigative agencies does not provide sufficient reasons to frame a charge, the case is dismissed. Our dependent variable captures the number of incidents at the state level recorded as federal crimes for which at least one young male aged 18-24 is suspected of the crime and has been arrested.

The state level crime data are collected by the Instituto Nacional de Estadística y Geografía (The National Institute for Statistics and Geography, INEGI). INEGI was formed in 1983 as a part of Ministry of Finance. In 2005, it was separated from the Ministry of Finance and became an autonomous institution. Its main task is to conduct regular population and economic censuses across Mexican states and municipalities. INEGI also collect and process all forms of crime data on a monthly basis based on input from the courts at the state level. Through its website, it provides data on crime incidents by suspected perpetrators for different age groups, from 1990 to 2010. The reported categories changed somewhat between 2008 and 2009. For both periods, there is a distinction between the "register year" and the "occurrence year". The former represents the year in which a crime was registered by the court of justice and the latter

[11] On December 2nd, 2012, the incoming Mexican President together with the two principal opposition political parties PAN and PRD, signed a document called "Pact for Mexico" as a part of larger judiciary reforms. One of the main features of this pact included the introduction of a single Penal Code and a single Penal Procedures Code for the entire country.

records the year in which the crime actually took place. The count based on 'register year' includes crimes dating back before 1990, hence we have relied on the 'occurrence year' data only. For this category we observed a sudden jump in crime figures across all age groups in 1997 and assume that data prior to 1997 has been subject to significant under-reporting.[12] Therefore, we only consider crime data starting in 1997.

[12] While data prior to 1997 appears to be significantly under-reported, INEGI recognizes that not every crime is reported, hence there could be a bias due to under-reporting for the period covered by this analysis (Síntesis Metodológica. Estadísticas Judiciales en Materia Penal, p. 6). However, we have no information suggesting that such underreporting could systematically bias the relationships that we are studying. Furthermore, systematic time period or geographical biases should in principle be picked up by the time and state specific dummy variables respectively.