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.

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 (Brett and Specht 2004). 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 recently become more prominent in the civil war literature (e.g. Hoelscher et al. 2012, Buhaug and Rød 2006, Urdal 2008, Østby et al. 2011, Vadlamannati 2011) as well as in social violence studies of global south contexts (e.g. Dreze and Khera 2000 on India, Hoelscher 2015 on Brazil, and Widner et al. 2011 on Mexico), such studies are still rare compared to cross-national analyses. 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 also 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 favourable 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 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 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

[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).

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 Gottfredson (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, Dell et al. 2019).

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. Rogers and Pridemore (2016: 259) note the lack of previous tests of this contextual relationship.

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), Pridemore (2011), and Rogers and Pridemore (2016) do not find statistically significant effects of age structure on crime among country-level panel data analyses.[2] 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.

[2] Though most studies fail to recognize the important distinction in the measurement of youth shares relative to the adult, as opposed to the total, population (see Urdal 2006 for a discussion).

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 former Federal district now officially known as the Mexico City, during the 1997–2010 period. We further describe the estimation specifications.

3.1 *Estimation Specification*

The baseline specification estimates the number of federal crimes committed by Mexican males in the age cohort 18–24 in state ***i*** during year ***t*** measured in per capita (log) (YC_{it}), as a function of a set of youth opportunity variables viz., YE_{it-1}, and control variables Z_{it-1}

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

Wherein, ν_i denotes state-fixed effects to control for unobserved state-specific heterogeneity in the panel dataset, λ_t is year specific dummies and ω_{it} is the error term. Note

that the Hausman (1978) test overwhelmingly favours 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[3] in state ***i*** in Mexico in year ***t*** measured in per capita (log). This data is reported by the National Institute for Statistics and Geography (INEGI hereafter) for the 32 states (including Mexico City) 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 and limitations of the dataset). Figure 1 shows the annual mean value of youth federal crime incidents reported across Mexican states during the 1997–2010 period. As seen, the states with the highest mean value of youth federal crimes are Baja California, Sonora, Jalisco, Mexico City, Chihuahua, and Sinaloa, many of which are heavily affected by drug-related violence. These states also have the highest relative shares of youth crimes in the country as illustrated in the right-hand side of the panel in Figure 1. Additionally, Map 1 shows that some of these states are close to the border with the United States. Other states in the middle of the country (Jalisco, Guanajuato and Michoacán) that have high federal crime rates are home to major drug trafficking organizations.[4]

Our main 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

[3] 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).

[4] The supplementary file offers a graph, which provides visualization of crime trends over time by state. They show an increasing trend in federal crime incidents by males (18-24) in these states during the 1997 to 2010 period.

and above, capturing the dynamics in the younger working-age segments.[5] The demographic data is sourced from Mexican population censuses carried out by INEGI across all the 32 Mexican states 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 originates from the census data, as well as the 2005 survey. This 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 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 specification (2):

[5] We 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.

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

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 year- 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):

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

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

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 high education stratum and youth bulge in

state ***i*** and year ***t-1*** in equation (4). These interactions help deduce whether the effect of youth bulge on violent crime are conditional upon unemployment rate in low or high education strata. As before, along with control variables we also include both year- and state-fixed effects.

Finally, the vector of control variables ($\mathbf{Z_{it-1}}$) includes other potential determinants of youth crime 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). Wary of the potential traps of "garbage can model" (Achen 2005) or "kitchen sink models" (Schrodt 2014), 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 (log)* in US$ 2003 constant prices[6] 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 (log)* 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 (2013) 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: $(12 - (\text{Mn} - 1))/12$, wherein *Mn* is the month in which the state governor election took place.

[6] 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$.

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.[7] The descriptive statistics are reported in Appendix 1 and the correlation matrix is in Appendix 2. The data sources and definitions are reported in Appendix 3. We estimate all our models with the Generalized Least Squares (GLS hereafter) controlling for two-way fixed effects. Using GLS 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

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. 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% increase in youth crime incidents per

[7] The results remain robust if we use a dummy for the Governor Election years.

capita (log), 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 (1.37%) above the mean increases the youth crime incidents per capita (log) by roughly 25%. These effects remain similar when we introduce various control variables in a stepwise manner in Column 2-4. Notice that these results marginally loses statistical significance in Column 5 when we include all control variables into the model.

In Column 2, we also include the youth education attainment ratio. As expected, higher levels of education have a 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% decline in youth crime incidents per capita (log). However, an increase in youth education by a standard deviation (4.29%) lowers the average youth crime incidents per capita (log) by 63%. Interestingly, our crude measure for male youth bulge has a negative association with youth crime (Column 4-5), 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 when all control variables are included in the model. 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.

As seen in Column 1 of Table 2, we find a positive effect of youth unemployment rate in the low education stratum, which is statistically significant at the 1% level in Column 1. The substantive effects suggest that a standard deviation increase in youth unemployment rate in the low education stratum is associated with a 24% increase in youth crime incidents per

capita (log), which is about 4% of the standard deviation of the dependent variable.[8] 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*

Table 3 presents 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 capita (log) and is significantly different from zero at the 1% level. This suggests that states with a higher percentage of male youth in their populations are more vulnerable to crime 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.

To better understand the interaction effect, we rely on margins plot in Figure 2 (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 2

[8] Note that in 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 of this measure on youth crime incidents per capita (log), though the unemployment in the "*low* education stratum" variable continues to be positive and statistically significant.

displays the marginal effect of a unit increase of the youth bulge and the 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 2, a unit increase in youth bulge decreases youth crime per capita (log) when the unemployment rate in the *low* education stratum is lower than 3.72%. For instance, at 0.72% of unemployment rate in the *low* education stratum crime per capita (log) is reduced 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 crime becomes positive but statistically insignificant once unemployment rate in the *low* education stratum is over and above 3.72%, 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, population, urbanization, among other factors. Note that the three terms are all jointly significant ($p< 0.0004$).

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 3. The y-axis in Figure 3 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 per capita (log) when the unemployment rate in the *high* education stratum is lower than 5%. However, the margin plot also shows that the impact of

youth bulge on youth crime incidents per capita (log) 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 'Density of Low Opportunity Cost Youth' capturing '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 cost 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. 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 4. The Figure 4 shows that a unit increase in youth bulge would decrease the youth crime incidents per capita (log) (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, at 2.7% and 3%, the impact of youth bulge on the youth crime incidents per capita (log) 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 16% increase in youth crime per capita (log). These results highlight that irrespective of the measure we use (unemployment rate or density), unemployment in the *low* education stratum is the best predictor of youth crime in Mexico.

Before moving forward towards robustness checks, we will briefly discuss the results of control variables in Tables 1-4. Interestingly, we find evidence of a positive association between per capita state GDP and violent youth crime. 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 capita 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. The other plausible explanation could be that it is not the level of income which matters for crime but how the income is distributed in the society which have far greater implications. Next, after controlling for time- and state-fixed effects we find our population (log) to increase youth crime. 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, Moura and Silveira Neto 2016). 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. This is similar to the political budget cycles theory to corruption suggested by Vadlamannati (2015) wherein an incumbent, in states within India, considers controlling

corruption based purely on political considerations.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 capita (log) 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. To determine the direction of causality, 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). It is noteworthy that while the Granger causality investigates causality between two variables in a time series data, it does not examine the true cause-and-effect relationship. Rather, it tests whether a particular variable comes before another in the time series. In other words, if we find Granger causality in the data it means variable *x* "Granger-cause" (i.e., precedes) variable *y*. 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 capita (log) on youth unemployment, education measures, and vice-versa. Following de Soysa and Vadlamannati (2012), 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. Granger causality test results for all four sets are reported in Table A – D shown in supplementary file. Our findings from all four sets reveal that we do not find any statistically significant effects of youth crime incidents per capita (log) on youth unemployment and education measures. The *joint F-statistics* show that none of the three lags in the youth crime 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 capita (log) as *joint F-statistics* is less than the thumb rule of 10 and remains statistically insignificant at all three lags (the results on granger causality are available in online appendix). Hence, our results reveal no significant reverse causality.

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 per capita registered under the age group of 18–30 years. Likewise, we also used the 18-30 age group to compute unemployment rates by level 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. These results are reported in Table E in the supplementary file. Second, we re-estimated our GLS 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 year- and state-specific dummies. The results (in Table F in the supplementary file) estimated using negative binomial methods remains similar to those reported in baseline models in Tables 1–4. Third, as an additional test for robustness, we exclude the few observations with extreme values in youth crime incidents reported. Excluding outliers, the baseline results remain robust (Table G, supplementary file), suggesting that results are not driven by extreme values. Next, we replicate the GLS fixed effects models with a simple OLS fixed effects estimator. Our results (reported in Table H) remain robust to using OLS estimator.

Finally, we attempt to control for some form of governance and institutional quality, which can significantly affect crime. Unfortunately, measures like rule of law, corruption or deployment of police, which are usually associated with governance strength are not available at the subnational level in Mexico. Therefore, we use two crude proxy measures for governance. First, we use a measure of federal crime agencies per capita (log). The crime

agencies are independent of state police and report the federal crime incidents. They also assist the state police force in carrying out investigations into these crimes. We use the data on number of federal crime agencies located in each state during 1998-2010 period. Thus, per capita crime agencies (log) is our measure of institutional effectiveness. Our baseline estimations remain robust to controlling for crime agencies, which in itself remains statistically insignificant (Table I, supplementary file). Second, we use a GINI coefficient index available across states as a proxy for governance. While not a conventional measure of governance, there is broad support among academics and policy makers that good governance provides the fundamental basis for economic development (e.g. Kaufmann et al. 2005). Good governance matters not only for income per capita but also for substantially reducing illiteracy and infant mortality. Hence, through reduction in illiteracy and health outcomes, good governance can help to change the access to opportunities for the most disadvantaged in society, leading to a reduction in inequality and hence in crime. It is important to note that inequality has been regarded as a determinant of violent crime and drug war in Mexico (Enamorado et al. 2016). Hence, in the absence of a more direct measure, we use GINI index as a crude proxy for governance. Our results remain robust to controlling for GINI measure, while the effect of GINI on youth crime remains statistically insignificant (Table J, supplementary file). The results of these robustness checks are available in an online appendix.

5. Conclusion

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 extensive challenge with youth across Latin America falling out of education as well as employment, generally referred to as '*ninis*', has been noted in recent policy analyses (de Hoyos et al. 2016). With *ninis* counting one in five youth between the ages of 15 and 24 in the region, Latin America is on a path to potentially passing on their 'demographic window of opportunity' (Birdsall & Sinding 2001) offered by the very low dependency ratios projected for the 2015-2035 period (de Hoyos et al. 2016: 6-7). With Mexico being one of the countries in the region with levels of *ninis* above the regional average, implications of the findings of this study strongly resonate with policy recommendations for extending education and reducing drop-outs through demand-side (e.g. financial incentives) as well as supply-side (e.g. pedagogical and early warning) interventions. More challenging yet are youth employment programs, with formal sector employment programs delivering mixed success, calling for context-sensitive diagnostics and implementation (ibid.: 31-32). With the combined potential for a lost demographic dividend and detrimental developmental consequences of political and criminal violence (Benyishay and Pearlman 2014, World Bank 2011), the underinvestment in young people's human capital represents a double-developmental challenge.

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Table 1: Effect of youth bulge and youth opportunity on youth crime

Dependent variable: Federal youth crime incidents per-head (log)

	(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.

Table 2: Effect of youth unemployment rate by education category on youth crime

Dependent variable: Federal youth crime incidents per-head (log)

	(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.

Table 3: Effect of youth unemployment rate - interactions with education category

Dependent variable: Federal youth crime incidents per-head (log)

	(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.

Table 4: Effect of the density of low-opportunity cost youth on youth crime

Dependent variable: Federal youth crime incidents per-head (log)

	(1)	**(2)**	**(3)**
	FGLS-FE	**FGLS-FE**	**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.

Map 1: Geographical distribution of Federal Crimes incidents by Males (18-24 years) during 1997-2010 period.

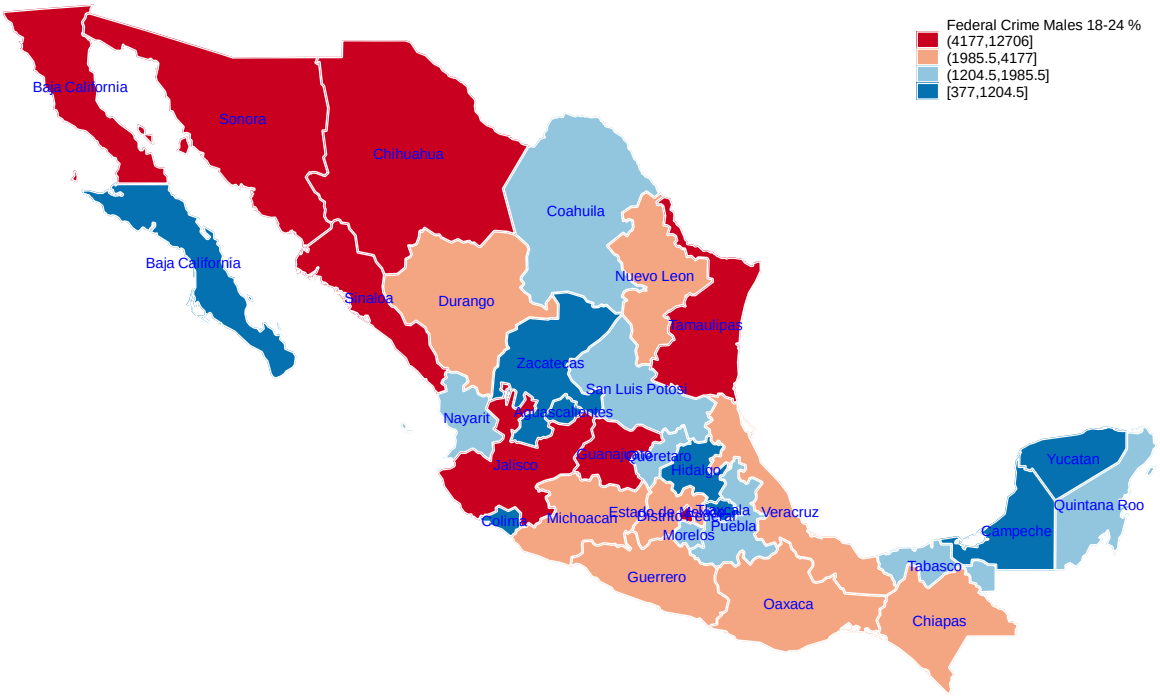
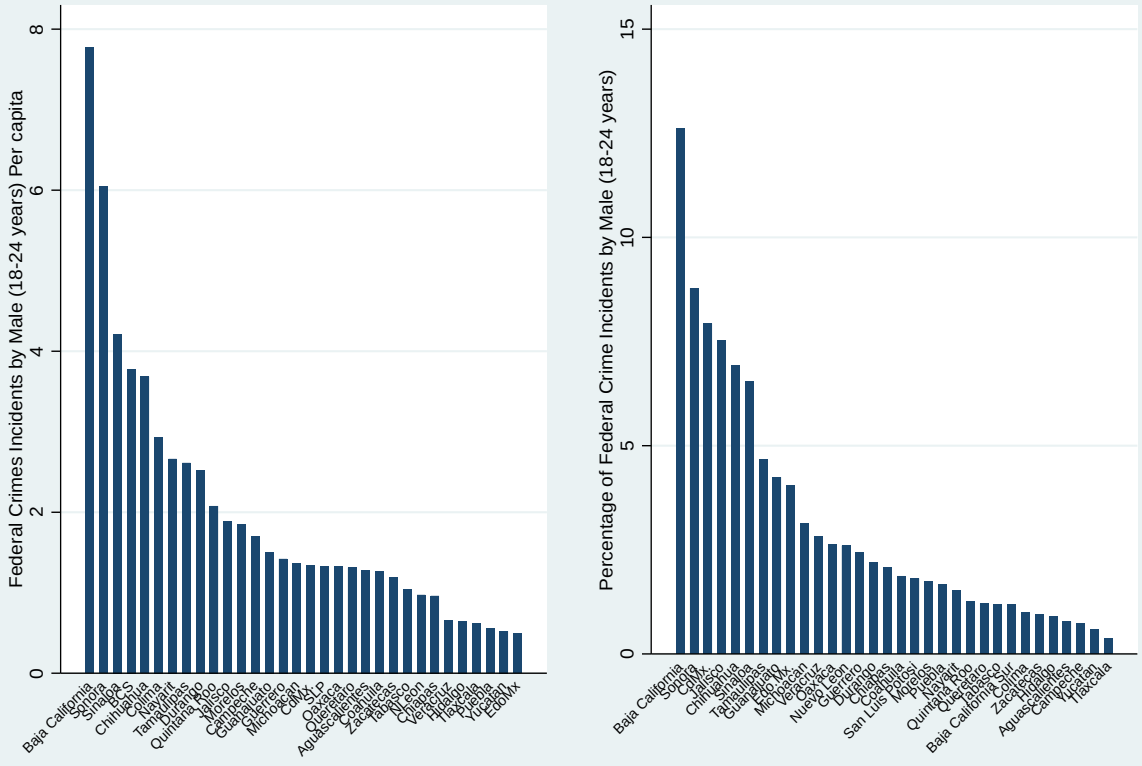


Figure 1: Federal Crime Incidents by Male (18-24 years) in Mexico during 1997-2010



Source: INEGI Data

Figure 2: Youth Bulge, Unemployment Rate in Low Education Stratum & Marginal Effect on Youth Crime

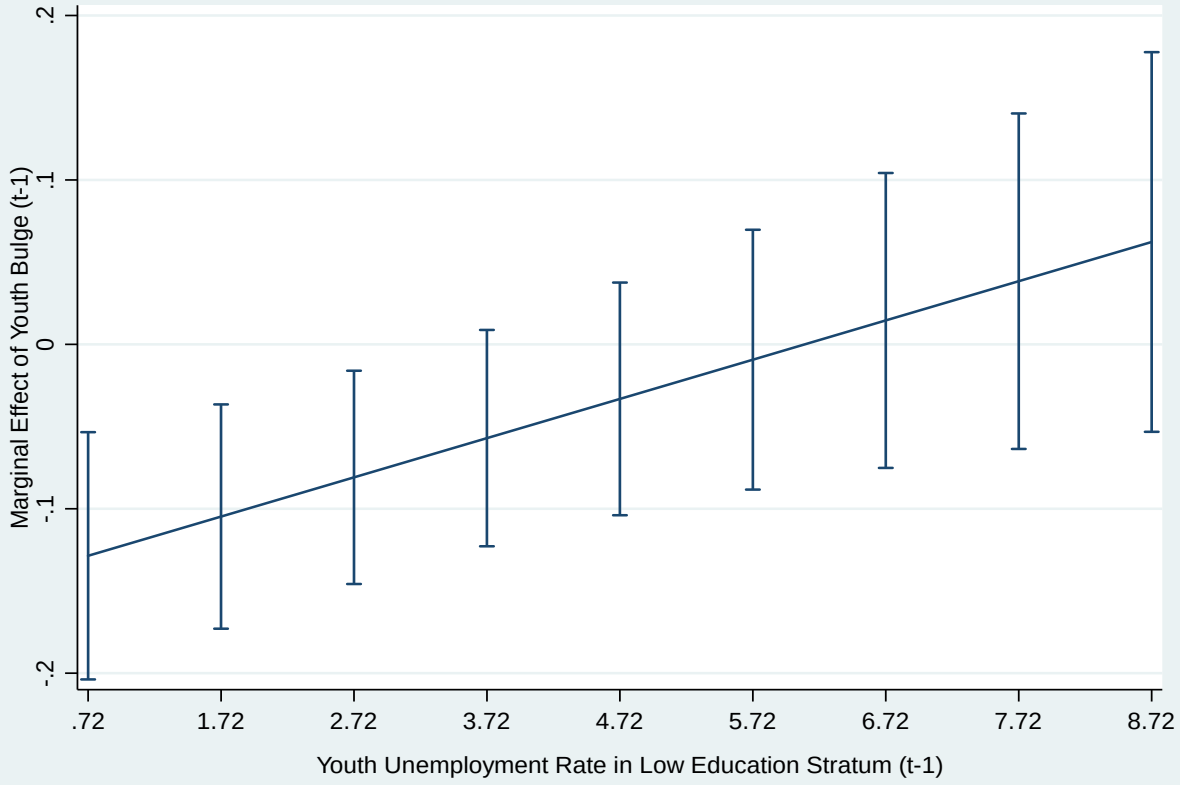
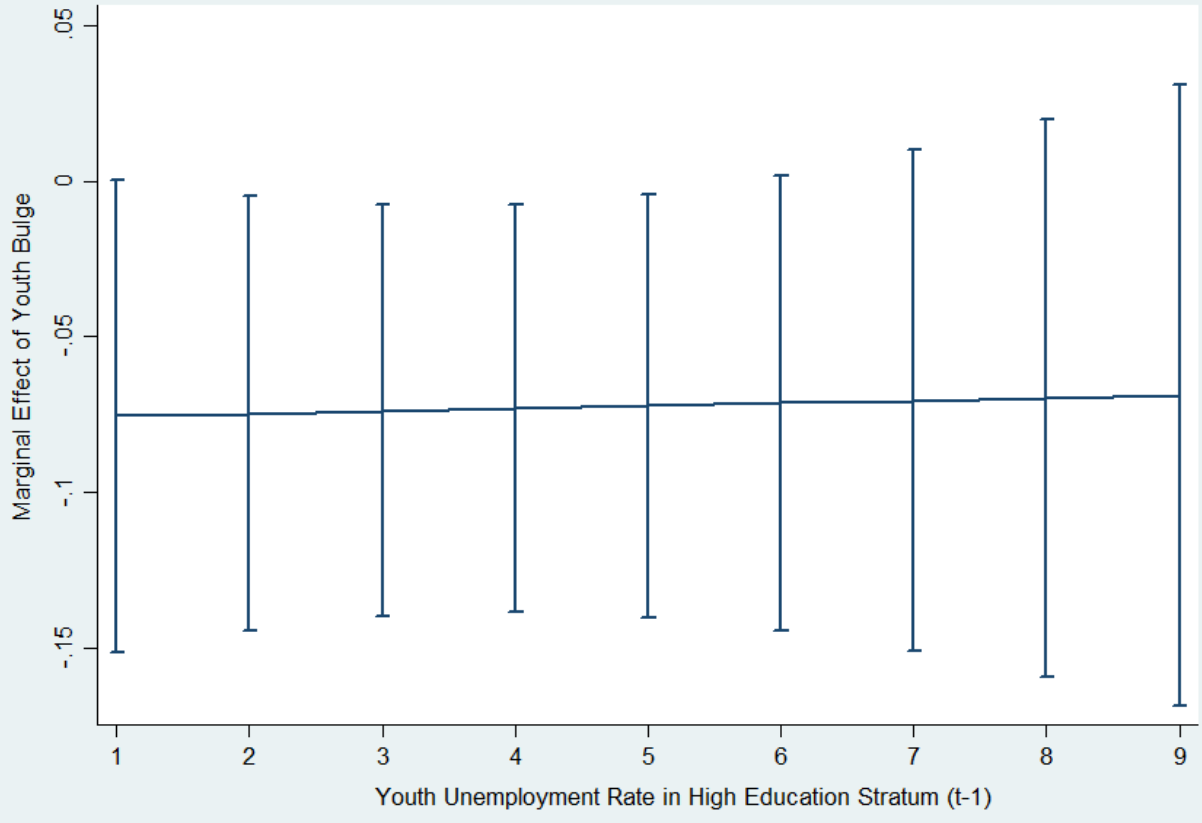


Figure 3: Youth Bulge, Unemployemeny Rate in High Education Stratum & Marginal Effect on Youth Crime



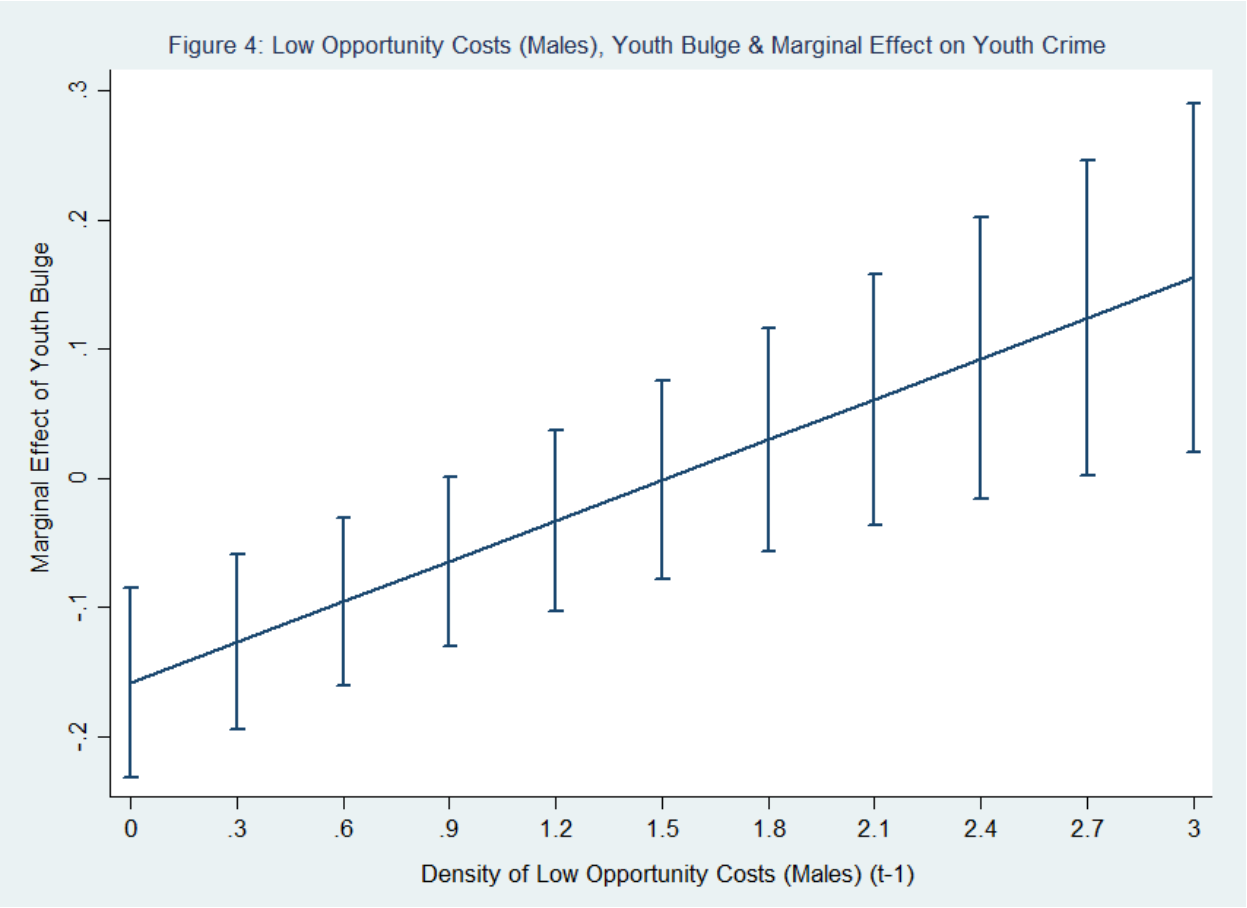


Figure 4: Low Opportunity Costs (Males), Youth Bulge & Marginal Effect on Youth Crime
Marginal Effect of Youth Bulge
-.2
-.1
0
.1
.2
.3
0
.3
.6
.9
1.2
1.5
1.8
2.1
2.4
2.7
3
Density of Low Opportunity Costs (Males) (t-1)