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Reassessing the Demography Hypothesis: the Great Brazilian Crime Shift

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Abstract

Mimicking the US in 1980 and 1990s, Brazil is a remarkable case of a major shift in homicides. After increasing steadily throughout the 1990s and the beginning of the 2000s, homicides reached a peak in 2003, and then fell. I show a strong time-series co-movement between homicide rates and the percentage of the population in 15-24 age bracket. Using a panel of states, I find a very high elasticity of homicide with respect to changes in the 15-24 year-old population (2.4), after controlling for income, income inequality, and state and year fixed effects. I then focus on the case of São Paulo, the largest state in the country, and whose shift in homicides has been particularly acute. City-level panel elasticities are similar to the state-level estimates. Furthermore, the demographic shift in São Paulo was more pronounced than the national one, explaining the particularly large shift in homicides in São Paulo. The large cohort born from the mid 1970 through the early 1980 is the result of a sharp reduction in infant mortality only belatedly followed by acceleration in the reduction of fertility. In line with the Easterlin Hypothesis (Easterlin [1980]), this large cohort faced tough economic conditions. Educational attainment ceased to improve for this cohort, and unemployment rates upon entering the job market were exceptionally high. Thus, the large homicide shift in Brazil is produced by a particularly large and socially fragile cohort.

KEYWORDS: Age Structure, Demographic Change, Homicides

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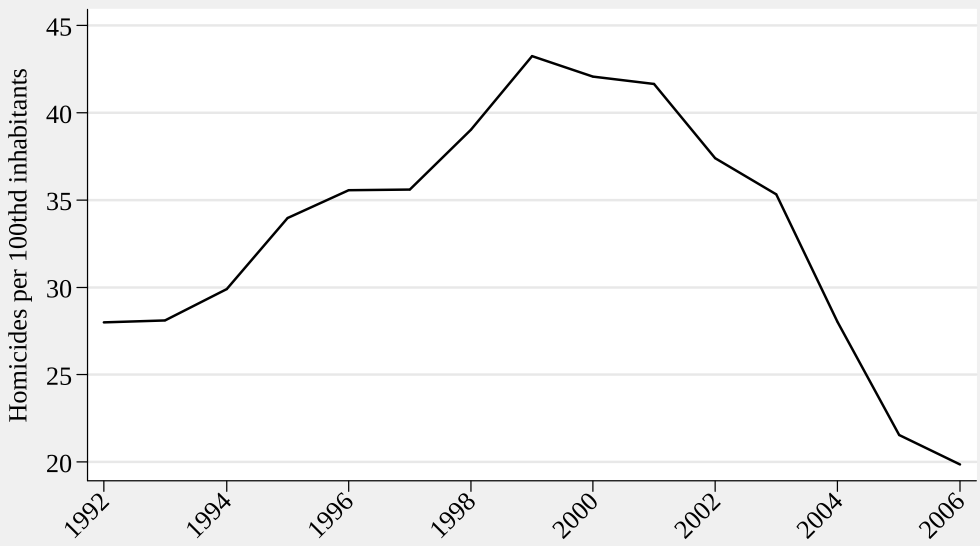
1. Introduction

The state of São Paulo in Brazil has received a significant attention in the domestic and international media for its sharp swings in the 1990s and 2000s decades, which practically mimic the time-series patter in the US cities a decade early.[1] Violence increased steadily during the 1990s. In the São Paulo Metropolitan Area (SPMA), the homicide rate jumped 54% in 1990s, from 28 per 100,000 inhabitants in 1992 to a peak of 43 homicides per 100,000 inhabitants in 1999, when they started to fall sharply. In 2006, there were 20 homicides per 100,000 inhabitants (28% lower than in 1992).

Fig. 1 Homicides Rates in São Paulo
1992-2006



Source: DATASUS - National Ministry of Health

This phenomenon was widespread: a similar pattern arises if we consider only the city of São Paulo, the São Paulo Metropolitan Area excluding São Paulo and other large cities statewide (see De Mello and Schneider [2010]).

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[1] In the last four years *The Economist* reported twice on the murder trends in São Paulo (*Protecting citizens from themselves*, Oct 20th 2005 and *Not as violent as you thought*, Aug 21st 2008).

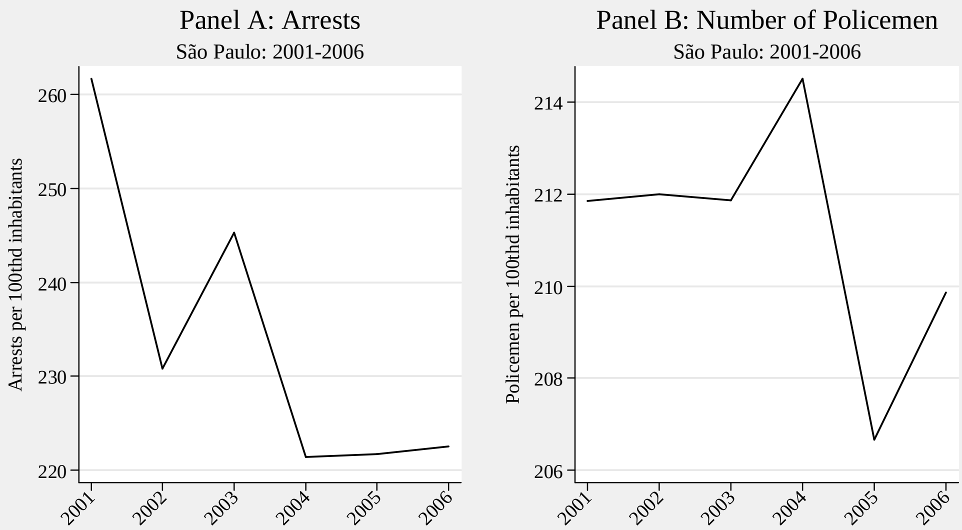
Again similar to the American case, candidates abound for explaining this major shift in violent crime (Zimiring (2007)). Improvements in enforcement occurred during the period. Among them, the most notable was the adoption of a unified data and intelligence system, INFOCRIM (a version of *Compustat*), although the timing and scope exclude INFOCRIM as a first-order explanation.[2] Incarcerated population, number of police officers and number of arrests are also common culprits. Figure 2 shows the number of arrests and policemen per 100,000 for the 2001-2006 period (for which we have data). Arrests and police, if anything, fell in the 2000s, suggesting reverse causality. Another candidate is incarceration. Figure 3 (borrowed from De Mello and Schneider [2010]) depicts the prison population per 100,000 inhabitants in the state of São Paulo from 1994 through 2006 (period of data availability). Incarceration rates rise monotonically, suggesting that for the period of the 1990s incarceration reacted to crime, and not contrary. Thus, it cannot explain both the increase and the reduction in homicides.[3]

[2] Data from the Secretaria de Segurança do Estado de Sâo Paulo, the state-level enforcement authority, shows that INFOCRIM started in 2000 in the city of São Paulo, a full year after the 1999 reversal. Slowly, precinct by precinct, it was implemented in other cities within the São Paulo Metropolitan Area. Only in 2005, implementation started outside the SPMA.

[3] Municipal level policies were also implemented. Among them a few are worth mentioning: the adoption “dry laws” (which are restrictions on the recreational sales of alcohol), the creation of municipal police forces, and the adoption of DISQUE-DENÚNCIA (an anonymous hotline to report crimes). Evidence in Biderman et al (2010) suggests dry laws had a 10% causal impact on homicides. Nevertheless, these policies have neither the right timing nor scope to account for the aggregate movements in homicide rates.

Fig. 2 Arrests and Number of Policemen

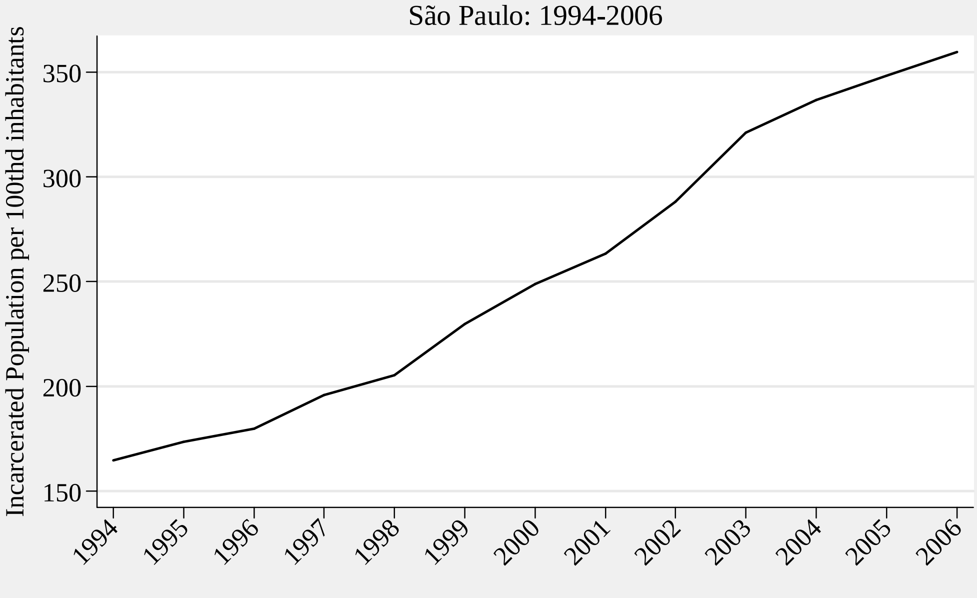
per 100thd inhabitants



Source : Secretaria de Segurança Pública do Estado de São Paulo

Fig. 3 Incarceration Rates

São Paulo: 1994-2006

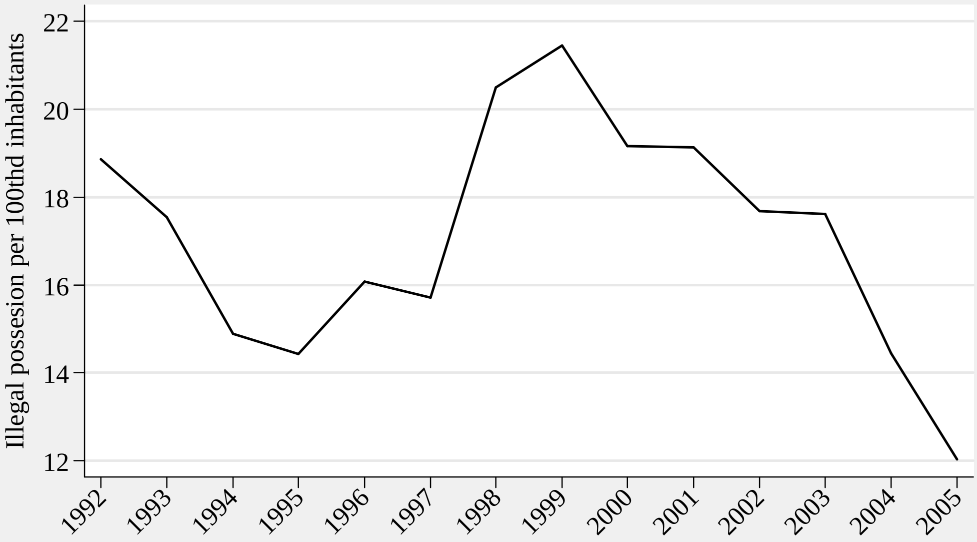


Source: Secretaria da Administração Penitenciária do Estado de São Paulo

Finally, gun control is a more serious candidate for explaining the aggregate trends. Starting in 1997 the state of São Paulo cracked down on the illegal possession of firearms, leading local analysts to attribute the sharp swings in homicides improvements in gun control (see *The Economist* 2005). Figure 4 depicts illegal firearm possession over the 1992-2005 period.

Fig. 4 Illegal Firearm Possession Rates

São Paulo: 1992-2005



Source: Secretaria de Segurança Pública do Estado de São Paulo

The time-series pattern of reported illegal firearm possession seemingly matches the pattern of the homicide data. However, a few subtleties obscure causal interpretation. First, there is the issue of measurement. Reported illegal firearm possession is a combination of the prevalence of firearms in the population (which one wishes to observe) and police enforcement (which one wants to isolate). Consider the increase in illegal possession starting in 1997. This movement is in line with the anecdotal evidence that police cracked down on illegal gun possession (see Goertzel and Kahn [2007]). Thus, the hike in late 1990s less is not due to an increase in firearm prevalence but to a tightening in enforcement. Notwithstanding this fact, if one is prepared to assume away the problem of measurement and consider that the movement in illegal possession in the late 1990s was in fact an increase in prevalence, then one must also assume it was so in the early 1990s. But then it becomes hard to reconcile the movements in firearm prevalence and homicides. In summary, it does seem that movement in guns prevalence might have played a role in the reduction of violence in the 2000s but it explains neither the increase in violence in the 1990 nor the reversal in the late 1990s

This paper argues that demographic changes play a crucial role in explaining the sharp shifts in violence. This assertion is supported using the following strategies. First, I show that the movements in homicides are not particular to the state of São Paulo. Much less heralded in the media, but equally important, the country as whole experienced similar shifts in homicide, albeit belatedly and somewhat less pronounced. We show that the trends in homicides are matched closely to the size of the population between 15 and 24 years old, the most crime prone age. Changes in the size of the crime prone cohort explain some 80% of the time-series variation in homicides at the national level. Competing explanations may rationalize the decline in homicides in 2000s (and most likely contributed to it), but are unable to explain the sharp in increase in homicides in 1990s. Only demographic rationalizes both phenomena.

In addition to the pure time-series evidence, I use a panel of Brazilian states during the 1990s and the 2000s to recover a more credible causal effect of age-structure. With both time-series and cross-state variation, on can control for all aggregate shocks and for all time-invariant heterogeneity among states. The only variation left to estimate the impact of demography is how the age structure changed differently in different states. This is important for interpreting the estimated elasticity as causal because the period is ripe with aggregate shocks, such as monetary stabilization that enriched the poor and reduced income inequality, or a few nationwide policies such as the "Disarmament Law" (see footnote 4). In addition, long-term age-structure differences between states (cross-section variation) could correlate with other demographics that affect violent behavior. Since most decisions that produce age-structure differences are made decades before, and because we control for state and year fixed-effects, I am quite confident that the variation in age structure used to estimate the homicide elasticity is exogenous, and thus we can interpret it as causal.[4] This is an additional advantage of a demographic explanation: most other variables that may affect violent behavior (police, guns, incarceration, etc…) are ripe with endogeneity problems, which prevents causal interpretation. In summary, our procedure is credible as it gets short of randomization of age structure, which is clearly impractical as a research design.

I recover elasticities between 1.5 and 2.6 of homicides to changes with respect to changes in the 15-24 year-old population. Since demography has a common component across states (and year specific effects are included), it is not surprising that demography has smaller explanatory power with panel data. But it still explains a large share of the variation in homicides. Using these estimates for the elasticity, changes in the 15-24 year-old population explain 60% of the nationwide variation in homicides.

The analysis goes one step further in terms of disaggregation. Borrowing from the analysis on De Mello and Schneider (2010), I use a panel of cities in the state of São Paulo to re-estimate the elasticity of homicides to changes in the 15-24 year-old population with another sample. The estimated elasticity is now stronger - 4 - but within the same range.

Besides documenting the reduced form impact of changes in age structure on violent crime, I show the mechanism through which the demographic shift produced a crime-prone generation. During the 1970s, infant mortality declined abruptly as a result in basic health and sanitation condition. Fertility is endogenous and depends on infant mortality rates, but only belatedly accelerated its rhythm of decline (see Soares and Birchenall [2009]).[5] This drop in mortality, itself a sign of improvement in social welfare, had a side-effect. In rapidly urbanizing country such as Brazil, with a precarious supply of public services, I show that this large cohort put an enormous pressure on the educational system, deteriorating the rates of improvement in educational attainment. The Easterlin Hypothesis (Easterlin [1980]) that the size of the cohort affects its economic fortune seems operative. I show that, as the late 1970s cohort entered the job market, youth unemployment increased (above and beyond unemployment in general) and real

[4] Migration may affect age structure contemporaneously. In section 7.D I show evidence that migration is not relevant enough empirically. Thus, it does not pose a serious threat to the identification strategy.
[5] Since Thompson's original work (Thompson [1929]) demographers have known that the decline in fertility only belatedly follows the improvements in mortality.

wages declined. In addition to their misfortune, members of late 1970s cohort faced a particularly bad job market for reasons other than demography: trade liberalization and technological change displaced unskilled labor in the short-run. *Mutatis mutandis*, labor market conditions improved in the 2000s. Thus, cohort size, and the attending fragility, played a major role in explaining *both* the spike in homicides during the 1990s *and* the decline in the 2000s.

Received literature is ambiguous as to the impact of age structure on aggregate crime. Early work found strong association between demographics (including age structure) and the rise of violence in the US during the 1960s and 1970s (Chilton and Spielberger [1971], Ferdinand [1970], Sagi and Wellford [1968]). Looking a longer trends, Fox (2000) shows that demographics match longer-term movements in homicides, but misses the sharp increase in violence in late 1980s. However, the author argues that current elasticity estimates suggest that demographic changes did have a role in the 1990s decline, albeit a small one. Zimring (2007) finds that demography is the only factor broad enough to explain the geographical scope of the "great American crime decline" in the 1990s. In contrast, Levitt (1999) finds only a small role for demography in explaining aggregate crime. See Marvel and Moody (1991) for a survey. Using elasticities normally recovered in the literature, it would be hard to explain quantitatively large shits in crime rates with changes in age structure. The literature normally interprets this fact as evidence of the limited role of demography (Levitt [1999], Zimiring [2007]). On the other hand, it is not uncommon for changes in demography to match movements in crime qualitatively (I show this for Brazil, and it is also true for the US except for the late 1980s spike in violent crime). I call this the "Demography Puzzle". Illustrative of the puzzle, consider Zimiring's, comment on the co-movement of long-term American and Canadian crime rates (Zimiring [2007]). He notes:

> "*But what joint causes might have operated in Canada and the United States throughout the 1990s? This uncomfortably open question is of obvious importance to **rethinking** the causes of the U.S. decline…What would explain the 30% or so of slow and steady decline over the nine years following 1991 in the United States and Canada? The **only***

traditional theory of decline supported by parallel U.S. and Canadian data trends is the ***decline of high-risk age groups as a percentage of the population****. But even if all the decline in youth share of population that occurred both in 1980s and 1990s is counted toward the crime decline that was confined to the 1990s, it would be difficult to find many criminologists who would expect that feature alone to produce a crime decline greater than 10%, and even that 10% should have been spread more evenly across two decades in both countries.[But* ***the demographic similarity****] between Canada and the United States over the period 1980 onwards invites, if it does not* ***demand****, a reconsideration of the magnitude of age structure effects on crime."* [6]

Perhaps the answer to the "Demography Puzzle" rests on the Easterlin Hypothesis. Decompositions in Levitt (1999) and Zimiring (2007) rest on the assumption that age-specific crime rates do not vary with group size. My results suggest that this procedure may understate the impact of changes age structure because the age-specific crime rates may vary with cohort size, at least with Brazilian data. With Brazilian data, age-group size does cause age-group crime rates.[7]

It is hard to overemphasize the importance of these results. According to the World Health Organization, in 2002 Brazil was the 11th most violent country among its 192 member, with a murder rate of 32.6 deaths per 100,000 (about six times the US rate).[8] Although it is hard to address age-structure issues with policy, shedding light on the underlying causes of violence in such a violent environment is important *per se*. Furthermore, our results suggest that the returns to investing in reducing youth fragility go beyond gains to productivity. The impact on violence should be factored in when computing cost/benefit ratios.

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[6] My emphasis.

[7] Levitt (1999) does provide evidence to defend the decomposition keeping age-specific crime rates constant. I show in section 7 that with Brazilian data, the "constant age-specific rates" assumption is not warranted.

[8] The WHO figure for Brazil is 8% higher than the figure from the Brazilian National Ministry of Health, which is the source. Half of this difference may be explained by the fact the WHO uses the 2000 population from the census when computing its violent death rate. The author has no explanation for the other half.

The paper has eight sections including this introduction. In section 2 contains an overview of the socio-economic trends in Brazil. A description of the institutional background on law enforcement is also provided. Section 3 describes the data used. Section 4 provides a review of the literature on demography and crime. It also examines the plausibility of the age-structure hypothesis with Brazilian victim and perpetrator data. Section 5 provides the nationwide time-series evidence linking the size of the 15-24 year-old group to aggregate homicides. Section 6 presents the panel evidence. One subsection contains state-level panel elasticity estimates. Another has the city-level panel evidence from the state of São Paulo. I use the estimated elasticities to show that changes in the size of the 15-24 age group explain between 60 and 80% of the aggregate movements in homicide in Brazil. Section 7 contains an in-depth discussion of the results. I start off by showing that the 15-24 year-old age-specific homicide rate increases with the size of this age group. Thus, decompositions based on "holding age-group homicide rates constant" are misleading with Brazilian data, which further justifies the idea of using elasticities estimated from panel-data regressions. Then I document several facts about the socio-economic conditions of the late 1970s-early 1980s cohort. First, I show the reaons why this cohort was so large. Then I present ample evidence that this cohort is not only large but fragile, re-enforcing the crime-prone nature of the 1990s. Lastly, I show that, *mutatis mutandis*, the following cohort, besides smaller, faced much less stringent socio-economic conditions (perhaps helped by the fact that it was smaller). Section 8 concludes.

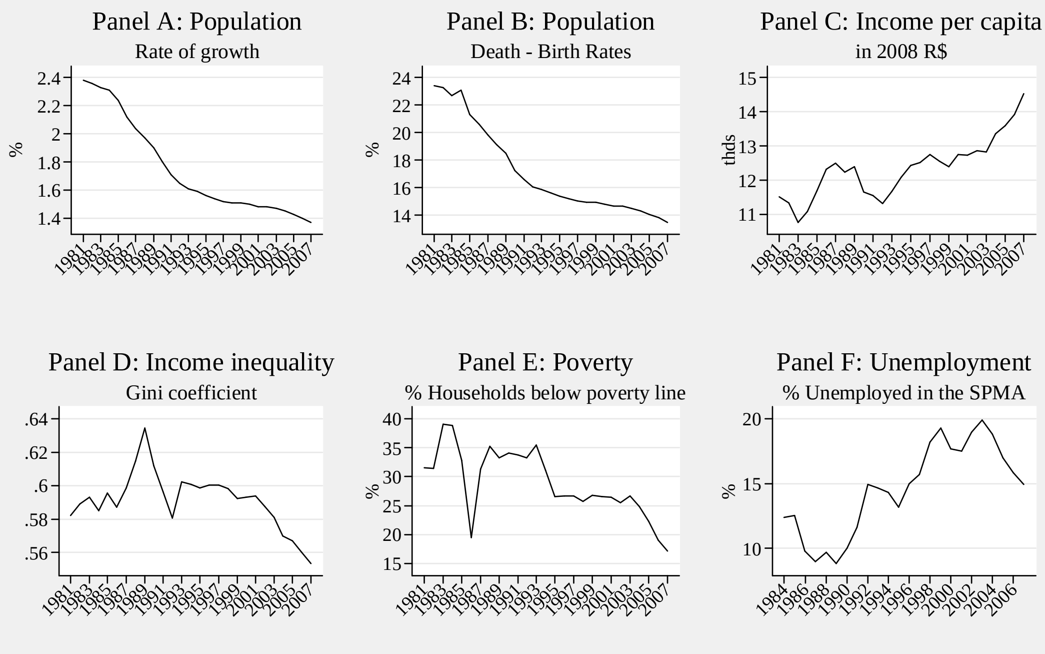
2. Socio-economic Trends and Institutional Background

2.A Socio-Economic Trends

Brazil is a large middle-income country. In 2006 the population was 187 million inhabitants with an income per capita of U$6,700 (in 2007 US dollars). After twenty years of military dictatorship, civil ruling returned in 1985 and the first president was elected by direct ballot in 1989. The 1980s and 1990s were a traumatic decades, economically and socially. After a decade of skyrocketing economic growth, the Latin

American debt crisis hit the Brazilian economy in 1982, slowing growth throughout the 1980s and early 1990s. Years of lax monetary policy culminated in cycles of super and hyper-inflation followed by unsuccessful stabilization plans. Finally, the 1994 stabilization plan, the first effective stabilization plan brought inflation under control, allowing the reorganization of the economy. Brazil also liberalized the economy in the 1990s, privatizing many state-owned enterprises, and opening its economy to foreign trade. The several panels in figure 5 depict the evolution of key socio-economic variables over the 1991-2006 period.

Fig. 5 Brazil: Macro Variables over the 1981-2007 period



Source: Instituto Brasileiro de Estatística e Geografia (IBGE) e Fundação SEADE

Panel A shows the demographic transition from the 1980s through the mid 1990s. After the rapid acceleration in the late 1970s - early 1980s, population growth slowed considerably in the 1990s. As we will see below this movement in population is crucial for the interpretation of the demographic hypothesis advanced in this paper. Comparing panels A and B one sees that birth and death rates respond for almost all movements in populations, i.e., migration is not an important phenomenon during this period. Panel C shows income per capita rather stagnant during the 1980s and 1990s. Economic growth picked up momentum after 2003, following the rapid acceleration of the world economy in the 2004-mid 2008 period. Income inequality fluctuated around high levels in the 1980s, increasing rapidly in the late 1980s and early 1990s, when inflation picked up momentum during. With stabilization in 1994 inequality returned to the levels of the 1980s; finally, since 2001 it has been declining steadily. Poverty follows a similar pattern (panel E). Finally, panel F depicts unemployment rates in the SPMA. Several structural shocks hit labor markets in the 1990s, most notably liberalization, privatization and technological changes that disfavored labor (Gonzaga et al (2006)). As a consequence, unemployment increased steadily during the 1990s, improving only in the mid 2000s. Among 15-to-24 year olds deterioration of labor market conditions was particularly strong, perhaps as a consequence of size of the cohort (seen section 7).

The story of figure 1 is compatible with the movements in aggregate crime rates during the 1990s and the 2000s. The rapid population growth in the late 1970s early 1980s produced a large young cohort in the 1990s. *Mutatis mutandis*, population growth abated in the late 1980s and early 1990s. Thus, the large crime-age cohort in the 1990s was followed by a small crime-age cohort in the mid 2000s. At the same time, economic conditions, especially in labor markets, which were generally unfavorable in the 1990s, improved considerably in 2000s.

2.B Institutional Background

Brazil is a federal republic with three layers of government: federal, state and municipal.[9] The main bulk of law enforcement is done at state-level. Executive and administrative authority rests with the state-level secretaries of security authorities (the *Secretarias Estaduais de Segurança Pública*), which respond directly the governor who allocates the budget to the secretary. The administrative and strategic decisions are done by the state security secretary, which is appointed by the governor. Some strategic decisions are determined by law. For example, by constitutional mandate, the number of policemen in the state of São Paulo has to be roughly constant in per capita terms across

[9] President, governor and mayor are elected by direct ballot.

cities. The execution of enforcement is shared between two corporations that respond to the secretary: the military police, responsible for patrolling and repression, and the civil police, which is investigative. The commanders of the two police forces are also appointed by the governor. Differently from the US, sheriffs are not elected but appointed among career officers. The institutional structure of state-level police is determined by the federal constitution.

The federal and municipal levels participate in law enforcement but to a much lower degree. The federal police force is rather similar to the American FBI, investigating mainly cross-state crime (mostly smuggling and drug trafficking), white-collar crimes and corruption. Differently from state-level *Secretarias*, municipal police forces (*Guardas Municipais*) are not mandatory by federal law but a choice of the municipality. In fact, in 2006 no more than 700 municipalities had a *Guarda Municipal*. In most cases *Guardas Municipais* do mainly the enforcement of traffic law, and the majority of *Guardas Municipais* do not carry firearms.[10]

Since the main bulk of enforcement is done at the state-level, nationwide articulated reactions to increase in homicides during the 1990s were rare (see figure 6 below). The only noticeable exceptions are: 1) the creation of the National Force in 2004, a federal police force to be deployed in extreme circumstances, or if a member states request help; 2) the "Disarmament Law" in December 2003 (see footnote 4 above).

3. Data

We use several sources of data. State-level murder data come from DATASUS, the hospital database of the National Ministry of Health. Although the data go back quite a long time, the taxonomy of violent deaths changed in 1996.[11] For the state-level panel model we use data from 1996 onwards to keep consistency over time.[12] For depicting

[10] There are exemptions. In the state of São Paulo, most *Guarda Municipais* carry firearms and are involved in community policing.
[11] From 1996 onwards, the system of morbidity taxonomy has been the 10th International Classification of Diseases (ICD-10), which substituted the previous system (the ICD-9). Differences in classification for deaths by external causes exist and the Brazilian ICD-9 and 10 series are not compatible with each other. More details can be found at the World Health Organization website at http://www.who.int/classifications/icd/en/
[12] Elasticity estimates are similar if the series is extended back to 1991.

national aggregates, when inconsistency is less costly, we use data from 1991. Also from DATASUS are data on the age distribution of homicide victims.

The city-level homicide data for the state of São Paulo come from *the Secretaria de Segurança Pública de São Paulo* (SSP-SP), the state-level enforcement authority. Hospital data are available, but I prefer police report data the following reason. At the city level, geographical location of hospital murder data is not obvious. The murder victim may be taken to a hospital in another city. Hospital data systematically overstate the homicide rate in larger cities, and, worse still, the bias may be changing over time because more hospitals at constructed in smaller cities at the margin. Both facts suggest that police data are superior.

Country-level demographics are from the *Instituto Brasileiro de Estatística e Geografia* (IBGE), the Brazilian equivalent of the Bureau of Statistics. For census years (1991 and 2000) full population counts by age-groups are available. For non-census years in the 2000s, population by age-group is available from Pesquisa Nacional Amostral de Domicílios (PNAD), an annual household survey conducted by IBGE. The PNAD has a sample design but it is representative at the state level. For non-census years in the 1990s, and for all years at the city level, population by age-group is projected based on the 1991 and 2000 census, and the population counts of 1996 and 2007.[13] For the state of São Paulo we use data from Fundação SEADE, the state-level think tank. Finally, data on the age distribution of perpetrators are from the Ministry of Justice.

4. The Age-Structure - Violence Hypothesis: Literature and Patterns in Brazil

4.1 A Brief Review of the Literature

At the individual level, the crime - age *nexus* is one of the most robust relationships in all social sciences (a very non-exhaustive list of more recent work include Wilson and Herrnstein [1985], Blumstein [1995], and Cook and Laub [1998]).

[13] Results are similar if for the 2000s use projections based on the 2000 census and the 2007 population count.

Recent US victim and perpetrator data strongly suggest a link from age-structure to violence. In 1993, an 18-24 year-old American was roughly 50% more likely to be murdered than a 25-34 year-old, the second highest category, with the difference increasing overtime (Fox [2000]; Rosenfeld [2000]). Furthermore, the 15-18 group has become increasingly relevant as victims of homicide (Fox [2000]).

Besides being preferential victims, members of the 18-24 year-old age group are the main perpetrators. At the height of the American crime cycle (late 1980s/early 1990s), the 18-24 age group offending rate was more than 2.5 times that of the 25-34 group (Fox [2000]). Interesting, the 25-34 group had higher victimization rate than the 14-17 group, but the offending rate of the later was twice as large.

Despite the individual level evidence, recent literature is ambiguous as to the importance of changes in age structure to explain aggregate crime. Fox (2000) find that demography explains the major homicide trends from the mid 1960s through the mid 1980s, but account neither for the increase in violence in late 1980s nor for the reduction in the early 1990s, perhaps because the two phenomena are one and the same. Holding age-specific murder rates constant, Levitt (1999) finds that changes in age-structure explain less than 10% of the aggregate time-series variation over the 1960-1995 period. Zimiring (2007) examines in depth all the explanations for what he calls “the great American crime decline”. He shows that demographic trends were favorable in 1990s. Similarly to Levitt (1999), Zimiring shows that, holding age-specific homicide rates constant (either *ex-ante* or *ex-post*), changes in age structure cannot account for the magnitude of the shift in homicides.

The interpretation of the decomposition hinges crucially on the assumption that age-specific homicide rates do not change with the size of the age group. With Brazilian data, I find that the size of the 14-25 age group affects homicide rate for ages 15-24, which invalidate procedures such as Levitt’s (1999) (see section 7). Brazilian data seem to confirm Easterlin’s Hypothesis (Easterlin [1980]) that larger cohorts face increased difficulty entering the job market, which deteriorates the prospects of careers in the legal system. In this case, the age-group crime rates are themselves a function of the group size. Section 7 contains: 1) direct evidence that age-group victimization rates are a function of population; 2) and evidence on the mechanism behind this fact, i.e., a deterioration of socio-economic indicators for the late 1970s – early 1980s cohort.

The issue of youth fragility and homicides has received considerable attention in the recent literature. Donohue and Levitt (2001) claim that the legalization of abortion in the early 1970s explains the shift in American homicides in the early 1990s. The specific claim is highly disputed (Rosenfeld [2004]; Joyce [2004]; Sorenson et al [2002]; Zimring [2007]). But the link between youth fragility and crime is plausible, and has support in the literature (see Zimiring (2007)). In a sense, this paper is bridge between the ideas of demography and fragility as explanations to violent crime. As argued by Zimring (2007), a large (small) 15-24 year population favors increases (reductions) violent crime. This effect, however, is compounded when youths are subject to unfavorable socio-economic conditions, as the Easterlin Hypothesis suggests. Furthermore, demographers suggest a quality-quantity trade-off in parental investment in offspring (Van Bavel [2006]). Thus, the very fact that a cohort is large reduces the quality of upbringing. These effects may be compounded when socio-economic environment is already fragile, as in the case of Brazil in the 1990s. It would be surprising if a particularly large cohort had a significant impact on crime rates in highly developed European welfare states.

4.2 Brazilian Victimization and Offense by Age Group

Brazilian victimization and offending data follow a typical pattern. Figure 6 shows the age distribution of homicide victims in two periods: 1996 through 2000 and 2001 through 2006. Persons aged between 15 and 24 years represent almost 40% of homicide victims. The second most victimized category is 25-34 with roughly 30% of victims. Overtime the 15 and 24 year-old group becomes slightly more important. If one uses data from the state-level secretaries of security, the 15-24 age group responds to a higher number of homicides, around 45% in 2005.[14]

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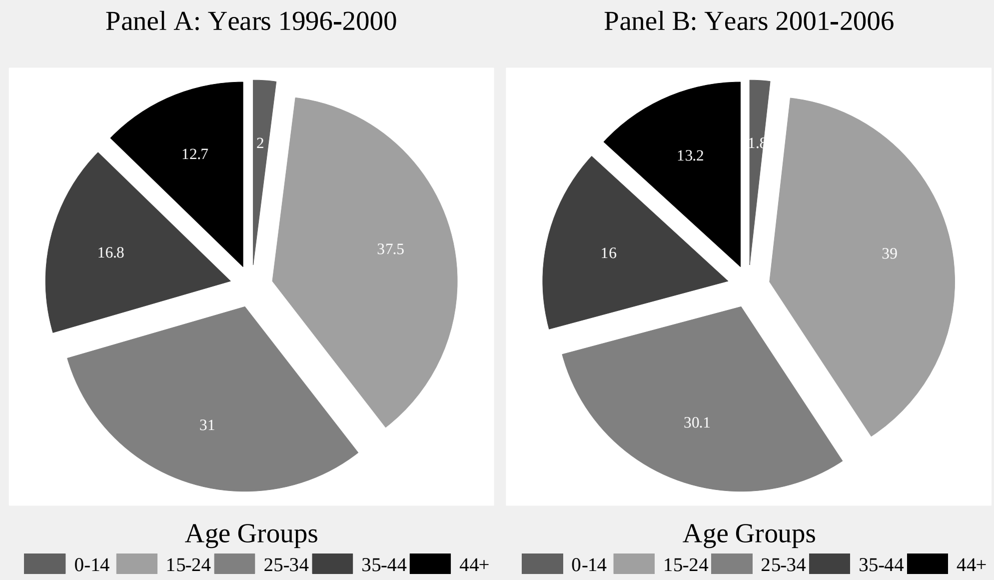
[14] See Ministério da Justiça (2006).

Fig. 6 Distribution of Homicides by Victim Age

in %



Source: DATASUS

In Brazil, no times-series on offenders by age is available but the Ministry of Justice compiled one snapshot (see footnote 10 above). From Jan-2004 through Dec-2005, almost 50% of the all homicides in 2005 were committed by persons in the 15-24 age group. In summary, victim and perpetrator data suggest that changes in demography could have a first-order impact on homicide rates.

5. Aggregate time-series patterns

Figures 7 and 8 summarize the story of the paper. Figure 7 shows the evolution of homicides rates in São Paulo and in Brazil excluding São Paulo. Some facts arise. Homicides increased in the 1990s countrywide and in São Paulo. São Paulo reached a peak in 1999, four years earlier than in the rest of the country. Finally, the decline in São Paulo has been more pronounced so far.

Fig. 7 Homicides Rates per 100thd inhabitants: Brazil versus São Paulo

1992-2006

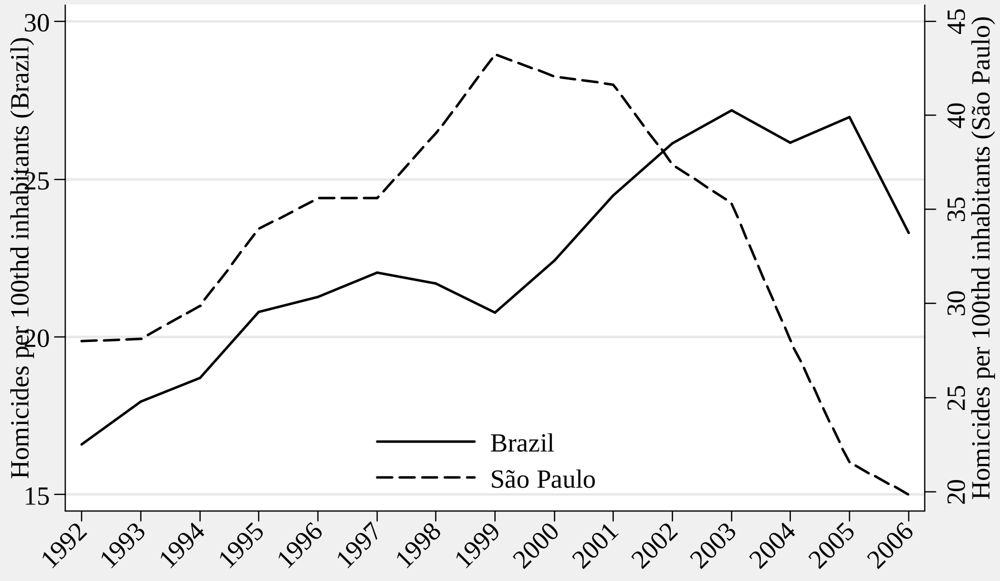
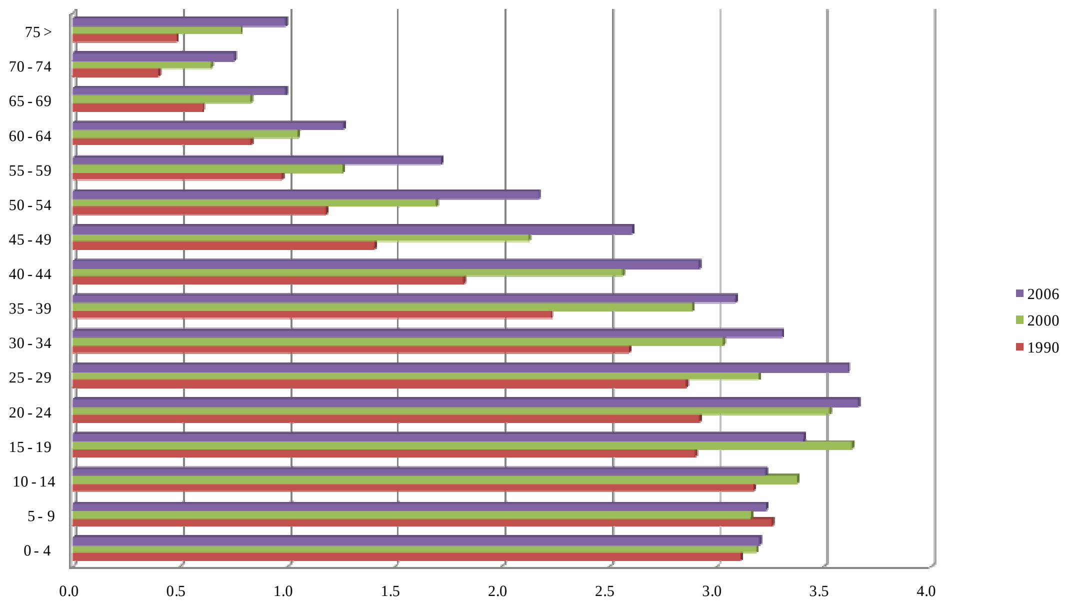
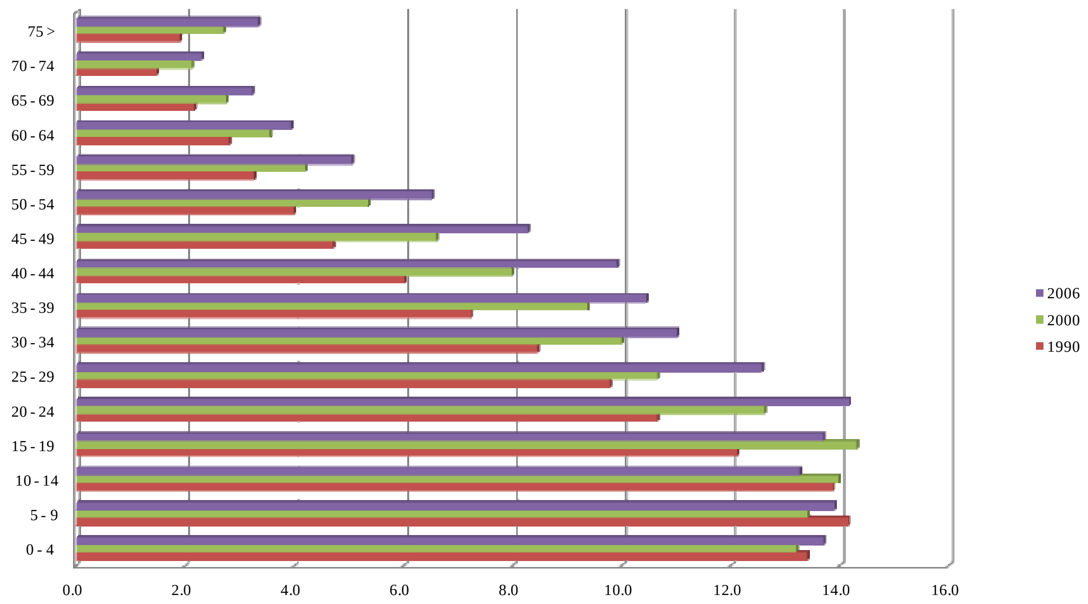


Fig. 8a Evolution of the Age Distribution in the State of São Paulo



Source: Fundação SEADE

Fig. 8b Evolution of the Age Distribution in Brazil excluding São Paulo



Source: Instituto Brasileiro de Geografia e Estatística (IBGE)

The two panels in figure 8 depict the age distribution since 1990 for Brazil and São Paulo. Mimicking the homicide trends, the size of the 15-24 year-old age group increased monotonically in the 1990s and then reached a peak. Similarly to homicides, São Paulo reached peak five years before the rest of the country. Although suggestive, the time-series correlation may be spurious. Table 1 sheds light on the interpretation of figures 7 and 8.[15] I estimate several regressions of homicides on the size of the 15-24 year-old population to check whether the pattern in Figures 7 and 8 survive some simple robustness checks.

[15] Figure 7 shows homicides up to 2006. In the regression we include the 2007 figure. The discrepancy is due to the availability of homicide data at the state level for the year 2007.

TABLE 1: Brazil over 1992-2007 period

Dependent Variable: Homicides per 100,000 inhabitants

	(1)	**(2)**	**(3)**	**(4)[B]**	**(5)[B]**
15-24 year-old population (in millions)	1.18	3.32	1.87	2.89	2.35
	(0.30)***	(0.56)***	(0.52)***	(0.27)***	(0.76)***
Population	No	Yes	Yes	No	Yes
Polynomial of Year?[A]	No	No	Yes	No	Yes
Observations	16	16	16	16	16
R^2	0.53	0.82	0.94	0.87	0.96

All standard errors are Newey-West corrected for heteroskedastic and first-order autocorrelation.

***: significant at the 1% level

**: significant at the 5% level

*: significant at the 10% level

A: Year, Year^2 and Log(Year)

B: Log-in-Logs model. Dependent variable is Log(Homicide Rate). Regressors are Log(Population 15-24) and Log(Population)

Source: DATASUS and Instituto Brasileiro de Geografia e Estatística (IBGE)

In column (1), I regress homicide rates on the population in the 15-24 year-old group. The (very precisely estimated) coefficient 1.18 means that a one million increase in the 15-24 year-old population is associated with an increase of 1.18 homicides per 100,000 inhabitants. Figure 8 shows that the 15-to-24 population in Brazil excluding São Paulo increase by 4.7 million between 1992 and the peak in 2005. Multiplying 4.7 by the 1.18, the estimated coefficient, the model in column (1) predicts an increase of roughly 5.5 homicides per 100,000 inhabitants. The actual increase was about 9 homicides per 100,000 inhabitants. For the drop in homicides from 2005 onwards a similar prediction arises. Thus, the size of the 15-24 year-old population alone explains 50% of the variation in murder rates. Other factors are not constant over time, most notably population itself. The model in column (2) includes population as a control. The impact of the 15-24 population is now much stronger. In fact, the model now over-predicts the increase in homicides by 6 (15 against 9), but under-predicts the reduction (2 against the actual 3.4).

Visual inspection of figures 7 and 8 suggest that the population and homicide series are non-stationary. We do not dwell into the complications of evaluating co-integration between series using 16 observations. We do however include a polynomial of time to see whether movements in the 15-24 year-old population are still associated with changes homicides. The coefficient is again very significant statistically and in practice. It predicts the increase in homicides accurately (8.8 against 9.1). For robustness (and for later comparison) we estimate a log-in-logs model, which produces coefficients that are interpretable as elasticities. Column (4) is equivalent to column (1). The estimated elasticity is high, 2.89, and very significant statistically. Over the 1992-2005 period, the 15-to-24 year-old population increase by 20%. Multiplying this figure by 2.89, the model predicts a 59% increase in homicides, closely replicating the actual increase (17 to 27.3 per 100,000 inhabitants).[16] Finally, column (5) has the estimates of a log-in-logs model equivalent to column (3), with similar results. In summary, the size of the 15-24 year-old population explains a significant proportion of the aggregate time-series variation in homicides. In addition, the time-series relation between these two variables is as robust as it can be with no more than 16 observations.

6. Panel Evidence

6.A State-Level Regressions

Identification of causal effects with pure time-series variation is challenging, especially with such a reduced number of observations. An alternative is panel data and explore how demography varied differently over time and across cross-section unit. In this subsection the cross-section unit is states; next section it is cities. As we shall see, panel and aggregate evidence are complements in identifying the impact of demography on violent crime, and produce remarkably similar results.

Demography and crime are not randomly determined but choices of the agents. Consequently, their relationship may suffer from the usual problems: reverse causality and omission of common determinants.

Demography has two pillars. One is fertility and mortality, which largely produced by decision made several years - if not decades - before. The second is

[16] The elasticity approximation to percentage increases is valid locally, for small changes in the regressors. Thus, the 59% effect should be viewed as a coarse approximation.

migration, a short medium-term decision. Regarding the first channel – fertility and mortality – it is unlikely that demography and crime have a common cause because crime is contemporaneous decision. Of course, fertility and mortality decisions made several years before may have been influenced by some hard-to-measure factor that is persistent over time, e.g. quality of institutional framework, and also determines crime. However, persistence is, by definition, somewhat constant overtime, and thus can be accounted for by the inclusion of state-fixed effects.

Migration is a potential stumbling block. Inflows to and outflows from abroad are just not relevant in Brazil during the period. Internal migration movements cancel out within the country. Thus, if unobserved time-varying heterogeneity across states caused by migration drives panel results, then pure time-series aggregate estimates should show no effect of demography on aggregate crime, which is not the case. Nevertheless, it is plausible that high-growth states attract both criminals and 15-24 year-old migrants, causing omitted variable bias (although we do control for income using GDP per capita). Migration may also produce reverse causality: violence may induce emigration. It should be noted that migration threatens identification of the causal impact of age structure only if it changes age distribution in the population. For example, if older people are more prone to emigrate due to violence, then our procedure would capture reverse causality. In the end, whether migration challenges the causal interpretation is an empirical question. In section 7.D, we measure the importance of migration in Brazil to show that it is not a sufficiently relevant phenomenon to pose a serious threat to our identification strategy.

Let *i* be a state and *t* be a year. The estimated model is:

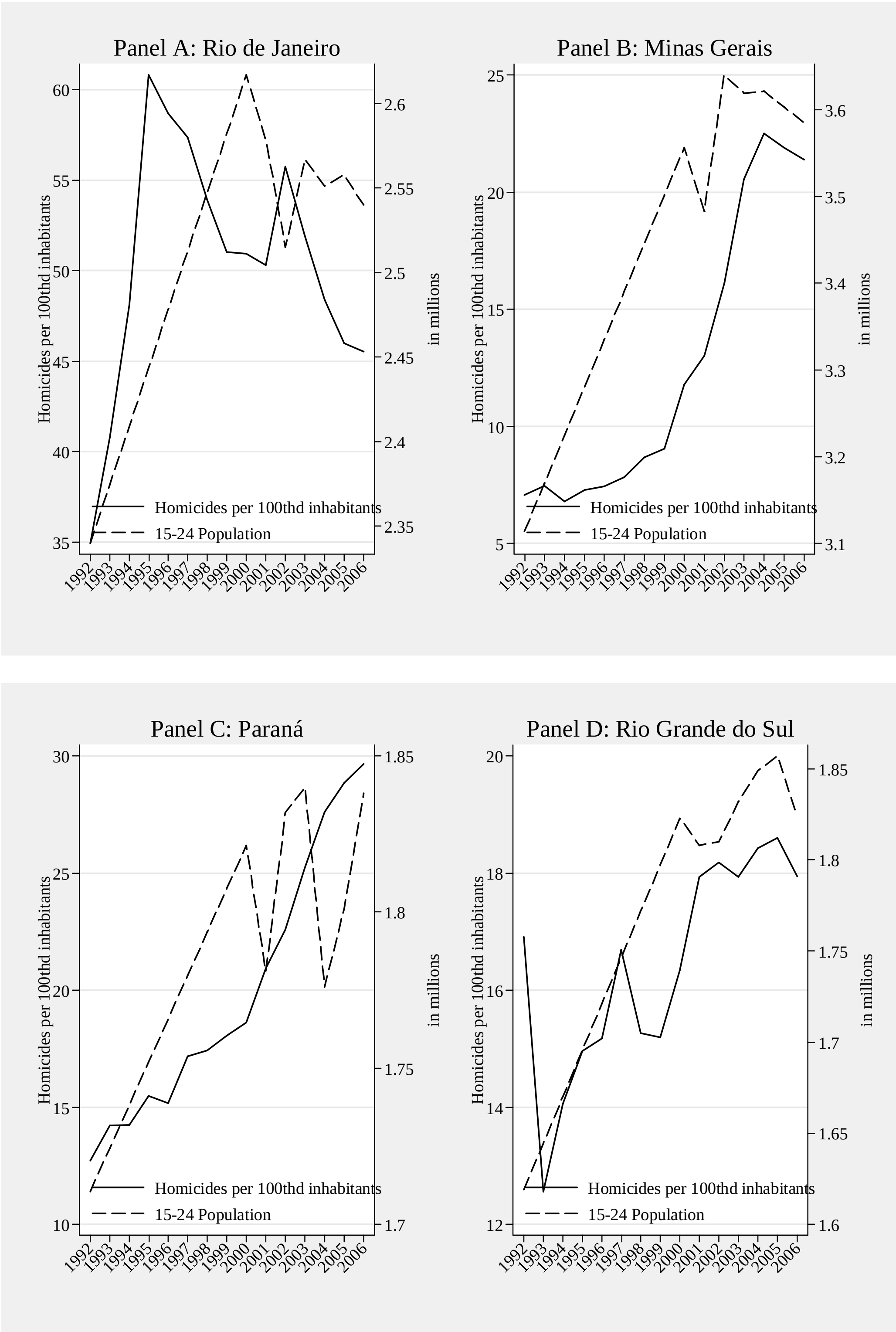
$$\log(Homicide)_{it} = \beta_0 + \beta_1 \log(1524)_{it} + Controls_{it} + \sum_{t=1}^{T} \tau_t TIME_t + \sum_{i=1}^{I} S_i STATE_i + \varepsilon_{it} \tag{1}$$

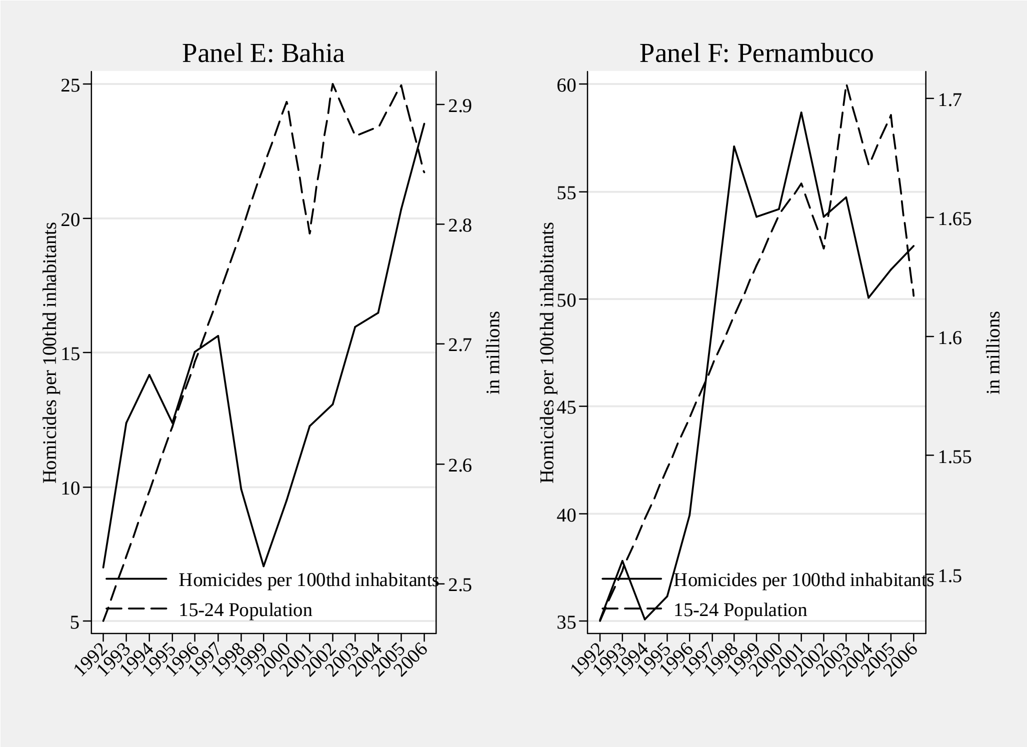
Homicides is the number of homicide per 100thd inhabitants, 1524 is the 15-24 year-old population. $TIME_t$ is a full set of year dummies, and $STATE_i$ is a full set of state dummies. *Controls* include the log of population, the log of the Gini coefficient, the log of the GDP per capita and the log of illiteracy rate and 15-24 year-olds. Population captures migration movements, the component of demography that is a product of current choices of agents. Economic activity, captured by GDP per capita, may have varied differently across states in the sample period. Education is a state-level attribution in Brazil, and the vulnerability of youngsters, measure by their illiteracy rates, varies across states. Finally, when estimating the parameters in (1) we weight observations by the state population for two reasons. First, homicide is notoriously noisy in small populations. Second, by weighting by population we emulate an elasticity representative of the country using state-level observations.

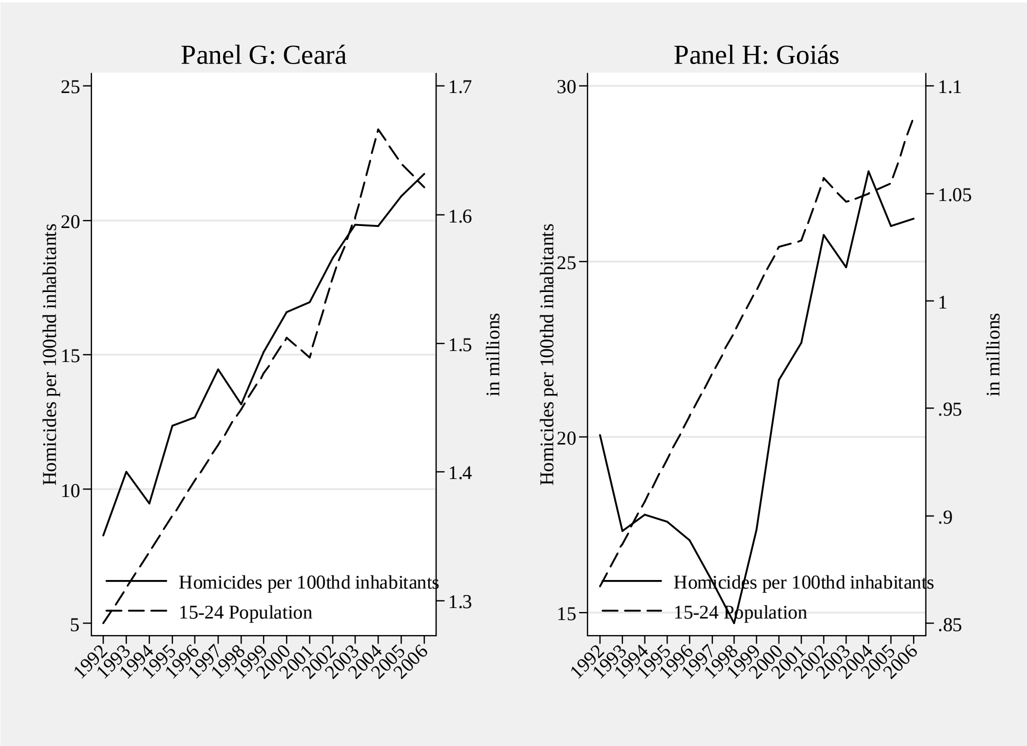
With a panel structure, one can discard *all* pure time-series variation (and all pure cross-city variation), leaving only demography changed *differently in different states* as a source of identifying variaton. Several more layers of coincidence are now necessary to produce the results spuriously. Second, we can account for all time-invariant heterogeneity among cities, which helps identifying the effect of demography. Figure 9 depicts the proportion of 15-24 year-old males and homicide rates for the largest states.[17]

[17] I do not depict all 27 states for conciseness.

Fig. 9: Homicides and 15-24 year-old population in several states







Source: DATASUS and Instituto Brasileiro de Geografia e Estatística

Figure 9 illustrates graphically the type of variation explored when estimating equation (1). In a couple of cases - Ceará and Goiás - the 15-24 year-old population increases monotonically and so do homicide rates. Even more interesting are two cases in which the 15-24 year-old population increases and then decreases: Rio de Janeiro and Minas Gerais. In all cases, homicides tend to mimic the behavior of the 15-24 year-old population (in Minas Gerais the 15-24 year-old population reaches a peak later than Rio de Janeiro, and so do homicide rates). Another interesting case is Pernambuco. The 15-24 year-old population and homicides increase until the early 2000s. Differently from Rio de Janeiro and Minas Gerais, the 15-24 year-old population fluctuates around this peak afterwards. Correspondingly, homicide rates stabilized at a high level. In Bahia and Paraná homicides and the 15-24 year-old population also show co-movement in the 1990s, but in both cases, although the 15-24 year-old population stabilized in the 2000s, homicides have not abated yet.

More importantly than co-movement between the two series, figure 9 shows that demography evolved differently in different states, which provides valuable variation to estimate the parameters in equation (1). The fact that homicide rates also varied differently in different states, in general mimicking demography, is suggestive of the causal relationship between demography and homicides in Brazil. When one estimates equation (1), one assess whether this relationship survives controlling for aggregate effects (year effects), state fixed effects, and for four time-varying covariates (population, GDP per capita, income inequality and illiteracy within the 15-24 year-old population. Table 2 contains the results.

TABLE 2: States of Brazil over the 1996-2006 period

Dependent Variable: Log of Homicide Rate per 100thd inhabitants

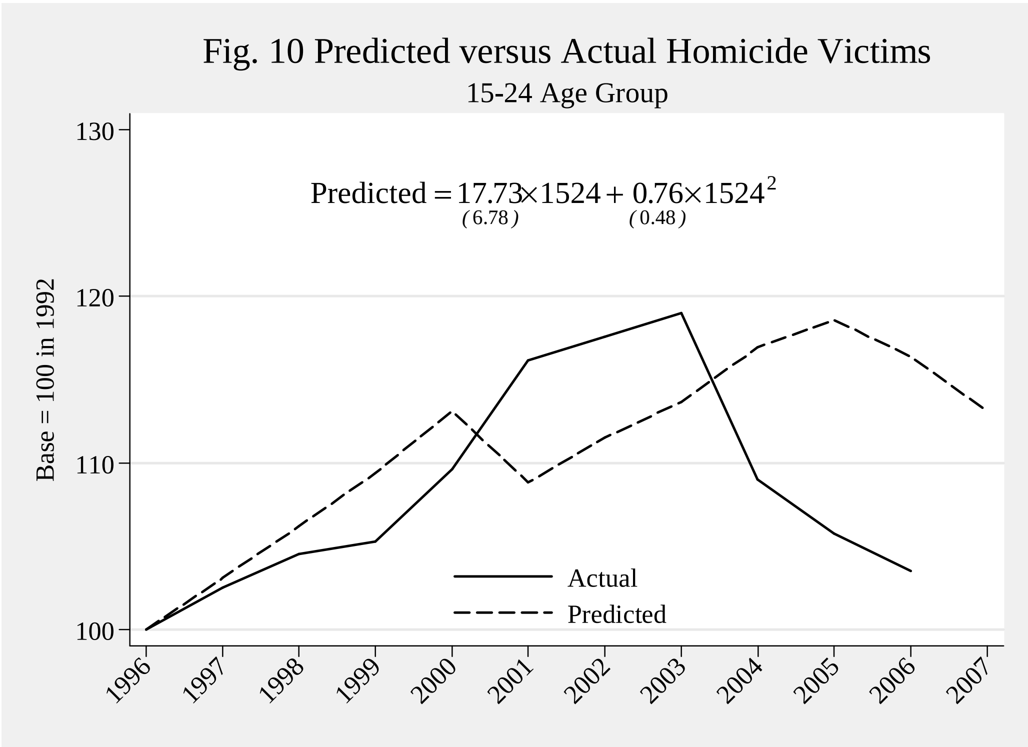
	(1)	**(2)**	**(3)[A]**	**(4)**	**(5)[B]**	**(6)[C]**
Log(1524)	2.53	2.18	1.49	2.43	2.18	2.18
	(0.90)***	(0.81)***	(0.64)**	(1.24)**	(1.24)*	(0.82)**
Covariates?[D]	No	Yes	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	No	Yes	Yes
# Observations	297	297	403	297	297	297
R^2	0.84	0.86	0.88	0.84	0.86	0.86

All standard errors are White-Huber heteroskedastic corrected, unless otherwise noted. All models include state dummies and the log of state population. In all models observations are weighted by population.

***: significant at the 1% level
**: significant at the 5% level
*: significant at the 10% level
A: Sample period is 1992-2006
B: Estimated standard errors corrected for within year correlation (clustered at the state level)
C: Estimated standard errors corrected for within panel correlation (clustered at the year level)
D: Covariates are the log of state GDP per capita, the log of the percentage of illiterate 15-24 year-olds, and the log of the state-level Gini Coefficient
Source: DATASUS and Instituto Brasileiro de Geografia e Estatística (IBGE)

All estimated models include state dummies and the log of population. The dependent variable is the rate of homicides per 100,000 inhabitants. Thus, the relationship between the size of the 15-24 population and the homicides is not driven by a mechanic size effect. In column (1) I include year dummies but not the covariates. It should be noted that the inclusion of year dummies deals with all concerns about whether the series are stationary. A 1% increase in the 15-24 population causes a 2.58% increase in the murder rate. Controlling for covariates reduces the impact slightly, but it is still quite significant both statistically and practically (column (2)). In column (3) we extend the sample period back to 1992. Despite the different criteria for homicide classification (see section 3), extending the sample serves two purposes. First, it provides yet another robustness check. Second, and more importantly, it allows for a comparison with the pure-time series estimates in table 1. Again, the impact of the size of the 15-24 population is significant both statistically and practically. In addition, the impact is remarkable is similar to the one in table 1. In columns (4) and (5) we check the robustness of the estimated standard errors to within panel autocorrelation and across panel correlation, something important with panel data. Estimated standard errors are slightly larger when one corrects for within panel autocorrelation, but results stand.

An estimated elasticity over 1 suggests that, in levels, homicide rates are a convex function of the size of the 15-24 year-old population. I estimate model (1) in levels and use the estimated coefficients the size of the 15-24 year-old population to predict homicide rates. Figure 10 shows the results (the estimate coefficient on the linear and quadratic terms of size of the population 15-24 also shown in the figure).



As expected, homicide in levels is a convex function of the size of the 15-24 year-old population. Predicted homicide rates matches quite well the variation in actual homicides: between 1996 and 2003, actual homicides increase by almost 20 percent, and so does predicted homicides. The predicted reduction is less acute than the actual drop, perhaps reflecting the fact that the predicted peak is couple of years later than the actual peak. Including the year 2007 in the predict series suggest that the predicted reduction is firm, and not just a peculiarity of the year 2006.

6.B City-Level Regressions (São Paulo State)

This section goes one step further into disaggregation. I estimate the elasticity of homicides with respect to the size of the 15-24 year-old population using a panel of cities in the state of São Paulo. The reason to use cities in São Paulo is data availability, as explained in section 3.

The estimated model is similar to equation (1), except that we have no time-varying control except population itself. The sample period is 1997-2006 because crime data is available for all cities in São Paulo starting only in 1997. Due to the large number of cross-section observations we first-difference the data instead of including city dummies.[18] In the case of city-level regressions weighting the observations by population is even more important than when state-level data are used. Homicide rates are very noisy in small cities. Results are in table 3.

TABLE 3: All Cities in São Paulo State, 1997-2006 period

Dependent Variable: ΔLog of Homicide Rate per 100thd inhabitants

	(1)	**(2) ^{A}**	**(3)^{B}**
ΔLog(1524)	4.61	4.61	4.61
	(0.36)***	(0.30)***	(0.40)***
ΔLog(Population)	0.55	0.55	0.55
	(0.23)**	(0.34)	(0.33)
# Obs	2108	2108	2108
R^2	0.25	0.25	0.25

All standard errors are White-Huber heteroskedastic corrected, unless otherwise noted. All models include time dummies in levels. In all models observations are weighted by population.

***: significant at the 1% level

**: significant at the 5% level

*: significant at the 10% level

A: Estimated standard errors corrected for within year correlation (clustered at the state level)

B: Estimated standard errors corrected for within panel correlation (clustered at the year level)
Source: IPEADATA and Secretaria de Segurança do Estado de São Paulo.

[18] The procedures are similar. In some cases they are algebraically the same. See Woodridge (2000).

Estimated elasticities are now even stronger than state-level's ones, about twice the magnitude. Estimated standard errors are no longer sensitive to accounting for within and across panel correlation among observations. City-level estimates again confirm the importance of the dynamics of demography in explaining violent crime.

7. Discussion

7.A Cohort Size and Cohort Violence

One major stumbling block with the demographic explanation of violent crime is the fact that age-structure changes slowly but homicides may shift vary sharply over a relatively short period of time, as it is the case in the US and in Brazil. In other words, even if changes in the age structure match shift in crime qualitatively, they come short of explaining magnitudes (Zimiring [2007]). In fact, a standard procedure in the literature is to perform a Oxaca-Blinder type of decomposition to find the impact of demography on aggregate type (Kitagawa [1964], Steffensmeier and Harer [1987], Levitt [1999]). Holding baseline homicide rates constant, one computes predicted aggregate homicide rates for subsequent years by multiplying the baseline age-specific homicides rates by the proportion of people in those age categories in that year. Levitt (1999) uses this procedure and finds a small impact of the changing age structure on aggregate crime. Using data from cities in the state of São Paulo, De Mello and Schneider (2010) find that simulated homicide rates match the timing of the reversal of the actual homicide trend very well, but fail to account for levels.

The validity of these decomposition hinges on a crucial assumption: age-specific homicide rates are not a function age-group size. Levitt (1999) presents regression evidence supporting the validity of this assumption for US data. With Brazilian data, we find strong evidence that this assumption is violated.

Figure 11 clearly shows that most, if not all, movement in homicides rate came from the 15-24 year-old age group, at least insofar as victimization is concerned. Thus, as James Alan Fox (1999) puts, the explanation is driven by the 15-24 year-old group, either by changes in its size or in the violence within the group.

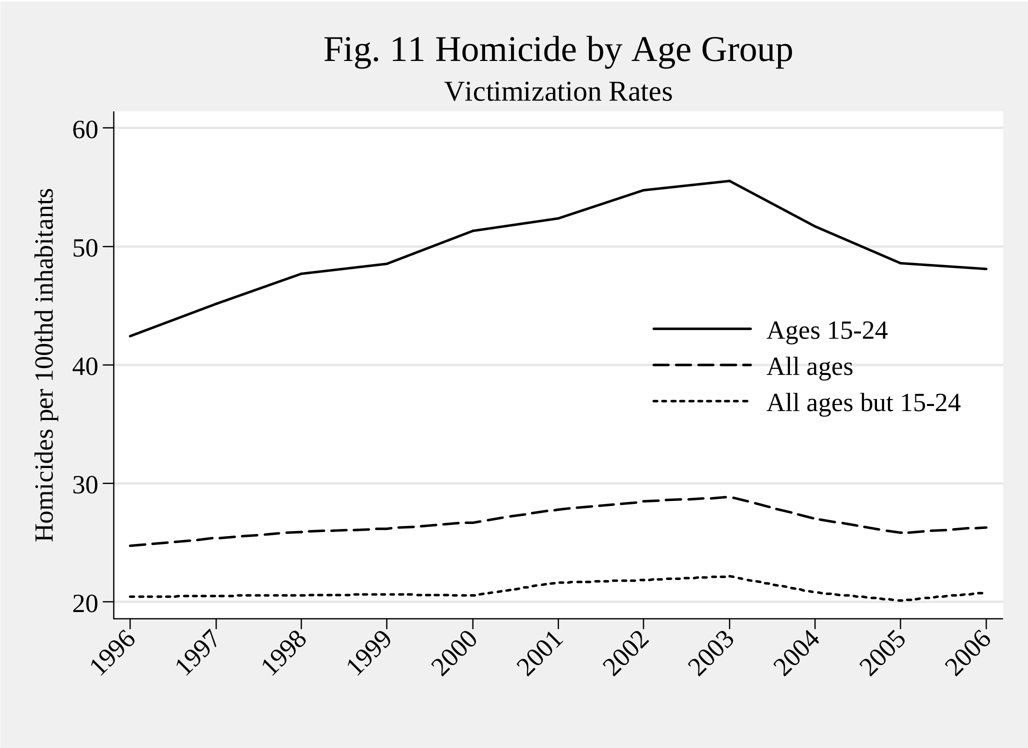


Figure 11 also suggests a non-linear relationship between the size of the 15-24 age group and the group specific homicide rates. To confirm this non-linear relationship, I estimate several versions of the following model:

$$\log(\text{Homicides1524})_{it} = \beta_0 + \beta_1 \log(\text{Population1524})_{it} + \beta_2 \log(\text{Total Population})_{it} + Controls_{it} + \sum_{t=1}^{T} \tau_t TIME_t + \sum_{i=1}^{I} S_i STATE_i + \varepsilon_{it} \qquad (2)$$

where i is a state and t is a year. Definitions of $TIME_t$, $STATE_i$ and $Controls_{it}$ are as defined in (1). Observations are again weighted by the state population. An estimated β_1 is more than 1 means that the homicides within the 15-24 year-old group increases by more than the population, implying that the age-specific homicide rate is increasing in age group population, violating the assumption that age-specific homicide rates are constant in age-group size. Inclusion of overall population and year dummies guarantee that β_1 does not capture a scale effect or other spurious time-series effects. Table 4 presents the results.

TABLE 4: States of Brazil, 1996-2006 period

Dependent Variable: Log of Homicide Victims in the 15-24 year-old group

	(1)	**(2)**	**(3)^{A}**	**(4)^{B}**	**(5)^{C}**
Log(15-24 year-old population)	2.81	2.42	1.77	2.42	-0.075
	(1.12)**	(1.02)**	(0.86)**	(1.58)	(0.28)
Covariates?^{D}	No	Yes	Yes	Yes	Yes
# Observations	297	297	297	297	297
R^2	0.91	0.96	0.84	0.96	0.97

All standard errors are White-Huber heteroskedastic corrected, unless otherwise noted. All models include state and year dummies, and the log of the state population, unless otherwise noted. In all models observations are weighted by population.

***: significant at the 1% level

**: significant at the 5% level

*: significant at the 10% level

A: Dependent variable is the log of the 15-24 age-group homicide rate (homicides per 100,000thd inhabitants). Regressor is the log of percentage of 15-24 year-olds in the state population. Log of state population not included.

B: Estimated standard errors corrected for within year correlation (clustered at the state level)

C: Dependent variable is the log of homicides in all ages except 15-24.Regressor is the log of population in all ages except 15-24.

D: Covariates are the log of state GDP per capita, the log of the percentage of illiterate 15-24 year-olds, and the log of the state-level Gini Coefficient

Source: DATASUS and Instituto Brasileiro de Geografia e Estatística (IBGE)

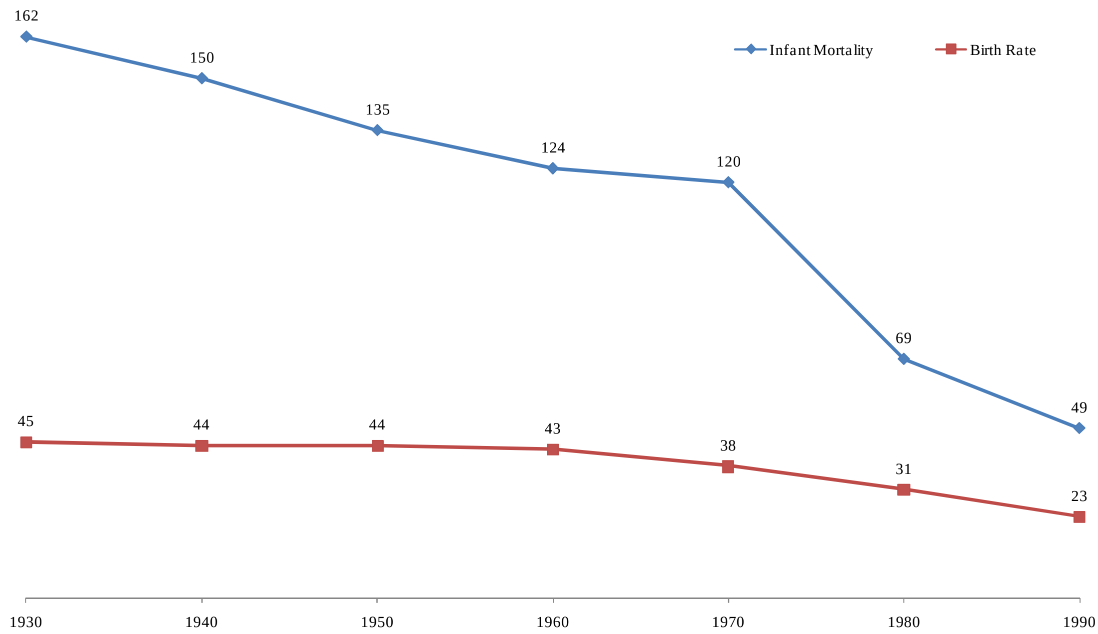
Column (1) has a stripped-down model, with no controls included. The estimated coefficient 2.82 means that a 1% increase in the size of the 15-24 year-old population causes an increase of 2.81% in total homicide within 15-24 year-olds. Thus the relationship is convex, not linear. In column 2 controls are included. Although the non-linearity is less severe, it is still quite significant, both statistically and practically. In column (3) the non-linear hypothesis tested in a different way. Instead of including the log of population as a regressor, I use the log of the homicide rate as a dependent variable. The coefficient 1.77 means that a 1% increase in the 15-24 year-old population increases the 15-24 year-old homicide *rate* by 1.77%. Notice that any estimated positive (not only above 1) would suggest that homicide rates increase with the size of population. In column (4) observations are clustered at the state-level; some precision is lost but the coefficient is still marginally significant (at 14% level). Finally, in column (5), the dependent variable is replaced by homicides in all age groups except 15-24 and the regressor is replaced by total population except 15-24 year-olds. The convex relationship between size of population and homicide is only valid for the 15-24 age group.

What are the theoretical mechanisms behind the age-group size – age-group crime rate *nexus*? At the family level, parents face a quality-quantity trade-off in offspring: larger families invest less (per capita) in their offspring, reducing human capital and thus lifetime earnings and social mobility (see Van Bavel [2006] for empirical on the quality-quantity trade-off evidence during a demographic transition similar to Brazil's). At the aggregate level, the Easterlin Hypothesis (Easterlin [1980]) states that larger cohorts face increased difficulty entering the job market, which deteriorates the prospects of careers in the legal system. In the next subsections we show not only that the late 1970s-early 1980s cohort faced tough conditions in the job market, but also faced difficulty in the educational system.

7.B What Caused the Especially Large Late 1970s Cohort?

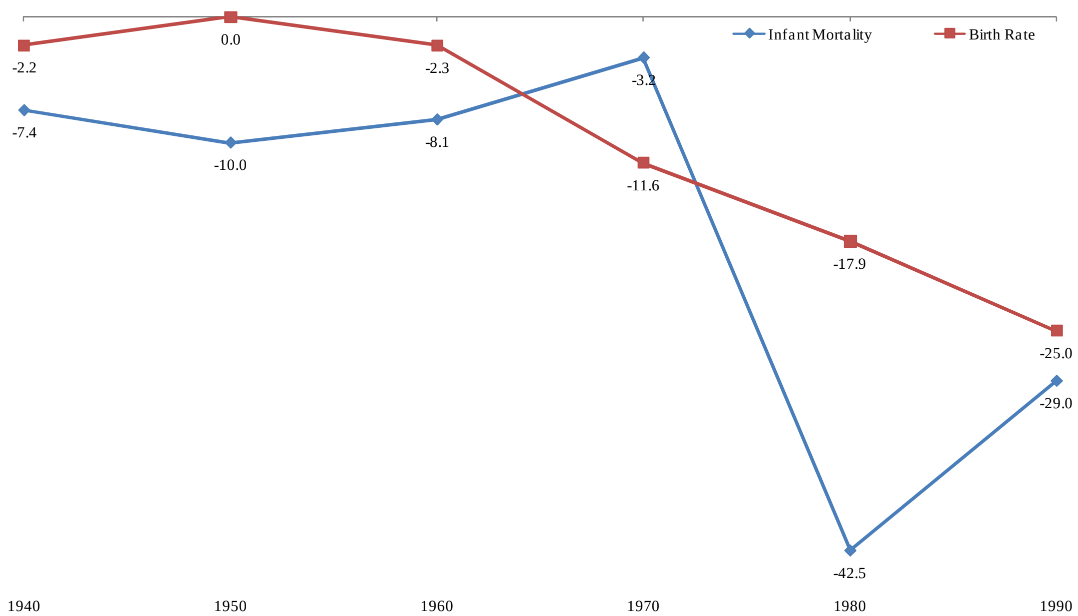
An important underlying question is what caused the large cohort born in the second half of the 1970s. Figure 12 has the clue.

Fig. 12a Infant Mortality versus Birth Rates: levels



Source: Instituto Brasileiro de Estatística e Geografia. Infant mortality is the number of deaths of infants less than a year old per 1000 births during a year. The birth rate is the number of born-alive infants per 1000 inhabitants during a year.

Fig. 12b Infant Mortality versus Birth Rates: % changes



Source: Instituto Brasileiro de Estatística e Geografia. Infant mortality is the number of deaths of infants less than a year old per 1000 births during a year. The birth rate is the number of born-alive infants per 1000 inhabitants during a year.

From 1940 through 1970, infant mortality dropped steadily, but birth rates stayed roughly constant, with slight acceleration in reduction between 1960 and 1970. This well-known pattern among demographers is not specific to Brazil, and explains exponential growth of the Brazilian population after 1940. After 1980, while the pace of reduction in infant mortality abated, reductions in fertility accelerated. This pattern is well-known to demographers (Galor [2005]). In the first phase of the demographic transition, factors such as vaccination, improvement in basic health services, and increases in food supply reduce mortality rapidly. Fertility rates respond only with lag, as households readjust to return to the optimal number of offspring.

Until the late 1970s, the Brazilian age distribution was a pyramid, with a large base and thin top. By the mid 1990s, one can see the impact of the movements in mortality and fertility rates. The shape changes (figure 13). Not only the late 1970s cohort is particularly large but the subsequent cohort is relatively small. This is the *demographic bonus*: a large cohort followed by a small one.

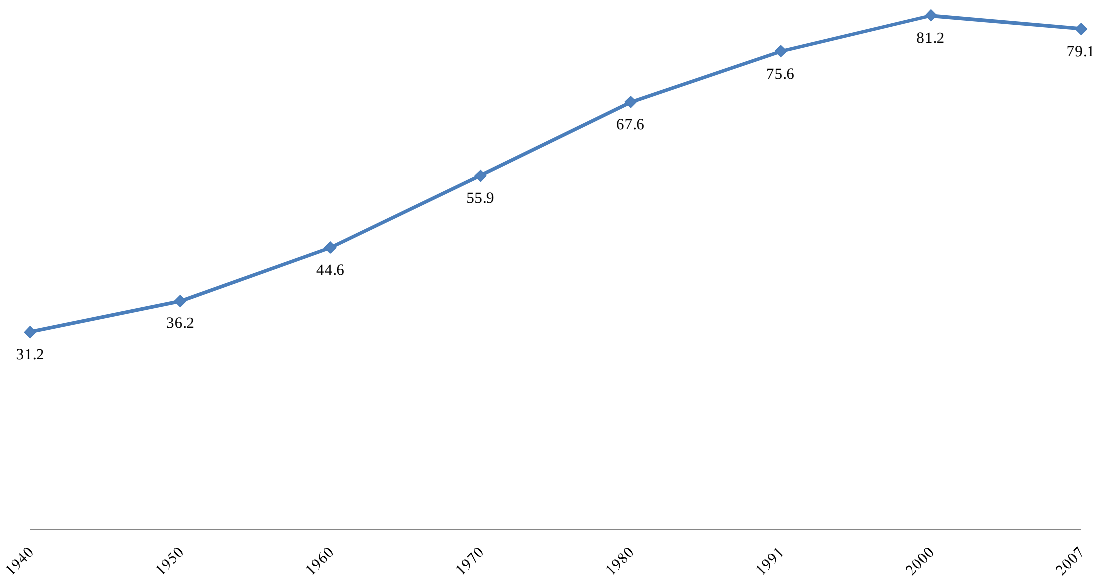
7.C The Socio-Economic Consequences of the 1970-1980 Cohort

The demographic bonus is normally beneficial, reducing the dependency ratio, improving economic growth and social security accounts (see Turra and Queiroz (2006)). While for the economy as a whole the demographic bonus might be beneficial, those born in the large cohort will usually face economic difficulty. Easterlin (1980) has outlined the theoretical mechanism, and showed empirical Evidence for the United States. When a large cohort reaches the job market, both the number of workers increases. This is outward shift in the supply of labor, which could increase unemployment in the short run (under sticky prices) and/or reduce real wages. If young workers are not perfect substitutes for workers in general, an additional effect to the same direction happen because the relative because the relative number of young workers increase as a large cohort enters the job market. The same argument can be made for the educational system: a large cohort, especially in a rapidly urbanizing country like Brazil in the 1970s and 1980s, puts pressure on the educational system, further deteriorating its economic fortunes. In this subsection, I document both facts using national-level data and data from the State of São Paulo.

7.C.1 Country Level

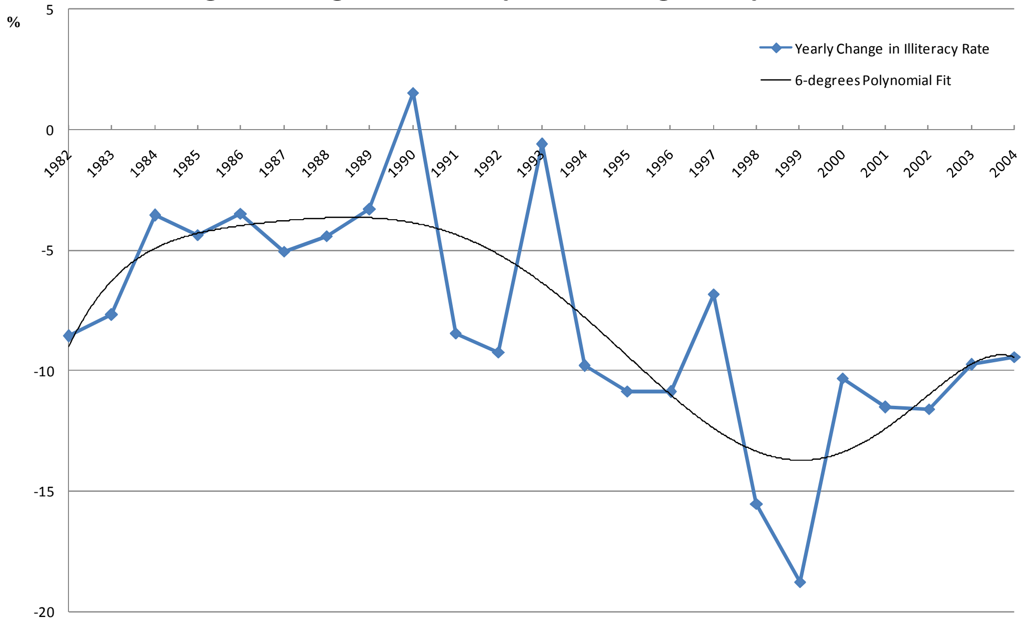
Besides producing a large cohort, the late 1970s saw the last period of rapid urbanization (see figure 14). We now argue that this large cohort put pressure on the provision of urban public services.

Fig. 14 Percentage of Urban Population



Source: Instituto Brasileiro de Estatística e Geografia.

Fig. 15 Changes in illiteracy rates among 15-17 year-olders



Source: Instituto Brasileiro de Estatìstica e Geografia.

Despite the difficulties in documenting the pressure on public services, some data are available. Figure 15 depicts changes in illiteracy rates among 15-17 year olds, and conveys the following pattern. The 1980s saw a reduction in the speed in which illiteracy rates were dropping. The lowest rate of change occurs in 1990, when illiteracy actually increased over the previous year. The reduction in the pace of improvement is not due to diminishing returns to investment in education. By the mid-1990s the illiteracy rate started to decline at increasing rates again. In fact, the best in year in terms of improvement in illiteracy rates was 1999.

Moving back 16 years from the early 1990s puts us in the mid 1970s, when the first members from the late 1970s-early 1980s cohort were born. In the mid 1990s the pace of improvement increased again, which is compatible with a smaller cohort following the late 1970s cohort.[19]

[19] If in 1985 the policy makers were completely forward looking agents that maximized inter-generational utility they should have anticipated the demographic bonus and borrowed against the future demographic dividend of the of late 1970s cohort to finance its education. This hypothesis requires not only tremendous farsightedness on politicians but also an amount of collective inter-generational benevolence that is not credible. At least not in Brazil.

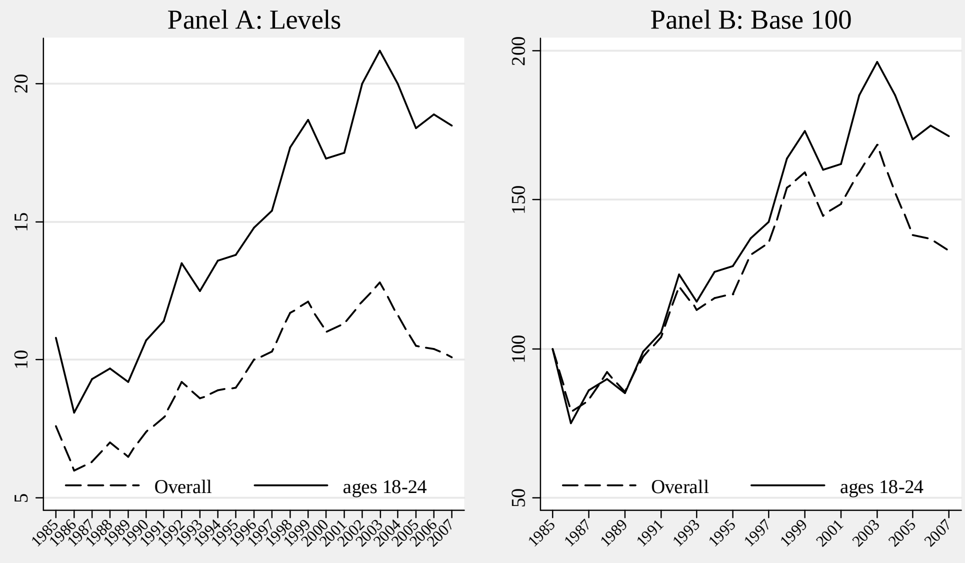
In summary, aggregate evidence suggests that a particularly large cohort put pressure on urban public services (i.e., public schooling) in a still rapidly urbanizing country. The late 1970s cohort was large and fragile. Fertility rates then dropped, and the subsequent cohort was much smaller. The quantity-quality trade-off faced by parents further exacerbates the differences between the 1990s and 2000s cohorts: parental investment in human capital was probably larger for the later cohort, which reduced fragility (Van Bavel [2006]).

7.C.1 São Paulo Metropolitan Area (SPMA)

Using data available for the SPMA, we can further investigation on the consequences of the large cohort. Figure 16 shows unemployment rates for the whole population and for the 18-24 age group.

Fig. 16: Unemployment rates in the SPMA

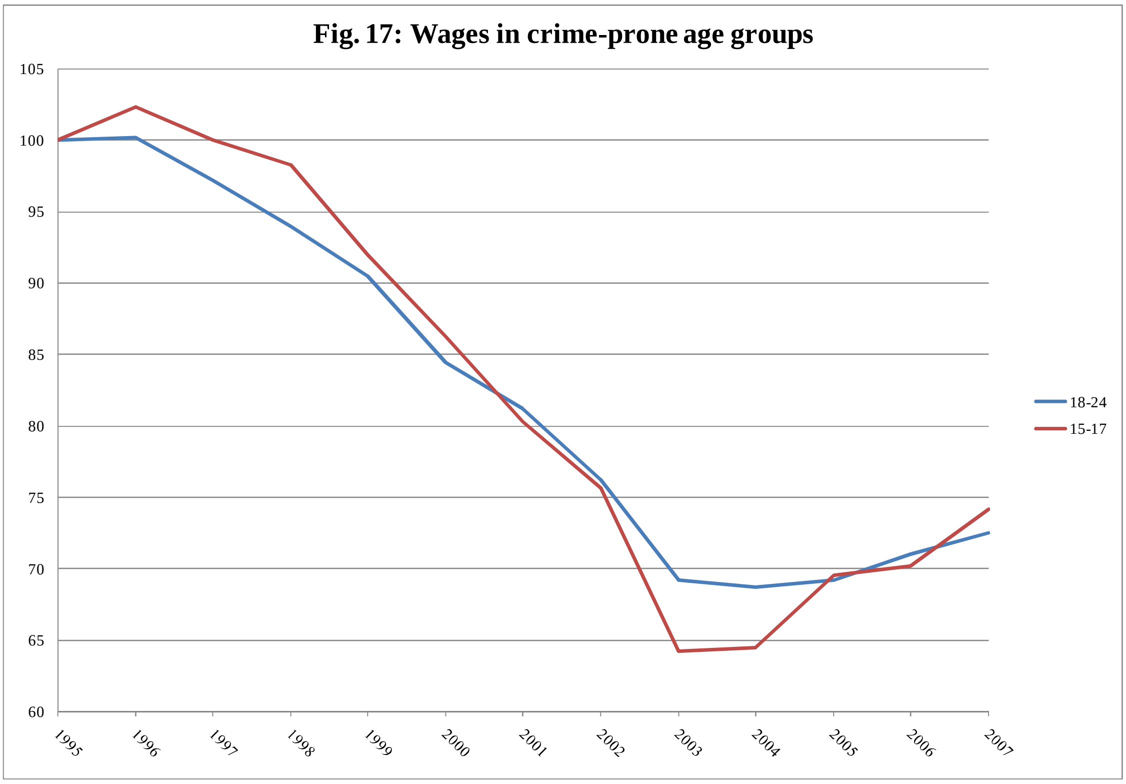
Overall versus aged 18-24



Source: DIEESE and Fundação SEADE

Several facts emerge from fig. 16. Panel A shows that unemployment rates within the age group 18-24 are much higher than the overall rate. Over the 1985-2005 period, the difference oscillates between 31 and 66 percent. After oscillating in the late 1980s, unemployment rates increased sharply and unabatedly during the 1990s. In panel B we see that, apart from the difference in levels, the series move very closely together in the 1980s. Starting in 1993, when the first members of 1975-1980 cohort turned 18, unemployment 18-24 age group start to rise faster than overall unemployment. The difference gains momentum in late 1990s. From then onwards, unemployment in general starts to abate.[20]

Wages in the legal sector are perhaps even more important than unemployment (Grogger [2000]).[21] Figure 17 shows average wages for the two crime-prone age groups: 15-to-17 and 18-to-24 year olds.



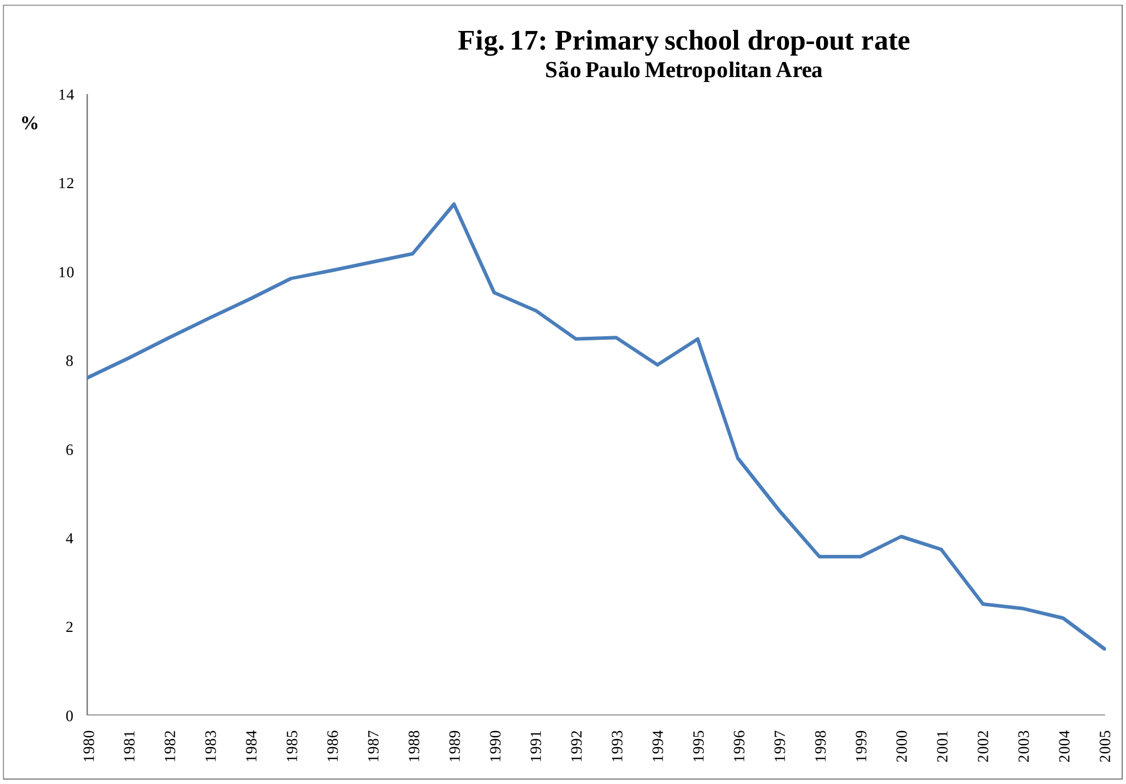
Source: Fundação SEADE

[20] Overall unemployment rates rose for unrelated reasons. The literature concludes than technological shocks favoring capital, skilled labor combined with trade liberalization, and lukewarm growth caused the spike in unemployment during the 1990s. See Gonzaga et al (2006).

[21] It is more natural to think that lower economic opportunity costs will increase property crimes. Insofar as property crime is instrumental to property crime, violent crime also increases. See Grogger (2000).

Figures 17 is the price mirror imagine of quantities in figure 16. Through the mid 1990s onwards, real wages for the crime-prone age groups drop steadily until 2003, when they stabilize, and then start rising again. Crime rates started to drop somewhat earlier (circa 2000). Nevertheless, dropping wages in the late 1990s again confirm the harsh jorb market conditions for young worker in the 1990s.

Figure 18 shows primary school drop-out rates from 1980 through 2005.[22]



Source: Fundação SEADE.

Children in primary school are aged between 6 and 14 years. We do not observe their average age, but drop-out rates for grades 5th though 8th are roughly three times as high as those for 1st through 4th. Thus, eleven is a reasonable guess for the average age of a primary school drop-out. The primary-school drop-out rate reached a peak in 1989, twelve years after 1978. In São Paulo, the large cohort was born between 1976 and 1980.

[22] We choose primary school rates instead of high-school rates for the following reason. Enrollment for primary school enrollment has been virtually universal in the SPMA. Thus, selection into enrolling does not determine. High-school drop-out rates are determined by previous decisions.

After 1990, drop-out rates start to drop; at first slowly, then quite sharply in second half of the 1990s, when the large cohort was between 16 and 20 years old and, thus, at least five years over the average primary school age.

In summary, data from the SPMA again suggest that the Easterlin Hypothesis is valid with Brazilian data. The late 1970s cohort was large and socially fragile. Not only they had worse performance in terms of educational attainment, but they faced very tough job market conditions. An unfortunate coincidence, general unemployment rose sharply as this fragile cohort was entering the job market.

7.D Are Differences in Age-Structure Across States Exogenous?

The use of non-experimental data calls for careful consideration of identification issues. Two problems may arise: omitted factors, and reverse causality.

Reverse causation could still arise for a mechanical reason. Homicide victims are concentrated in the male age bracket 15-24. Thus an increase in homicides reduces the relative number of 15-24 males. But murders are too few to make a significant difference. For an illustration, at its 2003 peak, there were 19,731 homicides in Brazil whose victims were 15-to-24-year-olds. Although the number is certainly very high, it amounts to no more than 0.05% of all 15-to-24-year-olds. In any event, this mechanical reverse causality force would bias results towards zero.

The age-structure in the 1990s-2000s is a product of three factors: 1) past reproductive decisions and mortality rates; 2) migration.

Reproductive decisions tend to be exogenous because they were take decades prior to crime decision. Arguably, theorize that decisions in the past will cause other differences across states twenty years down the road. Nevertheless, state (and city) fixed-effects are included in the regression. Thus, most structural differences across states (cities) that could be the result of decision made in previous decades are accounted for. In summary, omitted variable due to reproductive decision, although possible, is far-fetched.

Migration poses a more serious threat, but we can assess whether it is relevant empirically. Internal migration is just not very relevant. Data from 1991 and the 2000 censuses show that only 5% of the population on the 15-24 age group was composed of migrants who had been leaving outside their home states for 5 years or less. In addition, movements in migration have a trivial impact on the size of the 15-24 age group. Below, I show the results of a regression of the first-difference in 15-24 year-old population size on the difference in stock of migrants aged between 15 and 24 years that had been living outside the home state for five years or less. More precisely, the estimated equation is:

$$\Delta 1524_{it} = -\underset{(3.60)}{12.71}\, \Delta \text{Migration}1524_{it}$$

$$\text{Number of Observations} = 27,\ R^2 = 0.002$$

where t = 1991 and 2000. The impact of migration is *negative* but in practice it is zero (12 is a very small number compared with the average state population in 15-24 year-old (163,000)). Since migration has no significant impact on the size of the 15-24 year-old population, it is unlikely to cause reverse causality and omitted variable biases.

8. Conclusion

In this paper I show the crucial role of demography in explaining violent crime.. I recover a large age structure elasticity of homicides to the size of the 15-24 age group. This is a reduced-form object because it captures both the sheer effect of cohort size but also indirect effect that a large cohort has on age-specific crime rates. I also document a deterioration of socio-economic prospects for the late 1970s – early 1908s, especially in terms of job market prospects. This deterioration is in accordance with the Easterlin Hypothesis (Easterlin [1980]). Quite unfortunate, the large cohort also entered the job market in a period of high unemployment due to trade liberalization and technological changes that economized on unskilled labor. The Easterlin Hypothesis, corroborated with Brazilian data, suggest that age-specific crime rates are a function of age-group size, which rationalizes the large reduced-form elasticities I recover. My results shed light on the "Demography Puzzle": when age-specific crime rates vary with age-group size, then decompositions understate the true impact of demography.

Concerning policy implications, results in this paper are seemingly dismal. Demography is not a choice variable. However, age structure is one of the very few socio-economic variables that can be forecast with precision. Thus, policy-makers can anticipate crime prone circumstances, and invest ahead in reducing youth fragility in large cohorts.

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