TEXTO PARA DISCUSSÃO

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# Does Crime Affect Economic Decisions? An Empirical Investigation of Savings in a High-Crime Environment

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João M P De Mello
Eduardo Zilberman

DEPARTAMENTO DE ECONOMIA
www.econ.puc-rio.br

*Neither inflation nor unemployment. The most important concern for consumers in Rio de Janeiro ... is violence.*[1]

# 1 Introduction

Economists have been studying the potential determinants of crime, such as abortion, alcohol consumption, demographic structure and the police enforcement for many years.[2] However, the consequences of crime on economic activity remain largely unexplored. Since it is intuitive that people respond to crime, it is intriguing that so little research has focused on this topic. To the best of our knowledge, the literature contains only four empirical studies that attempted to investigate the relationship between crime and economic outcomes.[3]

The four studies that did analyze the affect of crime on economic outcomes include Pshiva and Suarez (2006) who measure the impact of crime on investment in Colombia, a high-crime country, and find that investment was adversely influenced *only* by kidnappings targeted at firms. Neither homicides, guerrilla attacks nor general kidnappings appear to have had any impact on investment. Freeman and Rodgers (1999) in their analysis find that youths have higher earnings and lower unemployment in low-crime areas. Using data on Uganda, Deininger (2003) finds that ordinary crime has no effect on start-up investment, and Cullen and Levitt (1999) show that crime causes depopulation—a rational for why crime could induce savings.[4]

Likely, the impact of crime on economic variables has not been established for two reasons: the choice of dependent variables previously selected and the level of the analyses. It is not obvious whether some economic variables respond to crime in an empirically meaningful way. Consider investment and unemployment. The transmission mechanism is through firm relocation and

[1] "Violence changes consumption," *Jornal O Globo,* August 10, 2004.

[2] See, for example, Becker, 1968; Donohue and Levitt, 2001; Corman and Mocan, 2000; Freeman, 1983; Levitt, 2002; Di Tella and Schargrodsky, 2004.

[3] A larger literature studies the related issue of how terrorism and civil unrest impact economic activity. Abadie and Gardeabazal (2003) estimate the economic costs of the conflict in the Basque region. Blomberg and Hess (2006) study the link between terrorism and international trade. Another example is Deininger (2003), who documents that civil strife, *not theft nor physical violence*, had an impact on start-up investment. Abadie and Dermisi (2006) associate variation in vacancy rates in Chicago's downtown buildings to the 9/11 attacks. However, terrorism and civil unrest are quite different from ordinary property crime. Thus, they may have quite different economic implications.

[4] Some older literature looks at crime and depopulation. For a list of references, see Cullen and Levitt (1999).

not through individual decision. Thus, the impact of crime on employment and investment is slower and dampened. Economic measures such as savings and consumption are arguably better choices as dependent variables because households can easily adjust their behavior as crime rates fluctuate. Incidentally, it is rare to have variation in crime and economic variables at an appropriate level of disaggregation. In Freeman and Rodgers, for instance, only state-level unemployment is observed, which forces the authors to estimate local unemployment using a rather elaborate procedure. In high-crime environments, such as Latin American cities, local data are even harder to find. Ironically, these cities are the most promising locales to observe a meaningful economic reaction to crime.

Why would households modify their savings decisions in response to crime? One reason is relocation from crime-plagued neighborhoods to safer districts, a phenomenon previously documented (see Cullen and Levitt). A second reason is that crime taxes certain types of consumption, such as conspicuous goods and dining out. This is particularly true for property crime, which threatens the right to drive a fancy sports car or wear the latest sneaker model. By taxing consumption, crime induces savings.[5] Precautionary savings is yet another reason, as crime increases the variance of "available" income.[6]

Crime may depress savings instead of encouraging it for three reasons. One is through the probability of death, thus it mostly applies to violent crimes. Crime also reduces returns on investments although the available evidence suggests that this is not empirically relevant (see Deininger; Pshiva and Suarez). Finally, crime can create expenses for private security. Our estimates partially account for private security spending because we control for employment in the private and public security sectors. However, not all private spending is included (e.g., locks, bars and alarms), and this omission biases results towards not finding an impact of crime on savings.

In this paper, we contribute to the economics of crime literature by examining the relationship between crime and savings in the cities of São Paulo, an affluent but crime-ridden state in Brazil. The decision to use the cities of São Paulo as the unit of observation is as important as the choice of dependent variables. Although wealthy by Latin American standards (in 2000, income per capita in São Paulo was US$6,567 in terms of 2005 dollar purchasing power), São Paulo is a crime-plagued state. In a statewide victimization

[5] Savings are postponed consumption. Thus, a rational forward-looking agent only reduces consumption in response to crime if she expects that crime will fall. Such rationality is unlikely because responses to crime are contaminated with fear, and behavior driven by fear is generally not rational.

[6] See Carrol (2004) for a theoretical treatment of precautionary savings.

survey, 18 percent of respondents reported that at least one family member had been the victim of a robbery or theft over a 12-month period.[7] In 2000, the homicide rate in São Paulo was 34.2 homicides per 100,000 inhabitants, higher than any state in the U.S.[8] In the same year, the vehicle theft and robbery rate was higher than all but two states in the U.S. Economic responses are more likely in violent places where people perceive crime as a serious problem. Moreover, in Brazil, credit markets are not fully developed, thus relocation requires an amassing of personal savings. In a victimization survey conducted in Rio de Janeiro, 51 percent of respondents expressed a desire to move because of crime.[9] Although victimization surveys exist in São Paulo, they contain no questions on economic reactions to crime. In any event, Rio de Janeiro and São Paulo are similar in terms of demographics and crime rates. In the victimization survey, 34 percent of respondents in Rio de Janeiro declared that they no longer went out at night due to violence. Another 16 percent reported going out less, day or night. Additionally, while an accurate measure of local consumption is not available in Brazil, a reliable measure of savings does exist at the city level.[10]

Using several sources, we construct a unique dataset that includes crime and savings information at the city level. Our data allow us to control for common determinants of crime and savings. When a panel structure is used, city- and year-specific effects are accounted for. When cross-sectional variation alone is used, an extensive list of covariates is included in the regression. Our cross-sectional results corroborate the panel estimates, which are more reliable. From a theoretical perspective, property and violent crime have different effects on savings. Thus, both types of crime are explored to identify the source of savings variation. Our analysis is restricted to cities in the State of São Paulo for data availability reasons, but this restriction has the advantage of guaranteeing a minimum level of homogeneity among observations because cities within the same state are subject to similar socioeconomic, political and cultural shocks.

[7] See Pesquisa de Condição de Vida (PCV) 1998, a survey on living conditions for the State of São Paulo conducted by Fundação SEADE, a state goverment think tank. Available online at http://www.seade.gov.br/produtos/pcv/index.php.

[8] Louisiana, with 12.5 per 100,000 inhabitants, had the highest homicide rate in the U.S. in 2000. In 1990, New York's homicide rate peaked at 29.8.

[9] See Pesquisa Estadual de Vitimização 04/2006, Instituto Brasileiro de Pesquisa Social, Rio de Janeiro. Available online at http://www.ibpsnet.com.br/ultimasprincipal3.asp.

[10] Consumption data are available from Pesquisa de Orçamento Familiar (POF), a household consumption survey. Unfortunately, the sample is restricted to the major metropolitan areas, and thus the variation in crime is too limited.

We subject our results to extensive sensitivity analyses. One important criticism is that people may respond to crime by shifting their savings to more secure instruments, leaving overall savings unchanged—bank deposits versus cash-at-home, for instance. If safety is the driving force, we would find an impact of crime on demand deposits, a secure but not remunerated instrument. Although not definite, the fact that we find no impact of crime on demand deposits renders credibility to the results. The effect of property crime on savings remains unchanged when the estimation is performed in different subsamples chosen by income and size of the city. Finally, we use household consumption data to corroborate the aggregate savings results.

It is difficult to exaggerate the policy interest in crime—a phenomenon that deserves both a complete understanding of its determinants and consequences. In terms of direct costs, crime amounts to some 3.6 percent of the GDP in Latin America, more than twice as much as in the U.S. (see Bourguignon, 1999). When welfare costs are considered, the picture becomes even more dramatic: 38 percent of the GDP in Brazil, which makes crime relatively "cheap" in the U.S. (13 percent of the GDP) (see Soares, 2006). Our results suggest that these welfare costs could be even higher once economic distortions are taken into account.

This paper is organized as follows. In section 2, the data are described. Section 3 explains our empirical strategy and contains the results. While we do not observe a relationship between violent crime and savings, a 1 percent increase in property crime is associated with a 0.035 percent increase in savings. For a sense of practical importance, the implied variation in property-crime-induced-savings growth across cities explains roughly 10 percent of the total variation in savings growth across cities, a small but nonnegligible fraction. In section 4, we outline the theoretical ideas behind the crime-savings *nexus*. Section 5 concludes.

# 2 Data, Measurement and Descriptive Statistics

## 2.1 Data Measurement

The choice of city-level variation in savings comes at a cost. Measures traditionally used in the savings literature, such as private savings from national accounts, are unavailable at the city level.[11] An alternative is constructing aggregate savings measures from micro-level data by subtracting consump-

[11] See Edwards (1996) and Loayza et al. (2000) for examples of cross-country studies of the determinants of savings.

tion from income or taking the first difference of wealth.[12] However, these approaches are not feasible because consumption data at the local level are unavailable.[13] Finally, the census is conducted every ten years, which rules out the first difference of wealth as a measure of savings.

As these standard measures are infeasible for São Paulo, banking data are used to construct a measure of savings. We observe savings account deposits for the period 1999–2004. These accounts are by far the most popular savings instrument in Brazil. In 2000, the Brazilian banking system had roughly US$78 billion in savings deposits, US$45 billion in demand deposits, and US$119 billion in time deposits. Time deposits cannot capture individual savings because roughly 65 percent are from firms and corporations, while the difference is made up by a small number of wealthy individuals.[14] As a consistency check, we also use demand deposits to reestimate the model. Being a noninterest bearing claim, we do not expect it to be used as a savings instrument. It is important to note that savings accounts are restricted to private citizens, and our measure of demand deposits considers only private citizens, not companies. The data used in our cross-sectional procedures for both savings and demand deposit accounts are from the Brazilian Central Bank (BACEN).[15] Two additional potential costs of using bank measures must be mentioned. The first is sample selection. Bank data are only available if a branch is located in the city. As a consequence, while São Paulo has 645 cities, we only have data for 566 cities. In section 3.3.4, we address the problem of selection and show that it is not relevant in our analysis. The second problem is savings deposits from nonresidents. This problem may be particularly acute because of the selection problem: some small cities have no bank branch. In section 3.3.1, we tackle this problem by estimating our main models for subsamples of different city sizes.

Annual property and violent crime rates per 100,000 inhabitants are available from 1997 onwards from the Secretaria de Segurança do Estado de São Paulo, the state-level enforcement authority. When examining only cross-city variation, crime rates are averaged over the 1997–2000 period to reduce noise, particularly in small cities where the occurrence of violent crimes such as homi-

[12] See Browning and Lusardi (1996) for a comparison of the advantages and disadvantages of both measures.

[13] The national household consumption survey, Pesquisa de Orçamento Familiar (POF), only identifies the metropolitan area.

[14] See the Brazilian Central Bank website at http://www.bcb.gov.br/ for more information.

[15] Bank liabilities at the local level are constructed from the call reports that commercial banks are required to send to the BACEN.

cide is rare. Among violent crimes, only intentional crimes are considered, which excludes manslaughter and unintentional assaults, although estimates are unchanged if these two categories are included as violent crimes.

The crime rates we use are based on police reports, thus they likely underestimate the true prevalence of crime.[16] Indeed, when compared with U.S. data, figures for São Paulo seem too low for crimes other than murder and, to a lesser extent, vehicle theft and robbery. Underreporting is a concern only if it varies systematically with our variables of interest, savings and crime. Since reporting depends on police reliability and law enforcement is done at the state-level in Brazil, we are not concerned that reliability varies systematically across cities within the same state. However, the reporting rate could be higher in small cities because of the proximity between victims and the authorities. Indeed, the only direct measure of underreporting for the State of São Paulo suggests that this is the case, although only weakly so. In 1998, 43.1 percent of theft and robbery victims reported the incident to the police in the São Paulo Metropolitan Area in contrast to 50.6 percent in the rest of the state.[17] Nevertheless, São Paulo crime data do not score poorly when compared to other countries.[18]

City-level demographics used in the cross-section procedures are from the 2000 census conducted by Instituto Brasileiro de Geografia e Estatística (IBGE), the Brazilian equivalent of the U.S. Bureau of Statistics. On an annual basis, only population and population distribution across ages are available, both based on projections. Thus, they are the only time-varying controls we are able to include in the panel procedures.

Finally, we use data from Pesquisa de Orçamento Familiar (POF), which is conducted by IBGE in metropolitan areas. POF is a household-level consumption survey. Its 2003 version contains a question on whether the respondent perceived violence in the neighborhood as a problem, which we can link to consumption data. Household data are used for corroborative purposes because we do not observe crime directly, only a vague perception of it.

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[16] Reported crime usually does underestimate real crime, as discussed in Levitt (1998), because of police inefficiency and the difficulty victims encounter when trying to report a crime. As long as a body is produced, murders are always "reported" since a police investigation is mandatory. Vehicle thefts are commonly reported for insurance reasons or to avoid a legal hassle if the car is used in criminal activities. Other crimes such as rape and petty theft often go unreported because of the "red tape" encountered when filing the report.

[17] See Pesquisa de Condição de Vida 1998, Fundação SEADE, http://www.seade.gov.br.

[18] Demombynes and Özler (2005) report a 60 percent underreporting rate for property crimes in South Africa, a figure comparable to São Paulo.

## 2.2 Descriptive Statistics

Tables 1 through 3 present summary statistics of the savings and crime data. In Purchasing Power Parity 2005 American dollars (PPP05US),[19] the stock of per capita savings account deposits was 1059. The average city had savings deposits of 404 PP05US with a large standard deviation of 409. Savings accounts and demand deposits are positively correlated, as one would expect, but not perfectly so. Monthly income per capita in PPP05US is roughly 520, making São Paulo comparable to a middle-income country (see the last column in Table 4).

**Table 1: Summary Statistics for the Dependent Variables**

| | Stock Savings Account per Capita | Stock Demand Deposits per Capita |
|---|---|---|
| **Mean** | 404.19 | 54.55 |
| **Standard Deviation** | 409.07 | 43.49 |
| **Minimum** | 39.98 | 3.38 |
| **Maximum** | 3512.35 | 339.54 |
| **Aggregate*** | 1059.00 | 100.53 |
| **Observations** | 566 | 566 |
| **Correlations** | | |
| **Saving Account** | 1 | |
| **Demand Deposits** | 0.6081 | 1 |

Source: Banco Central do Brasil. All numbers are in US$ Purchasing Power Parity of 2005.
*: State as a whole, not averaged across cities.

Table 2 shows the relative importance of different types of crime in São Paulo. Theft and robbery represent approximately 86 percent of all property crime of which about 25 percent is robberies and thefts of vehicles. Another telling statistic is the 841 kidnappings that occurred from 1997–2000, a striking finding considering that kidnappings tend to be underreported.[20] Additional crime statistics are in Table 3.

[19] To calculate the PPP05US, we use the IGP-M index calculated by Fundação Getúlio Vargas (www.fgv.br), the mean exchange rate (R$/US$) for December 2005 and the Purchasing Power Parity coefficient provided by the IMF (http://www.imf.org).

[20] Victims' families seldom report kidnappings to the police because of the kidnappers' threats and because the families' assets are frozen by law enforcement when kidnaps are reported.

**Table 2: Reported Crime by Type (1997-2000), Statewide**

| Property Crime | | | Violent Crime | | |
|---|---|---|---|---|---|
| Fraud | 185,881 | 5.10% | Manslaughter | 22,654 | 1.15% |
| Extortion via kidnapping | 841 | 0.02% | Felony murder | 47,934 | 2.43% |
| Other extortions | 3,514 | 0.10% | Involuntary assault | 471,586 | 23.95% |
| Achieved common† theft | 1,102,953 | 30.27% | Felony assault | 675,413 | 34.29% |
| Attempted common theft | 44,905 | 1.23% | Attempted murder | 38,745 | 1.97% |
| Achieved theft of vehicles | 417,755 | 11.47% | Other violent crimes | 713,093 | 36.21% |
| Attempted theft of vehicles | 4,815 | 0.13% | | | |
| Achieved qualified† theft | 392,729 | 10.78% | | | |
| Attempted qualified theft | 14,642 | 0.40% | | | |
| Achieved robbery | 776,265 | 21.30% | | | |
| Attempted robbery | 31,120 | 0.85% | | | |
| Achieved robbery of vehicles | 348,387 | 9.56% | | | |
| Attempted robbery of vehicles | 3,490 | 0.10% | | | |
| Robbery followed by murder | 2,442 | 0.07% | | | |
| Other property crimes | 313,907 | 8.62% | | | |
| **Total Reports** | **3,643,646** | **100%** | **Total Reports** | **1,969,425** | **100%** |

Source: Secretaria de Segurança do Estado de São Paulo. Categories are defined by the Brazilian Penal Code. Categories are nonoverlapping. "Total Reports" is the sum of all categories.
†: Qualified theft is a figure of the Brazilian Penal Code. Thefts are considered qualified if the object of the theft is destroyed, if the perpetrator abuses trust, if the perpetrator uses a false key or, the usual form, if two or more perpetrators are involved. If none of these circumstances apply, then the theft is considered common. In practice, sentences are harsher in case of qualified theft. See *Código Penal Brasileiro, Artigo 155 § 4.*

**Table 3: Crime Rates per 100,000 Inhabitants (2000), Statewide**

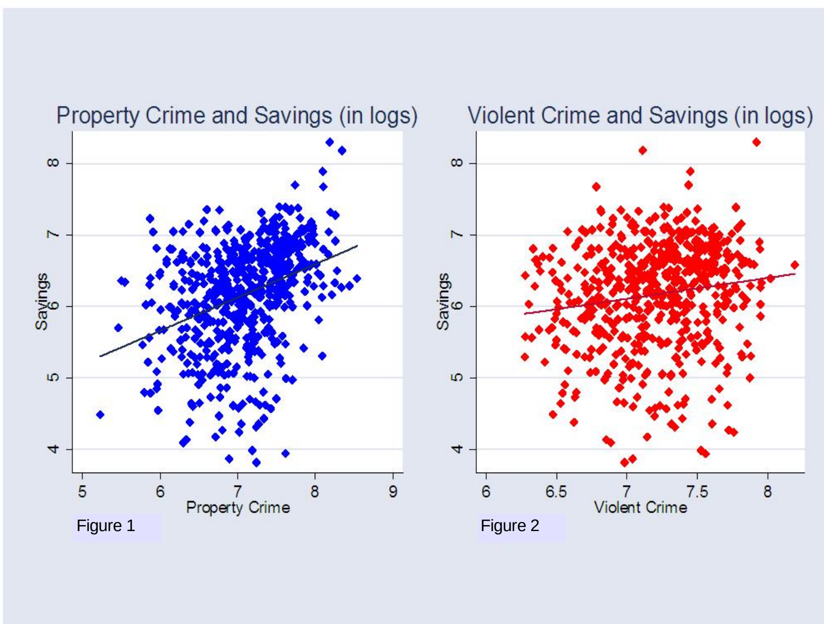
| | Mean | Stand. Dev. | Minimum | Maximum | Obs. | Aggregate* |
|---|---|---|---|---|---|---|
| Violent crime† | 1495.23 | 531.52 | 405.63 | 3629.40 | 566 | 1329.53 |
| Felony crime† | 649.65 | 244.13 | 158.08 | 1630.93 | 566 | 390.66 |
| Felony murder† | 11.80 | 13.27 | 0.00 | 92.35 | 566 | 25.89 |
| Felony assault† | 584.20 | 214.15 | 158.08 | 1618.76 | 566 | 364.77 |
| Property crime | 1561.02 | 876.97 | 79.72 | 5745.47 | 566 | 2459.77 |
| Robbery and theft (all) | 1021.47 | 651.61 | 127.79 | 4055.07 | 566 | 2050.97 |
| Robbery and theft (nonvehicle) | 673.35 | 420.15 | 68.92 | 2840.92 | 566 | 1268.63 |
| Robbery and theft (vehicle) | 108.91 | 159.81 | 0.00 | 1811.69 | 566 | 517.21 |
| Theft (qualified) | 239.21 | 229.84 | 0.00 | 2315.66 | 566 | 265.13 |
| Extortion and fraud | 95.62 | 57.20 | 5.70 | 414.47 | 566 | 128.43 |

Source: Secretaria de Segurança do Estado de São Paulo and IBGE.
*: State as a whole, not averaged across cities.
†: Felony categories includes only intentional crimes. Violent crime includes all categories.

Table 3 includes two important pieces of information across cities. First, crime rates per 100,000 inhabitants display considerable variation. While this variation allows us to measure the relationship between crime and savings, the data from small cities are quite noisy. Consequently, we perform a robustness check to ensure that our results are not driven by these small cities. Second, the crime indices are quite high in São Paulo. As noted above, for crimes with less reporting problems (vehicle theft, robbery and murder), São Paulo would be among the three most violent U.S. states.

# 3 Empirical Strategy and Results

Figures 1 and 2 provide additional motivation for our research. Each point on the graph represents a city in São Paulo. The two scatterplots suggest a positive relationship between crime and stock of savings, especially for property crimes. The following analysis assesses whether this relationship is indeed robust.




We implemented two procedures. When using both time-series and cross-city variations, we use city fixed effects to control for all time-invariant heterogeneity. We also ignore the time-series dimension and use only cross-city variation. Both procedures offer their own advantages.[21] Causal inference

[21] Endogeneity is a potential stumbling block. Suppose that savings provides a buffer against short-term variation in crime. In this case, more savings causes less crime. On the

is more credible under panel estimation because cross-sectional estimates are vulnerable to omitted variables bias. However, OLS and panel measure two different concepts. While the panel estimates capture short-term fluctuations, cross-sectional estimates capture a steady-state relationship between crime and savings. Another reason why cross-section results are interesting is that they allow us to assess whether the correlation between bank savings and demographics is as expected. Finally, in cross-sectional procedures, we average the crime rates from 1997–2000 to reduce the noise in crime measures.

Our benchmark models are:

$$\text{SavingsPC}_i = \alpha_0 + \alpha_1 \text{Crime100}_i + \Pi \mathbf{X}_i + \epsilon_i \quad (1)$$

$$\text{SavingsPC}_{it} = \alpha_0 + \alpha_1 \text{Crime100}_{it} + \Theta \mathbf{YEAR} + \Psi \mathbf{CITY} + \epsilon_{it} \quad (2)$$

The subscripts $i$ and $t$ represent a city and a year between 1999 and 2004, respectively. SavingsPC is the log of stock of savings per capita. Crime100 is the log of crimes per 100,000 inhabitants. **CITY** is a set of city dummies, and **YEAR** is a set of year dummies. **X** is a vector of controls, and $\epsilon$ is the error term. When estimating (2), we control for year-specific effects to avoid estimating a spurious relationship due to pure time series in crime and savings.[22]

In the cross-city procedure, **X** includes an extensive list of factors that may jointly determine crime and savings. A major common determinant is income per capita. The census has a direct measure of individual income, which is aggregated to the city level. We include both the log of income per capita and its square to account for nonlinearities in the income-savings relationship. However, systematic underreporting of income could lead to omitted variable bias. We attenuate this problem by including a measure of wealth per capita, which is estimated using information on households'

other hand, savings could have the opposite effect by providing criminals with more opportunities to steal. In a previous version of this paper, we presented an extensive Instrumental Variable analysis to account for such endogeneity using drug usage and drug trafficking as our instruments. While the cross-section, panel and robustness results are all robust to the inclusion of IVs, it is nevertheless difficult to show that drug trafficking is exogenous to savings. For this reason, we omit the IV results, but estimates are available from the authors upon request.

[22] The stock of savings tend to increase over time in sample. Suppose that people save 20% of income, producing a certain average stock of savings throughout the life-cycle. As income grows, the stock grows even if the marginal propensity to save remains constant. In addition, in poor-to-middle-income countries such as Brazil the marginal propensity to save tend to increase with income.

ownership of various durable goods and assets.[23] Similarly to income, we include both the log of wealth per capita and the squared log of wealth per capita.

Income inequality affects both savings and crime. For example, Demombynes and Özler (2005) find a positive relationship between local inequality and property crime. Thus, we include two measures of inequality: the Gini coefficient and the percent of people living below the poverty line. Finally, we include the log of the mean number of hours worked because labor market conditions affect the ability to save and the opportunity costs of engaging in criminal activities (see Lochner, 2004).

In addition to the income-related variables described above, regressors include the following city-level demographics, the ratio of males between the ages of 15 and 34, because most crime is committed by this demographic group (see Freeman, 1996; De Mello and Schneider, 2007); logs of both population and population density;[24] the divorce rate, which affects savings ability and is a determinant of crime;[25] educational achievement measured by the log of the mean number of years of study of residents aged 25 to 64; and the number of bank branches per 100,000 inhabitants, which affects savings via competition in the local banking market and crime via an increased presence of bank security. Finally, we include the ratio of the workforce employed as police and private security personnel, a determinant of both crime and income net of security expenses (see Levitt, 2002; Di Tella and Schargrodsky, 2004).[26] Table 4 displays descriptive statistics for the controls and shows that the cities in our sample are quite heterogenous in several dimensions, such as income, income inequality and population density.

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[23] We estimate a principal component model using a total of 22 components. Examples of included components are: a dummy variable indicating whether the individual owns their house, the ratio of house members to bedrooms and the presence of certain consumer durables, such as personal computers and automobiles. A full description of the procedure is available upon request.

[24] Glaeser et al. (1996) and Glaeser and Sacerdote (1999) document that social interactions cause crime.

[25] Results in Donohue and Levitt (2001) suggest a connection between family structure and criminal activity.

[26] The first stage of the omitted instrumental variables section is informative as to the relationship between property crime and other covariates. Most coefficients have the expected sign. For example, property crime increases with population and with the population bracket for ages 15–35. Further details are available upon request.

**Table 4: Summary Statistics for the Controls (level)**

| | Mean | Std. Dev. | Minimum | Maximum | Obs. | Aggregate* |
|---|---|---|---|---|---|---|
| Income per capita (PPP05US) | 340.00 | 107.86 | 128.04 | 1,026.68 | 566 | 520.56 |
| % Security personnel | 1.00 | 1.00 | 0.00 | 5.00 | 566 | 2.00 |
| Gini | 0.53 | 0.05 | 0.42 | 0.73 | 566 | 0.59 |
| % Male population ages 15-34 | 39.77 | 1.71 | 31.32 | 43.65 | 566 | 42.00 |
| % Divorce | 6.00 | 1.00 | 1.00 | 9.00 | 566 | 6.00 |
| Hours worked per week | 45.43 | 2.00 | 39.03 | 58.68 | 566 | 44.91 |
| Years of schooling (between ages 25-64) | 6.54 | 0.75 | 4.23 | 9.76 | 566 | 6.95 |
| Population | 14,086 | 447,391 | 795 | 10,426,384 | 566 | 36,974,378 |
| Bank branches per 100,000 inhabitants | 21.36 | 11.85 | 0.17 | 125.79 | 566 | 14.11 |
| Population density (per $km^2$) | 296.01 | 1,157.14 | 3.57 | 11,686.75 | 566 | 148.73 |
| Wealth per capita† | 6.69 | 0.55 | 4.99 | 8.11 | 566 | 6.62 |
| % Below poverty line | 19.78 | 8.58 | 2.89 | 59.38 | 566 | 14.37 |

Source: Instituto Brasileiro de Geografia e Estatística (IBGE) e Secretaria de Segurança Pública de São Paulo.
*: State as a whole, not averaged across cities.
†: Principal component measure based on the consumption of durable goods.

## 3.1 Main Results: Cross-City

Estimates of model (1) are presented in Table 5. OLS estimates confirm the graphical patterns in Figures 1 and 2. Column (1) shows a positive significant effect of local property crime on local savings: a 1 percent increase in property crime is associated with a 0.131 percent increase in per capita savings. On the other hand, violent crime is not related to savings, as shown in column (2). The positive association between property crime and savings is even stronger in column (3) where both types of crimes are included in the regression. Violent crime is still unrelated to savings.

The coefficients for the control variables are either indistinguishable from zero or have the expected sign. Savings increases with income, wealth, population density and in the presence of bank branches. On the other hand, education reduces savings, probably because the more educated use different savings or investment vehicles than a bank. In column (4), we observe a significant, nonlinear relationship between savings and income, as observed by the income squared term (see Carroll and Kimball, 1996, on the nonlinearities of the consumption and savings functions). The estimated relationship between property crime and savings is, if anything, stronger when allowing for such nonlinearities (0.179 against 0.158).

**Table 5: Cross-section Procedure†**

| Dependent variable: log (savings per capita) | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| ***Property Crime*** | **0.131** | | **0.158** | **0.179** | **0.129** |
| | **(0.051)*** ** | | **(0.056)*** ** | **(0.055)*** ** | **(0.059)** ** |
| ***Violent Crime*** | | **0.006** | **-0.074** | **-0.088** | **-0.089** |
| | | **(0.054)** | **(0.059)** | **(0.059)** | **(0.064)** |
| Bank Branches | 0.665 | 0.617 | 0.626 | 0.670 | 0.924 |
| | (0.066)*** | (0.067)*** | (0.067)*** | (0.066)*** | (0.036)*** |
| % Security Personnel | -0.048 | -0.044 | -0.043 | -0.019 | -0.103 |
| | (0.030)* | (0.030) | (0.031) | (0.030) | (0.036)*** |
| Income per Capita‡ | 0.629 | 0.727 | 0.561 | 9.702 | |
| | (0.266)** | (0.269)** | (0.274)** | (2.365)*** | |
| Income per Capita$^2$‡ | | | | -0.761 | |
| | | | | (0.196)*** | |
| Wealth per Capita | 2.277 | 2.073 | 2.345 | 2.797 | |
| | (0.352)*** | (0.369)*** | (0.358)*** | (6.536) | |
| Wealth per Capita$^2$ | | | | -0.032 | |
| | | | | (1.732) | |
| Gini‡ | -0.082 | -0.104 | -0.003 | -0.540 | |
| | (0.466) | (0.474) | (0.472) | (0.521) | |
| % Below Poverty Line | -0.008 | -0.007 | -0.009 | 0.005 | |
| | (0.007) | (0.008) | (0.007) | (0.008) | |
| Hours Worked | 1.155 | 1.146 | 1.120 | 0.752 | |
| | (0.549)** | (0.553)** | (0.554)** | (0.558) | |
| Population‡ | 0.266 | 0.275 | 0.264 | 0.285 | 0.318 |
| | (0.048)*** | (0.049)*** | (0.049)*** | (0.049)*** | (0.054)*** |
| % Male Population between 15-34 | 0.016 | 0.021 | 0.016 | -0.007 | -0.020 |
| | (0.039) | (0.041) | (0.039) | (0.032) | (0.038) |
| Population Density‡ | 0.480 | 0.463 | 0.481 | 0.592 | 0.610 |
| | (0.133)*** | (0.137)*** | (0.133)*** | (0.138)*** | (0.129)*** |
| % Divorce | -0.029 | -0.021 | -0.025 | -0.024 | -0.014 |
| | (0.018)* | (0.019) | (0.019) | (0.018) | (0.021) |
| Years of Schooling | -1.170 | -0.990 | -1.132 | -1.487 | 1.639 |
| | (0.468)*** | (0.467)** | (0.472)** | (0.476)*** | (0.401)*** |
| Constant | -11.387 | -10.898 | -10.967 | -38.185 | -1.301 |
| | (2.885)*** | (2.934)*** | (2.901)*** | (9.651)*** | (0.526)** |
| Observations | 566 | 566 | 566 | 566 | 566 |
| $R^2$ | 0.665 | 0.660 | 0.666 | 0.682 | 0.564 |
| $F$ - statistic | 81.138 | 79.258 | 75.232 | 81.086 | 72.958 |

All regressors are the same as in Table 4. ‡: In log.
†: White-Huber standard errors in parentheses.
***: Significant at the 1% level; **: Significant at the 5% level; *: Significant at the 10% level.

Our preferred cross-section estimate is column (4), which includes an extensive list of variables that capture income: wealth, income distribution, percentage of population below the poverty line, average number of hours worked and income itself. These variables are excluded from the model in column (5) to demonstrate the importance of properly controlling for income. The estimated coefficient on property crime is considerably lower than in column (4).

This suggests that omitting income biases results *negatively*.[27] Interestingly, educational attainment is now *positively* related to savings, which suggests that education captures the effect of income in column (5).

## 3.2 Main Results: Panel

Cross-sectional estimates may suffer from omitted variables bias. We mitigate this problem by introducing time-series variation to account for unobserved heterogeneity among cities. This subsection presents the estimates of the panel model (2).

The inclusion of city dummy variables only accounts for time-*invariant* unobserved heterogeneity. Since the span of the panel is short—five years—most determinants of crime and savings such as income inequality are stable. This mitigates the problem of time-*varying* unobserved heterogeneity. However, factors such as hours worked and income do fluctuate over the five-year period. Year-specific variation is captured by the time dummies. Thus, shocks to economic activity threaten the panel strategy only if the impact in different cities varies. Restricting the analysis to one state reduces this possibility. Moreover, we include two available covariates: the log of population and the log of the percentage of 15- to 34-year-old males. The inclusion of the percentage of the population in the crime-prone, age-gender group is particularly important because a large cohort reached the age of 30 during the time of the study. Evidence suggests that the coming-of-age of this large cohort has strongly influenced the Brazilian homicide rate (see De Mello and Schneider, 2007). As in Table 5, the model is estimated using property and violent crime as independent variables. Table 6 shows the results.

City fixed effects are included in all models. Column (1) shows that a 1 percent increase in property crime causes a 0.069 percent increase in savings after accounting for time invariant heterogeneity. Consistent with the OLS results, the presence of violent crime is irrelevant.

Fixed-effects estimates of the impact of property crime are smaller than the cross-sectional ones. One explanation is omitted variable bias inherent to cross-sectional estimates. However, it may not be justifiable to conclude that omitted variables bias is a problem because the panel and OLS estimates are not of comparable magnitudes.

[27] It is uncontroversial that higher savings follow higher income. In contrast, the relationship between income and property crime is unclear. On the one hand, higher income reduces the need to steal. On the other hand, higher income implies that more can be stolen.

**Table 6: Panel Procedure (1999-2004)†**

| Dependent Variable: log (savings per capita) | (1) | (2) | (3) | (4) | (5) | (6)‡ |
|---|---|---|---|---|---|---|
| ***Property Crime*** | 0.069 | 0.071 | | 0.033 | 0.035 | 0.035 |
| | (0.015)*** | (0.015)*** | | (0.012)*** | (0.012)*** | (0.013)*** |
| ***Violent Crime*** | 0.009 | | 0.019 | 0.002 | | |
| | (0.012) | | (0.010)* | (0.009) | | |
| Log (Population) | 0.168 | 0.161 | 0.308 | -0.897 | -0.895 | -0.894 |
| | (0.092)* | (0.092)* | (0.726)*** | (0.151)*** | (0.151)*** | (0.204)*** |
| Log (Male Population | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| between 15-34) | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.002) |
| Year Dummies? | No | No | No | Yes | Yes | Yes |
| City Dummies? | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3394 | 3394 | 3394 | 3394 | 3394 | 3394 |
| $R^2$ | 0.971 | 0.971 | 0.970 | 0.974 | 0.974 | 0.974 |

†: White-Huber standard errors in parentheses.
‡: Standard errors robust to clustering (within city autocorrelation) and panel-level heteroskedasticity.
***: Significant at the 1% level; **: Significant at the 5% level; *: Significant at the 10% level.

Columns (1) to (3) of Table 6 are subject to the criticism that shocks common to all cities may drive both property crime and savings. We address this issue by including year dummies. Results are in columns (4) to (6). Savings still increases with property crime, although the magnitude of the coefficient is smaller. However, if property crime causes savings, the time-specific shocks to property crime that are common to all cities *do* cause savings. This common component is discarded when year dummies are included. Thus, the numbers in columns (4) to (6)—between 0.033 and 0.035—are a lower bound to the elasticity of savings to property crime. City clustering is relevant for standard deviation estimation, and from now on all panel standard error estimates are robust to within-city correlation and panel-level heteroskedasticity.

## 3.3 Sensitivity Analysis

### 3.3.1 *Does heterogeneity in city size drive the results?*

The inclusion of municipalities of vastly different sizes poses at least three problems. First, small towns may lack banking services, forcing residents to deposit their savings in banks in larger cities. In our cross-city procedure described above, we include the number of bank branches per city as a covariate in an attempt to control for this effect. Nevertheless, if property crime is systematically higher in larger cities, the positive link between property crime and savings could still be spuriously driven because we have not properly controlled for the supply of banking services. Moreover, our panel estimates could also be biased if banks left small cities during the period under study.[28]

A second potential problem stems from underreporting of property crimes. As discussed above, the possibility exists that law enforcement officials in smaller cities are more effective because of the proximity between the authorities and residents, and this may induce victims in smaller cities to report crimes to the police. Therefore, if savings are systematically lower in smaller cities, as our summary statistics suggest, then our estimates are biased.

Finally, a third potential problem stems from the presence of metropolitan regions, which are municipalities clustered around an economically dominating city. São Paulo has three such regions, encompassing 62 cities. Individuals living in these regions typically live and work in two different cities, which brings into question the location of their savings accounts.

**Table 7: Misallocation--Estimates by Size of the City**

| **Dependent Variable: log (savings per capita)** | < 66 percentile | < 50 percentile | > 50 percentile | No Met Region |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | **A: Cross-City‡** | | | |
| ***Property Crime*** | 0.168 | 0.218 | 0.127 | 0.166 |
| | (0.066)*** | (0.082)*** | (0.057)** | (0.060)*** |
| ***Violent Crime*** | -0.145 | -0.180 | 0.040 | -0.103 |
| | (0.068)** | (0.078)** | (0.077) | (0.064) |
| **Observations** | 376 | 283 | 283 | 504 |
| $R^2$ | 0.642 | 0.598 | 0.774 | 0.664 |
| | **B: Panel§** | | | |
| ***Property Crime*** | 0.027 | 0.034 | 0.029 | 0.029 |
| | (0.015)** | (0.017)** | (0.015)** | (0.013)** |
| ***Violent Crime*** | -0.000 | 0.001 | 0.004 | -0.000 |
| | (0.011) | (0.012) | (0.011) | (0.011) |
| **Observations** | 2253 | 1695 | 1699 | 3021 |
| $R^2$ | 0.971 | 0.967 | 0.975 | 0.977 |

‡: Same controls as in Table 5, column (1). White-Huber standard errors in parentheses.
§ Same controls as in Table 6. City and Year dummies included. In parentheses, standard errors robust to clustering (within city autocorrelation) and panel-level heteroskedasticity.
***: Significant at the 1% level; **: Significant at the 5% level; *: Significant at the 10% level.

These three concerns are addressed by reestimating equations (1) and (2) using various subsamples that are stratified by population size. We run four regressions: two performed exclusively on medium- and small-sized cities (municipalities smaller than the 66th and 50th percentiles, respectively); one performed exclusively on large- and medium-sized cities (municipalities larger

[28] We mention this possibility because BANESPA, a large, state-run bank, was privatized in 2000. As of 2003, SANTANDER, the current owner, was present in all cities where BANESPA previously operated (see Arrigoni et al., 2007)

than the 50th percentile); and one performed on cities that do not belong to a metropolitan area. Results are displayed in Table 7.

The estimated coefficients are similar to their counterparts in Tables 5 and 6. The precision loss in several cases is due to smaller sample sizes.

#### 3.3.2 *Does unreported income drive results?*

The cities in our sample are very heterogeneous in terms of income. We attempt to control for this by including five income-related covariates in the cross-city procedure and by including city dummies in the panel procedure. However, it is possible that the error terms in equations (1) and (2) still capture movement due to unreported income. If this underreporting is systematic with income level, say due to tax evasion purposes, then our results could be biased. For instance, if richer cities exhibit both higher levels of underreporting and crime, a positive spurious relationship between property crime and savings arises. Bias could also arise in the opposite direction if underreporting is more prevalent in low-income cities.

As above, we address this problem by reestimating models (1) and (2) using various subsamples, now stratified by wealth. We consider two such subsamples: cities above and cities below the median level of wealth in our sample. Results are presented in Table 8.

**Table 8: Estimates by Wealth per Capita**

| **Dependent Variable: log (savings per capita)** | | | | |
|---|---|---|---|---|
| | **Cross-City‡** | | **Panel§** | |
| | Wealth above median | Wealth below median | Wealth above median | Wealth below median |
| | **(1)** | **(2)** | **(3)** | **(4)** |
| ***Property Crime*** | 0.164 | 0.168 | 0.037 | 0.043 |
| | (0.076)** | (0.080)** | (0.017)** | (0.023)** |
| ***Violent Crime*** | -0.127 | -0.010 | 0.023 | -0.022 |
| | (0.093) | (0.077) | (0.014)* | (0.015) |
| Observations | 283 | 283 | 1696 | 1697 |
| $R^2$ | 0.598 | 0.630 | 0.977 | 0.963 |

‡: Same controls as in Table 5, column (1). White-Huber standard errors in parentheses.

§: Same controls as in Table 6. City and Year dummies included. In parentheses, standard errors robust to clustering (within city autocorrelation) and panel-level heteroskedasticity.

***: Significant at the 1% level; **: Significant at the 5% level; *: Significant at the 10% level.

Savings are still positively associated with property crime and exhibit coefficient estimates similar to Tables 5 and 6. Again, precision is lost due to smaller sample sizes, but the coefficients are all significant at the 5 percent level.

### 3.3.3 *Reallocation towards safer forms of savings?*

Property crime may cause a shift among savings instruments—from unsafe forms, such as cash at home, to more secure forms, such as bank deposits. Consequently, our estimates could simply be capturing residents' reallocations of savings.

Although an overall measure of savings is not available, we address this issue by reestimating our models using demand deposits as our dependent variable. While savings accounts and demand deposits are issued through a bank, demand deposits are noninterest bearing assets and therefore are "dominated" as a savings instrument. Historically, demand deposits were more liquid than savings accounts in Brazil. If demand deposits also increase with property crime, then we suspect that our estimates are simply capturing residents reallocating their savings across different forms of assets. Table 9 contains the estimation results.

**Table 9: An Alternative Measure**

| **Dependent Variable: log (demand deposits per capita)** | All samples (1) | < 66 percentile (2) | > 5 percentile (3) | Wealth > median (4) | Wealth < median (5) |
|---|---|---|---|---|---|
| | | | **A: Cross-City‡** | | |
| ***Property Crime*** | -0.031 | -0.067 | -0.012 | 0.019 | -0.046 |
| | (0.053) | (0.106) | (0.054) | (0.082) | (0.072) |
| ***Violent Crime*** | 0.078 | 0.110 | 0.050 | 0.022 | 0.104 |
| | (0.067) | (0.131) | (0.067) | (0.116) | (0.082) |
| **Observations** | 566 | 188 | 537 | 283 | 283 |
| $R^2$ | 0.478 | 0.276 | 0.488 | 0.314 | 0.481 |
| | | | **B: Panel§** | | |
| ***Property Crime*** | -0.007 | -0.034 | -0.017 | -0.012 | -0.004 |
| | (0.029) | (0.047) | (0.032) | (0.038) | (0.046) |
| ***Violent Crime*** | 0.055 | 0.045 | 0.062 | 0.05 | 0.069 |
| | (0.029)** | (0.053) | (0.044) | (0.050) | (0.069) |
| **Observations** | 2980 | 746 | 2850 | 1558 | 1422 |
| $R^2$ | 0.913 | 0.883 | 0.914 | 0.907 | 0.904 |

‡: Same controls as in Table 5, column (1). White-Huber standard errors in parentheses.
§: Same controls as in Table 6. City and Year dummies included. In parentheses, standard errors robust to clustering (within city autocorrelation) and panel-level heteroskedasticity.
***: Significant at the 1% level; **: Significant at the 5% level; *: Significant at the 10% level.

Neither violent nor property crime are related to demand deposits. The estimates are unstable across models and statistically insignificant, suggesting that residents are not simply reallocating their savings to more secure assets.

### 3.3.4 *Sample selection?*

As mentioned above, 79 of São Paulo's 645 cities were omitted from our sample. Omission is due to the lack of bank branches in the city. In fact, two missing cities had bank branches, but had missing bank data. If these excluded municipalities are systematically different from those included in the estimation, then this sample selection could bias the results. In fact, the omitted cities are quite small—Potim, the largest of the 79 exclusions, had only 13,562 inhabitants in 2000, which is slightly smaller than the median city size of roughly 14,000. If the level of savings is systematically different in very small cities, endogenous selection on the dependent variable conditional on observables may arise. In fact, results in Table 5 suggest a lower level of savings in smaller cities. A simple way to address the potential problem of nonrandom selection is restricting the attention to sufficiently large cities. In this case, no city is excluded due to the lack of a bank branch. In fact, Table 7 contains informative results. In column (3), the sample is restricted to cities above the fifth percentile in terms of population.[29] Results are similar to those in the whole sample. Thus, endogenous selection does not seem to drive results.

#### 3.3.5 *Individual level consumption and savings data*

We use the Pequisa de Orçamento Familiar in 2002/2003, which is a household-level consumption survey, to corroborate the aggregate savings results. We compute the consumption and savings per member for each household. Results are similar if we use total consumption and add dummies for the number of adults and the number of children in the household. We then focus on two types of consumption: (i) basic needs, which are food, hygiene and cleaning, and (ii) clothing. These two categories are particularly well-measured. Other consumption categories are noisy, having many missing and zero observations. Savings is the sum in all financial instruments, including hard domestic and foreign currency. While the consumption dependent variables are in logs, savings is in levels because being a flow it may assume negative values.

We regress the three dependent variables on a dummy based on whether the respondent perceived violence in the neighborhood as a problem. This perception of violence includes both property and violent crime. As controls, we include income, age and educational level of the head of the household, their squares, dummies for the number of members in the household and state dummies. We use observations from all metropolitan areas, not only São Paulo's. For this reason, observations are clustered at the state level when computing the standard errors. Table 10 contains the results.

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[29] The second largest city without a bank branch had 2,893 inhabitants in 2000, which is slightly above the fifth percentile. So Potim seems to be a freak occurrence instead of a pattern of cities without bank branches.

**Table 10: Household Level Estimates†**

| Panel A: Dependent Variable Is the Log (Consumption of Food, Hygiene and Cleaning per Member) | | | | |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| ***Violence Dummy*** | -0.008 | -0.032 | -0.026 | -0.032 |
| | (0.023) | (0.017)* | (0.017) | (0.017)* |
| **Controls?** | No | Yes | Yes | Yes |
| **Dummies for Number of Members?** | No | No | Yes | Yes |
| **State Dummies?** | No | No | No | Yes |
| Observations | 36891 | 36891 | 36891 | 36891 |
| $R^2$ | 0.000 | 0.150 | 0.165 | 0.173 |
| **Panel B: Dependent Variable Is the Log (Consumption of Clothing per Member)** | | | | |
| ***Violence Dummy*** | 0.021 | -0.041 | -0.039 | -0.035 |
| | (0.028) | (0.021)* | (0.021)* | (0.020)* |
| **Controls?** | No | Yes | Yes | Yes |
| **Dummies for Number of Members?** | No | No | Yes | Yes |
| **State Dummies?** | No | No | No | Yes |
| Observations | 30049 | 30049 | 30049 | 30049 |
| $R^2$ | 0.000 | 0.310 | 0.321 | 0.326 |
| **Panel C: Dependent Variable Is the Total Savings per Member** | | | | |
| ***Violence Dummy*** | 654.119 | 520.988 | 558.955 | 450.456 |
| | (336.336)* | (309.731)* | (323.193)* | (253.066)* |
| **Controls?** | No | Yes | Yes | Yes |
| **Dummies for Number of Members?** | No | No | Yes | Yes |
| **State Dummies?** | No | No | No | Yes |
| Observations | 6825 | 6825 | 6825 | 6825 |
| $R^2$ | 0.001 | 0.040 | 0.045 | 0.063 |

†: Standard errors (in parentheses) are clustered at the state level.
***: Significant at the 1% level; **: Significant at the 5% level; *: Significant at the 10% level.

In panel A, the dependent is the log of consumption of basic goods (food, hygiene and cleaning products). In column (1), we run the model with no controls, and violence does not seem to affect consumption. However, once we add controls, the results increase. Column (4) shows the complete model. The dummy variable is associated with a 3.2 percent drop in consumption of basic items. This is very close to the reverse of the panel results for aggregate savings, a 3.5 percent increase in savings. For clothing (panel B), we see the same pattern, but estimated coefficients are slightly stronger. The complete model (column (4)) shows that the violence dummy is associated with a 3.5 percent reduction in the consumption of clothing. In panel C, we see that all models show violence causing an increase in savings in line with the city-level results. In the complete model, the violence dummy is associated with an increase of R$450,00 in annual savings, which represents roughly 15 percent of the average amount saved or not saved. This impact is larger than that at the city level, possibly because of data recording. A "missing" is assumed if the household had no change in asset position during the year (notice that the number of observations is drastically smaller in the savings models). Thus, savings results are conditional on the household having performed some financial transaction during the previous year.

Household-level results corroborate the city-level results. In addition, associating violence at the neighborhood level with consumption and savings at the household level considerably mitigates concerns with reverse causality and omitted variable bias, which are major concerns when working with aggregate data. Finally, the savings measure includes all categories, which reduces any concerns about capturing substitution from unsafe to safe forms of savings.

# 4 Discussion

Section 3 illustrates a striking relationship: cities with a higher prevalence of property crime also exhibit higher levels of savings per capita. In this section, we briefly discuss the theoretical reasons why property crime can affect savings.

## 4.1 Crime and Relocation

An important way in which crime can encourage savings is residential relocation because individuals living in high-crime neighborhoods may want to move to a safer place. Consequently, crime would increase the marginal benefit of savings. As the Brazilian credit market is underdeveloped, household savings represents the primary means for individuals to relocate their place of residence (for an overview of the Brazilian credit market, see Costa and De Mello, 2006).

The literature has documented that crime depresses real estate prices (see Gibbons, 2004) and induces urban depopulation (see Cullen and Levitt). Thus, individuals do indeed relocate in response to crime. Anecdotal evidence supports this "moving effect" in the State of Rio de Janeiro, which is similar to São Paulo in terms of demographics. Recent victimization surveys show a high proportion of the respondents (51 percent) showing a desire to move due to violence (see Pesquisa de Base Estadual sobre Vitimização, 2006, Instituto Brasileiro de Pesquisa Social).

## 4.2 Crime as a Tax on Consumption

Crime taxes consumption through three different mechanisms. First, the utility from consuming is lower in a dangerous environment—for instance, a night out on the town is less enjoyable if one is worried about getting mugged. Second, a higher crime rate increases the probability of a "bad state" occurring (being victimized), thus *distorting* an individual's preferences. For example, in high-crime environments, the consumption of flashy goods, such as sports cars or designer clothing, increases the probability of robbery and theft. Third, crime reduces consumption opportunities as businesses may respond to crime by reducing their hours of operation or relocating to a safer neighborhood. Broadly speaking, these three mechanisms increase the price of consumption.

Anecdotal evidence supports the assertion that crime taxes consumption. A Consumer Expectation Survey conducted in the city of Rio de Janeiro showed that violence was the main reason for consumer's pessimism, not inflation or unemployment. After news of a string of robberies or gang fights, customer presence in shopping centers was reported to drop 25 percent (see Jornal O Globo, 08/10/2004). Moreover, anecdotal evidence from newspapers suggest that individuals have substituted luxury automobiles for less conspicuous models out of fear of car theft (see Jornal O Globo, 04/16/2006). Two points should be noted. First, people may substitute "risky" (conspicuous) consumption for "safer" consumption: a night out at the cinema, for instance, may be replaced by renting a movie. Second, savings are postponed consumption. Therefore, increased savings in the presence of crime is only rational if individuals expect crime to fall in the future. Indeed, crime rates reached their peak in 2000 and have since dropped substantially.

### 4.3 Crime and Precautionary Savings

When the variance of future consumption increases, risk-averse consumers save more for precautionary reasons in an attempt to mitigate future consumption volatility (see Carroll, 2004, for a theoretical approach to this problem). Although the empirical literature has failed to convincing show the existence of a savings precautionary motive, such a motive can rationalize the crime-to-savings *nexus*. Following this train of thought, Lusardi (1998) argues that this motive exists, although its effect is small, while Gourinchas and Parker (2001) show that the motive is important for individuals with low wealth levels. Crime increases the volatility of future income and consumption flows through the taxes discussed above and the increased probability of injury or death, which may interrupt the flow of future wages.

### 4.4 Reasons Why Crime May Depress Savings

We find three reasons why crime may depress savings. First, if returns on investments are lower in high-crime environments, then the equilibrium interest rate would be lower, and, consequently, savings would fall in equilibrium. Although this is theoretically possible, this assertion has little empirical support (see Pshiva and Suarez; Deininger). Second, in infinite-horizon consumer models, the probability of death is normally modeled by introducing an additional discount factor. Therefore, any increase in the probability of death (tantamount to further discounting of future consumption) produces a lower savings rate in steady state (see Blanchard, 1985). Therefore, violent crime reduces savings as long as the crime increases the probability of death. Finally, crime may create expenses by making private security necessary (insurance, alarms, private security guards, LoJack and other protection devices). In this case, less would be saved.

# 5 Conclusion

In this paper, we have shown a positive relationship between property crime and savings. Our preferred estimate indicates that variations in property crime respond for 2.3 percent of the variation in aggregate savings over the 1999–2004 period, a small but nonnegligible fraction. We find a similar impact when using household data. This result is in congruence with the intuition that crime affects economic decisions. It is in contrast, however, with some of the papers in the literature that have investigated the crime-to-economic *nexus* (see Pshiva and Suarez; Deininger). We conjecture that this contrast is due to our main variable of interest, savings, as previous authors have used more "rigid" variables such as unemployment and investment. One avenue for future research could be shedding light on the conditions under which crime affects economic behavior. Our results call for a better theoretical and empirical understanding of the mechanisms by which crime, violence and stress affect economic decisions.

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Departamento de Economia PUC-Rio
Pontifícia Universidade Católica do Rio de Janeiro
Rua Marques de Sâo Vicente 225 - Rio de Janeiro 22453-900, RJ
Tel.(21) 31141078 Fax (21) 31141084
www.econ.puc-rio.br
flavia@econ.puc-rio.br