Income Shocks and Crime: Evidence from the Break Down of Ponzi Schemes*

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Abstract

This paper estimates the impact on crime rates of a large negative income shock led by the simultaneous crash down of various Ponzi schemes at the end of 2008 in Colombia. The crunch of Ponzi schemes affected hundreds of thousands informal investors who lost tens of millions of dollars. Using data on the spatial incidence of the Ponzi schemes and their crashing date we estimate the effect of the negative income shocks on crime rates at the municipal level. Using matching and difference-in-differences techniques on monthly frequency data, we find that the shock differentially exacerbated cash-grabbing crimes in affected municipalities relative to places with no presence of Ponzi schemes. In particular, we find a positive effect on mugging, commercial theft, and burglary, but no significant effect on major crimes like murder or terrorism. These findings are robust to controlling for differential pre-trends in crime levels. We further show that the escalation in crime rates is larger in municipalities where access to formal credit is limited and where judicial and law enforcement institutions are weaker.

Keywords: Ponzi schemes, Income shocks, Crime
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1 Introduction

This paper exploits the crash down of Ponzi schemes to estimate the short-term causal effect of aggregate negative income shocks on criminal outcomes at the municipal level in Colombia. At the end of 2008 the Financial Oversight Bureau of Colombia intervened several façade firms throughout the country. Effectively, these turned out to be a network of Ponzi schemes that illegally raised money and offered rates of return much higher than the market. Tens of millions of dollars invested in these firms by hundreds of thousands of individuals suddenly vanished, leaving investors broke. While individual deposits in the schemes were not verifiable due to the illegal nature of the business, the maximum amount that the intervening entity managed to return to each proved investor, given the assets confiscated, was about US$130. In contrast, press stories portrayed people who had invested tens of thousands of dollars of their family savings, often after having sold or mortgage their assets.

Using data on the spatial incidence of the Ponzi schemes and their crashing date we estimate the effect of the aggregate negative income shock on crime rates at the municipal level. Our identification strategy uses matching techniques to identify comparable districts that had no Ponzi schemes and hence where citizens were not negatively affected by their crash down in late 2008. We estimate a difference-in-differences (DD) model on the matched sample that includes two-way fixed effects as well as a differential pre-trend between treatment and control districts. Our results indicate that the generalized crunch of the illegal money schemes differentially increased cash-grabbing crimes like mugging and commercial theft in affected municipalities. In contrast, major non-money obtaining offenses like homicides and terrorism were not affected by the aggregate shock. Next, we explore potential heterogeneous effects depending on the severity of credit constraints to low income individuals and the presence and quality of policing, law enforcement and judicial institutions. We show that the estimated crime surges are driven by municipalities where individual have low access to microcredits, or municipalities where judicial or policing institutions are scarce or inefficient.

There is a large body of literature on the effects of negative income shocks on people's behavior. According to the classical *life cycle* and *permanent income* hypotheses (Modigliani, 1954; Friedman, 1957) individuals plan consumption patterns taking into account their expected lifetime income and try to smooth consumption overtime. In these theories savings are instrumental to achieve income smoothing. Hence the presence of credit constraints may explain why the earlier empirical studies found no support for them, suggesting instead that consumption is very sensitive to changes in income flows (Flavin, 1981; Zeldes, 1989). Indeed, in the face of credit rationing, negative income shocks may lead to large consumption drops that harm people's welfare. In weakly institutionalized environments this is likely to increase the incentives for individuals to try to counteract the negative shock with questionable practices, like resorting to criminal activities. In this paper we explore the differential effect that the negative income shock that followed the crunch of Ponzi schemes had on he incidence of crime in Colombian districts according to the presence of credit constraints and weak judicial and low enforcement institutions.

The welfare effects of negative income shocks are likely to be stronger on poorer households, which are more credit constrained and hence less able to carry out effective risk pooling (Banerjee and Duflo (2007).[1] Even if households do have access to insurance, for instance through informal community-specific mechanisms (Townsend, 1994), such forms of risk pooling would not protect households from aggregate shocks that span over the entire community. In any case, the available empirical evidence suggests that households even fail to protect themselves from idiosyncratic shocks (Deaton, 1992; Bardhan and Udry, 1999). Thus, households must resort to other practices to try to smooth consumption in the face of shocks: Rosenzweig and Wolpin (1993) suggest that they counteract shocks by selling productive assets like cattle; Larson and Plessmann (2009) and Dercon (1998) underline the importance of crop diversification; and Beegle and Weerdt (2006) and Guarcello, Mealli, and Rosati (2010) suggest shocks are counteracted by increasing child labor.

Another potential way of dealing with negative shocks is resorting to predatory activities, particularly if the local security and law enforcement institutions are weak. The tradeoff between (legal) productive activities and (illegal) criminal behavior was first formalized by Becker (1968) and Ehrlich (1973), who highlight that, ceteris paribus, a lower opportunity cost in the form of lower wages or income, increases the incentives to become criminal or engage in illegal activities. This theoretical mechanism is also present in Dal-Bo and Dal-Bo (2011) that incorporates predatory activities to a canonical trade model to study how commodity price changes affect the inter-sectoral labor allocation.

The empirical evidence on the relevance of the relationship between negative income shocks and crime or violence is scarce, perhaps due to the usual identification concerns. Miguel, Satyanath, and Sergenti (2004) exploit the variation in income explained by ex-

[1] There is mounting evidence that negative shocks have sizable consequences on the wellbeing of poor households, and that children are particularly at risk. Jensen (2000), for example, shows that income shocks in Côte d'Ivoire substantially increase children's malnutrition. Bengtsson (2010) documents that in Tanzania a 10% drop in income leads to a reduction in children's weight of about 400 grams. In Guatemala, India, Indonesia and Tanzania negative income shocks have been shown to increase child labor at the expense of school dropout (Guarcello, Mealli, and Rosati (2010); Jacoby and Skoufias (1997); Thomas, Beegle, Frankenberg, Sikoki, Strauss, and Teruel (2004); Beegle and Weerdt (2006), respectively).

ogenous variation in rainfall levels. Using an instrumental variables strategy, the authors show that negative economic shocks significantly increase the probability that armed conflict breaks out in Sub-Saharan Africa. Several papers have followed a similar identification strategy thereafter. For instance, Miguel (2005) shows that negative rainfall shocks (that proxy for economic downturns) increase the probability that elderly women (accused of witchcraft) are killed in rural Tanzania. In a similar vein, Sekhi and Storeygard (2011) show that drought-led negative shocks in India increase domestic violence and dowry deaths. The mechanism appears to be that impoverished men have incentives to kill their wives in order to marry another women and renew their dowry. In addition, Mehlum, Miguel, and Torvik (2006) show that rainfall driven cereal price-changes in 19th century Bavaria explain the incidence of property crimes. Hidalgo, Naidu, Nichter, and Richardson (2010) also use rainfall to instrument for negative economic conditions in Brazilian municipalities and show that negative income shocks increase the incidence of land invasions especially in more unequal municipalities.[2]

Another branch of the empirical literature, closer in spirit to our paper since does not rely on the (possible) exogenous variation provided by rainfall but instead exploits the variation given by quasi-natural experiments, arrives at similar conclusions regarding the effects of negative economic shocks on crime. Bignon, Caroli, and Galbiati (2011) exploit the variation in the timing in which different wine districts were affected by the *phylloxera* parasite in the second half of the 19th century in France. In line with our own results, the paper finds that the negative shock did not affect violent crimes but did increase property crimes significantly. The absence of an effect for major offenses is suggestive that people are breaking the law to try to substitute for the forgone income and not because they become savage overnight.[3] In a similar vein, Arnio, Baumer, and Wolff (2012) exploit cross-state variation in the US to find that the increase in mortgage prices had a positive effect on the incidence of armed robberies. Morse (2011) uses natural disasters as an exogenous shock to find that the existence of payday lenders mitigates California foreclosures as well as larcenies (but not burglaries or vehicle thefts). Finally Dube and Vargas (2013) show that negative exogenous changes in the price of coffee (and in general to labor-intensive agricultural commodities) exacerbate conflict-related

[2]Instrumenting economic shocks with rainfall is not free of criticism. Sarsons (2011) shows that rainfall is a good predictor of crime even in Indian districts in which structural characteristics make it less likely that rain is correlated with economics performance. The author interpret this as evidence that rainfall affects crime through different channels and hence questions the exclusion restriction of using rainfall as an instrumental variable.

[3]These findings contrast with the results Puech and Guillaumont (2006). Using a panel of countries for the period 1980-1997, the authors find that macroeconomic stability increases homicides more than theft. The identification strategy of this paper is however not comparable to that of Bignon, Caroli, and Galbiati (2011).

violence in the Colombian districts in which farmers' income depends more heavily on the coffee harvest.

Common to all these papers is the Beckerian idea that negative income shocks lower the price of carrying out (illegal) activities that can help offset the consequences of the shock. Because law breaking occurs as an attempt to substitute for the forgone income, the types of crimes that are likely to follow negative shocks typically exclude major offenses like homicides.[4]

To the best of our knowledge, there is no paper that systematically studies the effects of shocks induced by the crash down of generalized financial Ponzi schemes. This paper takes advantage of the quasi-natural experiment given by the crunch of 12 Ponzi schemes that affected over 10% of the Colombian territory and hundreds of thousands of investors, to assess the effect of an aggregate income shock on urban crime rates. Using a DD strategy over a matched sample, and controlling for municipality and month fixed effects, as well as a large set of time-varying controls; our findings suggest that money-grabbing crimes increased disproportionally in affected districts compared to places that had no Ponzi schemes operating before the crisis. This contrasts with the fact that major crimes such as homicide or terrorism do not present a systematic differential pattern across treatment and control municipalities.

Another contribution of our paper is the identification of heterogeneous effects of the negative shock on crime. These depend both on the extent of credit rationing for the lower income individuals and the presence and quality of institutions. We show that crime surges following the shock are driven by municipalities where individuals have low access to microcredits, or municipalities where judicial or policing institutions are scarce or inefficient.

The rest of the paper is organized as follows. Section 2 provides brief context on Ponzi schemes and their recent development in Colombia. Section 3 describes the data and the empirical strategy. Section 4 discusses the main results and robustness and section 5 concludes.

[4]However, this really depends on the institutional and cultural context that accompanies the shock. This is why income shocks are likely to induce homicides in witchcraft-believing villages in Tanzania (Miguel (2005)) or dowry-dependent rural India (Sekhi and Storeygard (2011)), but not in more urbanized Western settings. The latter is the case of the crunch of Ponzi schemes in Colombia, where the portfolio of money-extracting crimes (eg. theft or robbery) usually excludes murder or terrorism. The latter activities respond to other types of incentives (Cook and Zarkin, 1985).

2 Background: Ponzi schemes

"Ponzi schemes" were named in the US after Charles Ponzi, who created in 1920 a financial scheme that offered extraordinarily high returns to costumers under the motto: "Double the money within three months". In practice Ponzi could sustain such rates by rewarding early investors with the money of later participants (Zuckoff, 2005). Ponzi schemes are fraudulent investments that offer rates of return that are considerably higher than market rates. This is made possible by a pyramidal structure in which the investments from a larger number of investors at the base are used to pay high returns to a smaller number of investors at the peak. Thus returns to investors come from deposits from subsequent investors rather than from the profit of the firm's business. This is only sustainable as the pyramid becomes larger and larger, which can only happen by ensuring that the business expands at high rates. This structure makes such schemes especially unstable in the long run.[5]

It is worth noting that Charles Ponzi was not the creator of these kind of businesses. According to MacKay (1841), the first such fraud was recorded in 18^{th} century France, where the Scottish economist John Law engineered a scheme that triggered high levels of speculation and ended up in a financial collapse called the "Mississippi bubble".[6] Garber (1990) argues that although there is historical anecdotal evidence that the consequences of this fraud were large, it is not possible to assess the economic cost with accuracy due to the shortage of data for the period. In the 20^{th} century similar cases of Ponzi-like financial schemes that crashed with large negative consequences have been reported in Portugal (1970s), the US state of Michigan (1987) and Rumania (1990s). Perhaps the two cases that are most remembered in recent history are Albania (1997) and Haiti (2001). On the one hand, the crunch in 1997 of a number of Ponzi schemes created between 1993 and 1996 generated social disorder against the Albanian government (accused of lack of regulation and having led the businesses grow considerably) that ended up in what Jarvis (1999) calls a civil war, resulting in over 2,000 fatalities and the oust of the incumbent Albanian president. On the other, the crash in 2001 of several Haitian "cooperatives" endorsed both by the government and through TV commercial by local celebrities, costed the country about 60% of its GDP.

Ponzi schemes have recently been brought back to the public attention in the US by the infamous Madoff case. According to Drew and Drew (2010) Bernard Madoff, a

[5] There are similar fraudulent practices like "pyramids" and "financial chains". However, these practices share the essential pyramidal structure with Ponzi schemes, which makes them as unsustainable. In fact, in the Colombian context the schemes analyzed were called "pyramids" by the local press.

[6] The name comes from the fact that Law was granted privileges to develop the French colonies of the Mississippi valley.

US businessman and former chairman of the NASDQ stock market, managed the most successful Ponzi scheme in history. Indeed Madoff's US$ 50 billion fund operated for over two decades and its nature was only uncovered in 2008, at the peak of the global financial crisis, after a massive withdrawal of resources from fund investors. However, Gregoriou and L'habitant (2009) argue that there were various alerts that something abnormal was going on with Madoff's fund before the crisis, and that the US regulatory entities were negligent not to act then. In 2009, Madoff was sentenced to 150 years in prison for the creation and management of a Ponzi scheme that affected thousands of investors.

Colombia has seen the rise and fall of Ponzi schemes a few times in the past. However, the most recent episode is also the largest in terms of its negative consequences. Indeed, several Ponzi schemes were established throughout the country in the mid 2000s. The façade firms hiding the schemes first settled in rather small towns and then expanded to bigger cities when the business was growing and more costumers were needed. The 2008 crises started when a large scheme with branches thorough the country (DFRE) became unsustainable. This episode was followed by media speculation that other high-return investments were in similar trouble in other parts of the country. When some investors tried to get out of the business and other schemes went broke, the government intervened. It was later publicized by the press that high profile public servants and politicians had themselves made investments in large schemes like DFRE or DMG.

In mid-November 2008, through the Financial Oversight Bureau, the government issued Decree 4333, declaring a social state of emergency that permitted the closure of companies suspected to operate as Ponzi schemes, the seizure of their financial assets and the arrest of their representatives. The intervened firms were accused of fraud, illegal acquisition of money, and in some cases money laundering. The consequences of the intervention, that took place during November and December 2008, were soon evident. Large numbers of investors held protests in the towns where the headquarters of the affected firms were located. Demonstrators demanded the compensation of their losses from the government, who was accused of letting the Ponzi schemes become too big. Protests often led to small riots and the damage of both private commercial business as well as public facilities.

3 Empirical Strategy

In order to identify the effect of the negative income shock generated by crash down of Ponzi schemes on crime rates in Colombian districts we take a quasi-experimental approach. Our identification strategy exploits the longitudinal variation given by the fact that Ponzi schemes were present in some municipalities but no in others, and that all the schemes crashed within a one month period (mid November to mid December 2008, see Table 6). We thus use DD to estimate the differential increase in crime rates experienced in municipalities that hosted Ponzi schemes after the financial crash, relative to places with no such businesses. This strategy takes into account any pre-treatment difference in the level of the outcome under consideration across treated and control districts. The simplest DD specification we run is:

$$Crime_{i,t} = \alpha + \beta_1 Ponzi_i + \beta_2 Post_t + \theta(Ponzi_i \times Post_t) + \epsilon_{i,t} \tag{1}$$

where $Crime_{i,t}$ is the crime rate, normalized by 100 thousand inhabitants, in municipality i and month t. We look at various types of crimes: income-generating crimes including mugging, commercial theft and burglary, and major offenses including murder and terrorism. $Ponzi_i$ is an indicator of the municipalities affected by Ponzi schemes and $Post_t$ is a time-dummy that captures the period after the crunch of the Ponzi schemes and hence takes value one from January 2009 onwards and zero up to October 2008.[7] The coefficient of interest, θ, captures the differential change in crime rates after the schemes were dismantled, in treated municipalities relative to those where no schemes were present.

Equation 1 faces some challenges in order to credibly identify the causal effect of the Ponzi crash on crime. First, the main identifying assumption of DD is that, in absence of the treatment, the outcome variable would have followed a similar trend in treated and control municipalities. Suggestive evidence that this is likely to be the case comes from examining the trend in the outcome for both the treated and the control group *before* the treatment takes place. While visual inspection of the difference crime rates in municipalities with and without Ponzi schemes (Figure 2) suggests this is probably the case for murder, commercial theft and burglary but not for murder or terrorism, our preferred specification explicitly controls for any differential pre-trend in the outcome across treatment and control districts.

Second, there may be selection in which municipalities get Ponzi schemes and which not, which in turn may be a source of bias. In particular, Ponzi schemes may have settled in municipalities that were more prone criminal cycles to begin with. We tackle this issue in several manners. We add municipality and monthly fixed effects to control for any time-invariant municipal-specific heterogeneity that may be correlated with crime changes, as well as for aggregate shocks that may affect municipalities at a specific time. We also introduce time-varying variables at the municipality level that control for observable time-varying differences. Thus, to investigate the robustness of the effects found with model

[7]The months during which the Ponzi schemes were dismantled, November and December 2008 were excluded from the analysis.

1, we further run:

$$Crime_{i,t} = \sigma_i + \gamma_t + \theta(Ponzi_i \times T_t) + \delta X_{i,t} + \phi(Ponzi_i \times D_{t_0-T,t_0-1}) + \xi_{i,t} \qquad (2)$$

where municipality and monthly fixed effects are respectively captured by σ_i and γ_t.[8] The coefficient ϕ estimates whether there is a significant T-period differential pre-trend in the outcome in the Ponzi affected municipalities relative to the control group. Note that the so called 'parallel trends' assumption holds when estimates of ϕ are not significantly different from zero. Moreover, estimating ϕ makes our estimates robust to any difference in trends across treatment and control groups.

In addition, the vector $X_{i,t}$ controls for time varying, municipality specific characteristics, including one spatial lag of the outcome (to account for potential geographical spillovers), the total contemporaneous crime level (excluding the outcome) in municipality i (to control for the overall local security), and total tax revenues and commerce tax revenues (to control for the municipality economic performance in the absence of GDP figures at such level of disaggregation).

As an additional robustness exercise we use matched techniques to identify, within the entire pool of control municipalities, places that closely resemble those that received Ponzi schemes. We then estimate the DD model in equation (2) on the matched sample. This allows us to reduce any remaining selection bias that is correlated with the observables characteristics included in the model. For this purpose, we perform a propensity score matching using the Mahalanobis metric (Cochran and Rubin (1973)). Matching is performed over the lagged outcome, the average rate of all crimes in neighboring municipalities, and the sum of other crimes in each municipality.

4 Data

We constructed an original database that merges monthly data on the incidence of different types of crime at the municipal level, with information on the location and crashing date of each one of the 12 Ponzi schemes that operated in Colombia since the mid 2000s. Criminality data comes from the Colombian National Police. In turn, we constructed the Ponzi dataset from primary sources specifically for this project. In particular, we gathered Ponzi-related stories from national and regional papers and coded what munic-

[8] Note that the non-interacted terms $Ponzi_i$ and $Post_t$, present in equation 1 are not included in 2 as these dummies are captured respectively by the municipality and the time fixed effects.

ipalities hosted which of the firms that later on were revealed as effectively being façade Ponzi schemes. We complement this with information from publicly available judicial sentences on Ponzi investigations.

We identify 12 different Ponzi-like firms with presence in 110 municipalities. Even if this represents only about 10% of Colombian smallest administrative districts, the treated areas account for 55% of the country's population and 80% of the country's total tax revenue. This suggests that the magnitude of the shock we study in this paper is economically large. As illustrated by Figure 1, most such municipalities are located in Southwestern Colombia.[9] Although judicial files suggest that the Ponzi schemes began to settle in rather smaller towns, by 2008 they were also present in large cities, including the country's capital Bogotá.[10]

We were also able to identify the exact date in which each scheme was intervened by the authorities and effectively crashed down.[11] Table 1 summarizes the Ponzi data.

Crime data is available at the municipality level with monthly frequency for our entire period of analysis (June 2007 to December 2009). It records the offenses in different categories of crime. In our analysis we include outcomes that are arguably by and large money extracting activities: mugging, commercial theft and burglary, as well as major offenses that generally respond to other motivations (which we use as placebo): homicides and terrorism. All outcomes are measured as rates normalized by 100 thousand inhabitants.

Table 2 presents descriptive statistics for these variables. We report the incidence of each crime in both Ponzi and no Ponzi municipalities, and before and after the financial crash took place. This allows us to summarize the essence of our empirical strategy and present the basic DD estimates (as estimated from equation 1) of how are the criminal outcomes affected by the crash of Ponzi schemes. For instance, the incidence of mugging is larger in Ponzi areas both before and after the crash.[12] However the post-crash difference is significantly larger which suggests that the gap between the two types of municipalities increased after the crunch of Ponzi schemes. Indeed, the DD estimate suggests a significant gap increase of about 0.75 mugging episodes for each 100 thousand inhabitants. There is a similar behavior in the case of commercial theft and burglary: Crime rates are systematically higher in the Ponzi-affected areas but the gap is larger

[9] The department of Putumayo in the border with Ecuador is a special case in which there was at least one scheme in almost every municipality.

[10] To facilitate comparability, throughout our analysis we exclude the four largest cities in country, each with over one million inhabitants.

[11] Our analysis covers the period June 2007-December 2009 since not all 12 schemes were established before mid 2007.

[12] This is possibly explained by the fact that on average larger cities where affected by Ponzi schemes, and crime is usually more prevalent in bigger towns.

after the crash. The DD estimate is however not significant in the case of burglary.

The pattern is somewhat different in the case of murder and terrorism (columns 4, 5 and 6). The murder rate is significantly larger in Ponzi areas both before and after the financial crash, but this gap is smaller instead of larger in the post-crash period. The DD estimate is indeed *negative* (but insignificant). In the case of terrorism there is no evidence of statistically significant differences neither across Ponzi and no Ponzi municipalities, nor across periods. This is consistent with the idea that when facing large negative income shocks people may break the law to try to substitute for the forgone income, and not because they become savage overnight.

In the next section we will explore the robustness of these rough estimates to specifications that additively include our full set of controls, two-way fixed effects and a differential pre-trend, up the most demanding specification as represented by equation 2. We will do so only for the outcomes for which Table 2 provides evidence that are affected by the shock: mugging, commercial theft and burglary.

We have both time-invariant and time-varying controls. The first set of controls can only be estimated in specifications that do not control for municipality fixed effects. These include population density, the (census-based) poverty index, and the distance of each town to the capital of the department (equivalent to the US state). The sources of these data are, DANE (Spanish acronym for the National Department of Statistics) for the first two variables, and IGAC (National Geography Institute) for the last. Time-varying controls include one spatial lag of the outcome (to account for potential spillovers), the aggregate (excluding the outcome) contemporaneous crime level in the municipality, total tax revenues and commerce tax revenues. In the absence of municipal-level GDP data for Colombia, the latter two variables account for the relative economic size of the municipality. These come from the National Planning Department.

Table 3 reports the descriptive statistics of all the controls and compares the mean of each variable in Ponzi and no Ponzi municipalities. The key message of this comparison is that places that hosted Ponzi schemes are significantly different from places that did not in all observable characteristics.[13] This is of course challenging for the identification of the effect of the schemes' crash on criminal outcomes. However, as discussed in the previous section, our identification strategy deals with such concern in two distinct manners. First, the double difference in the DD strategy takes into account both any difference in the level of variables as well as in their trend, provided that the latter is the same across treated and control municipalities. Second, as a robustness check we pre-process the data

[13]Treated municipalities are more densely populated, are located nearer to the department capital, levy more taxes and have less incidence of poverty.

using matching techniques, which ensures that the two types of municipalities that enter the DD analysis in the matched subsample are very similar on observable characteristics.

Our review of the literature (see the introduction) suggests that negative shocks are more likely to affect people that have less access to credit and insurance, and that this is more likely to push individuals to criminal enterprises if policing and law enforcement institutions are less effective. In line with these two hypotheses, we test in section 5.2.4 whether our main results are driven by people in different municipalities facing differential degrees of financial constraints, or by municipalities having different level of institutional presence and efficiency. To this end, we use financial variables such as the (normalized) number of microcredits, and the per capita number of financial institutions present in each municipality. The source of the first variable is Asobancaria (the Colombian Association of Banks) and that of the second is the local NGO Fundación Social (FS). In addition, we use proxies for the presence and quality of judicial and law enforcement institutions. For instance, the per capita number of law enforcement institutions is the population normalized number of judiciary and jails at the municipal level. This variable, as well as that of the per capita number of police stations, comes from FS. Finally, as a proxy for the efficiency of the judiciary at the local level, we rely on an index computed by Fergusson, Vargas, and Vela (2013). The index is based on cases that entered the criminal justice system and is computed as follows:

$$\begin{aligned} \textit{Efficiency Index}_m &= \frac{\textit{Cases Closed}_m}{\textit{Total Cases}_m} \times \frac{\textit{Total Resolved Cases -Total Unresolved Cases}_m}{\textit{Cases Closed}_m} \\ &= \frac{\textit{Total Resolved Cases -Total Unresolved Cases}_m}{\textit{Total Cases}_m}. \end{aligned} \tag{3}$$

This measure can be thought of as the efficiency of judges, adjusted for quality: The first ratio in the first line of the expression measures the share of cases entering the judicial system that are resolved (efficiency). However, cases are often closed without resolution, meaning that either no one is found guilty, or terms expire and the judge is forced to close the case with no definite action. Thus, it is adjusted by the second ratio (quality): the difference between resolved and unresolved cases, normalized by total closed cases. The source for these data is the Office of the National General Attorney.

The financial and institutional variables are also included in Table 3. These too reveal significant systematic differences between Ponzi and no Ponzi municipalities.

5 Results

5.1 Baseline results

Baseline results are summarized in Table 4. We focus in this section on the the effect of the Ponzi crash on the economic crimes (mugging, commercial theft and burglary). The intuition that these are the crimes that are likely to be affected by the shock is confirmed by Table 2, which presents the basic DD estimates for all outcomes including murder and terrorism as well. In Table 4 we report the estimates of coefficient θ for four different version of equation 2 (columns 1 through 4). Column 1 only includes the full vector of controls $X_{i,t}$ including both the time-varying and time-invariant controls described in section 4. Columns 2 and 3 drop the time-invariant controls and include, respectively, municipality and time fixed effects: $sigma_i$ and γ_t. Column 4 further includes a differential pre-treatment trend.[14]

Results are in all cases positive and significant which point to a differential increase in all three types of economic crimes in municipalities that hosted Ponzi schemes after these collapsed at the end of 2008. The DD estimate is significant across all specifications for mugging and commercial theft. It is not significant for burglary except in the case when all time-varying controls and two-way fixed effects are included. That is, that rate of burglary loses significance when the differential pre-trend is included.

The estimated effect for both mugging and commercial theft are not only significant throughout specifications but also very stable in terms of magnitude. The exception is the inclusion of monthly fixed effects which doubles the estimated DD coefficient of mugging, though this shrinks again when the pre-trend is included.

Overall it it seems that the Ponzi crash positively affected all three economic crimes. Notice as well that the simpler the investment required to commit a crime the larger the estimated increase in it. Indeed, while the rate of mugging increases by 0.79 per 100 thousand inhabitants, that of commercial theft increases only by 0.3 and the rate of burglary by half the rate of commercial theft. This could be related to the complexity of the technology required to commit each type of crime. While the technology of mugging is simpler than the technology of robbing a store (in terms of the required capital, knowledge, planning and organization), the latter is arguably simpler than what it takes to engage in burglary.

[14]Recall that all specifications exclude the four biggest cities of the country (with populations above 1 million). Our results do not however hinge on dropping these outliers.

5.2 Robustness

5.2.1 Matching

Even under the assumption that in the absence of the shock the criminal trends of the Ponzi and the no Ponzi municipalities would have been the same, the validity of which is supported by both by Figure 2 and by the inclusion of a differential pre-trend in the empirical model, the results presented in Table 4 may be biased if Ponzi schemes settled in municipalities that are significantly different than others, especially in terms of their criminal profile. For this reason we check the robustness of our baseline results to estimating our DD model on a matched sample of municipalities. To ensure that we are comparing similar municipalities in terms of their security environment we conducted propensity score/Mahalanobis matching (Cochran and Rubin (1973)) using three covariates: the lagged outcome of interest, one spatial lag of the outcome and the aggregate incidence of other crimes in the municipality. The first variable ensures that municipalities have similar pre-shock levels of the outcome, the second that the potential spillover from neighboring municipalities is similar, and the third that municipalities are similar with respect to their overall security.

Table 6 shows that the matching procedure was successful in constructing a more comparable set of municipalities. Indeed, and in contrast to Table 3, the after-matching mean incidence of both the outcomes and the balancing covariates is very similar in Ponzi and no Ponzi municipalities. In other words, the percent bias reduction owing to the matching is quite large and significant in most cases.

While the matched sample reduces the number of observations to about a half (compare the number of municipalities of the regressions in Table 4 with that of Table 6), it ensures that the DD estimation is carried over districts that are very similar to begin with. Most importantly, the results presented in Table 6 are remarkably similar to the pre-matching estimates both in magnitude and significance: taking the point estimate of the most demanding specification of column 4 the rate of mugging increases differentially in Ponzi municipalities by 0.8 cases per 100 thousand inhabitants; that of commercial theft increases by 0.3 and that of burglary by 0.16. The latter is however not significant. Again, the effect on mugging is larger than on theft, and the effect on theft is larger than the effect on burglary. We interpret this asymmetry as evidence of the fact that people who are not professional criminals are more likely to resort to activities that require lower investments. This in turn is consistent with the idea that crime surges are driven by the efforts of individuals who, in the face of a large negative income shock, resort to criminal activities to try to recover the forgone income.

5.2.2 Duration of the crime surges

The shock we study in this paper is a transitory shock. While there is heterogeneity in the investments lost by the people who participated in the schemes, ranging from a few hundred dollars to life-time savings, the shock did not in principle affect people's jobs or productivity, or at least not systematically. Hence, we should expect that the effect of the shock on crime rates is only temporary.

Recall that the crash of Ponzi schemes in Colombia took place at the end of 2008, and that our period of analysis spans until December 2009. Thus, in the analysis presented the post-shock period is a full year long. We report the duration of the criminal upsurge graphically in Figure 3. The figure graphs, for each one of the outcomes of interest, the estimated coefficients θ_1 to θ_4 corresponding to the interaction of the $Ponzi$ dummy with an indicator of each one of the four quarters of 2009 in the following variation of regression model 2 (estimated on the matched sample):

$$Crime_{i,t} = \sigma_i + \gamma_t + \sum_{1}^{4} \theta_i (Ponzi_i \times 2009Qi_t) + \delta X_{i,t} + \phi(Ponzi_i \times D_{t_0-T,t_0-1}) + \xi_{i,t} \quad (4)$$

Figure 3 reveals two important points. First, crimes surges are relatively short-lasting: Three quarters for the case of mugging and two for the case of commercial theft.[15] Second, and related to the first point, because the baseline specification takes a year-long post-shock period as a reference, the average post-shock DD estimate computed in Tables 4 and 6 understates the actual increase in crime rates. Take for example the case of mugging (Figure 3.1): during the first half of 2009 the differential increase of this crime in Ponzi municipalities was 1.9 and 1.7 cases per 100 thousand inhabitants respectively. In the third quarter it is 0.9 and in the fourth 0.5 and not significant.

5.2.3 Inference

As Bertrand, Duflo, and Mullainathan (2004) show, standard errors in DD specifications may be severely biased due to serial correlation. As a solution the authors prove that collapsing the time-series information into a 'pre' and a 'post' period is a simple way of taking this problem into account. In this case, the dependent variable, $Crime_{i,t}$ is computed as the average crime rate over the entire time-windows before and after the Ponzi crisis. In Table A.1, in the Appendix, we show that our results are robust to pre-processing the data in this way (column 1). The estimated DD coefficient of the effect of the Ponzi crash on mugging is slightly smaller than the baseline case reported in Tables

[15] Burglary is not significant in any quarter.

4 and 6, and is still significant. Also, the coefficient on commercial theft is virtually unchanged and does not lose significance either. In turn, the estimated effect on burglary is about half the baseline effect and still not significant.

Another approach is to take into account the nature of the potential autocorrelation. Thus, if one thinks that the nature of the serial correlation is the fact that the same municipalities have different observations overtime then clustering the standard errors at the municipal level will improve the statistical inference. If instead the serial correlation is a consequences of municipalities being grouped in larger administrative units (departments, equivalent to US states), then clustering at the department level will suffice. Columns 2 and 3 from Table A.1 show that the baseline DD results estimated on the matched sample are robust to both ways of clustering the standard errors.

5.2.4 Heterogenous effects

We have discussed two potential mechanisms that may exacerbate the criminal effect of negative shocks of the sort we study in this paper. First, negative shocks are more likely to affect people that have less access to credit and insurance. Second, negative shocks are more likely to push individuals to criminal enterprises if judicial and law enforcement institutions are less effective. In this section we explore the extent to which our baseline results are driven by people in different municipalities facing differential degrees of financial constraints, or by municipalities having different level of institutional presence and efficiency.

Regarding access to financial markets, the idea is that municipalities where people face stronger credit constraints are likely to have witnessed more people resorting to informal investment mechanisms like Ponzi schemes and hence may explain a larger share of the average estimated upsurge in economic crimes, especially if those who are credit constrained are poorer individuals. This idea is consistent with recent research on the relationship between baking and crime. For example, using state branching deregulation to instrument for bank competition, Garmaise and Moskowitz (2006) show that bank mergers increase crime in US counties, because of higher loan interest rates that increase the share of people who are credit rationed. In turn, the availability of credit seem to mitigate criminal surges. An example of that is the paper by Morse (2011). The author finds that negative income shocks explained by (exogenous) natural disasters, induce an increase in crime in California. However the existence of payday lenders offset the surge in foreclosures and larcenies, but consistent with our findings, not in burglaries and vehicle thefts.

We explore this hypothesis is Table 7 where we estimate equation 2, after matching, on samples that we divide according to two different proxies of financial depth (as described in section 4): the (per capita) number of financial institutions present in the municipality (top panel), and the average access to microcredits (bottom panel). The odd columns report the DD estimates for the three outcomes focusing on the subsamples of observations below the mean of each proxy of financial access. The even columns use the subsamples above the mean access. The message is clear in that the average effects reported on Table 6 are driven by what happens after the financial crunch in municipalities where financial access is relatively low. Indeed, the estimated DD coefficients of the odd columns are positive and significant (in the top panel even for the case of burglary, for which the average effect reported in Table 6 was nil). In turn, the estimated differential impact of the Ponzi crash on income generating crimes taking place in municipalities that hosted Ponzi schemes is indistinguishable from zero when the sample is restricted to places with relatively high levels of financial access (even columns). As expected the DD estimates of the low-access subsamples are higher in magnitude than the average estimated reported in Table 6. In the case of mugging the effect is almost double in magnitude when the poorer individual are those who face the credit constraints (bottom panel).

We also look at the extent to which well functioning judicial and law enforcement institutions can deter crime. In particular, in Table 8 we study whether our baseline effects differ across municipalities that vary according to three proxies of the presence and quality of these types of institutions: a 'judicial efficiency' index (as explained by equation 3, top panel), a measure of the presence of institutions of law enforcement (the per capita number of judiciary and jails, middle panel), and the per capita number of police stations (bottom panel). The odd columns report the DD estimates for the three outcomes focusing on the subsamples of observations below the mean of each proxy of institutional strength. The even columns use the subsamples above the mean strength. The results suggest that a relatively high presence of law enforcement institutions as well as a relatively efficient judicial apparatus are able to deter the surges in income-generating crimes (mugging and commercial theft) experienced by municipalities with relatively weaker institutions. Interestingly, moreover, the lack of good quality judicial institutions (top panel) seems to be worse than the lack of presence of policing and law enforcement institutions (middle and bottom panels).

6 Conclusion

This paper exploits the crash down of Ponzi schemes to estimate the short-term causal effect of aggregate negative income shocks on criminal outcomes at the municipal level in Colombia. At the end of 2008 the Financial Oversight Bureau of Colombia intervened several façade firms throughout the country that were accused of illegally raising money and of providing short-term rates of return to investors that were significantly higher than the market rate. These businesses turned out to effectively be a network of Ponzi schemes in which hundreds of thousands of primarily middle-low and low income individuals had invested tens of millions of dollars.

We estimate a difference-in-differences model on a matched sample that includes two-way fixed effects as well as a differential pre-trend between treatment and control districts. Our results indicate that the generalized crunch of the illegal financial schemes differentially increased cash-grabbing crimes like mugging and commercial theft in affected municipalities. In contrast, major non-money obtaining offenses like homicides and terrorism were not affected by the aggregate shock. We also show the the positive effect of crime is temporary (lasting one to three quarters) and is exacerbated by the presence of credit constraints to low income individuals and by the presence and quality of policing, law enforcement and judicial institutions.

This paper is part of a growing empirical literature that tests the Beckerian idea that negative income shocks lower the price of carrying out (illegal) activities that can help offset the consequences of the shock. These incentives are likely to generate increases in crimes that provide resources to the perpetrator, but not in other forms of crime. However, the nature of such money-generating crimes depends on the institutional context that accompanies the shock.

To the best of our knowledge this is the first paper that assess in a systematic way the indirect consequences of the presence and crashing of Ponzi schemes. Another contribution of the paper is to identify the conditions under which the Ponzi-led surges in crime are exacerbated.

Our results point to several policy avenues that can help offset the negative consequences of unexpected income shocks. The importance of reducing credit barriers and extending financial access has been largely emphasized in the development literature. We provide another reason why policy efforts should target the reduction of credit constraints, namely that the lack of credit and insurance mechanisms push individuals who face negative shocks to practices that are often times illegal or dangerous. In addition strengthening the judicial apparatus at the local level is key to raise the cost to individual who plan to resort to criminal enterprises. Finally this paper points to one particular unexpected negative consequence of the state intervention of illegal financial businesses in developing countries.

One interesting avenue for future research is to explore to what extent the criminal surges that followed the crash down of Ponzi schemes in Colombia crowed out the judicial system that faced excess criminal activity and this resulted in longer term increases in other types of crimes as as results to the judicial congestion.

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Table 1: Municipal Incidence and Crash Date of Ponzi Schemes in Colombia

Ponzi scheme	Crash Date	No. Municipalities
DRFE	Nov. 12, 2008	50
DMG	Nov. 15, 2008	50
Gesta Grupo Profesional E.U.	Nov. 19, 2008	1
Inv. Raiz Network Colombia Ltda	Nov. 19, 2008	1
Palabras	Nov. 19, 2008	1
Sociedad Consorcio Preell S.A.	Nov. 19, 2008	1
Costa Caribe	Nov. 22, 2008	6
Global	Nov. 24, 2008	29
Euroacciones	Nov. 25, 2008	43
J & J Clean's Ltda	Nov. 28, 2008	16
H & R	Dec. 1, 2008	1
Tango Trading Ltda	Dec. 16, 2008	1

Source: Authors' own search from primary sources including electronic archives of national and regional newspapers, reports from the Financial Oversight Bureau, and public judicial files on Ponzi cases.

Table 2: Descriptive Statistics: Outcomes

	Ponzi (1)	No Ponzi (2)	Difference (3)	Ponzi (4)	No Ponzi (5)	Difference (6)
		Mugging			**Murder**	
After crash	8.013	2.539	5.474***	3.170	2.363	0.807***
	(11.128)	(7.043)	(0.222)	(4.667)	(6.118)	(0.177)
Before crash	6.413	1.684	4.729***	3.821	2.963	0.858***
	(9.223)	(5.297)	(0.148)	(5.418)	(7.635)	(0.190)
Difference	1.600***	0.855***	**0.745***	−0.651***	−0.600***	**−0.051**
	(0.374)	(0.074)	(**0.392**)	(0.190)	(0.085)	(**0.199**)
		Commercial theft			**Terrorism**	
After crash	2.383	0.850	1.533***	0.076	0.105	-0.029
	(3.437)	(3.561)	(0.105)	(0.630)	(1.216)	(0.035)
Before crash	1.874	0.659	1.215***	0.086	0.081	0.005
	(2.978)	(3.016)	(0.077)	(0.591)	(1.003)	(0.025)
Difference	0.509***	0.191***	**0.318****	-0.010	0.024	**−0.035**
	(0.118)	(0.040)	(**0.143**)	(0.023)	(0.013)	(**0.034**)
		Burglary				
After crash	3.160	1.400	1.760***			
	(4.907)	(5.090)	(0.150)			
Before crash	2.577	1.013	1.564***			
	(4.469)	(4.392)	(0.112)			
Difference	0.583***	0.387***	**0.196**			
	(0.173)	(0.057)	(**0.240**)			

Notes: Variables are rates per 100 thousand people. Source: Colombia's National Police Department. *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.

Table 3: Descriptive Statistics: Controls

Controls	Ponzi (1)	No Ponzi (2)	Difference (3)	Source (4)
Pop. density (people/Km^2)	479.20	104.97	374.23***	DANE
	(1,127.04)	(557.20)	(12.11)	
Distance to the capital (Km)	85.73	134.28	−48.55***	IGAC
	(88.48)	(106.52)	(2.00)	
Total tax rev (million COP per capita)	0.130	0.076	0.054***	DNP
	(0.133)	(0.093)	(0.002)	
Com. tax rev (million COP per capita)	0.044	0.018	0.026***	DNP
	(0.084)	(0.046)	(0.001)	
Poverty index (0 to 100 index)	32.00	46.42	−14.42***	DANE
	(19.50)	(20.28)	(0.38)	
No. microcredit (/10,000 inhabitants)	2,393.08	3,946.23	−1,553.15***	AB
	(2,637.46)	(3,966.89)	(80.21)	
Judicial efficiency (Index)	2.77	1.17	1.60***	FGN
	(5.75)	(2.00)	(0.05)	
Inst. law enforcement (per capita)	0.18	0.24	−0.06***	FS
	(0.13)	(0.23)	(0.004)	
Financial Inst. (per capita)	0.122	0.197	−0.075***	FS
	(0.072)	(0.170)	(0.003)	
Police stations (per capita)	0.11	0.15	−0.04***	FS
	(0.11)	(0.13)	(0.002)	

Notes: See section 4 for details on the variables. DANE (Departamento Administrativo Nacional de Estaística) is the Colombian official statistics agency. IGAC (Instituto Geográfico Agustín Codazzi) is the Colombian official geography agency. DNP (Departamento Nacional de Planeación) stems for National Planning Department. AB (Asobancaria) is the banking association. FS (Fundación Social is a local NGO). *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.

Table 4: Difference-in-Differences Specifications for Selected Outcomes

Dependent variable	(1)	(2)	(3)	(4)
Mugging				
Post crash × Ponzi	0.678*	0.677*	1.319***	0.793**
	(0.346)	(0.351)	(0.334)	(0.362)
R-squared	0.037	0.037	0.036	0.037
Commercial theft				
Post crash × Ponzi	0.296**	0.301**	0.388***	0.301**
	(0.132)	(0.134)	(0.125)	(0.130)
R-squared	0.011	0.011	0.012	0.013
Burglary				
Post crash × Ponzi	0.146	0.179	0.393*	0.158
	(0.206)	(0.211)	(0.201)	(0.249)
R-squared	0.029	0.029	0.030	0.031
Observations	29,708	29,708	29,708	29,708
Municipalities	1,061	1,061	1,061	1,061
Controls	✓	✓	✓	✓
Municipality Fixed Effects		✓	✓	✓
Monthly Fixed Effects			✓	✓
Trend				✓

Note: Ordinary Least Squares regression. Robust standard errors in parenthesis. Sample excludes the four largest cities of the country. Time-varying controls in all specifications include one spatial lag of the outcome (to account for potential spillovers), the total contemporaneous crime level (excluding the outcome) in the municipality, total tax revenues and commerce tax revenues. In addition column 1 includes the following time-invariant controls: population density, distance to the capital of the department and the poverty index. Column 1 also includes the non-interacted Post crash and Ponzi indicators. Column 2 includes only the Post crash indicator. Column 3 includes neither. The differential trend in column 4 is a dummy that captures the 6-months before the crash of Ponzi schemes interacted with the Ponzi indicator. *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.

Table 5: After Matching Balance Tests

	Mean		% Bias	p-val*
	Ponzi	No Ponzi	Reduction	
Mugging				
Lagged mugging	63.75	51.10	74.9	0.17
Neighborhood crime	773.59	930.54	89.1	0.77
Sum of other types of crime	180.49	155.52	69.6	0.09
Commercial theft				
Lagged commercial theft	19.12	16.35	78.1	0.32
Neighborhood crime	175.43	226.78	87.9	0.65
Sum of other types of crime	225.12	199.19	78.4	0.22
Burglary				
Lagged burglary	25.18	21.48	76.5	0.28
Neighborhood crime	215.67	285.19	84.2	0.56
Sum of other types of crime	219.07	194.38	78.9	0.23

Note: Propensity Score Matching using the Mahalanobis metric (see section 3 for details). Matched covariates include the lagged (pre-Ponzi crash) outcome, the (one level) spatial lag of the outcome, and the total contemporaneous crime level (excluding the outcome). * p-value of the after-matching difference in means t-test (null hypothesis is equality of means).

Table 6: Difference-in-Differences on Matched Outcomes

Dependent variable	(1)	(2)	(3)	(4)
Mugging				
Post crash × Ponzi	0.678*	0.671*	1.263***	0.802**
	(0.359)	(0.363)	(0.318)	(0.348)
R-squared	0.059	0.059	0.060	0.061
Observations	15,736	15,736	15,736	15,736
Municipalities	562	562	562	562
Commercial theft				
Post crash × Ponzi	0.254*	0.264*	0.378***	0.303**
	(0.141)	(0.143)	(0.123)	(0.129)
R-squared	0.017	0.017	0.019	0.019
Observations	16,352	16,352	16,352	16,352
Municipalities	584	584	584	584
Burglary				
Post crash × Ponzi	0.101	0.112	0.342*	0.157
	(0.221)	(0.224)	(0.196)	(0.244)
R-squared	0.049	0.049	0.052	0.052
Observations	16,576	16,576	16,576	16,576
Municipalities	592	592	592	592
Controls	✓	✓	✓	✓
Municipality Fixed Effects		✓	✓	✓
Monthly Fixed Effects			✓	✓
Trend				✓

Note: Ordinary Least Squares regression. Robust standard errors in parenthesis. Sample excludes the four largest cities of the country. Time-varying controls in all specifications include one spatial lag of the outcome (to account for potential spillovers), the total contemporaneous crime level (excluding the outcome) in the municipality, total tax revenues and commerce tax revenues. In addition column 1 includes the following time-invariant controls: population density, distance to the capital of the department and the poverty index. Column 1 also includes the non-interacted Post crash and Ponzi indicators. Column 2 includes only the Post crash indicator. Column 3 includes neither. The differential trend in column 4 is a dummy that captures the 6-months before the crash of Ponzi schemes interacted with the Ponzi indicator. *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.

Table 7: Heterogeneous Effects: Credit Rationing

	Mugging		Com. theft		Burglary	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Presence of financial institutions						
Post crash × Ponzi	0.886**	0.231	0.381***	−0.137	0.417*	−1.212
	(0.371)	(0.846)	(0.131)	(0.393)	(0.218)	(0.971)
Observations	10,640	5,096	11,368	4,984	11,256	53,20
Municipalities	380	182	406	178	402	190
R-squared	0.089	0.039	0.031	0.011	0.062	0.046
Access to microcredit						
Post crash × Ponzi	1.458***	−1.500*	0.361**	0.107	0.033	−0.051
	(0.438)	(0.775)	(0.164)	(0.238)	(0.313)	(0.429)
Observations	7,220	4,176	7,836	4,120	7,752	4,456
Municipalities	259	150	281	148	278	160
R-squared	0.148	0.058	0.059	0.016	0.099	0.044
Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Monthly Fixed Effects	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓

Note: Ordinary Least Squares regression. Robust standard errors in parenthesis. Sample excludes the four largest cities of the country. Time-varying controls in all specifications include one spatial lag of the outcome (to account for potential spillovers), the total contemporaneous crime level (excluding the outcome) in the municipality, total tax revenues and commerce tax revenues. All columns include municipality and time fixed effects, as well as a differential trend composed a dummy that captures the 6-months before the crash of Ponzi schemes interacted with the Ponzi indicator. Columns 1, 3 and 5 run the regressions fro the subsample above the mean presence of financial institutions (top panel) and the mean access to microcredit (bottom panel). Columns 2, 4 and 6 focus on the subsamples below the mean. *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.

Table 8: Heterogeneous Effects: Quality of Institutions

	Mugging		Com. theft		Burglary	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Judicial efficiency						
Post crash × Ponzi	1.328**	0.232	0.540***	−0.082	0.021	0.564
	(0.536)	(0.383)	(0.191)	(0.184)	(0.317)	(0.429)
Observations	11,200	3,612	11,312	3,920	11,760	3,948
Municipalities	400	129	404	140	420	141
R-squared	0.066	0.059	0.026	0.013	0.057	0.049
Presence of institutions of law enforcement						
Post crash × Ponzi	1.138***	−0.208	0.300**	0.322	0.229	−0.030
	(0.391)	(0.694)	(0.137)	(0.312)	(0.213)	(0.723)
Observations	10,360	5,376	11,200	5,152	11,172	5,404
Municipalities	370	192	400	184	399	193
R-squared	0.076	0.049	0.027	0.014	0.057	0.049
Presence of police stations						
Post crash × Ponzi	1.020**	−0.110	0.415***	−0.054	0.262	−0.116
	(0.396)	(0.642)	(0.140)	(0.304)	(0.225)	(0.763)
Observations	10,248	5,488	10,556	5,796	10,836	5,740
Municipalities	366	196	377	207	387	205
R-squared	0.086	0.041	0.026	0.014	0.065	0.042
Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Monthly Fixed Effects	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓

Note: Ordinary Least Squares regression. Robust standard errors in parenthesis. Sample excludes the four largest cities of the country. Time-varying controls in all specifications include one spatial lag of the outcome (to account for potential spillovers), the total contemporaneous crime level (excluding the outcome) in the municipality, total tax revenues and commerce tax revenues. All columns include municipality and time fixed effects, as well as a differential trend composed a dummy that captures the 6-months before the crash of Ponzi schemes interacted with the Ponzi indicator. Columns 1, 3 and 5 run the regressions fro the subsample above the mean presence of institutional efficiency (top panel), presence of institutions of law enforcement (medium panel), and presence of police stations (bottom panel). Columns 2, 4 and 6 focus on the subsamples below the mean. *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.

Figure 1: Map of Ponzi schemes' presence

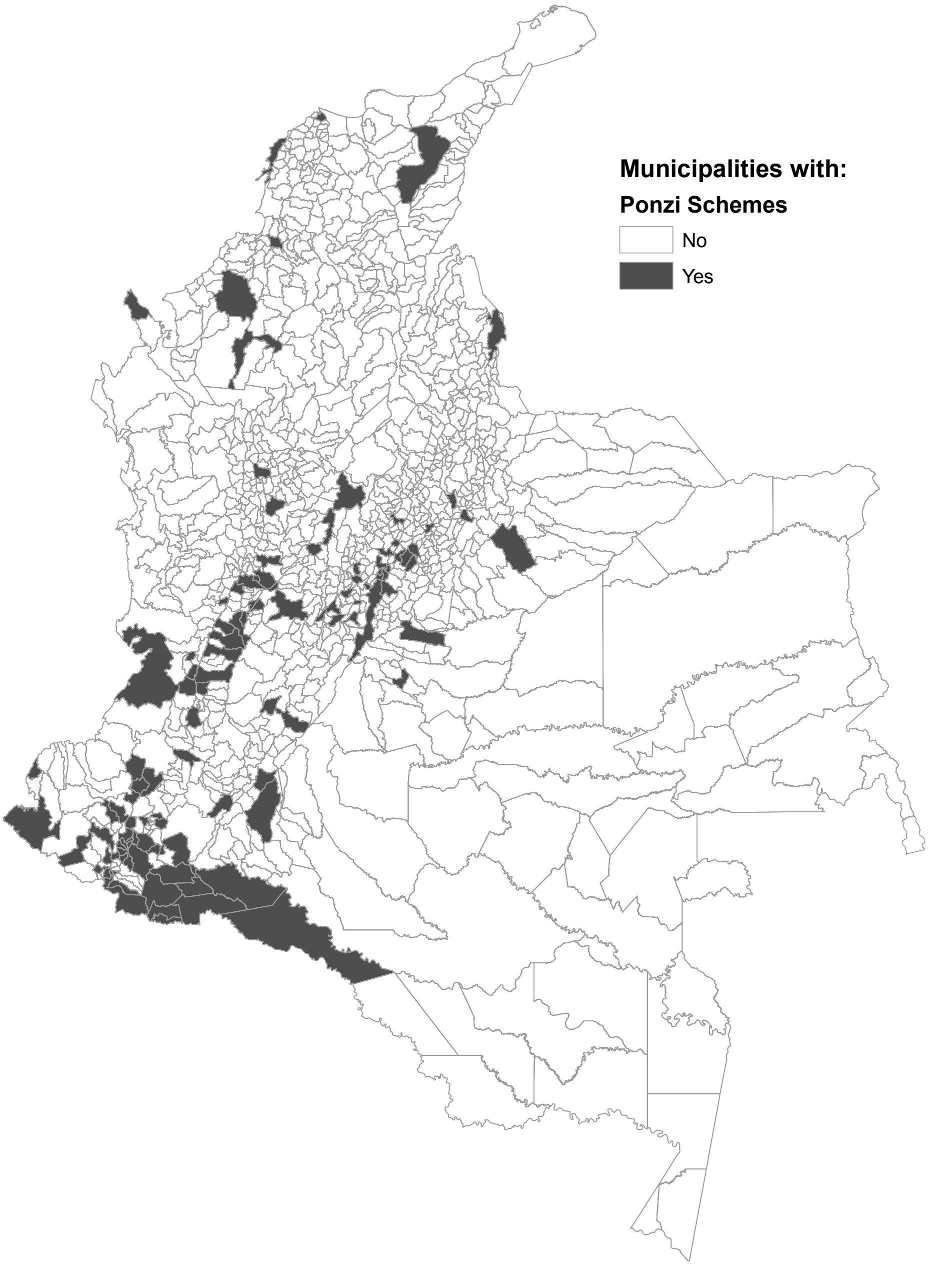


Figure 2: Visual Examination of Parallel Trends

Figure 2.1. Mugging

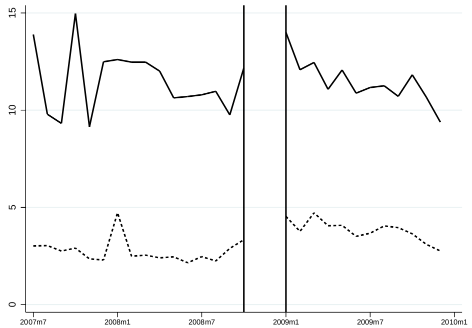


Figure 2.2. Commercial Theft

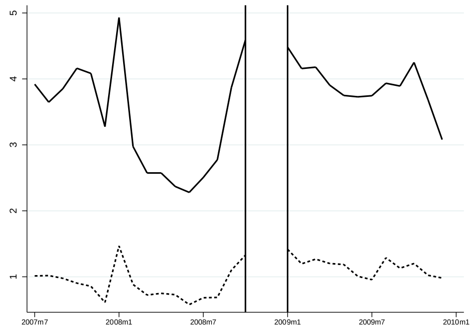


Figure 2.3. Burglary

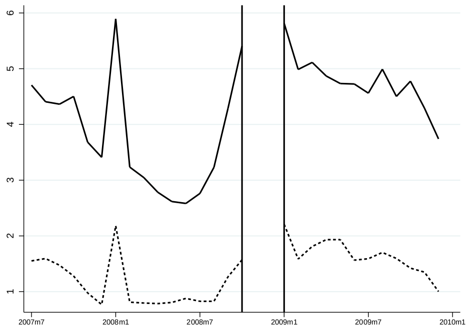


Figure 2.3. Murder

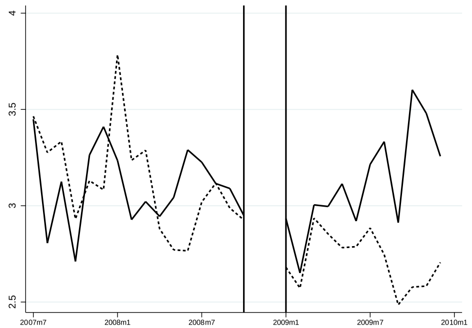
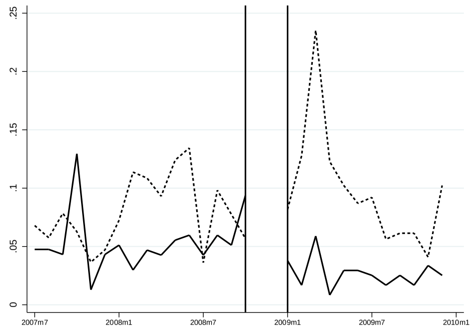


Figure 2.4. Terrorism



Note: Dark lines corresponds to the average crime in Ponzi municipalities and light line to the average crime in no Ponzi municipalities.

Figure 3: Duration of the Criminal Upsurge

Figure 3.1. Mugging

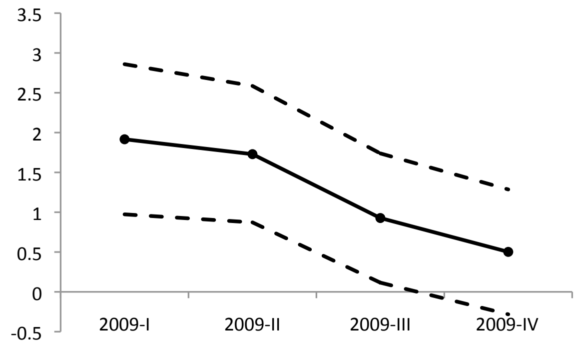


Figure 3.2. Commercial Theft

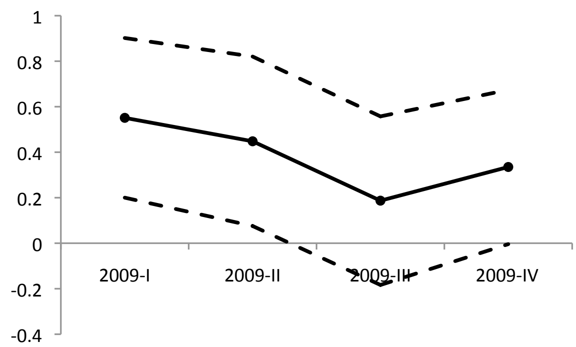
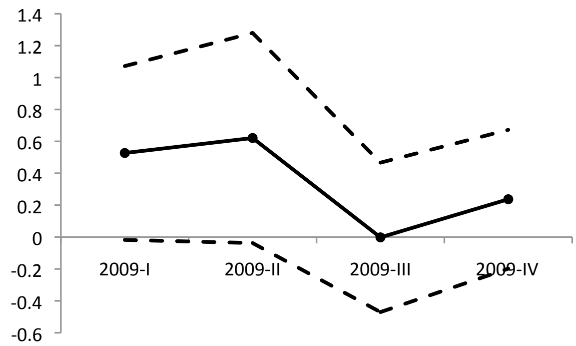


Figure 3.3. Burglary



Note: Confidence Intervals are calculated at 95% of significance using Robust Standard Errors.

Appendix

Table A.1: Additional Robustness: Inference

	Time collapsed (1)	Municipality clustering (2)	Department clustering (3)
Mugging			
Post crash × Ponzi	0.690**	0.802**	0.802*
	(0.312)	(0.348)	(0.481)
R-squared	0.339	0.061	0.061
Observations	1,137	15,736	15,736
Municipalities	569	562	562
Commercial theft			
Post crash × Ponzi	0.259*	0.303**	0.303***
	(0.136)	(0.129)	(0.084)
R-squared	0.143	0.019	0.019
Observations	1,179	16,352	16,352
Municipalities	590	584	584
Burglary			
Post crash × Ponzi	0.071	0.157	0.157
	(0.204)	(0.244)	(0.329)
R-squared	0.279	0.052	0.052
Observations	1,197	16,576	16,576
Municipalities	599	592	592
Controls	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓
Monthly Fixed Effects	✓	✓	✓
Trend	✓	✓	✓

Note: Ordinary Least Squares regression. Sample excludes the four largest cities of the country. Time-varying controls in all specifications include one spatial lag of the outcome (to account for potential spillovers), the total contemporaneous crime level (excluding the outcome) in the municipality, total tax revenues and commerce tax revenues. All columns include municipality and time fixed effects, as well as a differential trend composed a dummy that captures the 6-months before the crash of Ponzi schemes interacted with the Ponzi indicator. *** is significant at the 1% level. ** is significant at the 5% level. * is significant at the 10% level.