# Down and Out in Italian Towns:
# Measuring the Impact of Economic Downturns on Crime

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Abstract

The paper investigates the effect of local economic conditions on crime. The study focuses on Italy's local labor markets and analyzes the short-term response of crime to the severe slump of 2007-2009. It shows that the downturn led to a significant increase in economic-related offenses that do not require particular criminal skills or tools (namely, thefts); on the other hand, for offenses for which specific skills and criminal experience are essential (say, robberies) the impact of the crisis was negative. The results also suggest that: i) labor market institutions (i.e. wage supplementary schemes and pro-worker contractual arrangements) had a role in slowing down the effect of the economy on crime; ii) the link between the downturn and crime was weaker in areas where the presence of organized crime is relatively more intensive.

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*Poverty is what I'm writing about.*
*For, when you are approaching poverty, you make one discovery which outweighs some of the others. You discover boredom and mean complications and the beginnings of hunger, but you also discover the great redeeming feature of poverty: the fact that it annihilates the future.*
*Being a beggar, he said, was not his fault, and he refused either to have any compunction about it or to let it trouble him. He was the enemy of society, and quite ready to take to crime if he saw a good opportunity.*

- George Orwell, *Down and Out in Paris and London* (1933).

## 1. Introduction[1]

The economy is an important determinant of crime. The theoretical point was first made by Becker (1968), who argued that to the extent that criminals are rational agents, dwindling opportunities in the legitimate labor market make crime relatively more attractive. Before Becker's rationalization, the link between economic difficulties and criminal offenses was, in addition to being fairly widely recognized among social scientists, a source of extraordinary inspiration for novelists: George Orwell's autobiographic description of life in extreme poverty in Paris and London in the 1920s, quoted above, is an enlightening example. In Italy, following the severe financial and economic crisis of 2007-2009, this rationale appears to be shared by many. For instance, a survey[2] of local entrepreneurs residing in the richest region of the country (Lombardy) reveals that over 90% of them think that safety has declined following the downturn. Conversely, official data shows that during the 2000s criminality in Italy continued to decline (ISTAT, 2010), despite a surge in the social perception of insecurity. Therefore, a simple analysis of time trends could lead to misleading conclusions.

This paper assesses the causal impact of local economic downturns on short-term variations in criminal activity. Of course, this is not the first study to explore the link between the

[1] Our thanks to Roberto Galbiati, Giovanni Mastrobuoni, Paolo Pinotti, the participants at the "Bank of Italy-DONDENA" workshop (Bank of Italy, Rome, July 2011) and the seminar "*Criminalità, economia e politiche per la sicurezza in Italia*" (Bocconi University, Milan, December 2012). The views expressed here are those of the authors and do not necessarily correspond to those of the Institutions with which they are affiliated.
[2] See: http://tuttocamera.mb.camcom.it/upload/repos/stampa/3/1889/consigliocrimin.pdf.

economy and illegal activities.[3] However, our analysis extends the contributions of the previous literature in a number of different directions. First, it concentrates on recent developments – namely, an economic crisis of unprecedented gravity since the Great Depression. Since the international crisis in Italy has brought about not only cross-time volatility, but also high levels of sectoral and geographical variability in economic activity, it allows us to control for year fixed effects and to exploit exogenous sectoral shocks, while retaining meaningful variation in the variables of interest. This is particularly important, as the effect of economic downturns on local criminal activity needs to be isolated from many other confounding factors.[4] Second, compared with previous work, our study incorporates a number of technical advancements. For instance, it employs a spatial classification that is more suited to the accurate measurement of the link between the state of the local economy and crime. In addition, the data we refer to on criminal records are those used by the police department for investigation/punishment purposes. These data are very detailed as to the types of crime reported and are less contaminated by underreporting. They allow us to estimate the short-term impact of the slowdown on crime, which is interesting in terms of policy implications – it might help the authorities to counteract illegality more promptly – but nevertheless should be considered a conservative estimate if longer-term effects (for instance, due to increasing returns of crime or spillover effects) materialize.

Our results point to the slump having a significant effect on those criminal activities to which individuals can easily turn when their economic conditions worsen. For instance, a 1% decrease in local economic activity results in a 0.6% increase in thefts (*furti*) and a 1.0% surge in extortions (*estorsioni*). On the contrary, the impact of the crisis on economic-related offences that require some crime-specific human and physical capital, such as robberies (*rapine*), is negative. We also find that the slowdown does not have any effect on non-economic-related crimes, such as murders, assaults or sexual crimes. Our findings highlight that labor market institutions matter: the impact on easy-to-commit economic-related offences is higher when the local economy features a higher share of SMEs – which have limited access to wage supplementary schemes – and a higher share of youngsters – who have more flexible labor contacts (i.e. are easier to fire). Finally, we find a lower impact of

[3] A comprehensive review of the literature can be found in the Handbook on the Economics of Crime (Benson and Zimmerman, 2010). In that volume, Mustard (2010) lists the papers of the new generation (people he associates with the last ten years of research) that use identification strategies explicitly to deal with causality issues.

[4] For instance, a Collective Clemency Bill passed by the Italian Parliament in July 2006, which led to the early release of a large share of detainees.

the slump for regions where organized crime is pervasive. We interpret this last finding as suggesting that organized crime acts as an illegal monopolist, raising barriers to potential new entrants.

The paper is structured as follows: Section 2 summarizes the relevant literature; Section 3 reports on the data and the estimation strategy; Section 4 documents the results; Section 5 concludes.

## 2. Related literature

In economic models of crime such as Becker's (1968), a declining economy provides higher incentives for individuals to commit offenses. As underscored by Machin and Meghir (2004) and Edmark (2005), among others, this prediction is likely to be truer for crimes with direct financial motivation, such as thefts, but less important for non-economic-related crimes (such as homicides and rapes) that are affected by a completely different set of determinants. By the same token, some financially motivated offenses might be suitable alternatives to legitimate income opportunities, while others might require some specific criminal expertise that is difficult to accumulate in the short term.

As explained by Mustard (2010), the empirical results fail to show consistent evidence in favor of the link between slowdowns and illegality. One possible reason is that there could be countervailing forces: a region with lower incomes may provide greater incentives for people to substitute the legal labor market with the illegal one, but might also have fewer resources available for criminals to steal. Furthermore, the number of "victimless" crimes, like drug dealing or gambling, tends to respond to the demand of "customers", which may well decrease during economic downturns (Freeman, 1999). As Levitt (2004, p. 170) puts it "to the extent that activities that are associated with increased levels of either offending or victimization are normal goods – like alcohol consumption, frequenting nightclubs, and owing a car – the link between economic activity and crime is theoretically ambiguous."

The failure could, however, also be related to pitfalls in empirical strategies. In his survey, Mustard (2010) argues that the empirical literature has only recently started to tackle seriously the threats to identification that might arise. Omitted variables, reverse causation and measurement issues can be jointly tackled with IV strategies. This is the approach taken by, among others, Mehlum et al (2006) and Gould et al (2002). A different problem is related

to the spatial unit of reference. Freeman (1995) and Levitt (2001) take issue with the chance to learn from data at the national level. As variation in crime and economic activity is greater within rather than across states, research that uses smaller areas of analysis, like cities or neighborhoods, might provide clearer estimates of causal impacts (see Glaeser et al, 1996 and Glaeser and Sacerdote, 1999). Finally, it should be noted that the recent literature has started to consider proxies for measuring legitimate opportunities other than the traditional unemployment rate (see, for instance, Machir and Meghin, 2004) while the link between the economy and crime in the context of a pervasive presence of criminal organizations has been little investigated (with the notable exception of Bonanno, 2006).

## 3. Identification strategy, data and empirical issues

The impact of the economy on crime is analyzed using the following simple specification:

$$(1) \qquad CRIME_{i,t.r} = \alpha + \beta\, ECONACT_{i,t} + \Sigma_n (\delta_n X_{i,t}) + \gamma_i + \rho_{rt} + \varepsilon_{i,t,r}$$

where i indexes spatial units, t years, and r regions; ECONACT is a proxy of local (private) economic activity in the area[5] and $X_{i,t}$ indicates time-varying covariates at the chosen level of geographic aggregation. We have a (2004-2009) panel with area ($\gamma_i$) and time-region ($\rho_{rt}$) fixed effects.[6] Therefore, identification comes from within-area overtime contemporary correlation between economic conditions and crime. All the variables we use are taken in logs.[7] Thus, the coefficient $\beta$ represents the elasticity of crime to economic conditions.

A few aspects of our empirical analysis are worth highlighting.

3.1 *Exceptional circumstances*. The study exploits a severe episode of crisis. The contraction in economic activity during the period 2007-2009 was the greatest since the Second World War (see: Jenkins et al., 2011) and was accompanied by a remarkable upsurge in the incidence of poverty: in 2010, around 18% of Italians were at risk of poverty, while 16% of households were experiencing great difficulties in paying monthly bills (ISTAT,

[5] Since we include time dummies, the measure of economic activities is left in nominal terms.
[6] This differentiates out trends in economic activities at the region level. We also show results from specifications including only year dummies.
[7] For crime events, we add one to all observations in order to keep zeros in the sample.

2011). We find a similar setting in the empirical literature in Edmark (2005), who exploits a period of unprecedented difficulties in Sweden, between 1988 and 1999, and in Bignon et al. (2011), who exploit the Phylloxera crisis as an exogenous source of variation in agricultural income across French departments from 1826 to 1936. If the relation between the intensity of the slump and crime is non-linear (perhaps, because an individual starts to consider committing criminal acts only after exceeding a certain threshold of economic difficulties) we might be also capturing these types of extreme impacts. On the other hand, these exceptional circumstances, as mentioned above, allow us to exploit the large within-year and -region variation in economic activity, leading to a cleaner identification of the causal effect.[8]

3.2 *The spatial units of reference*. The analysis employs a more suitable spatial classification than the bulk of the previous work on the subject. The geographic units of reference are local labor markets (LLM), functional areas which approximate the spatial extension of labor market interactions.[9] According to Mustard (2010), the use of data at the local level delivers substantial benefits in the empirical strategy, as "national or state-level data mask much of the important variation that is needed to identify causation" (Mustard, 2010; p. 25). Note also that the choice of areas defined on the basis of self-containment should help minimize measurement, reverse causality and omitted variable problems that are frequent in regressions using units defined by administrative borders (see: Briant et al, 2010).[10] Even more importantly, local labor markets seem to reflect appropriately the state of

[8] Note, however, that since they are derived from an extraordinary episode, the external validity of our results might be low.

[9] Local labor markets are defined by the Italian National Institute of Statistics (Istat, 1997). They are aggregations of two or more neighboring municipalities based on daily commuting flows from place of residence to place of work as recorded in the 2001 Population Census. Local labor markets are thus largely "self-contained": within a given unit, both the share of working residents working locally and the share of employees residing locally must be at least 75%. This definition is consistent with the notion of "functional region", defined as "a territorial unit resulting from the organization of social and economic relations in that its boundaries do not reflect geographical particularities or historical events" (OECD, 2002). Italian local labor markets also roughly follow the criteria used to define Metropolitan Statistical Areas in the US, Travel to Work Areas in the UK, or Metropolitan areas and employment areas in France. Italian local labor markets span the entire national territory. In 2001, 686 of them were defined. They had an average population of 83,084 and a standard deviation of 222,418.

[10] In the literature of urban economics and regional science the choice of the appropriate spatial unit of reference comes under the heading of "modifiable area unit problem" (MAUP). Indeed, unlike in international comparisons (where country borders have a clear economic and political meaning) in a sub-national setting the shape and size of the spatial unit of interest often present many different options. However, it has been shown that economic estimates may present huge variations across different spatial classifications (Gehlke and Biehl, 1934; Openshaw and Taylor, 1979; Arbia, 1989; Menon, 2012). It is also worth noting that spatial units which maximize (across all viable alternatives) the self-containment of the phenomena under scrutiny (e.g. crime or unemployment) also minimize the spatial autocorrelation of the residuals, eventually improving the quality of the estimates. If, instead, the adopted spatial units poorly match the economic spatial dimension of the studied

the economy in a country where many shocks are localized, e.g., due to the widespread diffusion of highly-specialized industrial districts. Finally, using areas defined on the basis of labor market interactions seems to be the best way of answering our research question, i.e. the degree to which criminal activities substitute legal labor market opportunities.

Figure 1 maps the cumulative 2007-2009 (the years of severe contraction) variation for our proxy of LLM economic conditions (see below). Note that a territorially heterogeneous pattern prevails: LLMs with the highest contractions coexist with LLMs with no contraction at all, even within the same region.

3.3 *Better data on crime*. The study takes advantage of a *unique* dataset on criminal facts. The dataset we use, known as the "Sistema di Indagine" (Investigation System or IS), supplements the information collected in victim reports with that on criminal facts that the police department collects in its day-by-day investigation activity.[11] Therefore, the data we use should be considered as less spoiled by the underreporting issue that plagues the empirical analysis of crime. As the data are gathered for investigation/punishment purposes, it is more likely that the timing of the crime documented in the dataset accurately reflects the true timing of the offense (and this is clearly an important element for our study that uses overtime variation).[12] Along with the LLM and year (by region) fixed effects present in all our specifications (which absorb the local and temporal component of underreporting: see Bianchi et al., forthcoming), the good quality of the data should guarantee that there is no systematic measurement error in our dependent variables.

IS criminal facts are available at the municipality-level for the years 2004-2009, and include 34 sub-categories of crime. Among these, we focus on: thefts, extortions and robberies (for economic-related crimes) and on murders, assaults and sexual crimes (for the non-economic-related ones). Figure 2 maps the 2007-2009 cumulative variations for the IS-recorded thefts at the LLM level.

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phenomena, this adds a systematic correlation component to the error terms, leading to inefficiency or even inconsistency (Anselin, 1988).

[11] IS data are confidential. They were made available to the staff of the Bank of Italy within a joint Bank of Italy-Ministry of Interior project on crime and the economy.

[12] IS data are thus very similar to the most commonly used source in the US, which is the Federal Bureau of Investigation's Uniform Crime Reports (UCRs). In fact, the UCR also collects data from reports compiled by law enforcement officials.

3.4 *Proxies for the local economic outlook.* The adopted measure for the local state of the economy is the sum of the sales (*fatturato*) of all plants (belonging to private firms) located in the LLM. The variable is taken from the commercial archive Cerved, containing detailed balance sheet information on all Italian corporations. Using plant-level data is an innovation in the literature, which focuses on labor market statistics, and allows us to perform the analysis on a more suitable spatial scale. On the other hand, labor force survey data at such a fine-grained level (the LLM) are available only with large confidence intervals, due to the small number of individuals surveyed in a given locality; as a consequence, they are of little use in a regression analysis.

Compared to labor force survey data, our balance sheet data are *less* informative with reference to the labor status of the residing population. However, our data are *more* informative with respect to other aspects. For instance, labor force survey data do not capture labor market deterioration that does not lead to unemployment (i.e. resulting only in wage losses with diminishing worked hours).[13] Moreover, the standard unemployment rate fails to consider non-participation issues, which have been shown to be extremely relevant and dependent on economic conditions (Bank of Italy, 2010).[14] According to Mustard (2010), in order to find significant impacts on crime the measures of opportunities in the legitimate labor market should be more ample that those referring to unemployment only. Furthermore, studies that disentangle the effect of wages and unemployment tend to find that the former has a greater effect on crime (Gould *et al.*, 2002; Machin and Meghir, 2004)

Even though we consider firm data, the economic mechanism behind our results is essentially a labor market one. We provide some evidence in this regard (see below, par. 3.7), showing that in those LLMs where labor market channels are going to slow down the impact of the recession (basically, the availability of wage supplementary schemes and pro-worker contractual arrangements) the estimated impact on crime is lower.

[13] This may be particularly important in Italy, where the network of "family insurance" often replaces a formal system of unemployment benefits (Saraceno, 1994). As a consequence, the shrinking of a single wage may have major repercussions on the well-being of several related individuals.

[14] The similarity between Cerved data on private sector sales and labor force survey information can be checked at a higher level of geographical aggregation. Using NUTS3 (provinces) as the spatial unit of reference, we find a negative relation (with a correlation coefficient of around 30%) between the two series over the 2004-2009 period.

3.5 *Short-term responses*. Given the data availability, we can estimate only the short-term impact of the slowdown. Our results should be basically read as yearly contemporaneous effects of the economy on crime. That is, we do not focus on longer-term dynamics. This does not imply that those are not important: they could materialize if criminal activities have some cumulative effect (once someone starts committing crimes, net returns increase because the costs of investing in criminal-specific skills are sunk) or if there are spillover effects (committing crimes leads to additional crimes in the area). However, the short-run impact is exactly what policy makers and public opinion have in mind when they worry about the effect of a slowdown on crime.[15]

3.6 *Threats to identification*. The estimated overtime contemporaneous correlation between the local economic outlook and crime might be flawed due to the usual drawbacks related to omitted variables, reverse causality, and measurement error in the independent variables. Since we control for LLM fixed effects and exploit LLM overtime variability, omitted candidates should be LLM-level time-varying factors that can drive the observed correlations (in our 6-year interval). Note that in the empirical exercises below (see Section 4) we control for native and foreign LLM populations. These controls take care of potential omissions related to city size. Moreover, we allow each control to have a different confounding impact on crime.[16] However, we are aware that the list of potential omissions is never complete. For instance, if local authorities assume the existence of a link between local economic conditions and crime, they might decide to react to a deterioration in local wellbeing with an increase in the resources targeted to fight crime, and this would lead to underestimation. However, it is also possible that overestimation will prevail, if allocations are reduced as the crisis dents local public finances. Another example refers to reporting bias (Levitt, 1998). To the extent that the downturn increases/decreases the likelihood of a victim reporting the crime, or the police department accurately completing the IS files, upward/downward biases could materialize. By the same token, the possibility that local variations in crime deter economic activity (reverse causality), while quite implausible in the context of a short-term severe crisis episode, cannot be ruled out. Finally, measurement error in the independent variable might also be a relevant source of bias. This is because local

[15] Clearly, there could also be no impact. For instance, given that the individual penalties for committing crimes (incarceration) might have very long-lasting effects, it could be that a short-term variation in the state of the economy is not enough to push people into illegality.

[16] This is probably an unnecessary precaution, since previous literature has shown that there is no causal impact of the growth of non-native population on crime: see Bianchi et al., forthcoming).

economic conditions are likely to be measured with substantial error, due to the fact that our sources are financial statement data referring only to private corporations, and are therefore significantly different from the ideal variable we would have derived from economic theory (rather suggesting a proxy for local labor market conditions).

All these identification challenges can be tackled with a valid instrument, that is, a variable correlated with our endogenous variables (the proxy for local economic conditions) but uncorrelated with the crime variables for reasons beyond its effect on the endogenous repressor. We use a shift-share instrument for economic activity: the sum of the contemporaneous nationwide employment variation by sector, weighted by the sector share in LLM employment in 2001:

$$(2) \qquad INST_{i,t} = \Sigma_k \left( \ln \left( emp_{n,k,t} / emp_{n,k,t-1} \right) * share_{i,k,T} \right)$$

where *j* refers to LLMs, *t* to year, *T* to the year 2001, *n* to the nationwide value, and *k* to sectors.

The instrumental variable is a derivation of the well-established shift-share approach introduced by Bartik (1991) and Blanchard and Katz (1992), and used in a number of leading papers afterwards, including Moretti (2010). In the literature on crime, variants of this instrument have been used by Gould et al. (2002) and Fougere et al. (2009). The validity of the instrument relies on the fact that national shocks to individual sectors affect the local economies proportionally to the employment shares of those sectors in total employment. On the other hand, both the national shocks and the lagged industry shares can be considered to be exogenous to LLM changes in employment over time. An example may be useful to clarify the intuition behind the identification strategy: the impact of the downturn was higher for sectors open to international trade (since the international crisis was also a dramatic world trade crisis: see WTO, 2009). Economic activity in those sectors declined drastically and LLMs which were highly-specialized exporting sectors were the worst affected. Thus, those LLMs experienced a more intense adverse shock for reasons that can be considered independent from any other time-varying factor affecting the local economy.

As with all the instruments, the validity of our IV strategy relies on the plausibility of the exclusion restrictions. In this specific case, one crucial assumption is that both the sectoral

composition of the local economy in year 2001 and the nationwide shocks do not have any independent effect on the LHS variable. The independence of the nationwide shocks to local conditions is reassuringly plausible, once year (by region) dummies are included in the regression. Sectoral composition in year 2001, however, can be determined by previous local unobserved trends over the 1990s which may extend to the 2000s, eventually leading to spurious serial correlation between the instrumental variable and the incidence of crime.

Unlike in other studies focusing on a longer time period, the short period of analysis (only 5 years) reinforces the exogeneity of the lagged sectoral composition (as the mean effect of the unobserved trend is absorbed by the longitudinal fixed effect). Furthermore, all our results hold if we restrict the period of analysis to the 2007-2009 triennium, for which endogeneity concerns should be even less relevant (given that most of the variability originates from the crisis, which was an unexpected shock). However, in order to dissolve further doubts about the strength of the identification strategy, we design a number of falsification tests. For instance, we build a "placebo" endogenous variable which is identical to our main regressor (economic activity at LLM level) but is lagged 6 years, thus referring to the 1998-2003 period. We then regress the crime variables on the 6-years lagged economic activity (plus the other controls and the fixed effects). In the result sections, we show that the coefficient of the lagged endogenous variable is very close to zero and not significant. We also replicate the first stage regression, substituting the endogenous variable with the lagged one (restricted to the period 1998-2001) in order to check whether first-stage results are driven by serial correlation in growth rates, rather than by the exogenous sectoral shocks. Again, we will show that the first-stage coefficients turn out to be insignificant, thus supporting the validity of the exclusion restrictions.

On more technical grounds, our IV estimates are going to capture the impact of the slowdown on crime for those who are on the edge of switching from the legal to the illegal sector. They are the *compliers*, in the local average treatment effect (LATE) interpretation of IV (Imbens and Angrist, 1994). This is a desirable feature of our estimates, as correlations among average measures of crime and economic conditions might be biased by relevant composition effect (see the discussion in Mustard, 2010).

3.7 *Additional instruments based on labor market buffers*. The identification strategy discussed so far relates a private sector measure of economic activity with crime indicators.

However, the economic mechanism behind our results is essentially a labor market one: are those who suffer more from the downturn (either because they became unemployed or their income was cut, or they live in a family where one member has lost their job or experienced income loss; see Mocetti et al., 2010) more likely to commit crimes? As we discussed above, labor market data are not reliable at the level of spatial detail used here. However, to make some progress in this respect, we can still try to embed labor market channels in our identification strategy. In particular, we elaborate on two institutional aspects of Italy's labor market:

i. Small vs. big firms: in Italy, firms with fewer than 15 employees can fire workers more easily than larger companies; on the other hand, the latter have preferential access to the main welfare scheme available for workers in firms experiencing a shortage in demand i.e. the wage supplementary scheme (*Cassa Integrazione Guadagni - CIG*).[17] A parallel mechanism acting in the same direction relates to financial resources. It is well known that SMEs are more credit-constrained, in Italy as elsewhere. As a direct implication, large firms may be able to retain their workforce longer, since greater financial resources allow them to smooth the short-term impact of the downturn. Therefore, (exogenous) variations in economic activity are likely to have a more severe impact on local labor markets with a lower share of larger firms. Below we complement our main results with IV estimations obtained by using INSTBIG as the instrument, that is INST (equation 2) multiplied by the LLM share of larger firms in 2004 (derived from Cerved data).[18] In so doing, the correlation of the instrument with the endogenous variable is higher for the sub-group of LLMs with a higher share of larger firms. The LATE interpretation of IV would suggest that INSTBIG identifies the average impact of the downturn for those who are less exposed to the deterioration because they work for a firm covered under the wage supplementary scheme. Therefore,

[17] The CIG supplements the wages of workers who are temporarily dismissed by firms who meet a number of requirements. The wage supplementary scheme covers workers employed by small firms less widely. In particular, it does not cover workers in firms fewer than 15 employees. However, in the aftermath of the 2009 international crisis, the Italian government introduced an extraordinary extension of the CIG to firms that were not covered by the scheme.

[18] Since the coverage of employment data in Cerved is incomplete, we identify firms with fewer than 15 employees using a turnover threshold, estimated using information on employment where available. As a result, larger firms are defined as those with a total turnover of more than €4.5 million.

compared with the IV estimation of β that makes use of INT we should obtain lower point estimates.[19]

ii. Italy's cohorts of workers differ substantially by virtue of their contractual arrangements. Basically, older cohorts benefit from contracts that make it difficult (and very costly) for a firm to interrupt the work relation. By contrast, younger cohorts have very flexible agreements, which make interruptions easier (e.g. short-term or consultancy contracts). Again, we calculate a new instrument (INSTYOUNG), as INST multiplied by the LLM share of young workers in the total workforce (derived from ISTAT data). The LATE interpretation of IV would suggest that IV estimates of β obtained by using INSTYOUNG as an instrument should be higher than those obtained by using INST (and even higher again than those obtained using INSTBIG).

## 4. Empirical results

This section presents the results of our empirical analysis.

Table 1 reports the mean values of the measure of economic activities (both at per-capita level and yearly variations), of foreigners over the total population, and of the number of different categories of crimes per 1,000 inhabitants. As we can see, while in 2009 the effect of the international crisis on economic activity was clearly evident, the intensity of crime was slowly decreasing overall. Casual empiricism would therefore fail to recognize a link between the deterioration of economic conditions and a surge in criminal activity.

Tables 2 and 3 present the results we obtain by using as a dependent variable the LLM number of thefts reported in the IS.[20] The only difference across the two tables relates to the time-varying fixed effects: Table 2 includes only year fixed effects, while Table 3 includes year-region fixed effects. OLS estimates suggest a negative (and statistically significant)

[19] A similar methodology was proposed by Ichino and Winter-Ebmer (1999). As compared with the more traditional sample split, this approach has two advantages: it does not require an arbitrary split threshold, and it does not limit the size of the sample, which is particularly helpful with 2SLS estimations.

[20] We also run regressions using a slightly different specification for the dependent variables. We take the local number of crimes divided by the population (at the same time, we do not include population measures on the LHS). The results of these experiments, not reported but available from the authors, are very similar to those illustrated below.

correlation between the local economic outlook and this measure of economic-related crimes. The magnitude of the effect, however, seems to be quite low (a 1% decrease in economic activity is associated with an increase of 0.03% in thefts). IV results confirm the negative impact of the economic downturn on robberies. The estimated effect is now substantially larger, with an estimated elasticity of 0.4% when the time shocks are assumed to be common across the whole country and of 0.6% when the time shocks are allowed to be region specific. The latter coefficient implies a sizable impact:[21] it would suggest, for instance, that moving from the LLM at the 25$^{th}$ percentile of the distribution of the variation in economic conditions in year 2009 to the LLM at the 75$^{th}$ percentile leads to a decrease in stealing amounting to around 9%.[22] The circumstance that IV estimates are higher than their OLS counterparts suggest that among the sources of bias, those that deliver attenuation (such as the measurement error) are likely to play a prominent role. Note also that we find a rather strong first-stage relationship between the instrument and the overtime change in economic activity. Finally, the estimates provided by using INSTBIG and INSTYOUNG fall in a range that is coherent with the idea that labor market institutions have a role in slowing down the effect of the economy on crime. In fact, wage supplementary schemes act as a buffer that tends to hinder the propagation of the economic deterioration to the crime sphere, and so does the existence of pro-worker contractual arrangements.

Table 4 provides the evidence for another economic-related criminal activity: extortions. Extortions differ from thefts because the former include an interaction between the criminal and the victim, while this is not necessarily the case for the latter. Certainly, the act of extortion implies that the victim is persuaded by a credible intimidation to hand over a good (for instance, a sum of money). Compared to thefts, extortions might then require more crime-specific human and physical capital. Therefore, they might be less appropriate than thefts to represent short-run criminal alternatives for those who act out of necessity. On the other hand, someone not from a criminal *mileau* might be in a better position to find affluent and defenseless victims for extortions. As shown in the table, also in this case the IV estimates are substantially higher that the OLS ones. For extortion, moving from the LLM at the 25$^{th}$ percentile of the distribution of the variation in economic conditions to the LLM at

[21] To ease comparisons across tables, we only comment on the point estimates from specifications that include region-specific trends.
[22] Between 2008 and 2009, the economic activity of the LLM at the 25$^{th}$ percentile shrunk by 18%, while those of the LLM at the 75$^{th}$ percentile contracted by 3%. The difference between the two growth rates is therefore 15%.

the 75th percentile of that distribution in year 2009 leads to a decrease of around 15%. The percentage variation, however, has to be scaled to the low frequency of this kind of crime: according to our data, there are four extortions every 1,000 thefts. The results also suggest that there is no differential impact when using INSTBIG and INSTYOUNG (the differences in the coefficients are not statistically significant). Given the nature of the violation, this is not surprising: compared to thefts, the labor market channel should be less evident.

Table 5 looks at robberies. This crime represents an even less suitable alternative for those who are experiencing economic difficulties. Legally, robberies are identified as offenses for which there is a physical act of violence committed by the criminal against the victim (in most cases involving weapons). Unlike extortions, the goods targeted are not delivered by the victims but are taken away from them violently. As reported in the table, the impact of a local economic downturn on the number of robberies is very different to those identified for thefts and extortions: the LLMs experiencing less economic difficulties are those where the increase in robberies is higher. One tentative way to interpret these findings is to note that robberies might be practiced by criminal organizations, which may be very mobile over the national territory and more likely to target relatively affluent LLMs. Furthermore, rational criminals may reduce their activity during an economic downturn, as the expected income is lower, while the risk of being caught is unaffected. However, we do not want to push this interpretation too far, as our approach allows us to estimate local impacts and it is less suitable to assessing the effects of the downturn that go beyond the boundaries of the LLM.

Table 6 provides the results for non-economic-related crimes. These experiments can be seen as placebos intended to corroborate the previous findings. First, some violent crimes are arguably weakly dependent on short-term economic conditions, not being related to a condition of economic disability; second, violent crimes, such as murders or sexual abuses, are generally associated with psychological characteristics and skills (e.g. using a weapon) which are unlikely to be developed in the short term. Therefore, if we found that our measure of economic activity is also related to crimes for which no link is expected, we should suspect that we are mistakenly capturing something else (for instance, a surge in crime due to cultural factors that have nothing to do with the concomitant downturn). This does not appear to be the case. Our placebos find that the economy does not have any role in murders, violent and sexual crimes.

Table 7 reports some robustness tests. For the sake of brevity, we only report the results for thefts (however, by using the other crime outcomes the findings are very similar). A first concern relates to the timing of the effect of the downturn on crime. Indeed, if the deterioration of the local economy were affecting crime with some delay, a lagged measure of economic activity would be an omitted variable in our specification. Therefore, the first column shows the estimate of the effect of the one-year lag of the economic activity variable on the number of thefts. The related coefficient is not significant. The possibility that our estimates might reflect some dynamics referring to the period before 2004 is also ruled out in the second column, where we regress our crime outcome on a measure of lagged (1998-2003) economic activity and fail to find any significant correlation. In the third column, we show that the instrument is not correlated with economic trends before the year 2001, suggesting that the strong first-stage relationship is not due to correlation of the long-run trend with the industrial structure in year 2001. To further check the instrument's exogeneity, in the fourth column of the table we replace our instrumental variable with one built using the share in 1991, rather than in 2001. As the table shows, the results are almost identical and only marginally less precise. Finally, in the last column we present the results obtained by using the 2007-2009 window only: the results are unchanged.

Finally, in Table 8 we split our sample of 678 LLMs into 232 LLMs belonging to regions in which the control of the territory by organized crime is relatively more intense (Campania, Sicilia, Calabria, Puglia) and the remaining 446.[23] Where organized crime is pervasive, we find no impact of the slump on economic-related crimes. The lack of impact could be due to the fact that the weight of the public sector in organized crime regions is more significant than elsewhere: therefore the public sector might act as a buffer that reduces the impact of the slump on criminal activity. However, this does not seem to be case: by excluding those LLMs with the highest share of public sector (i.e., the top 25% of LLMs with the highest employment share in public administration, education, and health) the results do not differ from those presented in the table. Therefore, we suspect that a better explanation is that organized crime acts as an illegal monopolist that raises substantial entry barriers: in areas controlled by the Mafia, Camorra or 'Ndrangheta it is difficult (and dangerous) to turn to illegality when the reserve value of legal opportunities decreases.

[23] Although the activity of criminal organizations is widespread throughout Italy, including richer regions in the North, it is generally recognized that their pervasive control of the territory is relatively more powerful in their traditional settings i.e. in the regions of Sicily, Campania, Calabria and Puglia. For a detailed analysis of the territorial distribution of criminal organization in Italian regions see Pinotti, 2012.

## 5. Conclusions

As underscored in the *Handbook on the Economics of Crime* (Benson and Zimmerman, 2010), there is a gap between the theoretical claim according to which economic conditions have an impact on crime and the empirical evidence in favor of the claim. By exploiting the severe slump of 2007-2009 and making use of a number of technical advancements (i.e. highly-disaggregated spatial units, more reliable data on crime, an identification strategy that is suited to deal with threats to causality) this paper helps fill that gap. It finds that in Italy's local labor markets, economic-related crimes – such as thefts and extortions – significantly increased as an effect of the downturn. Interestingly, previous empirical studies for Italy focusing on different spatial units, data, and time periods, produce rather different results (e.g. Buonanno, 2006).

The idea that the slump is putting safety at risk also seems to be a pressing concern for the authorities. For instance, in Italy local government representatives (*Prefetti*) in charge of crime protection have been authorized to monitor local economic patterns to detect early warning signs of deterioration that should help to predict surges in crime requiring action. Our estimates might therefore be useful for the fight against crime. They highlight that the (short-term) impact of the slowdown is likely to materialize through an increase of economic-related crimes. Thus, they also suggest that national and local authorities should add criminality to the long list of social problems that should be tackled during an economic crisis. More generally, our work underscores the social and collective costs of local economic downturns, even in a high-income country like Italy.

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**Table 1. Descriptive statistics – economic activity and number of crimes per 1,000 inhabitants**

| Year | ECONACT (% change) | | ECONACT/POP | | Foreigners/POP | | Thefts | | Assaults | | Robberies | | Murders | | Extortions | | Sexual crimes | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. |
| 2004 | NA | NA | 12725 | 13375 | 0.03 | 0.02 | 14.53 | 9.30 | 1.08 | 0.51 | 0.26 | 0.40 | 0.04 | 0.06 | 0.08 | 0.08 | 0.07 | 0.07 |
| 2005 | 0.08 | 0.23 | 13531 | 14194 | 0.04 | 0.03 | 14.68 | 9.33 | 1.14 | 0.51 | 0.26 | 0.40 | 0.04 | 0.07 | 0.08 | 0.09 | 0.07 | 0.07 |
| 2006 | 0.12 | 0.23 | 15031 | 15623 | 0.04 | 0.03 | 15.36 | 9.68 | 1.19 | 0.55 | 0.26 | 0.40 | 0.03 | 0.05 | 0.08 | 0.09 | 0.07 | 0.07 |
| 2007 | 0.10 | 0.18 | 16323 | 16944 | 0.05 | 0.03 | 15.90 | 10.21 | 1.27 | 0.55 | 0.28 | 0.41 | 0.03 | 0.05 | 0.10 | 0.10 | 0.08 | 0.08 |
| 2008 | 0.01 | 0.18 | 16255 | 17465 | 0.05 | 0.04 | 14.27 | 8.85 | 1.31 | 0.57 | 0.27 | 0.37 | 0.04 | 0.06 | 0.09 | 0.09 | 0.08 | 0.09 |
| 2009 | -0.09 | 0.19 | 14263 | 15235 | 0.06 | 0.04 | 13.72 | 8.68 | 1.33 | 0.59 | 0.21 | 0.29 | 0.03 | 0.05 | 0.09 | 0.09 | 0.08 | 0.08 |

Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 2. The effect of local economic activity on thefts**

| Estimation method | OLS | IV (INST) | IV (INSTBIG) | IV (INSTYOUNG) |
|---|---|---|---|---|
| Dependent variable | | Number of thefts | | |
| | | **2nd stage** | | |
| ECONACT | -0.039** | -0.356** | -0.254** | -0.463*** |
| | (0.016) | (0.150) | (0.127) | (0.174) |
| NATIVES | -0.319 | -0.268 | -0.284 | -0.251 |
| | (0.224) | (0.241) | (0.232) | (0.253) |
| FOREIGNERS | 0.179*** | 0.263*** | 0.236*** | 0.291*** |
| | (0.037) | (0.058) | (0.052) | (0.064) |
| | | **1st stage** | | |
| INST | | 3.763*** | | |
| | | (1.005) | | |
| INSTBIG | | | 12.960*** | |
| | | | (3.298) | |
| INSTYOUNG | | | | 7.082*** |
| | | | | (1.596) |
| F-stat excl. instr. | | 14.04 | 19.70 | 15.45 |
| YEAR FE | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES |
| REG.*YEAR FE | NO | NO | NO | NO |
| Observations | 4,094 | 4,094 | 4,094 | 4,094 |

Notes: The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 3. The effect of local economic activity on thefts (region by year FEs included)**

| Estimation method | OLS | IV (INST) | IV (INSTBIG) | IV (INSTYOUNG) |
|---|---|---|---|---|
| Dependent variable | | Number of thefts | | |
| | | **2nd stage** | | |
| ECONACT | -0.034** | -0.628*** | -0.474*** | -0.750*** |
| | (0.016) | (0.214) | (0.178) | (0.239) |
| NATIVES | -0.485* | -0.543 | -0.528* | -0.555 |
| | (0.278) | (0.350) | (0.321) | (0.377) |
| FOREIGNERS | 0.129*** | 0.278*** | 0.239*** | 0.309*** |
| | (0.041) | (0.080) | (0.071) | (0.088) |
| | | **1st stage** | | |
| INST | | 3.700*** | | |
| | | (1.004) | | |
| INSTBIG | | | 6.952*** | |
| | | | (1.829) | |
| INSTYOUNG | | | | 13.435*** |
| | | | | (3.530) |
| F-stat excl. instr. | | 13.59 | 14.45 | 14.49 |
| YEAR FE | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES |
| REG.*YEAR FE | YES | YES | YES | YES |
| Observations | 4,094 | 4,094 | 4,094 | 4,094 |

Notes: The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 4. The effect of local economic activity on extortions**

| Estimation method | OLS | IV (INST) | IV (INSTBIG) | IV (INSTYOUNG) |
|---|---|---|---|---|
| Dependent variable | | Number of extortions | | |
| ECONACT | 0.013 | -1.015* | -1.180** | -1.151** |
| | (0.035) | (0.528) | (0.514) | (0.588) |
| NATIVES | -1.429** | -1.529*** | -1.545** | -1.542** |
| | (0.541) | (0.666) | (0.700) | (0.694) |
| FOREINERS | 0.055 | 0.313* | 0.354** | 0.347** |
| | (0.087) | (0.161) | (0.168) | (0.176) |
| YEAR FE | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES |
| REG.*YEAR FE | YES | YES | YES | YES |
| Observations | 4,094 | 4,094 | 4,094 | 4,094 |

Notes: The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 5. The effect of local economic activity on robberies**

| Estimation method | OLS | IV (INST) | IV (INSTBIG) | IV (INSTYOUNG) |
|---|---|---|---|---|
| Dependent variable | | Number of robberies | | |
| ECONACT | -0.020 | 1.104** | 1.055** | 0.930** |
| | (0.042) | (0.478) | (0.428) | (0.471) |
| NATIVES | -0.726 | -0.617 | -0.622 | -0.634 |
| | (0.514) | (0.622) | (0.615) | (0.593) |
| FOREINERS | 0.278*** | -0.003 | 0.009 | 0.040 |
| | (0.084) | (0.155) | (0.147) | (0.151) |
| YEAR FE | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES |
| REG.*YEAR FE | YES | YES | YES | YES |
| Observations | 4,094 | 4,094 | 4,094 | 4,094 |

Notes: The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 6. The effect of local economic activity on non-economic-related crimes**

| Estimation method | OLS | IV | OLS | IV | OLS | IV |
|---|---|---|---|---|---|---|
| Dependent variable | No. of murders | | No. of assaults | | No. of sexual crimes | |
| ECONACT | -0.049 | 0.140 | -0.011 | -0.023 | -0.008 | -0.669 |
| | (0.032) | (0.431) | (0.029) | (0.351) | (0.033) | (0.502) |
| YEAR FE | YES | YES | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES | YES | YES |
| REG.*YEAR FE | YES | YES | YES | YES | YES | YES |
| Observations | 4,094 | 4,094 | 4,094 | 4,094 | 4,094 | 4,094 |

Notes: All specifications include NATIVES and FOREIGNERS. First-stage results are reported in Table 3. The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 7. Robustness**

| Estimation method | Thefts | Thefts | ECONACT lagged (1998-2001) | Thefts | Thefts |
|---|---|---|---|---|---|
| Dependent variable | OLS | OLS | OLS | IV | IV |
| ECONACT | -0.029* | | | -0.571** | -0.325*** |
| | (0.018) | | | (0.223) | (0.124) |
| ECONACT LAGGED (1 year) | -0.006 | | | | |
| | (0.022) | | | | |
| ECONACT LAGGED (1998-2003) | | -0.021 | | | |
| | | (0.017) | | | |
| INST | | | 0.918 | | |
| | | | (1.098) | | |
| F-stat excl. instr | - | - | - | 14.06 | 31.10 |
| YEAR FE | YES | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES | YES |
| REG.*YEAR FE | YES | YES | YES | YES | YES |
| Observations | 4,076 | 4,076 | 2,714 | 4,094 | 2,042 |

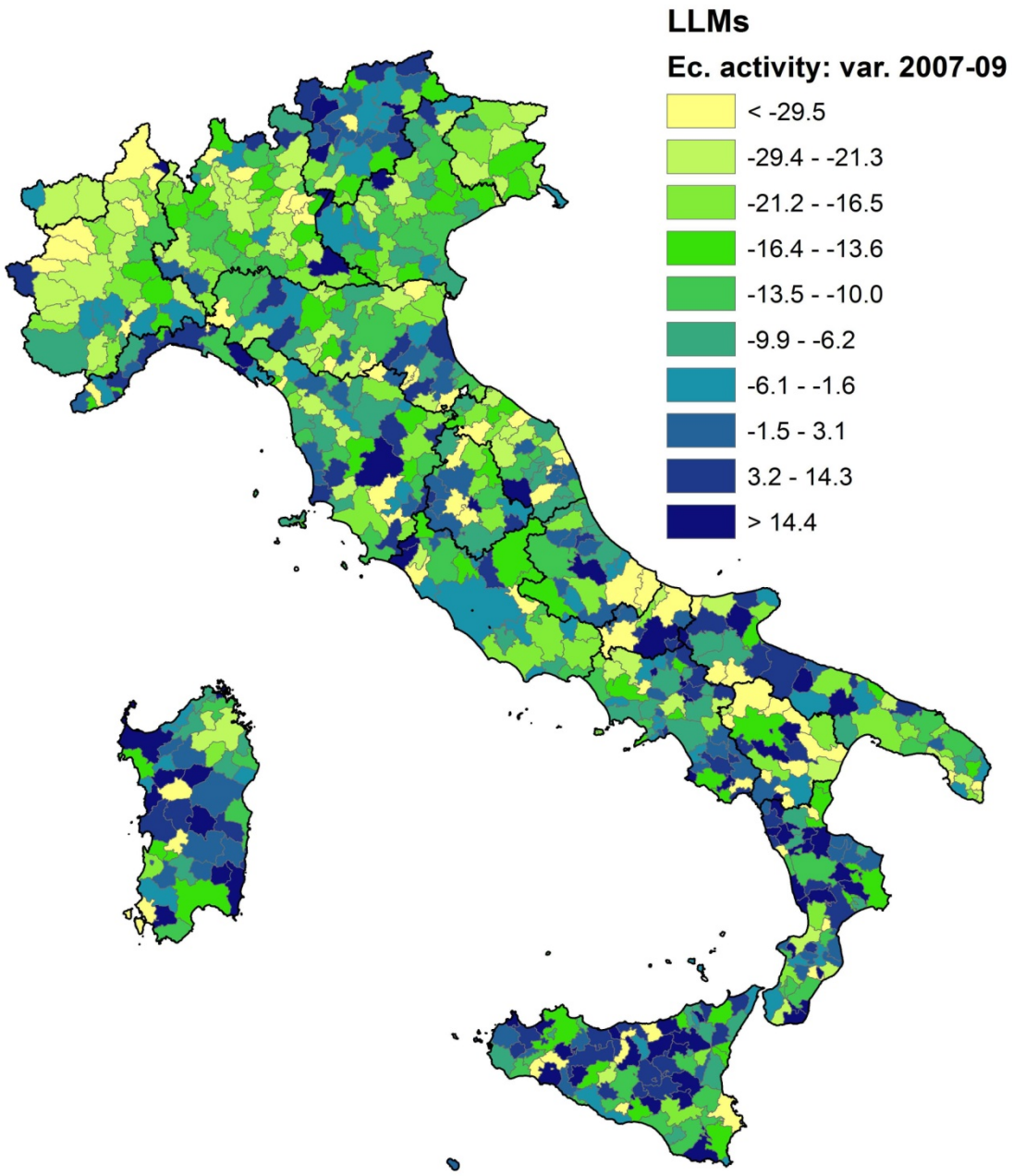
Notes: All specifications include NATIVES and FOREIGNERS. In the fourth column, the instrument is referred to 1991. In the fifth column, the estimation sample is limited to the period between 2007 and 2009. The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Table 8. The effect of local economic activity on thefts and extortions: region splits**

| Sample | Organized crime regions | | Other regions | |
|---|---|---|---|---|
| Estimation method | OLS | IV | OLS | IV |
| | | Dependent variable: thefts | | |
| ECONACT | 0.035 | 0.180 | -0.062*** | -0.910*** |
| | (0.039) | (0.318) | (0.016) | (0.313) |
| | | Dependent variable: extortions | | |
| ECONACT | -0.019 | -0.421 | 0.027 | -1.167* |
| | (0.084) | (0.846) | (0.035) | (0.659) |
| | | Dependent variable: robberies | | |
| ECONACT | 0.085 | 0.585 | -0.068 | 1.304** |
| | (0.062) | (0.721) | (0.043) | (0.623) |
| F-stat excl. instr | - | 6.193 | - | 8.875 |
| YEAR FE | YES | YES | YES | YES |
| LLM FE | YES | YES | YES | YES |
| REG.*YEAR FE | YES | YES | YES | YES |
| Observations | 1,384 | 1,384 | 2,710 | 2,710 |

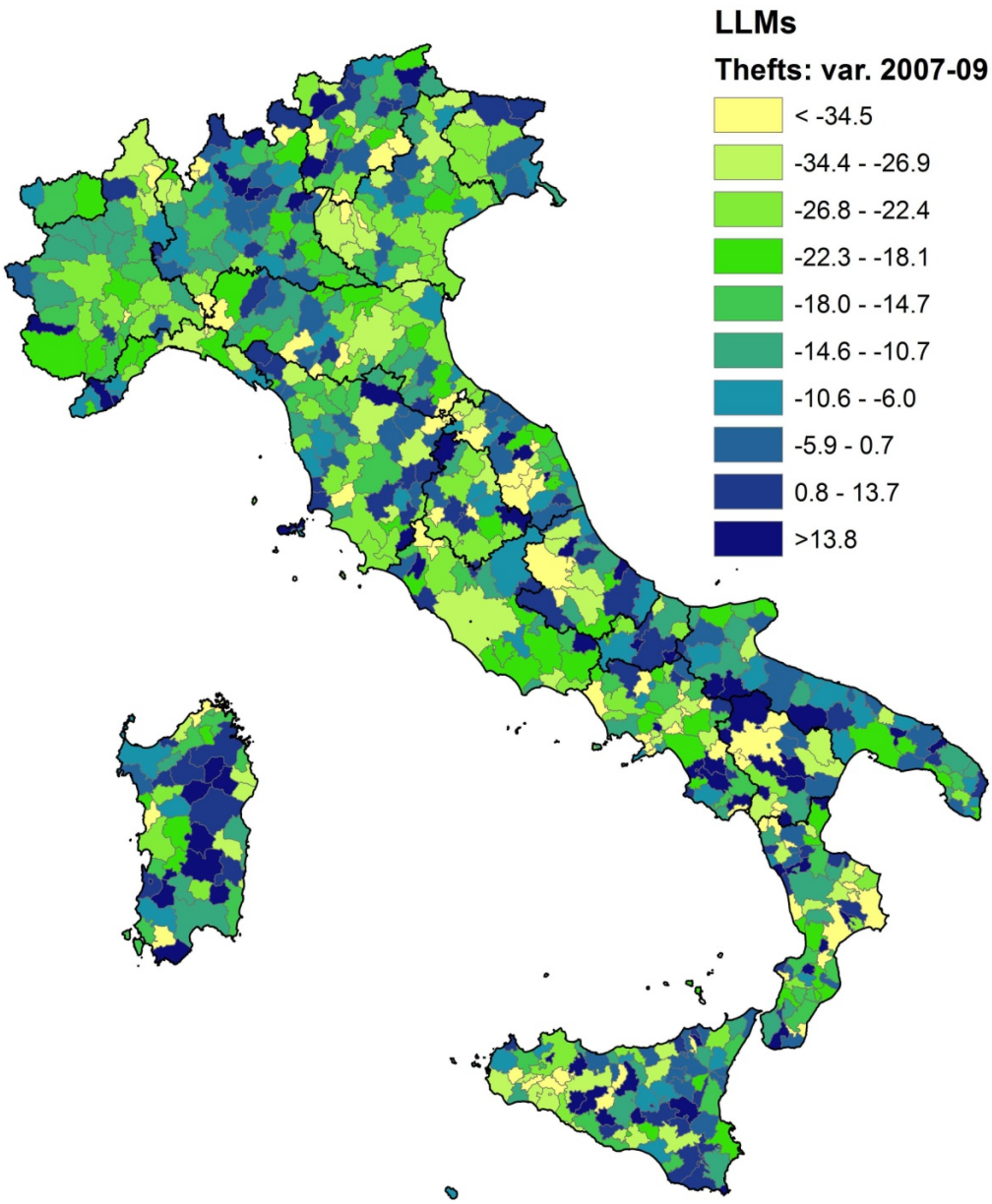
Notes: The unit of observation is the Local Labour Market (LLM). All variable are in logs. Robust standard errors clustered at LLM level in parenthesis. *** significant at 1% ** significant at 5% * significant at 10%.
Source: Authors' elaborations on ISTAT, CERVED, and IS database

**Figure 1. Percentage variation in economic activity, 2007-2009**




Notes: The map shows the percentage variation in our measure of economic activity across Italian LLMs between 2007 and 2009. Black lines identify regional borders.
Source: Authors' elaborations on ISTAT, CERVED, and IS database.

**Figure 2. Percentage variation in thefts, 2007-2009**




Notes: The map shows the percentage variation in thefts across Italian LLMs between the year 2007 and 2009. Black lines identify regional borders.
Source: Authors' elaborations on ISTAT, CERVED, and IS database.