Police and Crime: Evidence from Dictated Delays in Centralized Police Hiring

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Abstract

This paper exploits dictated delays in local police hiring by a centralized national authority to break the simultaneity between police and crime. In Italy police officers can only be hired through lengthy national public contests that the Parliament, then the President, and finally the Court of Auditors need to approve. On average 3 years pass between the legislative approval and the actual recruiting. We use *positive changes* in the number of police officers that are driven by public contests to identify the elasticity of crime with respect to police. The availability of data on two police forces that specialize in fighting different crimes provides convincing counterfactual evidence on the robustness of our results. Despite the apparently inefficient hiring system, regular Italian police forces seem to be almost as efficient in fighting crimes as the US ones, with two notable exceptions: auto thefts and burglaries.

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1 Introduction

Becker (1968)'s seminal work formalizes the intuition that increased enforcement might deter criminals from committing crimes.[1] Despite these unambiguous predictions it has been extremely hard to find empirical evidence about the causal relationship between police enforcement and crime. The main empirical issue is the endogeneity of police forces with respect to crime rates: governments tend to increase police enforcement in regions that have higher crime levels (Fisher and Nagin, 1978). This endogeneity is so strong that the correlation between the local number of police officers and local crime rates tends to be positive.[2]

Fisher and Nagin (1978)'s critique to the existing literature and the availability of richer data led to the use of more sophisticated econometric technics to break the simultaneity between crime and police. Marvell and Moody (1996) use twenty years of US state and city data to show that more police Granger causes crime rates to fall.[3] Levitt (1997) uses gubernatorial elections in 59 major US cities to instrument changes in police enforcement and finds that an increase of 10% in police forces implied a reduction 3 to 10% in crime rates.[4] More recently, Evans and Owens (2007) use *centralized federal* hiring grants that change the size of a police force, but are unrelated to past and (presumably) future changes in the crime rate, to instrument for the number of police officers. Their estimates are similar to the ones found in Levitt (1997, 2002).

Our identification strategy is also based on a centralized policy, but with some distinctions. We exploit documented delays in a nationwide centralized hiring system of police forces to estimate their deterrence effect on crime. We use data on police and crime in

[1] Almost two-hundred years earlier Cesare Beccaria had written that certain and severe punishments can deter people from committing crimes (Beccaria, 1786).

[2] Cameron (1988)'s survey of the literature reports that in 18 out of the 22 studies that try to estimate the relationship between the level of police and crime rates either find no relationship or a positive one.

[3] A variable "Granger causes" another when changes in the first variable precede changes in the second.

[4] In response to McCrary (2002) who challenged these results due to some coding errors, Levitt (2002) uses the number of fire-fighters and other municipal workers as instruments for the number of police officers and finds evidence of large deterrence effects.

Italian regions over the period 1980-1997. As we widely discuss in the next section, Italy displays very interesting features for the purpose of our analysis. In those years every police officer was hired through a centralized public contest (*concorso pubblico*). The parliament would sign a law establishing the total number of allowed hirings, over the following 2 to 3 years. In section 2 we document that it would take at least 3 years before these hirings were implemented.

This centralized hiring methods generates sharp exogenous increases in the number of police officers across time and across regions. We show that consistent with the documented long delays only the third lag in the level of crime predicts local changes in police forces. Negative changes instead are not centrally governed, and are consequently less likely to be exogenous. The elasticity of crime with respect to police is indeed asymmetric around 0: the elasticity is close to 0 for negative changes in police and positive and significantly smaller than 0 for positive changes. The estimated elasticity is around -12 percent. We also propose an instrumental variables approach to identify the elasticity at positive thresholds that are not necessary 0, and the results are robust to using larger thresholds.

Our identification strategies has some advantages over the one based on the COPS program analyzed in Evans and Owens (2007) i) local police offices do not need to apply to hire more officers; ii) several years pass between the centralized financial decision to increase the number of police forces and the actual hiring making it less likely that hirings depend on expected changes in crime rates; the relative changes in police forces that are driven by our centralized hiring system tend to be larger.[5]

Another set of papers have tried to solve the reverse causality problem between crime and police relying on small scale experiments or quasi-experiments: following a quasi-randomized experiment different districts in Kansas city received different levels of patrolling **?** finds little evidence of deterrent effect of police on crime rates. Exploiting

[5]Only 1 to 2 percent of police officers receive a COPS grant

a peculiar feature in the hiring process in New York Corman and Mocan (2000) find a deterrent effect of police on crime rates. Di Tella and Schargrodsky (2004), instead, show that after terrorists' attacks reinforced police deployment around mosques and temples in Buenos Aires generates sudden reductions in crime. While these papers certainly use clever identifying variations the external validity of their results might be limited.

Finally, this is the first paper to identify the causal effect of police enforcement on crime using Italian data.

2 Mass Hirings and the Data

Law n. 121 of 1981 rules the present organization of Italy's law enforcement. There are a total of five police forces: *Polizia di Stato*, *Carabinieri*,[6] *Guardia di Finanza*, *Polizia Penitenziaria* and *Corpo Forestale dello Stato*. *Polizia di Stato* and *Carabinieri* are responsible for maintaining public security and keeping public order, while *Guardia di Finanza*, which, as the name suggests, fights financial or white collar crimes, frauds, and smugglings. The *Polizia Penitenziaria* is responsible for security and surveillance in Italian prisons, while *Corpo Forestale dello Stato* officers are park ranger force responsible for protecting Italy's natural resources, the environment, countryside and ecosystems, especially national parks and national forests. Both these forces are not interesting for the focus of the present study.

Polizia di Stato and *Carabinieri* have identical functions. *Polizia di Stato* is a civil force that depends on the Minister of Interior, while *Arma dei Carabinieri* is a military force (gendarmerie) that depends on the Minister of Defense. Historically, *Carabinieri* was created by King Victor Emmanuel I of Savoy with the aim of providing the Kingdom of Sardinia with a police corps. After Italian reunification in 1861 the Carabinieri were appointed the "First Force" of the new national military organization. The *Polizia di Stato*

[6] *Carabinieri* is the shortened (and common) name for the *Arma dei Carabinieri*

was established in 1852 (they were called *Corpo delle Guardie di Pubblica Sicurezza*) and subsequently merged to the *Carabinieri* in 1922. In 1925 the Fascist regime decided that the ministry of interior had to oversee the *Polizia*. For our analyses we are going to focus on the *Polizia* and the *Carabinieri*, but given the crime specialization the *Guardia di Finanza* provides a reasonable placebo test.

As briefly discussed in Section 1, police officers are hired nationally through centralized public contests.[7]. The law establishes the procedure that need to be followed to hire new police officers. This generates a considerable time lag between the time the law gets approved and the time the newly recruited police officers become operational. Let us briefly describe how the procedure works and later provide a specific example to clarify the extent of these delays[8]. Bills need to be discussed in both chambers of the parliament, which typically requires around one year. Once approved, the law needs to be signed by the President of the Republic and, every time the law needs at least some funding, to be approved by the Corte dei Conti (the Italian Court of Auditor). In order to become effective, the law must be published in the Gazzetta Ufficiale (G.U.). For example, in 1986 the Minister of Interior started the procedure to hire 3,000 police officers. The decree was approved by the Corte dei Conti January 9, 1987 and then published on the G.U. on March 3, 1987. Once published, potential candidates need to apply for the position within one month. The oral and written examinations took place one year later, on February 23 and 24, 1988. On average after six months candidates are notified about the results of the examination. Successful candidates must complete a one-year training course (Law n.121/1981). Thus, on average new police officers become effective 3 years after the approval of the law. This hiring procedure introduces a significant and sizable lag, that might help breaking the simultaneity between police and crime. In addition, in many cases the laws itself established the year in which new police officers should be

[7]Hiring procedures were set in 1981 (art. 47 and 48 of Law n. 121). This law was later modified in 1982 (Decree Law n. 335) and in 2000 (Decree Law n. 234)

[8]A complete list of laws is presented in Table 1

hired.[9]

Since the hiring system is centralized, new police officers need to be allocated to regional police offices. The rules that govern these allocations are not transparent and publicly available. In order to understand the allocation process we contacted police officers that were enrolled during the time considered in our analysis. A new police officer at the end of his/her training course had to express up to three preferences for his/her geographical destination. However, the Minister of Interior could allocate police officers disregarding these preferences, following a detailed manning schedule (or *pianta organica*) that determines the number of police officers each province and city should have.[10]

In order to exploiting the peculiarities of this hiring system we use a balanced, yearly, regional panel over the period that goes from 1980 to 1997. Our main explanatory variable is the total number of *Carabinieri* and *Polizia di Stato* force, in short "police force."[11] Crime data are taken from the official crime statistics that are recorded by the police and are published yearly by the Italian Statistics Institute (ISTAT) at region level and by type of crime. For the purpose of our analysis we consider: robbery, murder, assault, burglary, car theft, bag snatching, larceny, fraud and smuggling.

We also collected a set of socioeconomic and demographic variables that are usually included in crime regressions. We include the percentage of men aged 15-35. Young men are said to be more prone to engage in criminal activities than the rest of the population (Freeman, 1991, Grogger, 1998). Turning to the socioeconomic variables, we include the (log of) real GDP per capita and the unemployment rate which measure the legitimate and illegitimate earning opportunities (Ehrlich, 1973, Gould et al., 2002, Raphael and Winter-Ember, 2001). We complete our dataset by including education measures: the percentage of population with high school diploma, and the percentage

[9]For example, Law n. 410/1985 established how many of the 8,800 new *Carabinieri* had to be hired in each year up until 1989.

[10]We found out that a ministerial decree published on March 16, 1989 changed the preexisting schedule, but even the police labor unions have no access to these schedules.

[11]These data have been used by Marselli and Vannini (1997), and provided to us by the authors.

of population with university degree.[12] Our list of control variables is likely to be incomplete. In order to control for unobserved factors we exploit the panel structure of our data either differencing the data or including region-specific fixed effects. We also include year dummies in order to adjust for exogenous shocks in crime rates that are common to all regions.

Table 2 presents the summary statistics of the variables used in our analysis. Over the period 1980 to 1997 there were on a yearly basis an average of close to 3,000 crimes per 100,000 inhabitants. It clearly emerges that the vast majority of recorded crime are property crimes. Italian crime rates differ from US ones in many ways. Having in mind that crime categories may not be perfectly comparable due to significant differences in the judicial system, crime rates in Italy are significantly lower than in the United States. While for our empirical analysis we use crime data for a panel of 59 U.S. large cities from 1970 to 1992[13], to compare the Italian crime rates to the US ones we use the 1995 Uniform Crime Reports. For instance, both property crimes and violent crimes seem less frequent in Italy (respectively 1,880 versus 4,590 and 96 versus 684). Because of their specific nature some types of crimes allow a better comparison, like murders and motor vehicle thefts. Nevertheless, even for these crimes these differences remain: Italy had on average 1.87 murders per 100,000 inhabitants, the US 8.2. The differences are lower for motor vehicle thefts: 324 per 100,000 inhabitants in Italy versus 560 in the US.

[12]Education may have a sort of "civilization" effect reducing crime over and above its effect through labor market opportunities (Buonanno and Leonida, 2009, Fajnzylber et al., 2002, Lochner and Moretti, 2004).

[13]See Levitt (1997) for a complete and detailed description of the data

3 Empirical Evidence

3.1 Evidence on endogeneity in the US and Italy

The identification strategy to estimate the effect police enforcement has on crime–overcoming the endogeneity of police enforcement–, is to exploit Italy's previously discussed centralized decision making in police hiring practices, coupled with the documented massive delays between the passing of the national parliamentary hiring laws and the actual hiring. We start with some visual evidence about the endogeneity that we might call "Italian style."

Figure 1 shows the average total number of crimes and total number of police forces across Italian regions, with (right panel) and without (left panel) controlling for year and region fixed effects. The raw data clearly shows that over time crime and police move together, at least until 1991. The correlation between the residual of crime levels and police levels after controlling for region and year fixed effects is instead negative (right panel). One possible interpretation of these patterns is that Italy presents an endogeneity issue when analyzing the country as a whole (more police officers are hired when crime goes up), but that centralism and delays in the allocation of police forces across space make the endogeneity issue fade away once we control for time effects.

Next we show that the same is not true in the US, where police forces are notoriously hired at a more local level (Levitt, 2002, McCrary, 2002). In the first column of Table 3 we simply regress the logarithm of total crime rates on the logarithm of the number of police forces per 100,000 inhabitants. The elasticity is clearly positive, significant, and large (75 percent). Controlling for year fixed effects reduces the elasticity by one half, but the elasticity keeps on being positive and significant. Columns 3 and 4 show that even controlling for potential confounders does not eliminate the endogeneity of police, while columns 5 and 6 shows that first-differencing the data does not prevent the elasticity from being positive and significant. What this means is that for US cities the evidence is that local police hiring depends on local changes in crime rates. Fixed effects panel data estimates would thus be unable to identify the effect of police on crime, requiring the use of alternative strategies, for example instrumental variables (Levitt, 2002), or regression discontinuities (Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005).

In Table 4 we show, in line with Figure 1, that Italy presents some similarities but also striking differences with respect to the US. Column 1 shows that the raw elasticity between police and crime is only slightly lower than in the US (50 versus 75 percent). Controlling for year and region fixed effects the similarities end. Column 2 shows that adding the fixed effects the elasticity goes from positive 0.50 to negative 0.24 percent. Remember that for the US cities after controlling for the same fixed effects the elasticity would be smaller but would still be positive. Column 4 shows that controlling for additional confounders does not alter this result, while column 5 and 6 show that first-differencing the data lowers the elasticity from -22.6 to -12.5 percent. This difference might be due to measurement error in the number of police officers. Since we don't have a way to assess the importance of measurement error bias we are going to benchmark our results to the -12.5 percent figure. It is also worth noting that after first-differencing the data all the other independent variables stop being significant, while police forces don't.

3.2 Identification based on mass hirings

We just showed that differencing the data alleviates the endogeneity issue, but how do we know that it solves the issue altogether? We devise a test for whether the whole endogeneity bias has been eliminated. It rests on a comparison between the OLS estimate (-12.5 percent) and estimates based only on positive variations in the number of police officers, the ones that are driven by national mass hirings. Given that each hiring is based on national contests, one way to identify the elasticity is to separately estimate the effect of police on crime depending on whether the change in police is positive, thus due to mass hirings or negative, thus more likely to be endogenous. Empirically it is enough to separately control for positive and negative changes in police forces in our crime regressions.

Another and more flexible way to identify elasticities based on thresholds is to use local instrumental variables. The estimation proceeds in two steps. In step 1 we identify the region-year observations that show positive increases in police forces, or increases that are larger than a given threshold, and in step 2 we instrument changes in the (log) number of police forces with the subset of large increases in the (log) number of police forces. The intuition for the test can be easily grasped looking at the scatterplot shown in Figure 2. We simulated data where police generally responds to increasing crime rates by hiring additional police forces (the endogeneity issue). This corresponds to the main scatter plot. Then there are some years where some regions get large relative increases in the number of police forces that generate relative reductions in crime equal to 50 percent. This corresponds to the scatter plot that is downward sloping. A linear regression would generate a flat prediction due to the typical positive relationship between crime and police forces. Instead, when an instrumental variable strategy is used where changes in police forces are instrumented using a changes that are larger than a given threshold (in the figure we use the largest 1 percent of the increase in police forces) the estimated slope is very close to the true one.

The first two columns of Table 5 show the OLS results as a benchmark. In columns 3 and 4 we split the changes in (*log*) police forces depending on the sign of these changes. A clear discontinuity emerges: positive changes induce a negative and significant elasticity of 15.4 percent. Negative changes instead lead to an elasticity that is not significantly different from zero.

Columns 5 to 10 present the IV results using three different thresholds: 0, 5, and 10 percent. We instrument changes in the log number of police officers $\Delta \log P_{rt}$ with just the positive changes $\Delta \log P_{rt} \times 1(\Delta \log P_{rt} > \delta)$ varying the thresholds δ. In Table 6 we show the distribution across years of the different thresholds $1(\Delta \log P_{rt}) \gtrless \delta$ used in Table 5 (the lower thresholds) and later in the "placebo" Table 8 (the upper thresholds). The lower thresholds have be chosen to be reasonably large and thus able to capture changes in police staffing that is driven by mass hirings, and large enough to provide a reasonable amount of variation. There are still 6 percent of observations with changes above 10 percent. The first 3 columns of the Table show that there is variability both across time and across space in mass hirings, defined as an increase in police forces of at least 0, 5, or 10 percent. 1987 stands out as the year where most regions (100 percent) had a positive increase in the number of police officers. In that same year 63 percent of regions increased the number of police officers by more than 10 percent. Notice that these changes happen 2 years after the "hiring" law number 150, signed on April 19, 1985, while in 1985 and in 1986 none of the regions show an increase of at least 5 percent. This is direct evidence of the time that passes between the signing of the law and the actual hiring of the recruits.

Going back to Table 5, comparing the OLS with the corresponding IV columns it is clear that the IV estimates tend to be larger in absolute value, but that overall the differences are negligible: -12.5 versus -15 percent. While we cannot completely rule out that some simultaneity bias might persist, the relatively small difference between the OLS estimates and the IV ones suggests that such bias would be small.

3.3 Elasticities for different crimes and crime categories

Analyzing just the total number of crimes that get reported to the police might hide heterogeneity in the elasticities across crime categories. We categorized crimes to render them comparable to the uniform crime reports used in Levitt (1997, 2002), McCrary (2002), and Evans and Owens (2007). Moreover, as discussed in Section 2, Italy has a police force that specializes in fighting frauds, smugglings, financial crimes, and tax evasions. This specialization allows us to see whether these crimes respond to the presence of the financial police and not to the presence of the regular police forces, and viceversa for the non-financial crimes. For this reason Table 7 analyzes “frauds” and “smugglings.” We also added bag-snatching, a highly visible crime that might respond to increased police patrolling.

Table 7 shows 2SLS estimates using the threshold equal to 0 for many different crimes and controlling for the regressors used in the even numbered columns in Table 5. *Panel A* shows the results using as regressors only regular police officers, *Panel B* using only “financial” police officers, and *Panel C* using both police forces separately. Let us first start by noting that running the regressions for the two police forces separately or together has little effect on the coefficients.[14] The coefficient in the last column corresponds to the sum of all crimes using just the regular police, and is therefore a simple replication of the result shown in column 6 of Table 5. The same column in *Panel B* shows that the financial police has no significant influence on total crimes. But columns (5) and (6) in *Panel C* show, not surprisingly, that the *Guardia di Finanza* has a large negative effect on frauds (-41 percent) and smugglings (-86 percent). With the exception of assaults financial police shows no significant negative effect on the remaining crimes. Some elasticities are even positive, and we have no explanation for this, other than sampling variability.

But let us now move to the regular police. There is clear evidence that the strongest impact are on violent crimes (-25.6 percent). In particular, murders show the largest responsiveness (-80 percent), followed by robberies (-55.7 percent) and assaults (-30.4 percent). These results are similar to the ones found in Levitt (2002) and Evans and Owens (2007). In Evans and Owens (2007) the lowest elasticities are found for robberies (-122 percent), assaults (-91 percent), and murders (-74.5 percent). In Levitt (2002) murders

[14]The simple correlation between *log* regular police and *log* financial police is 95 percent, but once we difference the data the correlation drops to 15 percent.

and robbery elasticities, respectively -91.4 percent and -45 percent, are very similar to our results. Some property crimes, instead, seem to respond differently in the US and in Italy to the presence of police. The elasticity of auto thefts is -85 percent in Evans and Owens (2007) and -170 percent in Levitt (2002), while it is precisely estimated to be 0 in our sample. The only property crime that responds to the presence of regular police is bag-snatching. Larcenies and burglaries show no significant changes when more police is employed, but this result is common to Levitt (2002) and in part to Evans and Owens (2007) (they find a negative and significant elasticity for burglaries, -54 percent, but no effect for larcenies).

3.4 Semiparametric and placebo regressions

Table 8 shows that we obtain elasticities that are not significantly different from zero when we use several different negative thresholds, which is in line with the elasticity that corresponds to negative changes in Table 5.

Instead of fixing some thresholds, in our final robustness check we are going to estimate "running elasticties" along the distribution of the changes in police. We start by estimating the elasticities based on just the first 40 percentiles of the changes in police forces.[15] These elasticities together with the corresponding (pointwise) 90 percent confidence intervals are shown at the beginning of the two plots shown in Figure 3, one for Italy and one for the US. The left y-axes show the elasticities, while the right ones show the log changes in police. The very first point on the left tells us that around the 20th percentile (±20%) the elasticity is close to zero in Italy and close to +20 percent in the US. The corresponding average changes in police forces are slightly larger than -3% in Italy and slightly lower than -4% in the US. The next point uses the data that lies between the 2nd and the 41st percentile of the changes in police. The last point, the 80th one uses the data

[15] We chose the number of percentiles to smooth the elasticities trying to minimize the bias.

centered around the 80th percentile (percentile 60 to 100). Several interesting differences emerge between the US and Italy: i) the elasticities tend to be larger in the US than in Italy; ii) in Italy the elasticities tend to be negative for positive changes in police forces (right of the vertical line) and positive for negative changes, which is consistent with the evidence related to mass hirings shown before; iii) in the US instead the elasticities tend to be small around small changes in police and large when changes in police are large, no matter whether positive or negative. Political pressure or concave hiring and firing costs might be driving this heterogeneity.

4 Conclusions

This paper uses a new identification strategy to provide evidence on the effect police has on crime: it exploits Italy's bureaucratic hiring procedures of state employees, in particular police officers. Positive changes in the number of police officers that are driven by mass hirings following lengthy hiring procedures lead to sizeable reductions in crime, especially the more violent ones.

Despite the apparent inefficiencies in the allocation of Italian police forces which persists to these days (the last mass hiring decree happened on June 26, 2008), the estimated elasticities of crimes with respect to regular police officers are only slightly lower than in the US, with two notable exceptions: auto thefts and burglaries.

The evidence leads to another open question that we leave for future research: Do the benefits of centralized hiring (lower risk of nepotism and corruption; internalization of potential spatial spillovers) outweigh the costs related to the hiring delays and the potential inefficient allocation of police forces?

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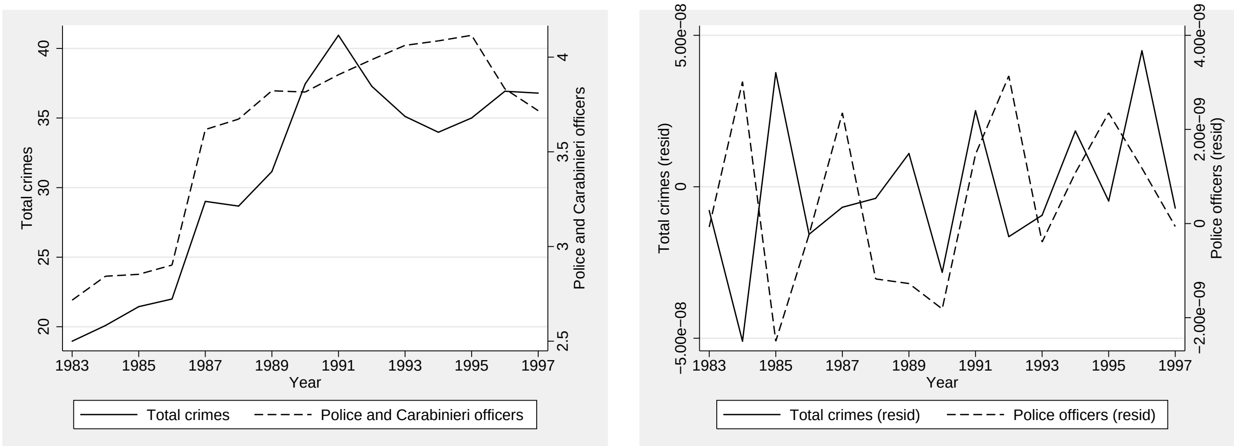

Figure 1: Crime and Police, with (right panel) and without controlling for time and region fixed effects.

Source: ISTAT Statistiche Giudiziarie Penali 1983-1997.

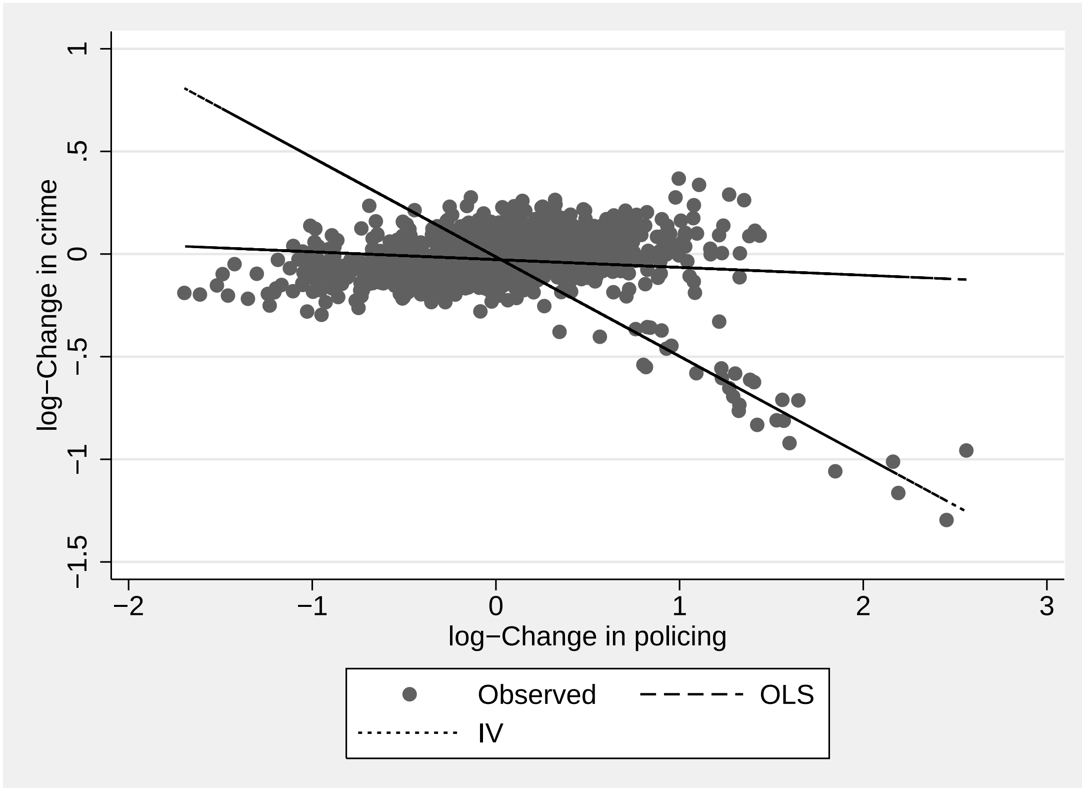

Figure 2: Simulated scatterplot and IV identification

Notes: The scatterplots are simulated assuming that police officers are hired based on crime levels, but that 5 percent of the times mass hirings happen and crime responds to increased police forces.

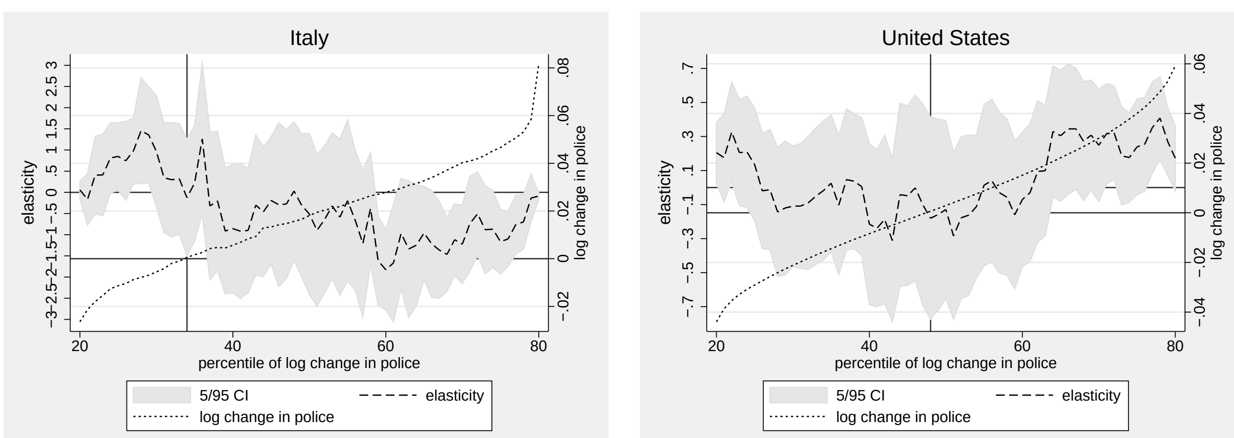

Figure 3: Crime and Police, with (right panel) and without controlling for time and region fixed effects.

Source: ISTAT Statistiche Giudiziarie Penali 1983-1997 and McCrary (2002).

Table 1: Police Force and Carabinieri Recruiting Laws

Law	Contents
Law. n. 121/1981	Set hiring procedures (art. 47 and 48 of Law n. 121). This law was later modified in 1982 (Decree Law n. 335) and in 2000 (Decree Law n. 234)
DPCM (*Decreto Presidente Consiglio dei Ministri*) March 2, 1984	Recruiting procedure for 5,000 *Carabinieri*
DPCM January 21, 1985	Recruiting procedure for 6,700 *Carabinieri*
Law n.150/1985	Recruiting procedure for 5,206 Police Officers (2,000 in 1985, 1,500 in 1986 and 1,000 in 1987)
Law n. 410/1985	Recruiting procedure for 8,800 *Carabinieri* (1,500 in 1985, 1,500 in 1986, 1,500 in 1987, 1,500 in 1988 and 1,800 in 1989)
Minister of Interior November 10, 1986	Recruiting procedure for 3,000 Police Officers
Decree Law n. 9/1992	Recruiting procedure for 3,799 Police Officers (in 1993 and 1994)

Table 2: Summary Statistics

variable	obs	mean	std.dev.	min	max
Police officers	285	359.67	132.82	86.87	750.40
Financial police officers	285	101.41	61.86	25.77	278.68
Population	285	3,014.29	2,211.196	330	8,974
Fraction pop. aged 15-35	285	.325	.019	.28	.36
Percentage of population with high school diploma	285	.17	.04	.08	.27
Percentage of population with university degree	285	.04	.01	.02	.08
Gross domestic product	285	14.13	3.68	7.49	21.97
Unemployment rate	285	9.22	3.98	3.19	23.48
Total crimes per 100,000 inh.	285	3,098.77	1,318.66	1,031.57	7,709.80
Property Crimes	285	1880.72	888.36	394.68	4,823.08
Burglary	285	281.11	120.84	62.05	743.11
Autotheft	285	323.50	263.92	43.11	1,174.16
Bag snatching	285	203.95	184.78	6.29	1,072.83
Larceny	285	901.22	519.72	141.29	3,005.91
Fraud	285	53.88	27.83	7.21	206.01
Smuggling	285	30.28	84.10	0.00	667.14
Violent Crimes per 100,000 inh.	285	95.55	37.71	38.64	243.27
Robbery	285	35.61	35.88	3.28	186.83
Murder	285	1.87	2.19	0	15.18
Assault	285	36.52	15.67	10.88	90.42
Rape	285	1.61	.70	.30	4.99

Table 3: Police and Crime in the US

	(1)	(2)	(3)	(4)	(5)	(6)
		log total crime			Δlog total crime	
	OLS	OLS	OLS	OLS	OLS	OLS
log or Δlog Police officers	0.749***	0.368***	0.180*	0.174	0.167***	0.132***
	(0.139)	(0.104)	(0.098)	(0.105)	(0.052)	(0.048)
log or Δlog SMSA % pop 25-29			0.552**	0.463		0.391
			(0.214)	(0.384)		(0.344)
log or Δlog State real income per capita			0.547**	0.099		-0.127
			(0.225)	(0.372)		(0.185)
log or Δlog % city pop black (interpolated)			0.052	-0.015		0.021
			(0.088)	(0.089)		(0.058)
log or Δlog State unemployment rates			0.175***	0.181***		0.086***
			(0.038)	(0.066)		(0.021)
log or Δlog real State+local educ			0.340***	0.378***		-0.009
spending per capita			(0.099)	(0.137)		(0.053)
log or Δlogreal State+local public welfare			-0.048	-0.048		0.019
spending per caita			(0.068)	(0.081)		(0.042)
Year and region effects	no	yes	no	yes	yes	yes
First stage F-stat						
Observations	1,332	1,332	1,084	1,084	1,259	1,015
R-squared	0.611	0.803	0.787	0.835	0.374	0.404

Notes: In columns (1)-(4) regressors are in *log*, while in columns (5) and (6) regressors are expressed in *log* changes. Regressions are estimated using ordinary least squares. Clustered (by city) standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Police and Crime in Italy

	(1)	(2)	(3)	(4)	(5)	(6)
		log total crime			Δ*log* total crime	
	OLS	OLS	OLS	OLS	OLS	OLS
log or Δ*log* Police officers	0.501**	-0.238***	-0.100*	-0.244***	-0.109***	-0.105***
	(0.229)	(0.049)	(0.050)	(0.044)	(0.037)	(0.034)
log or Δ*log* Fraction pop. aged 15-35			4.388***	0.411	0.000	0.000
			(0.907)	(1.555)	(0.000)	(0.000)
log or Δ*log* Gross domestic product			2.191***	1.188***		-0.843
			(0.393)	(0.382)		(2.053)
log or Δ*log* Percentage of population			0.043	-0.107		0.032
with high school diploma			(0.117)	(0.065)		(0.228)
log or Δ*log* Percentage of population			0.060	0.116		0.023
with university degree			(0.116)	(0.116)		(0.037)
log or Δ*log* unemployment rate			-0.056	0.008		-0.048
			(0.083)	(0.069)		(0.094)
Year effects	no	yes	no	yes		-0.085
First stage F-stat						(0.060)
Observations	285	285	285	285	266	266
R-squared	0.704	0.947	0.922	0.952	0.549	0.553

Notes: In columns (1)-(4) regressors are in *log*, while in columns (5) and (6) regressors are expressed in *log* changes. Regressions are estimated using ordinary least squares. Clustered (by region) standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Instrumental variables regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimation method	OLS	OLS	OLS	OLS	IV	IV	IV	IV	IV	IV
Threshold			0 percent		> 0 percent		> 5 percent		> 10 percent	
Δlog Police officers	-0.109***	-0.105***			-0.123***	-0.121***	-0.133***	-0.132***	-0.122***	-0.120***
	(0.037)	(0.034)			(0.036)	(0.033)	(0.035)	(0.032)	(0.032)	(0.030)
Negative Δlog Police officers			0.095	0.106						
			(0.107)	(0.108)						
Positive Δlog Police officers			-0.130***	-0.127***						
			(0.042)	(0.038)						
Δlog Fraction pop. aged 15-35		-0.843		-0.900		-0.886		-0.917		-0.884
		(2.053)		(2.049)		(1.851)		(1.844)		(1.852)
Δlog Gross domestic product		0.032		0.011		0.027		0.023		0.027
		(0.228)		(0.222)		(0.206)		(0.207)		(0.207)
Δlog Percentage of population with high school diploma		0.023		0.022		0.024		0.025		0.024
		(0.037)		(0.037)		(0.034)		(0.034)		(0.034)
Δlog Percentage of population with university degree		-0.048		-0.046		-0.051		-0.052		-0.050
		(0.094)		(0.093)		(0.084)		(0.084)		(0.085)
Δlog unemployment rate		-0.085		-0.088		-0.083		-0.081		-0.083
		(0.060)		(0.059)		(0.053)		(0.053)		(0.053)
First stage F-stat					8490	10508	3221	5990	3832	6756
Observations	266	266	266	266	266	266	266	266	266	266
R-squared	0.549	0.553	0.551	0.555	0.548	0.552	0.548	0.552	0.548	0.552

Notes: IV regressions are estimated using two stage least squares. Changes in the log number of police officers $\Delta \log P_{rt}$ are instrumented with changes above a given threshold $\gamma \; \Delta \log P_{rt} \times 1(\Delta \log P_{rt} > \delta)$. Clustered (by region) standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Distribution of the threshold dummies

IV threshold	larger than			smaller than		
	0%	5%	10%	0%	-3%	-6%
1983	0.00	0.00	0.00	0.00	0.00	0.00
1984	0.89	0.37	0.05	0.11	0.05	0.00
1985	0.53	0.05	0.00	0.47	0.05	0.05
1986	0.68	0.11	0.00	0.32	0.05	0.00
1987	1.00	0.84	0.63	0.00	0.00	0.00
1988	0.84	0.26	0.11	0.16	0.11	0.11
1989	0.89	0.21	0.00	0.11	0.00	0.00
1990	0.58	0.05	0.00	0.42	0.11	0.05
1991	0.74	0.32	0.00	0.26	0.00	0.00
1992	0.89	0.05	0.00	0.11	0.05	0.00
1993	0.74	0.16	0.05	0.26	0.11	0.00
1994	0.47	0.11	0.00	0.53	0.05	0.00
1995	0.53	0.11	0.00	0.47	0.00	0.00
1996	0.11	0.00	0.00	0.89	0.58	0.47
1997	0.05	0.00	0.00	0.95	0.42	0.05
total	0.60	0.18	0.06	0.34	0.11	0.05

Notes: The table shows the frequency of the thresholds used in Tables 5 and 8

Table 7: Instrumental variables regressions for different crime categories

	(1) Burglary	(2) Auto theft	(3) Bag-snatching	(4) Larceny	(5) Fraud	(6) Smuggling	(7) Economic	(8) Robbery	(9) Murder	(10) Assault	(11) Violent	(12) Total
Panel A: Police officers only												
Δlog Police officers	-0.057	0.048	-0.357***	0.028	0.210	0.583**	-0.017	-0.539***	-0.780*	-0.328***	-0.258***	-0.121***
	(0.053)	(0.083)	(0.073)	(0.033)	(0.254)	(0.257)	(0.040)	(0.163)	(0.452)	(0.067)	(0.061)	(0.033)
R-squared	0.262	0.446	0.243	0.470	0.137	0.245	0.528	0.296	0.096	0.135	0.224	0.552
Panel B: Financial police officers only												
Δlog Financial police officers	-0.054	0.137**	0.063	0.059	-0.376*	-0.769**	0.069*	0.107	0.091	-0.300***	-0.052	-0.047
	(0.067)	(0.059)	(0.109)	(0.050)	(0.216)	(0.301)	(0.042)	(0.080)	(0.258)	(0.076)	(0.059)	(0.049)
R-squared	0.260	0.449	0.198	0.473	0.147	0.227	0.530	0.253	0.083	0.140	0.200	0.543
Panel C: Police officers and financial police officers												
Δlog Police officers	-0.052	0.035	-0.368***	0.023	0.249	0.666**	-0.024	-0.557***	-0.801*	-0.304***	-0.256***	-0.118***
	(0.054)	(0.084)	(0.074)	(0.032)	(0.258)	(0.261)	(0.040)	(0.166)	(0.463)	(0.068)	(0.060)	(0.032)
Δlog Financial police officers	-0.047	0.132**	0.115	0.056	-0.411*	-0.863***	0.073*	0.185**	0.205	-0.257***	-0.016	-0.030
	(0.065)	(0.058)	(0.100)	(0.051)	(0.213)	(0.309)	(0.040)	(0.083)	(0.240)	(0.069)	(0.056)	(0.043)
Observations	266	266	266	266	266	264	266	266	264	266	266	266
R-squared	0.262	0.450	0.241	0.472	0.155	0.234	0.531	0.294	0.095	0.156	0.224	0.552

Notes: IV regressions are estimated using two stage least squares. Changes in the log number of police officers $\Delta \log P_{rt}$ are instrumented with changes above 0. Clustered (by region) standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Placebo regressions

	(1)	(2)	(3)	(4)	(5)	(6)
	Δlog total crime					
	IV	IV	IV	IV	IV	IV
IV threshold	< 0 percent		< -3 percent		< -6 percent	
log Police officers	0.031	0.044	0.019	0.033	-0.018	-0.010
	(0.072)	(0.071)	(0.085)	(0.085)	(0.068)	(0.068)
log Fraction pop. aged 15-35		-0.424		-0.454		-0.574
		(1.922)		(1.889)		(1.863)
log Gross domestic product		0.085		0.081		0.066
		(0.193)		(0.196)		(0.204)
log Percentage of population		0.012		0.013		0.016
with high school diploma		(0.035)		(0.035)		(0.035)
log Percentage of population		-0.026		-0.028		-0.034
with university degree		(0.099)		(0.099)		(0.095)
log unemployment rate		-0.105		-0.103		-0.098
		(0.070)		(0.069)		(0.069)
First stage F-stat	796.5	666.3	453.2	957.7	311.7	650.0
Observations	266	266	266	266	266	266
R-squared	0.532	0.535	0.535	0.537	0.542	0.545

Notes: IV regressions are estimated using two stage least squares. Clustered (by region) standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.