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Abstract

I review recent developments in the economic analysis of crime, focusing in particular on organized crime and corruption. I first discuss the main challenges to the empirical identification of causal relationships -- namely, measurement error due to endogenous reporting of crime and the fact that randomized controlled trials are rarely an option when studying crime. I then discuss recent advancements made possible by the combination of detailed micro-data and quasi-experimental methods.

JEL Classification: K42

Keywords: Economics of crime, Measurement error, identification, Quasi-experiments

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The credibility revolution in the empirical analysis of crime

Paolo Pinotti*

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Abstract

I review recent developments in the economic analysis of crime, focusing in particular on organized crime and corruption. I first discuss the main challenges to the empirical identification of causal relationships – namely, measurement error due to endogenous reporting of crime and the fact that randomized controlled trials are rarely an option when studying crime. I then discuss recent advancements made possible by the combination of detailed micro-data and quasi-experimental methods.

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1 Introduction

Crime is inherently an economic phenomenon, as individuals decide whether to commit a crime or not by weighing its expected benefits against its expected costs. As a consequence, crime decreases in response to harsher sanctions, higher probability of arrest upon committing a crime, and higher opportunity cost (as driven, for instance, by greater legitimate earning opportunities). This is the essence of the economic model of crime, as formalized half a century ago by Gary Becker (1968), and anticipated even earlier by Enlightenment philosophers such as Beccaria (1764) and Bentham (1864). Over the years, this simple and elegant theoretical framework has become a cornerstone of the choice-theoretic approach to the analysis of criminal behavior (see, e.g., Lazear, 2015).

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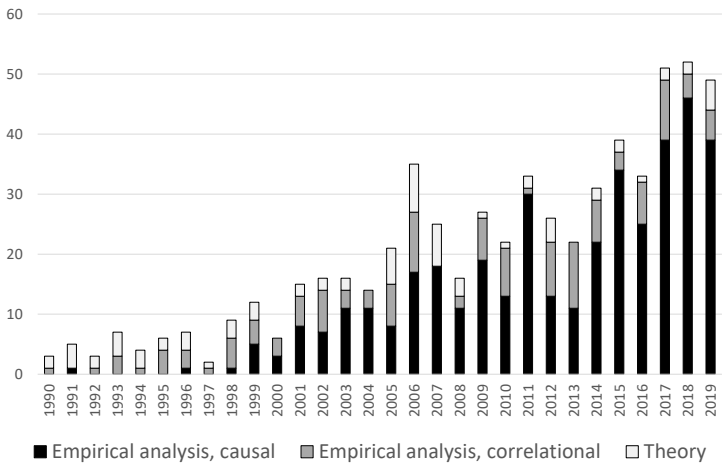
On the empirical side, testing the model predictions and achieving a credible identification of the parameters of interest (e.g., the crime-police elasticity or the deterrence effect of prison) has proven very difficult, for two main reasons. First, crime data typically entail a large degree of (non-classical) measurement error, as offenders do all they can to hide their actions from enforcement authorities. Second, randomized controlled trials are seldom available when studying crime, due to obvious ethical, social, and political concerns. The combination of these two challenges greatly complicate the identification of causal effects, which are most interesting from both a scientific and a policy perspective.

In spite of these difficulties, the field witnessed a spectacular expansion during the last two decades. Figure 1 shows the number of articles on crime published in leading peer-reviewed journals in economics, distinguishing between purely theoretical contributions, correlational empirical analyses, and causal empirical analyses (the source of these data is Doleac, 2020).

Figure 1: Articles on crime published in economics journals, by type of analysis



This figure shows the number of articles published in 21 general interest and top field economics journals (source: Doleac, 2020) by type of analysis: theoretical, correlational empirical analysis, and causal empirical analysis

The total number of articles on crime increased tenfold since the early 1990s – from less than five to about fifty per year. Interestingly, this trend was entirely driven by the marked increase in the number of causal empirical analyses. These analyses overcome measurement issues and the impossibility of running controlled experiments by the means of increasingly detailed datasets and clever quasi-experimental designs; I discuss these developments in the next Sections 2 and 3.

Both measurement and identification issues become more severe when studying the activities of criminal organizations engaged in complex criminal enterprises, as opposed to individual offenders committing relatively simple crimes. As a consequence, we know

much less about the causes and consequences of organized crime than we know about individual offenders, although the former likely imposes the largest costs on societies. In Section 4, I discuss a few recent papers that attempt to make progress in this direction, including some of the papers in the Italian Economic Journal's Special Issue on Economics of Crime.

2 Measurement of illicit behavior

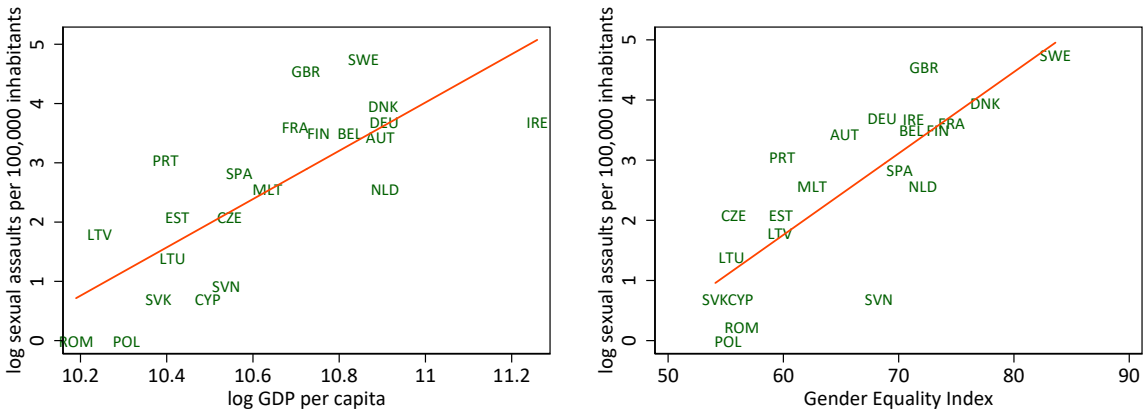
Measurement error is ubiquitous in empirical research, particularly in social sciences. The very definition of many important concepts in social sciences (e.g., "quality of institutions" or "social capital") involves a large degree of discretion. In addition, such variables are often measured from relatively small samples surveyed from much larger populations. As it is widely known, "classical" measurement error in explanatory variables (i.e., errors that are correlated neither with the outcome of interest nor with the explanatory variables itself) should bias the estimated regression towards zero, so one can still sign the effect of interest and bound its magnitude from below (see, e.g., Hyslop and Imbens, 2001).

However, mis-measurement of crime typically reflects rational and willful acts by offenders (or their victims) aimed at hiding illicit conducts. As a consequence, criminal statistics – as derived, e.g., from police reports or judicial proceedings – will systematically under-state the true number of committed crimes. Most importantly, the extent of under-reporting may vary with the expected benefits and costs from crime, level of enforcement, social values and beliefs, offender's characteristics, and so on. All these other factors are potential determinants of crime. Therefore, measurement error will be non-classical, and may severely bias the measurement of crime as well as its relationship with other variables.

As an example, the willingness to report sexual assaults – and the very definition of such crime – may well depend on societal attitudes towards this type of crime. Interestingly, the (log) rate of reported sex assaults across European countries is on average higher in countries that are richer and characterized by greater gender equality, such as Nordic countries, as compared to countries in Southern and Eastern Europe; see Figure 2. This evidence suggests that crime statistics about sexual assaults are hardly informative for comparing the extent of the phenomenon in different socioeconomic contexts.

The article by Gara and Pauselli (2020), included in this Special Issue, discusses measurement issues in another context, namely banks' reporting of suspicious transactions to supervisory authorities. In this specific case, banks may have an incentive to *over-report* due to the asymmetric treatment of Type I and Type II errors, as sanctions apply only to omitted reports (Takáts, 2011, provides a theoretical framework). Exploiting detailed, restricted use information at the report-level, Gara and Pauselli (2020) characterize variation in over-reporting across different banks and geographical areas, and draw

Figure 2: Sexual assaults reported to the police, GDP per capita, and gender equality



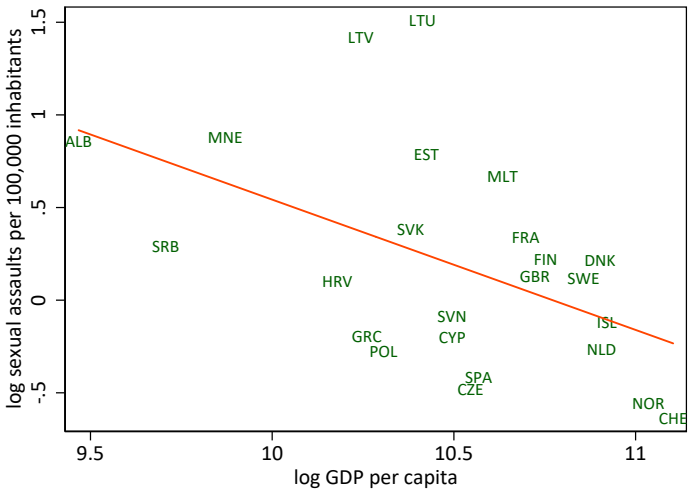
The left and right graphs show the relationship between the log number of reported sexual assaults per 100,000 inhabitants with, respectively, the log of real GDP per capita and the Gender Equality Index. The source of the first two variables is Eurostat, while the Gender Equality Index is a composite indicator computed by the European Institute for Gender Equality (see Bericat, 2012, for a description). All data refer to year 2017.

its implications for the activity of supervisory authorities.

We next discuss three possible solutions to measurement issues. First, the potential extent for underreporting (or differential reporting) varies greatly by type of crime. For instance, the definition of homicide does not vary across countries and the extent of underreporting is negligible for this type of crime, because soon or later the corpse is (almost) always discovered. For this reason, homicide rates are most informative for comparing the incidence of (violent) crimes across countries. Indeed, Figure 3 shows that – differently from the previous evidence on sexual assaults – homicide rates are negatively correlated with country GDP per capita (see also Soares, 2004). As for property crimes, underreporting should be minimal for car thefts, because the owner has clear incentives to report the theft to the police, both because the stolen good is typically valuable and because stolen cars could be used to commit other crimes (Buonanno et al., 2009). Therefore, a potential solution to measurement issues consists in limiting the analysis to crimes that are less subject to underreporting.

A second solution consists in imposing some restrictions on the structure of measurement error and differencing it away in the econometric specification. For instance, let C_{rt} and C^*_{rt} be, respectively, the number of reported crimes and the number of actual, unobserved crimes committed in region r and year t. If one is willing to assume that (i) reported crimes are proportional to actual crimes, $C_{rt} = \alpha C^*_{rt}$, and (ii) the constant of proportionality is the product of region-specific and year-specific components, $\alpha = \alpha_i \alpha_t$, a log linear specification would absorb measurement error into region and year fixed effects. In practice, if the degree of underreporting differs across regions – possibly, in ways that are correlated with the actual number of committed crimes or with the other

Figure 3: Homicide rate and GDP per capita



The graph shows the relationship between the log number of homicide rates per 100,000 inhabitants and the log of real GDP per capita. All data refer to year 2017, the source is Eurostat.

variables of interest – but remains parallel over time, the resulting measurement error would bias estimates in levels but not estimates in growth rates. This approach is used, among others, by Levitt (1996) and Bianchi et al. (2012).

The third solution exploits the fact that, although criminals try (and often succeed) to hide their action from authorities, such actions may nevertheless leave a "statistical trace" in official statistics. In a famous paper, Duggan and Levitt (2002) detect match-rigging in Sumo wrestling based on a clear anomaly in the distribution of wins within 15-bout tournaments, namely a missing mass of players with 7 wins and an excess mass of players with 8 wins. Since the payoffs of an additional win increase discontinuously between 7 and 8 wins, the authors conclude that players entering the last bout with 6 wins are selling the match to players entering the last bout with 7 wins. Notice that this evidence remains very compelling even in the absence of any judicial evidence. Although Sumo is not so relevant to most economists – or for that matter, other social scientists – a similar approach has been followed in later years to study other types of illegal behavior that are arguably more relevant, such as corruption in public procurement Di Tella and Schargrodsky (2003), tariff evasion (Fisman and Wei, 2004), skimming on road building (Olken, 2007), and drug consumption (Kilmer et al., 2011). Zitzewitz (2012) labels this approach "forensic economics" and provides and interesting review.

Of course, these methods do not exhaust the possible solutions to the problem of measuring illicit behavior. In general, any such solution must rely on a careful knowledge of the institutional context, often coupled with the availability of detailed (possibly restricted-use) data allowing us to gauge the extent of under/over-reporting. For instance, it may be useful to have data on arrests *in flagrante*, which should be less biased by differential enforcement compared to other arrests or convictions (Pinotti, 2017; Britto

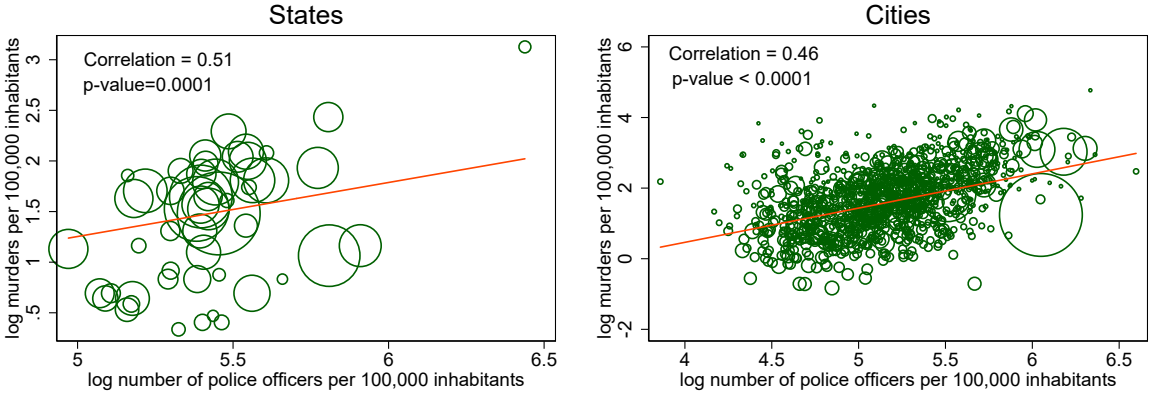
et al., 2020) or data on suspect offenders, which should be less affected by false negatives (Decarolis et al., 2019).

3 The search for causality

During the last two decades, empirical research in economics – and in other social sciences – put increasing emphasis on the distinction between correlation and causation. Correlation describes systematic associations between two or more variables (e.g., crime and poverty) but remains silent about the underlying causal effects – indeed, correlation is neither necessary nor sufficient for the existence of any such effect. Instead, causal analysis isolates the effect of variable X on outcome Y (the effect of poverty on crime) from reverse causation (the effect of crime on poverty, which may also be relevant) and from other confounding factors affecting both X and Y (e.g., racial segregation). This type of analysis is definitely more challenging than correlational analysis, but is essential for understanding both the causes and the consequences of crime as well as for correctly evaluating the effect of alternative counter-policies.

As a leading example, consider the problem of estimating the elasticity of crime to the size of police forces. This parameter is of utmost importance, both from a scientific and a policy perspective – for instance, it may inform the cost-benefit analysis of additional policy hirings. A simple plot of (log) crime rates against number of police officers across US states and cities – respectively, left and right graph of Figure 4 – reveals a positive correlation between the two variables, which is clearly driven by policy responses of local authorities to the level of criminal activity. From this correlation, it is thus impossible to identify the causal effect of police on crime.

Figure 4: Homicide rates and police officers per capita across US states and cities, 2018



The figure plots the relationship between the log number of homicides and police officers per 100,000 inhabitants across US states (left graph) and across cities with more that 10,000 inhabitants (right graph). The size of each marker is proportional to the resident population. The source of these data is the Uniform Crime Report for year 2018.

In principle, randomized controlled trials (RCTs) provide a solution to this identification problem. RCTs are social experiments arranged specifically for the purpose of estimating the effect of a given "treatment" (e.g., an anti-crime policy) by randomizing experimental units into one of two groups, treated and controls, and administering the treatment only to the former group. By the virtues of randomization, the two groups should be ex-ante comparable – at least, for a large enough sample – so any difference in outcomes during the following period can be attributed to the causal effect of the treatment (see, e.g. Gerber and Green, 2012). This approach has gained increasing importance in applied economic research, culminating with the attribution of the last Nobel Prize in Economics to Abhijit Banerjee, Ester Duflo, and Michael Kremer, who popularized this approach for the study of economic development (see, e.g. Duflo et al., 2007; Banerjee and Duflo, 2009).

However, RCTs may be practically unfeasible for ethical, political, or technological reasons. While this is often the case in general, RCTs in crime research are particularly problematic. Back to the previous example of police and crime, the ideal RCT would randomly allocate the number of police officers across US cities or states. Such experiment would be strongly opposed by local politicians and citizens alike, though policing experiments have been conducted on a smaller scale. For instance, Ratcliffe et al. (2011) show that foot patrolling in downtown Philadelphia reduced crime rates by randomizing "hot spots" (i.e., areas with abnormal levels of criminal activity) into treated and control groups. However, the small scale of the randomized intervention limits the external validity of the results and is problematic for the likely presence of displacement effects (Blattman et al., 2017).

In general, RCTs are simply not an option for evaluating policy interventions that are most relevant for studying crime, such as capital punishment and incarceration. In addition, crime may be itself the "treatment" of interest – e.g., when trying to estimate its economic and social costs – in which case RCTs are even less advisable. For this reason, the empirical analysis of crime mostly relies on natural experiments (or "quasi-experiments"). Differently from RCTs, natural experiments are not arranged on purpose by researchers or policy makers. At the same time, they generate "quasi-random" variation in exposure to treatment, which may then be leveraged to identify the treatment effect of interest (see, e.g. Angrist and Pischke, 2010; Dunning, 2012). Typical examples of natural experiments include policy changes induced by plausibly exogenous factors, such as the European Union enlargement in Mastrobuoni and Pinotti (2015); historical natural experiments, as recently reviewed by Cantoni and Yuchtman (2020); and variation in whether conditions, as in Miguel et al. (2004) and Brückner and Ciccone (2011).

Considering once again the effect of police on crime, Di Tella and Schargrodsky (2004) and Draca et al. (2011) exploit terrorist attacks as an exogenous shock to the intensity of police presence in Buenos Aires and London, respectively, to identify its deterrent effect

on other types of crime. Another landmark natural experiment in crime research has been the Collective Clemency Bill enacted in Italy on July 2006 – the so-called *Indulto*. In the wake of severe prison overcrowding, the government pardoned three years of sentence to all prison inmates, so that anybody with less than three years of residual sentence was immediately released – about 25 thousand people, on an initial prison population of over 60 thousand. Buonanno and Raphael (2013) and Barbarino and Mastrobuoni (2014) exploit this massive release to estimate the incapacitation effect of prisons – between -0.17 and -0.4 percent reduction for a 1 percent increase in incarceration. In addition, pardoned individuals re-arrested within 5 years were going to serve the pardoned sentence in addition to any new sentence. This provision generates plausibly exogenous variation in expected punishment for new crimes across pardoned offenders, depending on the timing of previous offenses. Drago et al. (2009) leverage this variation to identify the deterrent effect of prison sentence, and Drago and Galbiati (2012) also estimate spillover effects to peers sharing time in the same prison. According to their results, a one year increase in expected prison sentence reduces recidivism by 1.9 percentage points – 16.5 percent over the baseline recidivism rate – and has a similar effect on peers.

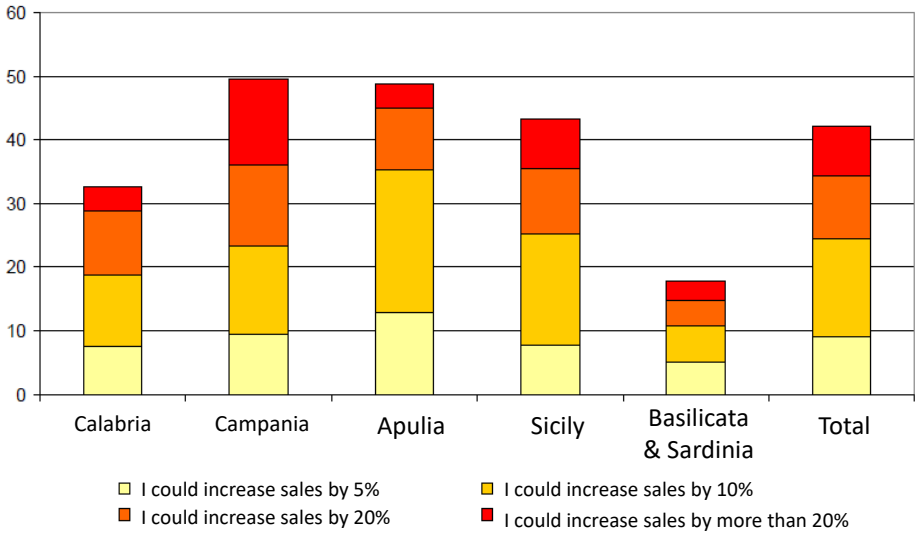
The article by Mastrobuoni and Rialland (2020) in the present Special Issues exploits the Italian *Indulto* for yet another purpose, namely developing a data-driven approach to detect networks of co-offenders. Previous studies mostly rely on ex-ante definitions of peer groups – for instance, sharing the same country or region of origin and having spent time together in the same prison, as in Drago and Galbiati (2012). In contrast, Mastrobuoni and Rialland (2020) infer the structure of co-offending networks from the exact date of (re)arrest of pardoned prison inmates recidivating after the *Indulto*. They then document a high degree of homophily among peers in terms of age and nationality (but not of education, former employment status, and type of offense). In this case, the natural experiment mainly serves the purpose of detecting information on criminal networks, as opposed to estimating causal effects. Given the measurement issues discussed in the previous section, this is certainly an interesting avenue for future research.

4 Organized crime and corruption

The methodological issues discussed in the previous sections become all the more challenging when we move the focus from individual offenders to criminal groups, and from simple crimes such as thefts or assaults to complex criminal enterprises such as drug trafficking or corruption. For starters, in the legislative system of most countries membership in structured criminal organizations is not defined, let alone sanctioned. In Italy, a country plagued by the presence of mafia-type organizations at least since the end of the XIX century, this vacuum was filled only in 1982 with Article 416-bis of the Penal Code.

Most importantly, people may be reluctant to report the presence of criminal organizations due to fears of retaliation, especially where such organizations are more powerful. For this reason, reported perceptions may be little (or even inversely!) correlated with the real presence and strength of organized crime. This problem is apparent when looking, for instance, at the results of a survey conducted among entrepreneurs in Italian regions with a high presence of organized crime (CENSIS, 2009). The survey asked each respondent by how much s/he could have increased her/his sales in the absence of organized crime – a measure of perceived costs. Strikingly, respondents in Apulia admitted higher costs than respondents in Sicily and Calabria (see Figure 5), though criminal organizations are arguably stronger in the latter regions than in the former.

Figure 5: Perceived costs of organized crime, survey evidence



The graph shows the results of a survey conducted among entrepreneurs in Italian regions with a high presence of organized crime (CENSIS, 2009). The survey asked: "In your opinion, by how much would you be able to increase your revenues in the absence of organized crime?"

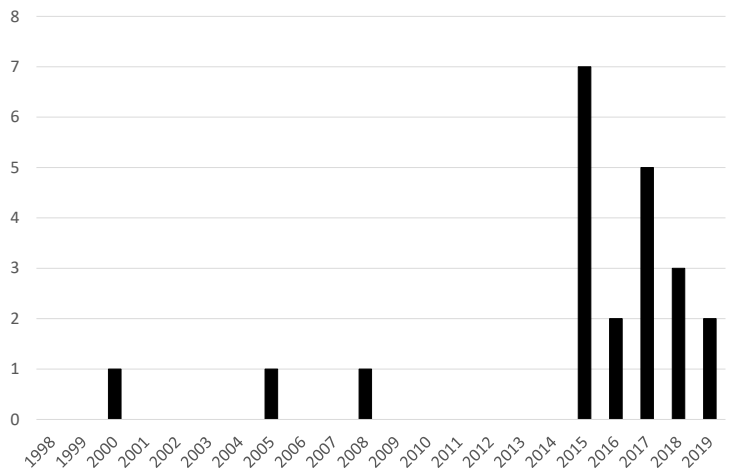
Similar challenges are present when trying to measure political corruption, a crime that is often connected with the presence of criminal organizations (Dal Bó et al., 2006; Alesina et al., 2019). Benefits from corruption are concentrated on a very small number of individuals (e.g. a bribing company and a corrupt politician) while its costs are spread among the entire society. This structure of payoffs generates a "tragedy of the commons" in which no single victim has a clear incentive to report corruption and, possibly, to start a judicial case that may be extremely long, complex, and expensive. Of course, people may still decide to report corruption out of civicness and other values usually embedded into the concept of "social capital". However, the latter variable may be an important confounding factor when evaluating the effects of corruption (e.g., on economic development). One way of addressing this issue has been to focus on connections between politicians and firms that may facilitate corruption but are *per se* legal – and, as such,

are often disclosed to the public (see, e.g., Fisman, 2001; Faccio, 2006).

The identification of the causes and consequences of organized crime and corruption is also complicated by the fact that their existence and diffusion is deeply intertwined with a host of social, economic, and political conditions. For instance, Gambetta (1996) traces the origins of the Sicilian Mafia to the power vacuum experienced in Southern Italy after the country's Unification, as weak democracy and lack of trust and enforcement generated demand for private protection (see also Buonanno et al., 2015; Acemoglu et al., 2020). To the extent that these conditions persisted over time, it will be hard to disentangle the effectiveness of different counter policies – or, for that matter, the effects of organized crime on other economic and political outcomes – from the effect of these pre-existing factors (Pinotti, 2015a).

The measurement and identification challenges just described have long delayed empirical research on organized crime. Figure 6 shows the total number of papers on this topic in economics journals – using the same data and criteria from previous Figure 1. The graph shows that research on organized crime gained momentum after the publication of a special issue in the *Economic Journal* in 2015, which provided some of the first causal evidence on the causes and consequences of organized crime (Buonanno et al., 2015; Mastrobuoni, 2015; Pinotti, 2015b; Daniele and Geys, 2015).

Figure 6: Articles published in economics journals about organized crime



This figure shows the number of articles on organized crime published in 21 general interest and top field economics journals. Source: Doleac (2020), available at www.jenniferdoleac.com/resources

Most of the papers included in the Italian Economic Journal's Special Issue on Economics of Crime contribute to this research agenda. Cingano and Tonello (2020) evaluate the effects on local crime rates of local government dismissals for organized crime infiltration in Southern Italy. Local government dismissals were first introduced in 1991 to

curb the influence of mafia-type organizations on local politics; 344 local governments have been dismissed since then, 312 of which in Sicily, Calabria, and Campania. Understanding the effects of dismissals is of primary importance from a policy perspective. In addition, they have been extensively exploited as useful experiments to understand the implications of organized crime presence along several dimensions, such as political selection (Daniele and Geys, 2015), public investment efficiency (Galletta, 2017), and public procurement (Fenizia, 2018). The empirical analysis by Cingano and Tonello (2020) points at a persistent decrease in petty crimes, while there is little effect on offenses more closely related to the activity of organized crime (e.g., homicides, extortions, and drug-trafficking). These findings speak to previous papers, discussed above, about the deterrent effect of increased policing after terrorist attacks on other types of crime (Di Tella and Schargrodsky, 2004; Draca et al., 2011). At the same time, they suggest that local government dismissals per se may not be sufficient to eliminate the influence of criminal organizations.

The paper by Calamunci and Drago (2020) exploits another important counter policy enacted by the Italian government against members of criminal organizations, namely asset seizures, to estimate the spillover effects of criminally-connected firms on other (legal) firms operating in the same market. Taking advantage of longitudinal firm-level data, the authors detect significant increases in firm performance and turnover in response to judicial seizures of infiltrated, competing firms. These results describe an important source of allocative inefficiency imposed by organized crime on legal economic activities.

Checchi and Polo (2020) highlight another way in which criminal organizations may infiltrate the legal economy, namely appropriation of public subsides for investment in renewable energy. In line with the findings of previous judicial investigations, the authors document an abnormal presence of wind farms in Sicilian municipalities with a higher presence of Mafia. Interestingly, investment into renewable energy across municipalities and over time seems more responsive to rent-seeking incentives – as determined by the regulatory framework – than to geographic features favoring energy production. The opposite is true in Apulia, where organized crime is less powerful and does not seem to be involved in the business of renewable energy.

Finally, De Angelis et al. (2020) focus on corruption, a phenomenon that is intimately related to the presence of organized crime. Using detailed, restricted-use data on corruption cases across Italian municipalities, they show that financial funds received from the EU brought a 4 percent increase in the number of white collar crimes. These results are consistent with previous evidence on the negative implications of windfall government revenues for politicians' accountability (see, e.g., Brollo et al., 2013).

Overall, these papers advance our knowledge of the causes and consequences of organized crime and corruption in Italy, a country that has been traditionally plagued by these phenomena. These findings will hopefully contribute to an informed debate on

these issues, from both a scientific and a policy perspective, and to the design of appropriate counter policies contributing to alleviate the burden imposed on entire countries and regions in the world.

5 Conclusions

It is hard to understate the relevance of crime, particularly "big" crime such as organized crime and corruption, from a social and economic perspective. At the same time, the measurement and identification challenges discussed in this review held back research on these issues compared to other areas of applied economics. One lesson that can be drawn from recent studies that made a dent in the scientific debate is that the empirical study of crime should meet the same standards of proof that are currently imposed in other areas of the economics discipline, no matter how hard this might be due to the complexity of organized crime and corruption, the difficulties involved in their measurement, and the impossibility of conducting randomized controlled trials in this area of research.

This lesson is especially relevant for young scholars that are just approaching the study of crime, who are often moved by a strong ethical passion alongside scientific curiosity. Such passion provides a powerful motivation for pursuing research on these themes. At the same time, it should always be combined with rigorous research designs and methodological tools, because the latter will ultimately make the difference between a solid scientific paper and more descriptive and/or anecdotal pieces of evidence.

I would like to conclude with a personal memory of Alberto Alesina, with whom I had the privilege to work with during the last few years. In 2015, Salvatore Piccolo and myself stepped into Alberto's office to ask his advice about some ideas we had on the interdependencies between organized crime and politics. It took less than 5 minutes for Alberto to become fully involved into the project, so much that the three of us started to work together on it. This enthusiasm reflected an immediate recognition of the topic's relevance more than the merits of our specific project, which at that time was still very preliminary. The unsurpassed sense of Alberto for big questions and themes – ranging from the causes and consequences of political institutions, cultural norms, ethnic fragmentation, discrimination, and immigration – is possibly the best demonstration that criminal behavior, organized crime, and corruption are phenomena of utmost importance from an economic and political perspective, and that they deserve further attention by economists and social scientists more generally.

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