Crime

SUMMARY

Contrary to common perceptions, today both property and violent crimes (with the exception of homicides) are more widespread in Europe than in the United States, while the opposite was true thirty years ago. We label this fact as the 'reversal of misfortunes'. We investigate what accounts for the reversal by studying the causal impact of demographic changes, incarceration, abortion, unemployment and immigration on crime. For this we use time series data (1970–2008) from seven European countries and the United States. We find that the demographic structure of the population and the incarceration rate are important determinants of crime. Our results suggest that a tougher incarceration policy may be an effective way to contrast crime in Europe. Our analysis does not provide information on how incarceration policy should be made tougher nor does it provide an answer to the question whether such a policy would also be efficient from a cost-benefit point of view. We leave this to future research.

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Crime in Europe and the United States: dissecting the 'reversal of misfortunes'

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1. INTRODUCTION

Despite the interest of policymakers in crime and the long tradition of economic analysis of delinquent behaviour, there is a surprising lack of quantitative research on the determinants of crime and on the effects of crime control policies outside the United States, particularly in Europe. Much of what we know is based on analyses of American data,[1] and is summarized by Levitt (2004) and Levitt and Miles (2007).[2] The primary goal of this paper is to fill this gap: we employ data on crime in Europe as well, and perform a cross-country empirical investigation of crime trends during the last 40 years. Here and in what follows, by Europe we mean Austria, France, Germany, Italy, the Netherlands, Spain and the United Kingdom.

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[1] An exception is Cook and Khmilevska (2005), who compare crime rates across countries.

[2] Other important contributions in criminology and economics to the understanding of crime trends in the US include Blumstein and Wallman (2000), Cook and Laub (2002), and Zimring (2006).

Although this choice is primarily driven by data availability, these seven countries account for more than 80% of the pre-2004 population of the current European Union, with an aggregate population above 300 million – a figure comparable to the US population.

It is well known that the United States experienced an unexpected drop in crime rates after 1990. In Europe, on the contrary, crime rates have been on the rise since at least 1970. Contrary to common perceptions, crime is today more widespread in Europe than in the United States, while the opposite was true 30 years ago. This fact, which we label the 'reversal of misfortunes', is documented in Figures 1–3. Figure 1 shows the dynamics of the total crime rate (crimes of any kind reported to the police per 1,000 inhabitants) in the United States and in Europe. In 1970 the aggregate crime rate in the seven European countries we consider was 63% of the corresponding US figure, but by 2007 it was 85% higher than in the United States. This striking reversal results from a steady increase in the total crime rate in Europe during the last 40 years, and the decline in the US rate after 1990. The reversal of misfortunes is also observed for property and violent crimes. Figure 2 documents the trends in the property crime rate. Although in this case Europe and the United States have been moving along a common

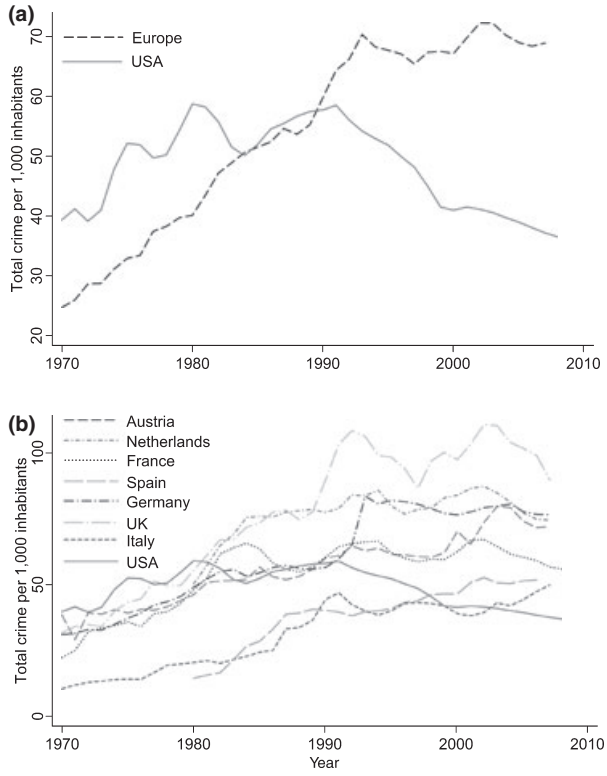

Figure 1. Total crime in the United States and in Europe

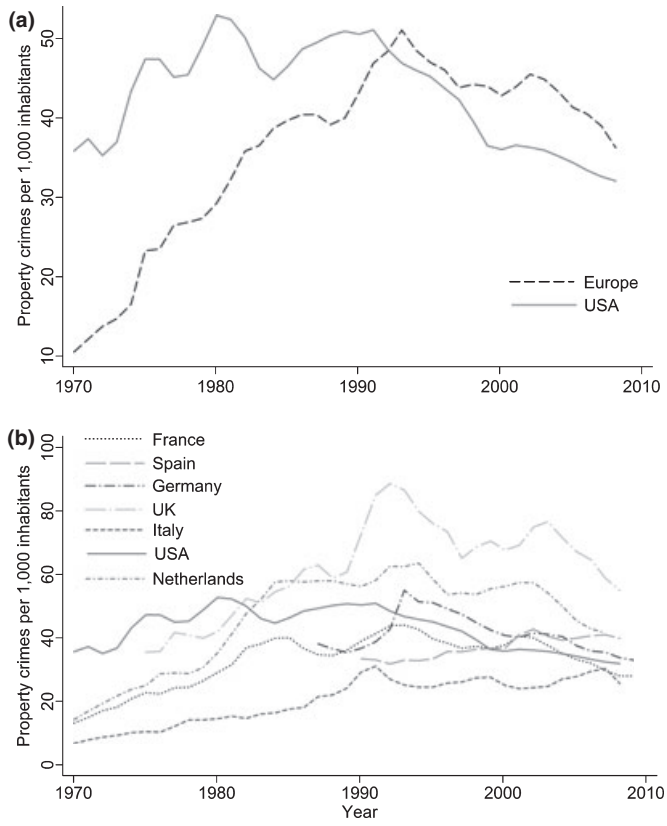

Figure 2. Property crimes in the United States and in Europe

trend since the early 1990s, the European rate in 2007 was still 20% above the US rate, while in 1970 the Europe/US ratio was below 30%. The same pattern is found when looking at individual countries, with the exceptions of France and Italy. Figure 3 shows the reversal for violent crimes: in 1970 the violent crime rate in Europe was 62% of the corresponding rate in the United States. By 2008 it was more than twice the US figure. We discuss later how varying reporting rates may alter these patterns.

These pictures reveal a substantial divergence between crime trends in Europe and the United States. Apparently, the American experience is a story of success in crime control when compared to what happened on the other side of the Atlantic. This makes a cross-country investigation of the determinants of crime rates an interesting research question: although the United States and Europe have different social and economic structures, it is natural to ask whether the different dynamics of the crime rates in the two areas can be explained by the different dynamics of the factors emphasized by the economics of crime.

We address this question by using a subset of such explanatory variables, for which we have been able to collect long-term cross-country series: demographic

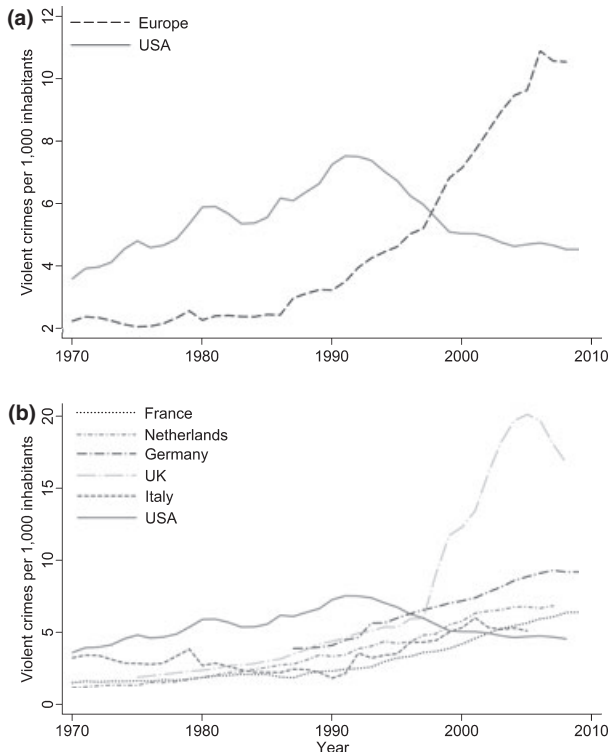

Figure 3. Violent crimes in the United States and in Europe

Note: The jump in the UK violent crime rate after 1997 reflects a discontinuity in definitions and recording practices. The Home Office (2008) notices that 'the number of violence against the person offences recorded by the police increased by 118 per cent as a result of the 1998 changes [...]. Much of this increase resulted from a widening of the offence coverage to include assaults with little or no physical injury, and offences of harassment (again with no injury)' (p. 60). In the inferential part of the paper this will be controlled for by year fixed effects. The dynamics after the discontinuity, though is genuine.

changes, incarceration rates, abortion, unemployment and immigration. The channels through which these variables may affect crime are well known. The demographic structure of the population is important because different demographic groups have different propensities to engage in crime. Young males, for instance, are disproportionately more prone to crime than women or seniors. Incarceration may have both a deterrent effect (the threat of being locked up reduces the net expected benefit of crime) and an incapacitation effect (those who are imprisoned cannot commit crimes). Past abortions, as Donohue and Levitt (2001) argue, may have removed from society individuals who, because of the socioeconomic conditions of their families, would have had a higher propensity for crime had they reached adulthood. As for the unemployed, they have a lower opportunity cost of engaging in crime due to their lack of legal sources of labour income. Finally, immigrants are characterized by both socioeconomic traits and living and working conditions that make them statistically more likely to commit crimes: they are

typically young, poorly educated, low income, and male, relative to their counterparts in the host country.

However, identifying the causal effect of these variables is challenging. First, these variables and crime rates may share common unobserved trends. Second, even if one treats the demographic structure of the population and the legalization of abortion as exogenous with respect to crime, the remaining variables (incarceration, unemployment and immigration) are all potentially endogenous. In view of these identification problems we opt for a conservative identification strategy. First, in addition to the inclusion of country fixed effects and common year dummies, we allow for flexible deterministic country-specific time trends. These help to remove spurious correlations induced by unobserved common time effects, although they may remove genuine correlations as well – this is why we call this approach conservative. We will conduct sensitivity analysis on the choice of such trends. Second, for the three aforementioned endogenous variables we employ a set of instrumental variables that are widely accepted in the literature. To identify the effect of incarceration rates on crime rates we exploit amnesties and collective pardons. Collective clemencies are quite common in Europe (particularly in France and Italy) and lead to a significant release of inmates during certain years for reasons that are mostly political and so are arguably unrelated to crime rates (Barbarino and Mastrobuoni, 2008; Drago *et al.*, 2009). For unemployment, we exploit the interaction between the price of oil and the share of manufacturing in GDP to construct a country-specific shifter of labour demand (Blanchard and Katz, 1992; Raphael and Winter-Ebmer, 2001; Lin, 2008). Finally, for immigration we resort to exogenous supply-push components of migration by nationality as an instrument for the immigrant population in Europe and in the United States (Card, 1990; Angrist and Kugler, 2003; Munshi, 2003; Saiz, 2007).

Despite the limitations deriving from these identifying assumptions and from the additional implicit assumptions about the microstructure underlying our aggregate regressions (discussed in the concluding section), our cross-country analysis is able to shed some light on the different crime trends in Europe and in the United States. The original contribution of the paper is twofold. First, we document the 'reversal of misfortunes', a surprising fact that contrasts with the stereotype of a safe Europe relative to an unsafe America. The literature on crime in Europe typically focuses on a single country and particular case studies (e.g. Bianchi *et al.*, 2011; Drago *et al.*, 2009; Drago and Galbiati, 2010; Buonanno *et al.*, 2009; and Draca *et al.*, 2010), but we are unaware of any cross-country studies of the determinants of recent crime trends in Europe, nor of US-Europe comparisons. Second, we confirm in a cross-country setting previous findings about the effects of incarceration and demographic changes on crime, while we find no evidence in favour of the abortion channel. Using our estimated elasticity of crime rates to prison population (about −0.4) we quantify at 17% the causal contribution of different incarceration policies in Europe and in the United States to the overall reversal. When looking at prop-

erty or violent crimes, this figure becomes 33% and 11%, respectively. As we discuss in more detail below, our estimates are policy relevant and point to potentially important directions for research and policy.

The rest of the paper is organized as follows. Section 2 describes the dataset and discusses the five explanatory variables we use in the analysis. Section 3 illustrates the identification strategy. Results are reported and discussed in Section 4. Section 5 concludes. The paper is complemented by a rich Web Appendix that contains additional data analyses that could not be included here due to space limitations. The Web Appendix is available on line at https://sites.google.com/site/crimeeuropeusreversal/. All data are posted at this journal's website, as well as at the authors' research pages.

2. DATA

2.1. Measuring crime

We collected data on crime and the explanatory variables of interest for seven European countries (Austria, France, Germany, Italy, Spain, the Netherlands and the United Kingdom) and the United States from 1970 to 2008. Our main measure of criminal activity is the total number of offences reported to the police per 1,000 inhabitants. All explanatory variables are also normalized by the size of the population. In addition to focusing on total crime, we distinguish between property and violent crime when possible, and also look (in the Web Appendix) at homicides separately.[3] The classification of property and violent crimes may vary across countries, because of different criminal codes: an act that is a property crime in country A may be classified as a violent crime in country B. For instance, in Italy the police record robbery as a property crime while in many other European countries and in the United States robbery is classified as a violent crime. As a consequence, if one wants to work with a homogeneous measure of crime rates across these different countries, then the total number of crimes (of any kind) recorded by the police is what should be used: this measure of crime minimizes measurement error in a cross-country setting. We will nonetheless, in addition, report results for both property and violent crime separately. Another issue is that reporting rates differ across countries and vary in time in a non-uniform way, as is suggested by comparing data from surveys of victims and from reports to the police (see, for instance, Soares, 2004 and VanDijk *et al.*, 2007). One may wonder whether different and varying reporting rates bias the picture we want to render. This is not a concern

[3] We classify property and violent crimes based on the definitions of the national police or the national statistical office. Typically, 'property crime' includes: any kind of theft, larceny, breaking in, burglary and fraud. 'Violent crime' includes: homicide, serious or aggravated assault, robbery and sexual offences. An aggregate property crime measure is not available for Austria, while it is available only after 1989 for Spain and after 1986 for Germany. Moreover, we were unable to find violent crime data for Austria and Spain over the entire 1970–2008 period.

when doing inference (employing country fixed effects, year fixed effects, and country-specific trends absorbs the resulting variation), but a bias could be present when looking at plain sample statistics.

The Web Appendix expands on these measurement issues. First, we take a separate look at voluntary homicides, which have the same definition everywhere and whose reporting rate is virtually 100% (very few voluntary homicides are not known to the police or misclassified as, for instance, suicides). Second, we correct crime rates using reporting rates from victimization surveys when possible. In both cases we produce evidence consistent with the reversal of misfortunes: this does not seem an artefact of measurement errors.

2.2. Crime in Europe and in the United States: stylized facts

Figures 1–3 reveal four important facts:

- Crime rates in Europe tend to move in parallel;
- Crime rates in Europe increased sharply from 1970 to 1990; the total crime rate stabilized afterwards, with property crimes decreasing since the early 2000s and violent crimes increasing steadily (with a few exceptions);
- Crime rates in the United States increased from 1970 to 1980, have no obvious trend in the 1980s and decline sharply in the 1990s. The rate of decline is less sharp from 2000 onward;
- Crime rates in the United States were above the corresponding rates in Europe in 1970, but they have been below European levels in recent years (with a few exceptions for property crime).

The decline in the US crime rates that began in the 1990s is discussed extensively in the literature. Given the trend in the 1970s and 1980s, this decline was a surprise and puzzled many analysts. According to Levitt (2004), increased incarceration, more police, the decline of crack and the legalization of abortion played an important role in this process. Imrohoroglu *et al.* (2004) offer additional explanations for the decline in property crimes: a higher probability of apprehension, the stronger economy, and a change in the demographic structure – most notably a decline in the demographic weight of young men. The further decline during the last ten years is consistent with these findings. First, the number of police officers in the United States has grown further since the early 2000s. Second, in the United States a substantial fraction of the criminally active population at the end of the 1990s was born prior to the legalization of abortion. The fact that cohorts born after the legalization of abortion reach adulthood is consistent with the decreasing trend if the Donohue and Levitt (2001) explanation is correct. Third, the demographic weight of young Americans has further declined. Fourth, consistently with the slowdown in the US crime trend during the 2000s, crack-related crimes and prison population apparently reached a steady state after the end of the 1990s.

2.3. Five factors that may explain crime trends

As discussed in Section 1, the economics of crime suggests taking into account at least two groups of variables that can explain crime rates. The first group includes factors that directly influence the possibility of committing a crime and its opportunity cost: police numbers, incarceration and unemployment. In particular, police numbers and the prison population affect deterrence and incapacitation, and are under the direct control of policymakers. Unfortunately, we are unable to include police numbers in our analysis for two reasons. First, there is a lack of reliable annual data on police forces in Europe before the mid-1990s. Second, we lack a credible instrumental variable for which adequate data can be put together. The second group includes sociodemographic variables, namely the age structure of the population, and the abortion and immigration rates. The explanatory variables we use are the following: the share of males aged 15–34, the stock of immigrants relative to the population, the abortion rate (defined as the proportion of unborn children who would have been at least 18 years old in a given year over the total population), the unemployment rate and the incarceration rate. In the remainder of this section we discuss in detail what is known about the relation between these variables and crime, and we document their dynamics in our sample.

2.3.1. Demographics. It is well known in criminology that young males are statistically more likely to be offenders than any other demographic group. Levitt and Lochner (2001) note that 18-year-old individuals are five times more likely to be arrested for a property crime in the United States than their 35-year-old counterparts. For violent crime this ratio is 2:1. The same authors document that in 1997 those between 15 and 19 years old constituted 7% of the population but accounted for over 20% of arrests for violent offences. In the light of these facts, the profound demographic change caused in the United States by the ageing of the baby boomers has been considered a potentially important driver in the decline in crime rates in the United States during the 1990s. Levitt (2004) notes that people over 65 have per-capita arrest rates approximately 2% the level of 15- to 19-year-olds, but claims that the change in the age structure of the population played little role in explaining the drop in crime. Imrohoroglu *et al.* (2004) observe that the share of the population between 15 and 25 years old declined from 20.5% to 15.1% in the same period, and claim that this accounts for 11% of the total drop in property crime (the rate per 100 inhabitants in the United States dropped from 5.60 in 1980 to 4.65 in 1996). As for Europe, it is well known that in the period 1970–2008 most countries first experienced an increase in the proportion of young individuals (1970-990) and then a generalized growth in the proportion of seniors, about 10 years after the proportion of seniors picked up in the United States. Figures 4a and 4b illustrate the share of males between 15 and 34 in our sample.

2.3.2. Immigration. There are several reasons why immigrants and natives may have different propensities to engage in crime. Part of this difference reflects the migration process. Immigrants from less developed to more developed countries are typically young, poorly educated and male. This makes them statistically more likely to engage in crime. Thus, if we draw random samples of immigrants and natives in a given country after conditioning on certain socioeconomic characteristics, the two groups would probably have very similar criminal attitudes. In this sense immigration may mechanically affect delinquency rates in receiving countries. Furthermore, natives and immigrants may have a different opportunity cost of crime. For instance, they may face different labour market prospects of working legally, and they may face different probabilities of being convicted and different costs of conviction, for instance because of the possibility of being deported.

Few empirical papers have investigated the relationship between immigration and crime. Using individual data, Butcher and Piehl (1998b, 2005) find that current immigrants in the United States have lower incarceration rates than natives, while the pattern is reversed for the early 1900s (Moehling and Piehl, 2007). Using aggregate data from US metropolitan areas in the 1980s, Butcher and Piehl (1998a) conclude that the inflow of immigrants had no significant impact on crime rates. Finally, Borjas *et al.* (2010) argue that recent immigrants have contributed to the

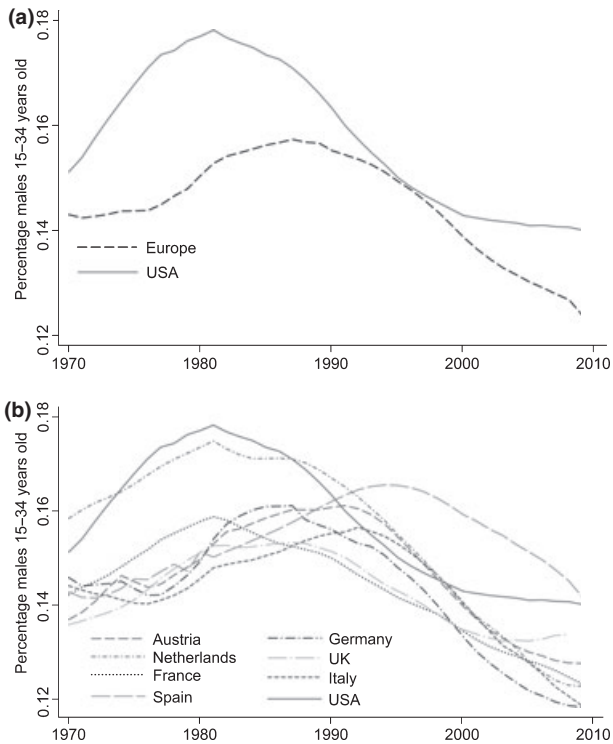

Figure 4. Share of young males (15–34) in the United States and in Europe

criminal activity of native black males by displacing them from the labour market. Evidence for European countries is even scarcer. Bianchi *et al.* (2011) examine the empirical relationship between immigration and crime across Italian provinces during the period 1990–2003. Drawing on police administrative records, they first document that the size of the immigrant population is positively correlated with the incidence of property crimes and with the overall crime rate. Then, using instrumental variables based on immigration toward destination countries other than Italy, they find no evidence of a causal effect of immigration on crime in Italy.

Figure 5 illustrates the dynamics of immigration rates in Europe and the United States. There are heterogeneous patterns in Europe. In particular, while Spain and Italy have experienced a dramatic increase in immigration since the early 1990s, in the Netherlands and in Germany the proportion of immigrants grew more modestly.

2.3.3. Abortion. According to Donohue and Levitt (2001), the legalization of abortion in the United States following the *Roe v. Wade* Supreme Court decision (410 US 113, 1973) had a causal impact on crime via the high number of abortions performed in the United States just after the Supreme Court decision. The underlying (and quite controversial) theory is that unwanted children are at greater risk of committing crime. Therefore, the legalization of abortion ultimately reduced the

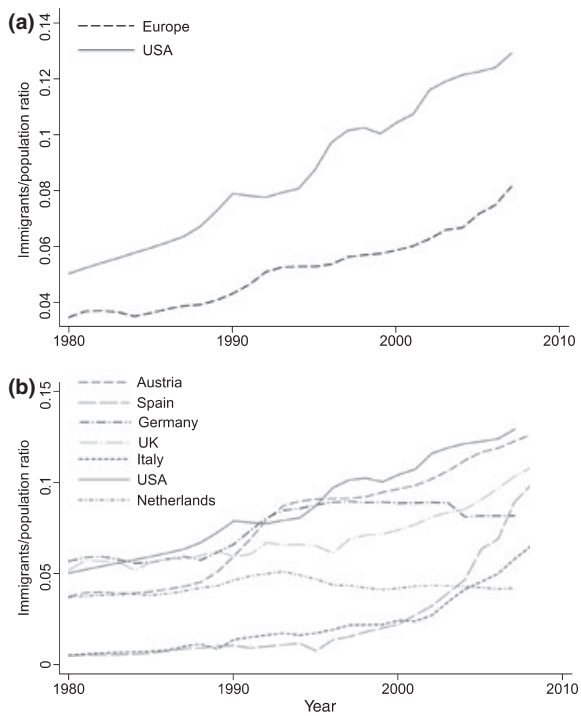

Figure 5. Immigration rates in the United States and Europe

birth of children who, had they been born, would have been at greater risk of committing crimes when they reached their teenage years. Levitt (2004) refers to various studies supporting this hypothesis and showing that children born because their mothers were denied abortion were substantially more likely to be involved in crime, even when controlling for the income, age, education and health of the mother. Further recent evidence supports this idea by showing that the legalization of abortion reduced out-of-wedlock teen childbearing (Donohue *et al.*, 2009). A lively debate followed the article by Donohue and Levitt (2001),[4] but the key findings of the original article have so far proved robust. A discussion of the abortion hypothesis is beyond the scope of this paper but the extension of the analysis to our panel of European countries is important to provide further evidence along this line of research.

In Europe the legalization of abortion did not follow a uniform pattern. Some countries in our dataset legalized abortion in the 1970s, as did the United States, others later.[5] To date, to the best of our knowledge, the only papers studying the relationship between abortion and crime in European countries are Pop-Eleches (2006) and Kahane *et al.* (2007), who focus on Romania and on England and Wales, respectively. The evidence from these studies is mixed. Pop-Eleches (2006) exploits abortion bans during the Ceausescu era in Romania as a source of identification and finds results in line with Donohue and Levitt (2001), while Kahane *et al.* (2007) employ panel data and show that Donohue and Levitt's result does not hold in England and Wales when one uses a different abortion index.

Figures 6 and 7 illustrate the relevance of abortions in the United States and Europe. Figure 6 reports the number of abortions relative to (roughly) the number of pregnancies. The abortion rate in the United States is always higher than in Europe as an aggregate, except after the early 2000s. In the first decade after the *Roe v. Wade* decision in the United States we observe a dramatic jump in the number of abortions leading to a ratio of abortions over the overall number of pregnancies of about one-fourth. In Europe the initial increase is less dramatic due to the staggered legalization in the continent. Figure 7 shows that the number of unborn children that would have been adult in a given year starts growing in the 1990s both in the United States and in Europe. In the empirical analysis we will construct the share of aborted adults using an approach similar to that of Donohue and Levitt (2001). The only important difference is that, due to data limitations, we cannot weight abortion rates by the percentage of arrests in a cohort over the total crime in a given country for a benchmark year.

[4] See Foote and Goetz (2008), Joyce (2004) and Donohue and Levitt (2004 and 2008).

[5] The UK legalized abortion in 1968, France and Austria in 1975, West Germany in 1976, Italy in 1978, the Netherlands in 1980 and Spain in 1985, but only for mental or physical danger within the 12th week. Abortion for other reasons was fully legalized in Spain only in 2009.

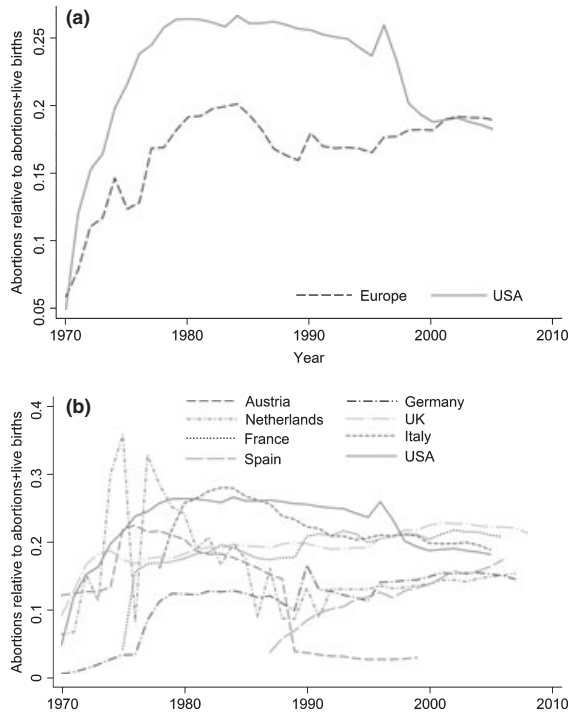

Figure 6. Abortion rates in the United States and Europe

2.3.4. Unemployment. According to the benchmark economic model of crime (Becker, 1968; Ehrlich, 1973), labour market opportunities may affect a rational individual's decision to engage in crime: if legal income opportunities are less lucrative than the expected gains from crime, individuals will opt for the latter. Therefore, unemployment may lead to an increase in crime through two channels. First, the expected returns from legal work decrease if the probability of being unemployed is higher. Second, given a downward sloping labour demand curve, more unemployment is associated with a lower wage rate. These two effects contribute to reducing the opportunity cost of crime. Hence, we can expect unemployment to have a positive effect on the crime rate. Recent empirical studies employing panel data at the state or regional level (Raphael and Winter-Ebmer, 2001; Gould *et al.*, 2002; Lin, 2008; Oster and Agell, 2007; Fougere *et al.*, 2009) reach a consensus that increasing unemployment contributes to an increase in property crimes (although the magnitude is not large) and does not significantly affect violent crimes. Imrohoroglu *et al.* (2004), on the contrary, conclude that the effect of unemployment on crime is negligible. Figure 8 reports the dynamics of the unemployment rates for the eight countries under investigation.

2.3.5. Incarceration. Of all the explanatory variables we are considering, the most striking difference between Europe and the United States pertains to incarceration

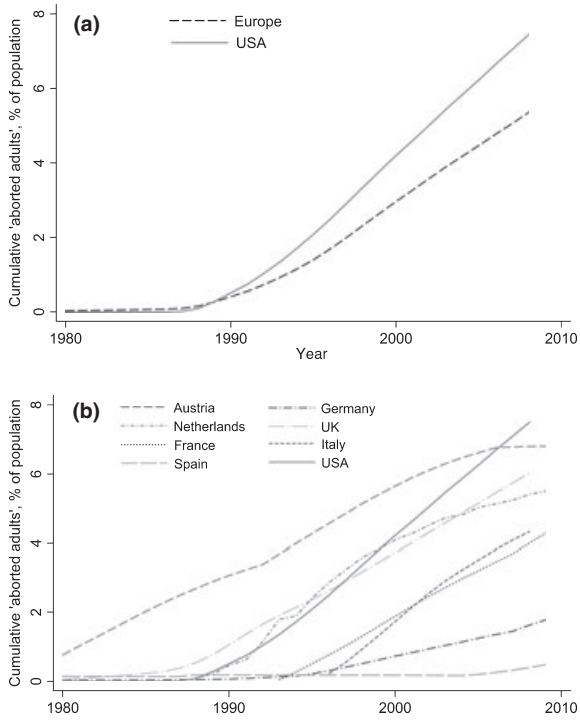

Figure 7. Aborted children who would have been adults in a given year

rates. This fact is illustrated in Figure 9. Between 1970 and 2008 the prison population per 1,000 inhabitants increased by a factor of more than 4.5 in the United States and by a factor of 3 in Europe, but there is a dramatic difference in levels. Back in 1970 the US/Europe ratio between incarceration rates was 4. Between 2007 and 2008 it was an astonishing 7. Although there are different patterns across the seven European countries we are considering and although the dynamic is similar to the United States, nowhere in Europe are incarceration rates comparable to

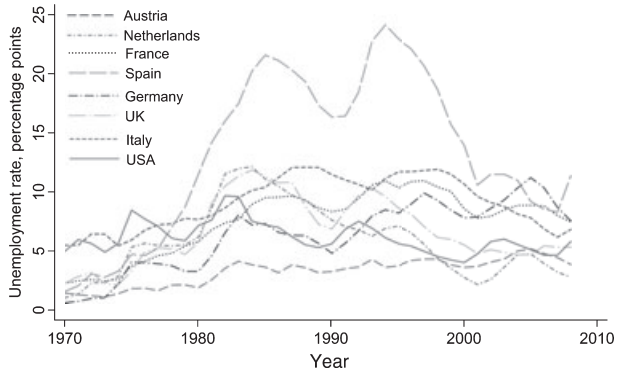

Figure 8. Unemployment rates in the United States and Europe

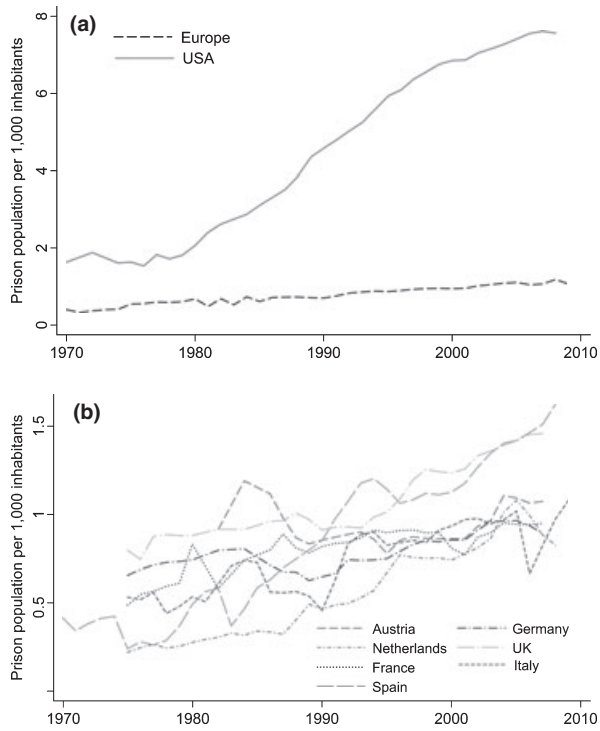

Figure 9. Incarceration in the United States and in Europe

what we see in America. According to Levitt (2004), the prison population was one of the main factors explaining the decline in crime rates in the United States during the 1990s: this variable accounts for 12% of the reduction in homicides and violent crimes from 1991 to 2001. The impact of the prison population on crime rates should be interpreted as the sum of two effects: deterrence (a large prison population implies a high probability of incarceration for potential criminals) and incapacitation (people who are locked up cannot commit crimes). Although it is not possible to distinguish between deterrence and incapacitation in a reduced-form framework, the effect of the prison population is of interest because the severity of punishment is a variable that can be manipulated by policymakers.

3. EMPIRICAL STRATEGY

In order to understand how the different factors reported above might have affected crime rates and the 'reversal of misfortunes', we estimate the following model:

$$\ln(crime_{it}) = \alpha_i + \beta \ln(\mathbf{x}_{it}) + \mathbf{t} d_i + \lambda_t + \varepsilon_{it}$$

where $crime_{it}$ is some measure of the crime rate (total, property, or violent) in country i at time t, α_i is a country fixed effect, $\mathbf{x}_{it}$ contains explanatory variables for

country i at time t (a constant, the share of males between 15 and 34 years old in the population, the immigration rate, the share of potential adults aborted, the unemployment rate, and the incarceration rate), $\mathbf{t}$ is a polynomial in time, d_i is a country dummy (so that $\mathbf{t}d_i$ is a country-specific deterministic trend), and λ_t is a common year dummy. By taking logs of all variables we will be able to interpret coefficients as elasticities with respect to the relevant rates.

Unlike unemployment, the immigration rate and incarceration, abortion rates and the age structure of the population are determined at least 15 years earlier than actual crime rates. We maintain the assumption that these two variables are exogenous with respect to crime after including country-specific time trends and year and country fixed effects. Abortion rates are computed as follows. We first calculate the annual abortion rate as a proportion of unborn children who would have been at least 18 years old in a given year over the total population. The impact of the variation in abortion rates for a given cohort plausibly depends on the extent of criminal behaviour within that cohort.

For this reason, Donohue and Levitt's measure of the abortion rate is computed as the sum of the abortion rate of each age group weighted by the percentage of arrests in that cohort over the total crime in the United States for a benchmark year. Unfortunately, crime data by cohort are not available for Europe. Therefore, we use unweighted abortion rates. This produces a particular abortion rate ($abort_{ikt}$) in country i for any age k, in the current year, t. It is important to understand that this particular abortion rate measures the number of aborted children in the past that would have been adult in year t. A causal effect is identified by the within country variation in access to abortion.

All other variables are endogenous to crime rates and for each of them we adopt an instrumental variable strategy, described in detail in the remainder of this section.

3.1. Immigration

Crime and immigration are likely to be simultaneously determined in equilibrium, hence the identification problem. Following Card (2001), researchers have constructed outcome-based measures of supply-push factors using total migration inflows by nationality toward the destination country of interest. Examples of this approach are Ottaviano and Peri (2006), Cortes (2008), and Card (2009). The idea is that new immigrants of a given nationality tend to settle in the same areas as previous immigrants from the same country (e.g. Munshi, 2003; Jaeger, 2006; McKenzie and Rapoport, 2007). This approach, however, has some limitations. To mention one, total inflows by nationality may be correlated with local demand-pull factors, including those originating in the 'crime market'. If all immigrants from a given country moved to the same receiving country, it would be impossible to disentangle push and pull factors based on total inflows by nationality. Bianchi *et al.* (2011), in an application to Italy, solve this problem by using a measure of supply-

push factors based on bilateral migration inflows toward destination countries other than Italy. We follow a similar route and resort to an exogenous supply-push component of migration by nationality as an instrument for shifts in the immigrant population across European countries and the United States. Supply-push factors are all the events in origin countries that increase the propensity to migrate, such as civil wars, ethnic wars, ethnic violence, economic crises, political turmoil and natural disasters (Card, 1990; Angrist and Kruger, 2003; Munshi, 2003; Saiz, 2007). As an exogenous measure of supply-push factors by origin country one would ideally use data on all the origin countries by destination countries. This is virtually impossible to do for all countries, but it can be done for a relevant subset of them. We have built an original database containing information on civil wars, ethnic wars and ethnic violence for 38 origin countries which are heavily represented in European countries and in the United States. Specifically, we consider the top ten sending countries for each European country and the United States. Since the top ten sending countries tend to overlap, in particular for European countries, the final number of origin countries considered is 38. The list of origin and destination countries used to construct the instrument is reported in the Web Appendix.

Identification exploits an instrumental variable labelled *IVWar*. For each destination country, we restrict our sample to the period 1980 to 2008, since data on the immigrant stock are not available for all countries in our sample before 1980. Moreover, we exclude France because data on the stock of immigrants are available only every 5 years. We define the top sending countries, and for each sending country and year we build a dummy variable 'War' which assumes value 1 if a civil war, ethnic war or ethnic violence took place in the sending country in that year, and zero otherwise. For example, if a civil war, ethnic war or ethnic violence took place in Kenya in the year 2000 the dummy 'War' will assume value 1 for every receiving country that has Kenya as a top-10 sending country. We then generate our instruments as follows:

$$IVWar_{it} = \Sigma_j (pop_{jt}/pop_{it}) War_{ijt}$$

where i, j and t index respectively receiving country, sending countries and year, while *pop* is country population. The crucial identifying assumption is that this measure of the supply-push component in origin countries is uncorrelated with unobservable determinants of crime rates in the receiving country. To the extent that the events defining variable *War* are exogenous to receiving countries (as they arguably are) this is not a concern.

3.2. Unemployment

Following Raphael and Winter-Ebmer (2001) and Lin (2008) we employ a state-specific measure of oil price shocks as an exogenous shift for unemployment. The relation

between the unemployment rate and the price of oil has been discussed and analysed by Blanchard and Katz (1992), among others. This approach allows us to isolate the exogenous (with respect to crime) component of variations in unemployment in the data and so to identify a causal effect of unemployment on crime. The instrument is constructed as follows. For each country and each year, we define the proportion of employment in the manufacturing sector. This allows us to roughly measure the relevance of energy-intensive industries. Next, by using the world price of crude oil (spot price, West Texas Intermediate), we construct a measure of state-specific exposure to oil shocks by interacting the proportion of employment in manufacturing and the price of oil. The idea behind this instrument is simple. Since there are no short-run substitutes for energy in manufacturing and since the price of oil is presumably unaffected by the economic activity of the eight countries in our panel, any variation in the price of oil generates an exogenous variation in unemployment – manufacturers must reduce their consumption of energy by reducing output and employment. The identifying assumption is that such shocks do not affect crime directly.

3.3. Incarceration

The incarceration rate is endogenous because it may simply reflect the extent of crime: the more people engage in crime the larger the prison population. Ideally, one would like to exploit quasi-experiments that exogenously alter the imprisonment rate. This is what Levitt (1996), Drago *et al.* (2009) and Barbarino and Mastrobuoni (2008) do for the United States and Italy, respectively. Levitt uses prison overcrowding litigation: court decisions in the United States may limit the growth rates of inmates in state prisons for reasons that have nothing to do with the incidence of crime. This generates an exogenous source of variation in the prison population relative to control states. Drago *et al.* (2009) use a collective pardon implemented in Italy in 2006 to reduce the population in overcrowded prisons to identify the deterrent effect of an increase in expected prison sentences.[6] Barbarino and Mastrobuoni (2008) exploit a series of prison amnesties in Italy to estimate the incapacitation effect of prison. These studies have the advantage of exploiting credible sources of exogenous variation in the prison population or expected prison sentences. However, they refer to single countries.

We follow this line of research and exploit amnesties across different countries to construct an instrument for prison population. Specifically, we first consider prison population lagged by one year – the number of inmates in prison statistics refers to the stock at the end of a given year. Therefore, our aim is to identify the effect of the prison population at time t–1 on crime rates at time t. The prison

[6] The collective pardon bill commuted actual sentences into expected sentences for those with less than three years of residual sentence. Thus, by exploiting the plausible exogeneity of residual sentences, Drago *et al.* (2009) can provide an estimate of the deterrent effect of prison sentences.

population at time t–1 in a given country is instrumented with a dummy that is equal to one if in the same year (t–1) an amnesty was passed in that country. The identifying assumption is that an amnesty affects crime rates only via the induced variation in prison population. We believe this assumption is credible. Many of the collective pardons and amnesties implemented by European countries between 1970 and 2008 were officially motivated by either political or humanitarian reasons. In many cases this is unquestionably so, as with the three consecutive amnesties approved in Spain after the end of the Franco dictatorship. However, one might suspect that in some cases such official motivations mask attempts to quickly reduce prison overcrowding. If this were the case, then amnesties would be predictable by looking at the past evolution of crime rates. Below we present evidence that this is not the case. We first report in Table 1 the timing and description of the amnesties used in our dataset. The official motivations seem to be unrelated to crime rates. As supporting evidence of this exclusion restriction, we report in Table 2 the results a regression of amnesties at time t on crime rates in earlier years. A significant relationship between past crime rates and amnesties would cast doubts on the validity of the identification strategy: it would suggest that amnesties are determined by or correlated to past trends in criminal activity. Table 2, however, shows that crime rates in earlier years do not systematically predict amnesties. This is also true when we include crime rates with three lags. Given that in some countries amnesties have occasionally been passed every other year, it is reassuring that crime rates one year, and two and three years earlier together do not predict amnesties.

As in all instrumental variable estimates, the elasticity of crime rates to prison population should be interpreted as a local average treatment effect (LATE). This raises the question of whether our results can be generalized to countries (such as the Netherlands, the United Kingdom and the United States) where no amnesty was passed during the 40 years on which we focus. An additional issue is that the only logically possible effect of amnesties is a reduction in the prison population, while the interpretation of the elasticity we estimate is two-sided (it includes a deterrence and an incapacitation effect). While these problems arise commonly when employing instrumental variables, it is important to keep them in mind when attempting out-of-sample policy exercises.

3.4. Country specific time trends

It is common in longitudinal analyses to include a time trend to account for the possible exogenous dynamics of the dependent variable of interest. Without doing so, such dynamics could be wrongly interpreted as caused by some explanatory variable that moves along a trend correlated (because of other underlying forces) with the trend of the dependent variable. In other words, a time trend removes possible spurious correlations. This comes at a price, though: superimposing an exogenous

Table 1. Timing and description of collective clemencies

Country	Year	Description
Austria	1995	Collective pardon for the 50th anniversary of the restoration of independence.
France	1980,1985	Collective graces, providing for a reduction of 15 days of the remaining time to be spent in prison for each month of original sentence.
	1981,1988,1995	Presidential amnesties for the election or re-election of a president.
Germany	1996, 1997	Pardons granted to prisoners after serving half of their sentences for failure to pay a fine.
Italy	1970	Amnesty of five years for crimes concerning the illegal holding of fire arms and violation of custom laws. Three years amnesty for theft and property crimes. Two years pardon for crimes covered by the military penal code.
	1978	Two years pardon for theft, crimes related to public health and sex crimes.
	1981	Two years pardon for theft, crimes related to public health and sex crimes. Three years' amnesty for non-financial crimes with the exclusion of recidivists and criminals sentenced to more than 10 years.
	1986	One-year pardon for theft, crimes related to public health and sex crimes. Three years' amnesty for non-financial crimes with the exclusion of recidivists and criminals sentenced to more than 10 years.
	1990	Four years' amnesty for crimes related to the illegal holding of arms, crimes related to strikes, crimes committed by criminals under age 18, crimes related to the tobacco monopoly.
	2003	Two years' pardon for criminals who had already served at least half of their original sentence with the exclusion of recidivists and professional criminals.
	2006	Three year pardon with the exclusion of crimes related to the mafia, paedophilia and terrorism.
Spain	1975	First amnesty for political prisoners under Franco's dictatorship, on the occasion of the coronation of the King of Spain.
	1976	Second amnesty for political prisoners under Franco's dictatorship.
	1977	Third amnesty for political prisoners under Franco's dictatorship.

time trend may remove genuine correlation as well, that is, produce overfitting. We prefer to take a conservative approach and pay this price. This is even more important in a cross-country study, where trends may be country-specific. The question remains of which trend is the appropriate one.

To estimate the causal effects of our independent variables, we include in the basic specifications a country-specific deterministic quartic trend, that is, a polynomial of degree four in time with country-specific coefficients. Such a specification strikes a reasonable balance between the need for flexibility in time trends and degrees of freedom. In particular, a quartic trend allows us to avoid bias in the

Table 2. Effect of earlier crime rates on amnesties

	1	2	3	4	5
Log total crime ($t-1$)	0.05	0.24	0.40		
	(0.20)	(0.28)	(0.31)		
Log total crime ($t-2$)		−0.18	−0.50		
		(0.26)	(0.36)		
Log total crime ($t-3$)			0.50*		
			(0.27)		
Log property crime ($t-1$)				0.43	
				(0.40)	
Log property crime ($t-2$)				−0.84	
				(0.52)	
Log property crime ($t-3$)				0.80**	
				(0.40)	
Log violent crime ($t-1$)					−0.70*
					(0.35)
Log violent crime ($t-2$)					0.54
					(0.39)
Log violent crime ($t-3$)					−0.16
					(0.34)
F-stat on lagged crime rates	0.05	0.40	1.33	1.34	1.38
Obs.	282	274	266	213	182
Countries	8	8	8	7	6

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses.

Significance levels: * 10%; ** 5%.

estimation of crime growth at the beginning and at the end of the period under investigation. This problem was discussed in the empirical labour literature by Murphy and Welch (1990), who showed that a quartic trend outperforms quadratic and cubic specifications when fitting life-cycle earnings profiles: quadratic and cubic specifications generate substantial spurious growth or fall particularly at the beginning and the end of a worker's career. Figure 10 suggests that the same problem arises, *mutatis mutandis*, in our panel. This figure shows, for each country we consider, the residuals from a regression of the total crime rate on a polynomial function of time: first (solid), second (dots and dashes), third (dots), and fourth order (dashes), respectively. Although a common choice elsewhere in the crime literature, a linear trend would be a very poor choice for the case at hand as it performs quite badly relative to non-linear (in time) specifications, not surprisingly. While quadratic and cubic trends improve a lot over the linear specification, they are outperformed by the quartic trend, most notably at the edges of the time interval we are considering. Country-specific quartic trends are illustrated in Figure 11. Although we do not know exactly what a quartic deterministic trend removes in this case (which is why we stress we are taking a conservative approach) the inclusion of country-specific trends and common year dummies improves our estimates in two distinct senses. First, only deviations of crime from such trends are left to be explained. This raises the bar of the explanatory power required of the regressors. Second, it

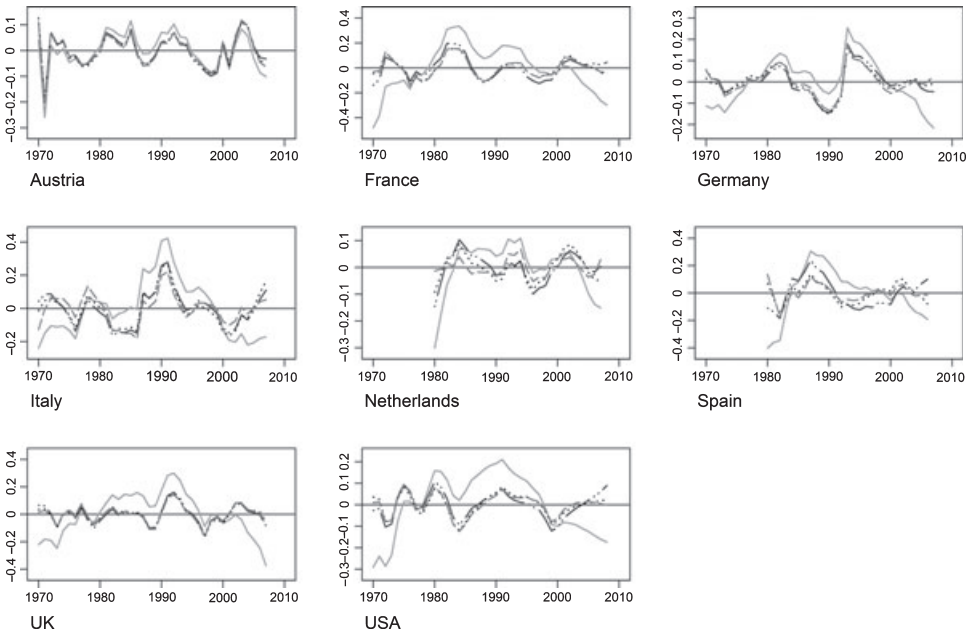

Figure 10. Residuals from polynomial time trends, total crime

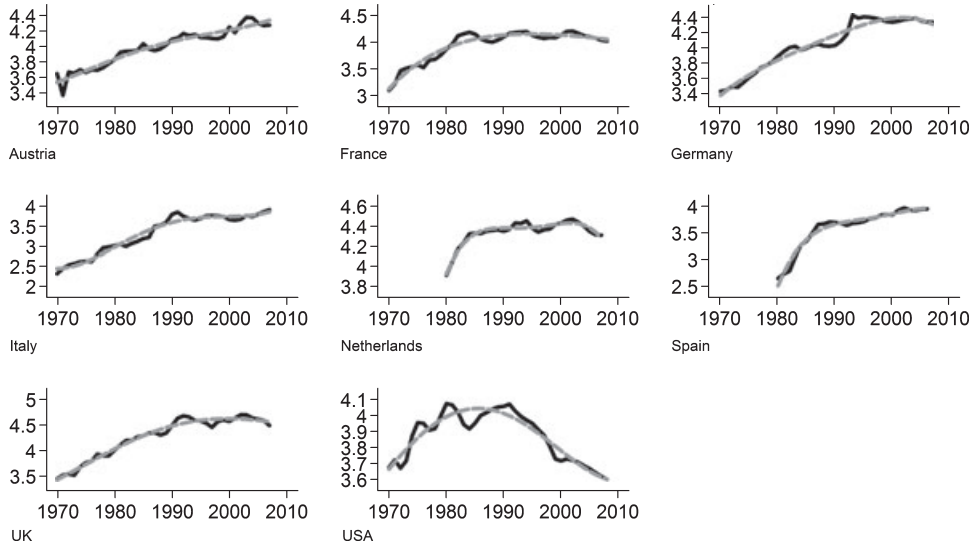

Figure 11. Quartic country-specific trends, total crime

improves the precision of the estimates. A thorough analysis of time effects in our sample (e.g. into whether the trend is in fact deterministic or stochastic, or the cointegration relation between the variables we use) goes beyond the scope of the paper.

In the Web Appendix we further discuss the issue of country-specific time trends. In particular, we perform a sensitivity analysis on the inclusion of such trends or not, and on the order of the polynomial in time when included. It turns out that, in general, our main result is robust to including or not a country-specific trend, and to the type of trend included (linear, quadratic, cubic, or quartic). The choice of a quartic trend actually produces estimates at the lower end of the range for total and property crimes.

4. RESULTS

Our results are reported in Tables 3 and 4. Results for total crime are reported in Tables 3a and 4a, and results on property and violent crime are reported in Tables 3b and 4b, and 3c and 4c, respectively. Odd and even columns report the results for the full sample and for Europe only, respectively. To understand the bias of the OLS relative to the IV estimates, Table 3 reports OLS estimates and Table 4 IV estimates.

4.1. OLS estimates

For the reasons discussed above, one cannot interpret the OLS coefficients as causal effects. However, it is informative to look at them. Tables 3a–3c show that, when using OLS, incarceration has a negative and statistically significant impact on crime across different specifications. This coefficient should be downward biased if higher crime rates are associated with higher incarceration rates. These estimates also show that the effect of age structure is positive and statistically significant,

Table 3a. OLS estimates, total crime

	1	2	3	4	5	6	7	8
ln(*incarceration*)	−0.33***	−0.33***	−0.32***	−0.32***	−0.29***	−0.29***	−0.26***	−0.25***
	(0.05)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)
ln(*abortion*)	0.00	−0.00	0.00	−0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
ln(*males 15–34*)	1.76**	1.79*	1.69**	1.68*	2.91***	2.84**	2.86***	2.92**
	(0.79)	(1.00)	(0.79)	(1.00)	(1.03)	(1.24)	(1.00)	(1.21)
ln(*unemp. rate*)			0.07*	0.06			0.12***	0.14***
			(0.04)	(0.05)			(0.04)	(0.05)
ln(*immigration*)					0.28***	.029***	0.30***	0.30***
					(0.07)	(0.07)	(0.06)	(0.07)
Sample	Full	Europe	Full	Europe	Full	Europe	Full	Europe
Observations	239	203	239	203	189	161	189	161
Countries	8	7	8	7	8	7	8	7

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses.

Significance levels: * 10%; ** 5%; *** 1%.

Table 3b. OLS estimates, property crime

	1	2	3	4	5	6	7	8
ln(*incarceration*)	−0.45***	−0.49***	−0.44***	−0.47***	−0.38***	−0.42***	−0.38***	−0.41***
	(0.06)	(0.07)	(0.06)	(0.07)	(0.08)	(0.09)	(0.08)	(0.09)
ln(*abortion*)	0.00	−0.00	0.00	0.00	0.00	−0.00	0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
ln(*males 15–34*)	0.22	1.65	0.48	2.54	1.39	−0.08	1.93	0.83
	(1.06)	(1.69)	(1.05)	(1.71)	(1.78)	(2.27)	(1.87)	(2.47)
ln(*unemp. rate*)			0.08*	0.11**			0.05	0.06
			(0.04)	(0.05)			(0.05)	(0.06)
ln(*immigration*)					0.03	−0.05	0.05	−0.03
					(0.09)	(0.10)	(0.09)	(0.10)
Sample	Full	Europe	Full	Europe	Full	Europe	Full	Europe
Observations	207	171	207	171	156	128	156	128
Countries	7	6	7	6	7	6	7	6

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses.

Significance levels: * 10%; ** 5%; *** 1%.

Table 3c. OLS estimates, violent crime

	1	2	3	4	5	6	7	8
ln(*incarceration*)	−0.00	−0.03	−0.00	−0.04	0.19**	0.18*	0.19**	0.15
	(0.09)	(0.08)	(0.09)	(0.08)	(0.09)	(0.10)	(0.09)	(0.11)
ln(*abortion*)	0.00	−0.00	0.00	−0.00	−0.01	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ln(*males 15–34*)	−1.57	7.21***	−1.53	6.82***	4.59**	7.01**	4.26**	5.27*
	(1.44)	(2.05)	(1.48)	(2.20)	(1.91)	(2.62)	(2.03)	(3.00)
ln(*unemp. rate*)			0.00	−0.03			−0.03	−0.10
			(0.06)	(0.07)			(0.06)	(0.09)
ln(*immigration*)					−0.01	0.04	−0.02	−0.01
					(0.13)	(0.17)	(0.14)	(0.17)
Sample	Full	Europe	Full	Europe	Full	Europe	Full	Europe
Observations	174	138	174	138	123	95	123	95
Countries	6	5	6	5	6	5	6	5

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses.

Significance levels: * 10%; ** 5%; *** 1%.

whereas the coefficient on abortion is close to zero and imprecisely estimated. Given that the age structure and abortion are predetermined variables, the only bias on the estimated coefficients in these specifications derives from the inclusion of other endogenous variables such as incarceration, unemployment and immigration. The coefficients on immigration are positive and precisely estimated only for total crime (Table 3a), whereas the results on unemployment are somewhat mixed. Unlike incarceration however, it is difficult to establish the sign of the possible bias in the coefficients of unemployment and immigration.

Table 4a. IV estimates, total crime

	1	2	3	4	5	6	7	8
ln(*incarceration*)	−0.37***	−0.44***	−0.36**	−0.34	−0.32*	−0.40**	−0.43*	−0.43**
	(0.14)	(0.14)	(0.15)	(0.48)	(0.18)	(0.19)	(0.25)	(0.20)
ln(*abortion*)	0.00	−0.00	0.00	0.01	0.00	0.00	0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
ln(*males 15–34*)	1.78***	1.83**	1.29	−1.35	4.26***	4.60***	5.06**	4.66***
	(0.67)	(0.84)	(0.80)	(5.85)	(1.29)	(1.51)	(2.06)	(1.60)
ln(*unemp. rate*)			0.33*	1.92			−0.18	−0.08
			(0.19)	(3.11)			(0.27)	(0.21)
ln(*immigration*)					−0.27	−0.32	−0.56	−0.36
					(0.25)	(0.27)	(0.58)	(0.31)
Weak instruments:								
First-stage *F*	17.88	15.87						
Cragg–Donald			3.98	0.36	1.78	1.77	0.32	0.99
Stock–Yogo critical value (10% max IV size)			7.03	7.03	7.03	7.03	>7.03	>7.03
Sample	Full	Europe	Full	Europe	Full	Europe	Full	Europe
Observations	239	203	236	202	188	161	188	161
Countries	8	7	8	7	8	7	8	7

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses. The endogenous variables instrumented are prison population, unemployment, and migration. The IVs are amnesties, IV-war, and the interaction between the price of oil and industry share.

Significance levels: * 10%; ** 5%; *** 1%.

Table 4b. IV estimates, property crimes

	1	2	3	4	5	6	7	8
ln(*incarceration*)	−0.28**	−0.38***	−0.23	−0.55	−0.76	−1.61	−1.33	−1.68
	(0.14)	(0.14)	(0.16)	(2.18)	(1.00)	(2.76)	(2.47)	(2.86)
ln(*abortion*)	0.00	−0.00	0.00	−0.03	0.00	−0.03	−0.00	−0.03
	(0.00)	(0.00)	(0.01)	(0.16)	(0.02)	(0.05)	(0.04)	(0.07)
ln(*males 15–34*)	0.02	1.34	0.88	−60.33	7.60	7.06	8.74	4.16
	(0.91)	(1.41)	(1.64)	(329.58)	(10.68)	(21.74)	(20.01)	(30.85)
ln(*unemp. rate*)			0.15	−5.95			−0.45	−0.20
			(0.28)	(32.13)			(1.28)	(1.20)
ln(*immigration*)					−2.86	−5.97	−5.41	−6.31
					(3.17)	(9.30)	(10.30)	(10.00)
Weak instruments:								
First-stage *F*	25.81	19.42						
Cragg–Donald			0.85	0.03	0.07	0.30	0.05	0.05
Stock–Yogo critical value (10% max IV size)			7.03	7.03	7.03	7.03	>7.03	>7.03
Sample	Full	Europe	Full	Europe	Full	Europe	Full	Europe
Observations	207	171	200	166	152	125	152	125
Countries	7	6	7	6	7	6	7	6

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses. The endogenous variables instrumented are prison population, unemployment, and migration. The IVs are amnesties, IV-war, and the interaction between the price of oil and industry share.

Significance levels: * 10%; ** 5%; *** 1%.

Table 4c. IV estimates, violent crimes

	1	2	3	4	5	6	7	8
ln(*incarceration*)	−0.48**	−0.37**	−0.58	−0.37**	−0.37	−3.67	−1.55	−3.98
	(0.21)	(0.17)	(0.36)	(0.19)	(0.60)	(86.91)	(5.08)	(164.67)
ln(*abortion*)	0.01	0.00	0.01	0.00	−0.01	−0.15	−0.04	−0.16
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(3.44)	(0.10)	(6.29)
ln(*males 15–34*)	−1.03	7.55***	3.42	8.81	9.56	11.40	16.17	12.55
	(1.32)	(1.73)	(3.52)	(10.76)	(6.09)	(90.75)	(36.19)	(383.33)
ln(*unemp. rate*)			0.77*	0.11			−0.62	0.05
			(0.44)	(0.82)			(2.17)	(12.09)
ln(*immigration*)					−1.30	−9.13	−6.27	−9.85
					(1.73)	(222.41)	(20.51)	(404.22)
Weak instruments:								
First-stage *F*	18.81	14.49						
Cragg–Donald			1.29	2.51	0.27	0.05	0.00	0.00
Stock–Yogo critical value (10% max IV size)			7.03	7.03	7.03	7.03	>7.03	>7.03
Sample	Full	Europe	Full	Europe	Full	Europe	Full	Europe
Observations	174	138	170	136	122	95	122	95
Countries	6	5	6	5	6	5	6	5

Notes: All specifications include country fixed effects, year fixed effects and a quartic country specific time trend. Standard errors in parentheses. The endogenous variables instrumented are prison population, unemployment, and migration. The IVs are amnesties, IV-war, and the interaction between the price of oil and industry share.

Significance levels: * 10%; ** 5%; *** 1%.

4.2. IV estimates

Tables 4a–4c report IV (2SLS) estimates, paralleling the OLS estimates reported in Tables 3a–3c. Checking the strength of the instruments is crucial, because a weak instrument may induce a bias similar to the OLS bias and reduce efficiency at the same time. In the case of a single endogenous variable (i.e. columns 1 and 2), the appropriate test is the standard first-stage F test on excluded instruments. The test shows that amnesties, with an F well above 10, are a remarkably strong instrument. First-stage estimates (not reported) indicate that the average effect of an amnesty in our sample is to reduce the incarceration rate by about 13% in the year the amnesty is passed. However, with $n > 1$ endogenous variables and n instruments we have n first-stage regressions. In this case the rule of thumb formalized by Staiger and Stock (1997) – that is, $F > 10$ – can no longer be applied, and the appropriate test is the one developed by Stock and Yogo (2002). This is a generalization of the univariate F test on excluded instruments to the multivariate case, based on the Cragg–Donald statistics. As a benchmark to interpret the magnitude of the Cragg–Donald statistics, notice that this is approximately equal to the first-stage F statistic on excluded instruments in the case of a single endogenous variable. The Stock–Yogo test immediately reveals that the instrument for unemployment and (even more) the instrument for immigration are very weak: we can never reject the

null hypothesis of weak instruments when including either of these two variables. The coefficients (and all coefficients) from specifications including these two variables are therefore unreliable as estimates of causal effects: even if the exclusion restrictions hold the point estimates are not credible because the model is very weakly identified. This also leads to very imprecise point estimates. An additional reason for this imprecision is that data on immigration are available only from 1980 onwards: when using them, we lose about 30% of the observations. Similar specifications (not reported) with a single endogenous regressor (either immigration or unemployment) confirm the weakness of these two instruments: the first-stage F statistic in this case is equal to 1.5 (immigration) and 5 (unemployment). The only specification that provides credible causal effects is, therefore, the one in columns 1–2 of Tables 4a–4c, where in addition to identifying the elasticity of crime to incarceration we identify the causal effect of abortion and age structure. This is our preferred specification both because it avoids weak instruments issues and because it considers the three factors (incarceration, abortion and age structure) that according to Levitt (2004) and Imrohoroglu *et al.* (2004) are among the main factors behind the drop in crime rates in the United States in the 1990s.

We deliberately choose to include in the regression only plausibly exogenous variables (or instrumented endogenous variables). For example, despite the fact the real GDP per-capita is strongly correlated to all economic fundamentals that, according to the standard economic model of crime, potentially affect crime (e.g. poverty, business cycle, property rights, human development, and even happiness), this is not included in our regressions because it is potentially endogenous to crime rates. It is worth noting, however, that our results are essentially unchanged when we include GDP as a control. This is illustrated in the Web Appendix.

In the remainder of this section we discuss the estimates from our preferred specification in more detail.

4.2.1. Incarceration. As mentioned above, first-stage estimates (not reported) indicate that the effect of amnesties on the prison population is large and significant. Passing one amnesty in one year leads to a reduction in the prison population rate of about 13% in our preferred specification. Comparing the estimates on incarceration in Tables 3 and 4, most of the OLS coefficients are generally lower than the corresponding IV, suggesting that OLS underestimate the effect of the prison population on crime, as expected. The exception is the elasticity of property crime to incarceration, which turns out to be upward biased when using OLS. Tables 4a–4c show that the elasticity of total crime per capita to the incarceration rate, using IV in our preferred specification, is −0.37 for the full sample and −0.44 for Europe only. A similar pattern is found for property crime (−0.28 for the full sample and −0.38 for Europe only) and for violent crime (−0.48 for the full sample and −0.37 for Europe only). As discussed above, superimposing a quartic country-specific trend is a conservative approach: in fact, had we not employed such trends the coef-

ficients on incarceration rates would have been larger, except for violent crimes (see Web Appendix). Furthermore, our estimate of the effect of incarceration is robust to excluding abortion and demographics from the regression (results not reported). This robustness confirms the presumption that amnesties do provide a source of exogenous variation for incarceration rates. Overall, our results on incarceration are in line with previous estimates by Levitt (1996), who exploits overcrowding litigation across US states as an instrument and finds an elasticity in the range −0.3 to −0.4.

We perform a simple back-of-the-envelope calculation to quantify the contribution of the prison population to the 'reversal of misfortunes'. The calculation works as follows. Start with the crime rates in Europe and in the United States in 1970, and simulate the dynamics of crime using incarceration as the only explanatory variable, with the elasticities reported in the first column of Table 4a, 4b, 4c. Then compare the simulated difference between crime rates in 2008 and in 1970 with the actual difference. The ratio of the two is the contribution of the different dynamics of prison population to the reversal. This calculation reveals that incarceration explains 17% of the reversal for total crime, 33% for property crimes alone, and 11% for violent crimes alone.

4.2.2. Abortion rates. As Tables 3 and 4 show, we do not find evidence supporting the hypothesis that abortion rates decrease crime rates as Donohue and Levitt (2001) find for the United States. Most of the point estimates have a positive sign (which is the 'wrong sign') and are not precisely estimated. It is likely that the quartic time trend absorbs all the variation in abortion rates, a slow-moving variable like the demographic structure of the population. In fact when we remove the time trend or use a linear trend we obtain a negative and significant point estimate (see Web Appendix). However, even in this case the effect of abortion on crime rates disappears when we consider European countries only (not reported). A word of caution, however, is in order when comparing our findings with those of Donohue and Levitt. As mentioned above, their measure of abortion rates is computed as the sum of the abortion rate of each age group weighted by the percentage of arrests in a cohort over the total crime in the United States in a given year. This measure captures what they call the *effective* abortion rate, that is, the measure that is relevant to crime in a given year. For European countries, data on arrests for each cohort are not readily available every year so that we can only use the unweighted abortion rates in our analysis. It is interesting, though, that the significant point estimate obtained when removing time trends or using linear trends is completely driven by the United States. A possible explanation is that in Europe a strong welfare state, easy access to good education and strong family ties work as risk-reducing factors that weaken the link between unwanted childbearing and crime. Overall, one of the key elements explaining the drop in crime rates in the United States seems to be ineffective in Europe. This result certainly deserves further research.

4.2.3. Age structure. The elasticity of crime to the weight of young males in the population is about 1.5 (IV estimates, preferred specification) and it is slightly larger for European countries. Keeping constant the population, an increase of 1% in the share of males between 15 and 34 years of age leads to a 1.5% increase in the total crime rate. This effect is imprecisely estimated when we use property crimes as a dependent variable. As for violent crimes, we obtain a very large estimate for the European group in isolation. While a large coefficient for violent crime is not surprising, a point estimate of 7 seems quite large. The same back-of-the-envelope calculation made for incarceration rates suggests that the different dynamics in the age structure between European countries and the United States cannot explain the reversal of misfortune, a fact that was already made clear by Figure 4.

5. CONCLUDING REMARKS

In this paper we have explored the evolution of crime rates in Europe and in the United States since 1970. We have documented a 'reversal of misfortunes' and have attempted the identification of the causes of this reversal. We have estimated the impact of demographic structure, incarceration and abortion on crime rates. Unfortunately, due to weak instruments, we cannot provide reliable evidence on the causal effects of migration and unemployment rates. The OLS estimates for these two variables suggest that migration and unemployment increase crime rates, but it is not possible to assess the OLS bias. We uncovered two significant causal channels, though: both the demographic structure of the population and the incarceration rate have a non-negligible influence on crime rates. Back-of-the-envelope calculations based on our estimates indicates that the different dynamics of the prison populations in Europe and the United States explain 17% of the reversal of misfortunes for total crime, 33% for property crimes, and 11% for violent crimes. We do not find evidence that abortion rates reduce crime rates in Europe as much as previously found for the United States. Understanding why is beyond the scope of this paper, but future research should investigate this different response of crime rates to abortion in Europe.

On the methodological side, we are well aware of the limitations of our analysis. We acknowledge them here to emphasize that we regard our empirical exercise as a starting point for further research rather than a conclusive word on an admittedly complicated question. First, as Durlauf *et al.* (2008, 2010) show, aggregate crime regressions like those used in this paper are consistent with a benchmark microstructure only under strong assumptions about the distribution of unobserved individual heterogeneity. This points to a second limitation, namely the use of a reduced-form approach that mimics experimental variations via instrumental variables to uncover causal effects. There is much controversy about what policymakers can learn from such exercises (see, for instance, the *Journal of Economic Perspectives*, Spring 2010, symposium on 'Con out of economics'). We regard such controversy as a constructive step towards a better empirical economics. In the meanwhile we

warn readers that our results are to be taken with caution, because the reduced form approach we adopt is not able to pin down the channel through which the factors that we analyse influence crime rates. Furthermore, reduced-form parameters are likely not policy-invariant. As a consequence, it is difficult to make reliable out-of-sample predictions. Third, there is no consensus on the use of time trends in policy evaluation exercises (see, for instance, the discussions in Wolfers, 2006, and Durlauf *et al.*, 2008). We have shown that a quartic trend is a reasonable choice for the problem at hand. However, this remains a matter of judgment. Finally, we consider a selected set of explanatory variables. While the five factors we consider are those emphasized in the economics literature on crime, we have no framework dictating that these and only these should be considered.

What are the policy implications of our analysis? The first main finding – that is, the existence of a reversal – should make policymakers in Europe aware of the fact that crime (and violent crime in particular) is a very relevant issue, more than we are accustomed to thinking when making casual comparisons with the United States. The fact that the homicide rate is much higher in the United States than in Europe (as documented in the Web Appendix) seems to generate the wrong perception that Europe is a safer place. But homicides are only a small fraction (although very important for their consequences) of violent crimes. The second main finding – an elasticity of crime to incarceration of -0.4 – implies that incarceration works. Therefore, a tougher incarceration policy may be an effective way of reducing crime in Europe. While this causal effect is informative, it raises two issues. First, without knowing why incarceration works, it is hard to decide in what sense incarceration policy should be tougher. If it works because of incapacitation, then convicting more criminals to longer sentences is the sense in which the policy should be tougher. But if incarceration works because of deterrence, then inflicting long sentences and placing criminals on parole, for instance, would be better policy. It is impossible to resolve this first issue in a reduced-form framework like the one we have employed: more research is needed on the channels that make incarceration work. Second, our finding that incarceration is crime-effective does not imply that it is a cost-effective policy. To conclude that more incarceration or longer sentences are needed in Europe, we should understand how, at the current incarceration rates, the marginal cost of a prison inmate to society compares to the marginal cost of crime to victims in Europe. If the cost (to society) of incarcerating an additional individual is below the cost (to victims) of additional crimes that this individual would commit if left free, then more incarceration is efficient. For this type of cost-benefit analysis, one needs two parameters: the elasticity of crime to incarceration and the marginal costs of both crime and incarceration. In this paper we estimate the first parameter. Calculating the marginal cost of crime and of incarceration is hard because it involves things that are difficult to estimate. For instance, recent research shows that incarceration may have a criminogenic effect on recidivism in the long run (e.g. Chen and Shapiro, 2007; Bayer *et al.*, 2009; Nagin *et al.*, 2009;

Drago *et al.*, 2011). Such dynamic, general equilibrium effects are not captured by our estimates (which are based on a static panel data model) and may crucially alter cost-benefit calculations. In particular, they may increase the marginal cost of incarceration. Moreover, the 'cost of an inmate to society' includes not only the direct cost of incarceration, but also the indirect costs in terms of additional general equilibrium effects, from the distortionary effects of taxation needed to finance the judiciary and prison systems to the effects on labour market equilibrium. This does not mean that the cross-country evidence we have produced is of little use: while these two outstanding issues prevent us from drawing strong policy conclusions, our estimates are quite relevant for crime policy because they provide the basis to understand if more incarceration in Europe would be efficient. But more research on this point is needed.

Discussion

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This is a careful and interesting analysis that brings for the first time comparable data across Europe and the US on various types of crime and its determinants. The authors have made great effort to investigate the causal effect of many determinants of crime. However, I even found the descriptive statistic of great interest. The authors concentrate on five determinants of crime, which the previous literature has identified as important. These include demographics, immigration, unemployment, incarceration rates and abortion rates. The study leaves out other determinants such as the role of education and the differential timing of drug waves (such as crack-cocaine). The role of education in shaping crime has been investigated by Lochner and Moretti (2004) among others. Presumably, the authors have left out this variable for lack of reliable data across countries for the period of analysis. The authors control for GDP per capita as a measure of income. However, what may be important are changes in the unskilled wage or minimum wage (Machin and Meghir 2004). Another determinants of crime, or at least of its cyclicality, could be selective fertility induced by business cycles (as described by Dehejia and Lleras-Muney 2004). As with selective abortion, it could change the composition of the young population. The strength of the paper is in its use of instrumental variables to deal with the endogeneity of some of the explanatory variables. The validity of this procedure relies on the quality of these instruments. While I have little problem with the supply-push factors to explain migration, other instruments requires stronger assumptions. For instance, amnesties are used to instrument incarcerations. The authors do a great job to show that amnesties are not correlated with past crime. However, is it politically feasible to pass an amnesty when current crime is high or its growth rate is high? Moreover, during amnesty, the most dan-

gerous criminals are usually not pardoned. What this exercise estimates is the effect of releasing low-level criminals, not a random selection of offenders. Finally, as usual in this literature, the incarceration variable is measuring both a deterrence effect and a lock-in effect. Distinguishing between both channels would be an interesting exercise, although beyond the scope of this paper.

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This is an interesting paper on a policy-relevant and under-researched topic. The paper is well structured and the analysis is straightforward, transparent and clear. The authors document that crime rates in Europe have become much higher than in the US since 1990. In the US, crime rates substantially dropped during the last 20 years. The authors present a well-executed empirical analysis on the determinants of crime in Europe so as to explain this divergent EU-US crime pattern. Their main finding is that crime rates can be causally explained by incarceration rates and the fraction of the male population aged 15-34. They find that the elasticity of crime with respect to incarceration rates for EU-countries is about the same size as estimates obtained for the US. The authors use quite a convincing identification strategy by resorting to prison amnesties to generate exogenous variation in incarceration rates. Similarly, the authors assume quite reasonably that demographic variation is exogenous. Moreover, the authors demonstrate that unemployment (instrumented with oil-prices) and migration flows (instrumented with civil and ethnic wars or ethnic violence) have little explanatory power to explain crime rates. A number of comments can be made regarding the paper. Although apparently often suggested in the crime literature, it is not so obvious that immigration should have an impact on crime. One may suspect that there is a large amount of heterogeneity in the immigrant population, ranging from asylum seekers, family reunification, to labor migrants. Heterogeneity among immigrants is also caused by country of origin, income levels and skill levels. Moreover, within the EU there is free migration accounting for 27 percent of total immigration in the OECD and 44 percent of immigration in the EU-area (OECD, 2010). Probably, illegal immigrants are overrepresented in crime statistics, but the latter group is not captured by the official migration statistics, which are employed in the current paper. Similarly, it would be surprising that labor immigration (including migration of spouses) – which covers about 17 percent of total OECD-immigration – would be strongly associated with crime, since migrants obtain a visa only conditional upon having work. Similarly, family reunification – accounting for 18 percent of total OECD-immigration – is possible only when certain age and income requirements are met. The figures are drawn from OECD (2010). Hence, one should be careful in presuming that immigration and crime are intimately linked. In addition, definitions of migration stocks can vary across countries due to differences in the definition of who is

considered to be an immigrant. The paper and the web appendix do not provide details on the way migration stocks are measured and whether the data are comparable across countries and over time. For all these reasons, a weak correlation between crime and immigration might not be surprising in the first place. Using weak instruments for migration tends to weaken this correlation even further. The authors are aware of potential shortcomings of their empirical strategy. In particular, they are very clear about the strength of the instruments they employ to obtain exogenous variation in the explanatory variables. I would have liked them having tried improving this part of the paper and to obtain more reliable instruments for the unemployment and migration variables. As regards unemployment, the authors follow Blanchard and Katz (1992) by using oil-prices to generate exogenous variation in unemployment rates. The instrument does not seem to work very well, perhaps because there is no cross-country variation in oil-prices and little time-variation in sectoral structure across countries. As an alternative instrument, one could possibly look at how changes in world trade affect employment in European countries. Most EU-countries are small-open economies that have a limited (if not negligible) impact on world-trade. If one believes that world-trade is exogenous to small-open economies, then this could be a reasonable variable to instrument unemployment rates. Cross-country variation can be introduced by interacting world-trade with the openness of a country. Similarly, numerous European countries have implemented structural labor market reforms in recent decades. Arguably, these reforms were not implemented to reduce crime rates, but to reduce structural unemployment. Therefore, dummy variables indicating whether there have been structural reforms in labor markets could be suitable instruments for unemployment as well. As regards immigration, the authors should be applauded for compiling their own data set with variables based on civil war, ethnic war or ethnic violence. Unfortunately, these instruments also appear weak in explaining migration patterns, as also indicated by the authors. I wonder whether this is due to the fact that migration flows predicted by these variables presumably mainly reflect asylum seekers. Asylum seekers are not representative for migrant populations in many countries. Asylum migration accounts for only 5 percent of total OECD-migration (OECD, 2010). Immigration flows are also determined by, for example, free movement of individuals in the EU, labor migration, marriage and family reunification. These immigration flows are not so much caused by war or violence, but by, among others, colonial and guest-worker histories and the Schengen Treaty. Hence, the instrument cannot capture a lot of the variation in migration flows. A suggestion is then to seek for instruments that relate to e.g. colonization patterns decades or even hundreds of years ago to predict current migration flows. Similarly, many countries have adopted guest-worker policies in the 1950s and 1960s when large numbers of guest workers were recruited. Variables related to guest-worker migration decades ago could therefore be reasonable instruments for current migration flows, since current crime rates are arguably not

caused by colonial and guest-worker histories. Finally, the introduction of the Schengen Treaty could be exploited as a natural experiment to generate exogenous variation in migration flows. The paper documents a very intriguing pattern in the crime rates in Europe and the US; Europe has overtaken the US in crime since the 1990's. The authors take a first step in explaining this pattern using a number of explanatory variables suggested by the literature: demographics, incarceration rates, abortion rates, migration rates and unemployment rates. However, only the demographic and incarceration variables appear to be causally explaining crime. For future research, one should probably explore other variables to explain crime as well, such as income (growth), drug-related activities, family and social problems, psychological traits, etc. However, one should be careful not to replicate research that is already done in, for example, criminology, psychology or sociology. As it seems, the time-series pattern in crime in the US and Europe discussed in the current paper appears to be well-known in criminology, see, for example, PEW Center on the States (2008). Finally, the policy conclusion is reached that higher incarceration rates are beneficial in reducing crime. I would like to raise a word of caution against policies explicitly aiming to raise the total incarceration rate. Police officers often say that prison provides the best education for a career in crime. In the short-run, higher incarceration rates could indeed reduce crime, either through deterrence or through incapacitation. However, the long-run effect could be that the stock of 'criminal human capital' grows and an ever increasing number of individuals need to be incapacitated to prevent them from committing new crimes. The US seems to have had exactly this experience, as the current US-incarceration rate is about five times bigger than 40 years ago, about 7.2 million individuals were under some form of correctional supervision (probation, prison, jail, parole), and more than half of released offenders went back to prison within three years (Bureau of Justice Statistics, 2011).

Panel discussion

Volker Nocke wondered whether there was any change in the composition of the prisoner cohort in countries over time. He speculated that in East European countries there is a high level of organized crime and many of these people facilitated by European Union accession may now operate in other European countries. Dalia Marin referred the authors to previous research which argued that an important determinant of crime among young males was their marriage status. Findings in a *Quarterly Journal of Economics* paper suggest that the introduction of the contraceptive pill led to a decline in shotgun weddings which reduced the number of young men with familial responsibilities and their need to commit crime in order to provide for their family. Eric Hanushek noted that in the United States the average level of

educational attainment has remained more or less constant while it has increased across European countries. This would appear to contradict the claim that higher levels of education reduce the crime rate.

Many of the panellists focused on specific aspects of the empirical strategy pursued by the authors in the paper. Marco Pagano believed that the oil shocks variable was not a good instrument for unemployment as it was likely to explain only a small percentage of its variation and urged them to search for alternative instruments. Michael Kiley advised the authors to continue with their instrumenting strategy. Even though their instruments are found to be weak they may still uncover robust findings. Andrea Ichino believed their approach is useful for understanding the differences in the crime rates between the United States and Europe; however analysis of individual country amnesty events could provide specific insights into the effect of incarceration on crime which would better inform policy. Some of the panellists believed that many of the country-level determinants of crime such as abortion rates to be relatively stable over the time period analysed. The inclusion of fixed effects eliminated much of the variation in these variables and therefore the likelihood of identifying their effect on crime. Fabrizio Perri believed unemployment and immigration are more important determinants of crime than the analysis suggests. He suggested the authors should estimate country-specific regressions and also estimate their pooled regressions without time trends to see if the effects of unemployment and immigration become more meaningful. Refet Gürkaynak focused on the role of the large downward trend in the US crime rate in the data. He suggested that a US specific regression was likely to yield insignificant results. He wondered how much of the current variation in the main model was explained by the inclusion of the United States.

Richard Portes wondered if there is any research on the link between the age at which alcohol can be legally consumed and violent crime. He also made the point that the large proposed budget cuts in the United Kingdom include a substantial cut to the prison budget and is likely to lead to a reduction in the incarceration rates which would provide an interesting experiment on the link with crime rates. Given there are higher crimes rates in cities, Daniele Terlizzese wondered if the authors had considered the structure of cities as an explanation for the larger crime rate in Europe compared to the United States.

In response to comments by Fabrizio Perri and other panellists, Giulio Zanella remarked that they adopted a conservative approach to ensure the robustness of the identified effect of incarceration on crime. In model estimations without quadratic time trends they find that a number of the demographic variables become significant, the effect of abortion on crime remains insignificant while the incarceration coefficient is higher. In reply to Eric Hanushek's comment, Giulio Zanella agreed it was important to control for education attainment; however, the variable is likely to be endogenous with crime rates and they would need to find another instrument.

Web appendix

Available at http://www.economic-policy.org.

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