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A Vicious Cycle of Regional Unemployment and Crime? - Evidence from German Counties

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Abstract: Much research has been done showing that unemployment can cause crime, and that crime adversely impacts economic activity. However, very few authors have considered a simultaneous relationship. Using an IV-setup and regional panel-data, I find evidence for the possibility of a vicious cycle, with unemployment leading to higher crime rates and crime rates raising unemployment. I further find that especially employment in low-skill service jobs is adversely affected by crime, that many types of crime are impacted by unemployment differently and that both apartment rents and GDP-growth decrease if crime increases. The spatial dependencies found further raise the possibility that these vicious cycles could spill over into neighboring regions.

Keywords: Crime, Unemployment, Amenities, spatial autregressive model, SARAR, endogenous regressors.

JEL: J21, J32, K42, R11, R23, R30

1 Introduction

In 2009, the company *Wolf-Garten*, a garden equipment manufacturer in the county of Altenkirchen, declared bankruptcy. It being the height of the financial crisis, other businesses also struggled, and people found it hard to find new jobs, and so the unemployment rate shot up roughly 1 percentage point in the county. At the same time, crime rates also increased. Especially assault, vandalism and all kinds of 'street crime', that is crimes taking place in public, went up to a similar degree (see Figure 1).

And Altenkirchen was not alone in this trend, during the financial crisis similar spikes in both crime and unemployment could be seen in many counties across the country (see Figure 6 in the appendix). In the coming years, the labor market rebounded, and the crime rates also fell. This raises an obvious question: Are crime and unemployment related?

The idea that people commit crimes to get things they could not afford otherwise is very intuitive, and so one would expect unemployment to have a positive impact on the crime rate. More recently researchers have begun asking the question how crime rates might affect the economy, with many hypothesizing that it dampens economic activity and causes higher



FIGURE 1. CRIME AND UNEMPLOYMENT IN ALTENKIRCHEN (WESTERWALD)
Note: Standardized with base-year 2007.

skilled workers to move. However, these two strands of research mostly stood alone, even though it seems quite intuitive to combine them. Because if unemployment causes more crime, and more crime in turn lowers economic growth, one would expect crime to increase unemployment. If true, this could give rise to a vicious cycle, where unemployment lets people turn to crime, thereby worsening the economic prospects of their neighborhood, thereby further increasing unemployment. This question has until now received very little attention, and the few studies that have considered this even tangentially have not found strong evidence either way.

My work then contributes to the existing literature as it is the first that examines this question from a regional perspective, using fairly granular panel data. It is also the first using an IV-set-up. As a result I indeed find some evidence that a simultaneous relationship between crime and unemployment does exist.

This has strong policy implications. It suggests that crime and unemployment are both social problems that can spiral out of control if policy makers do not intervene. But it also suggests that policy interventions lowering either crime or unemployment could in turn lead to a virtuous cycle of lower crime rates leading to lower unemployment and that in turn lowering crime. The existence of spatial autocorrelation further suggests that such feedback loops are not isolated to one region, instead a rise or a decrease in either crime or unemployment could spill over into neighboring regions and cause a vicious or virtuous

cycle to start there, which in turn would then again spill over into other regions and so on.

The paper is structured as follows: First, I will give a short overview of previous research and derive hypotheses from it. Then an overview of the data used to examine this question is given, followed by a discussion of the econometric methodology. Finally, the results are presented and discussed.

2 Literature & Hypotheses

The prevalent economic theory regarding crime is the Becker-Ehrlich model (Becker 1968; Ehrlich 1973). They propose a rational choice model of crime, where individuals face a choice of pursuing legal or illegal activity to generate income and compare the expected pay-outs to decide. The expected payout of legal employment depends on the probability of losing ones job. In the aggregate and given identical agents, they argue, this probability corresponds to the unemployment rate in a given period. Generally, in empirical studies this model and its derivatives performed very well, with almost all of them confirming its main implications (see Freeman 1999; Entorf and Spengler 2002 or Draca and Machin 2015 for a literature overview). However, early on, the impact of unemployment on crime often could not be shown (Chiricos 1987; Entorf 2008). To get more definitive results, researchers subsequently either focused on just youth unemployment, where this effect could be shown reliably (Fougère et al. 2009; Grönqvist 2011) or used data with a lower level of aggregation like the county level (Baller et al. 2001; Gould et al. 2002; Messner et al. 2013; Tarling and Dennis 2016), street level (Morenoff et al. 2001; Andresen 2006; Ha and Andresen 2017; Liu and Zhu 2017) or even individual level (Entorf 2008; Bennett and Ouazad 2016). Both these approaches were indeed fruitful and found the hypothesized link between unemployment and crime. Yet interestingly Raphael and Winter-Ebmer (2001) were also able to show the same with highly aggregated data and for overall unemployment by controlling for the possibility of simultaneity between unemployment and crime using 2SLS among other strategies. This already suggests that the lack of evidence found in earlier studies might also have been due to misspecified models, as no other study mentioned so far accounted for this possibility.

The other workhorse theory trying to explain the socio-economic determinants of crime is the "social disorganization" theory, rooted in the work by Sutherland (1924) and Shaw and McKay (1942) among others. This theory also predicts a positive link between unemployment and crime, but proposes a different causal relationship. They argue that these are linked via the local community and its ability to exercise social control, which in turn depends on how well organized the community is, i.e. how tight the friendship-networks and how strong local institutions are. This in turn depends on the economic means of the community, which should decline as unemployment increases. As its implications are fairly similar to those of the Becker-Ehrlich model it is not surprising that empirical investigations also found it to work well (for example Sampson and Groves 1989; Miethe et al. 1991; Glaeser and Sacerdote 1999; Gould et al. 2002; Entorf and Spengler 2002). The only theory of note which suggests that unemployment might lower crime rates is that of Cantor and Land (1985). They argue that since unemployed people tend to stay home more, they are both less likely to be the victim of a crime and more likely to be a witness, thus acting as a deterrence. They further believe that these effects can be disentangled, because the deterrence effect of unemployment should set in immediately, while any motivational effect might take time to manifest, implying that contemporaneously unemployment should lower crime, while increasing it after a time lag. Some empirical testing of this hypothesis has been done, but with mixed results, with Hale and Sabbagh (1991); Phillips (2006); Phillips and Land (2012); Ha and Andresen (2017) confirming this hypothesis, but Levitt (2001) or Arvanites and Defina (2006) finding no such evidence. Fajnzylber et al. (2000) even find that unemployed people are more likely to be victims of a crime, which contradicts Cantor and Land's theory.

The idea that crime rates can influence unemployment is not entirely new, either. Some research has been done in the framework of amenities (Roback 1988; Palumbo et al. 1990; Boarnet 1994; Gottlieb 1995; Willis 1999, Berger et al. 2008; Nuñez et al. 2017). The argument here is that crime is a dis-amenity, which causes companies to set up shop elsewhere and/or causes high-skilled individuals to move, leading in turn companies to go where they have access to this pool of labor. Most studies have found some evidence for this hypothesis, but with the exception of Willis (1999), did not control for the possibility

of a simultaneous relationship. Recently, Diamond (2016) found crime to be a dis-amenity which specifically causes highly skilled individuals to move, while lower skilled people often do not have the means to do so. Outside of the amenity framework, some authors pointed out that crime might incur higher costs for firms, either because they have to pay more for security, because they incur losses from thefts, burglaries etc., or because they have to pay workers a premium to work in an certain area (Entorf and Spengler 2002; Sandler and Enders 2008; Torres-Preciado et al. 2015). Independently, there exist a range of studies trying to estimate the economic costs of crime, with estimates ranging from roughly 3% of GDP in Italy (Detotto and Vannini 2010) to 12% in the US (Anderson 1999). Besides this impact on economic activity and thereby labor demand in an area, there is also a strain of literature showing that crime impacts labor market by destroying the human capital stock and thereby labor supply. For one, young people endeavoring into a career of crime will likely not invest further into their education and often drop out of school (Lochner and Moretti 2004; Machin et al. 2011). Further, people with a criminal past often have trouble finding legitimate work even if they try (Kling 2006), and whether they try is largely determined by how difficult they expect it to find employment (Entorf 2008), with recidivism having an inverse relationship to job market opportunities after release from prison. In addition Bindler and Ketel (2019) found that victims of crime also were more likely to become unemployed after the crime took place, either because of mental health issues caused by a traumatizing experience, or because they had to move, leaving their job.

Given that there is this much evidence suggesting that unemployment has an impact on crime rates and also a fair amount that crime rates can impact labor demand and supply, it is somewhat surprising that most research has ignored the possibility of a simultaneous relationship, and only very few studies explicitly investigated it. Thornberry and Christenson (1984) used a simultaneous equation model to do so, but only on an individual level, where it seems unsurprising that a career in crime is not compatible with a day-job. Further, there exist a number of studies that looked at whether crime rates and unemployment have a co-integrating relationship. Hale and Sabbagh (1991) found no evidence for this, while Masih and Masih (1996); Mauro and Carmeci (2007) and Detotto and Pulina (2013) did. The latter further tested the Granger causality of this relationship, with all

studies finding a strong evidence for crime being Granger-caused by unemployment, but little or no evidence for causality flowing the other way around. One possible reason for this is the use of national-level and therefore highly aggregated data. Both crime-rates and unemployment are determined locally to a large extent (Levitt 2001; Elhorst 2003b) which is also made apparent by the great regional variation of these variables we can observe (see Figure 3). Therefore, analysis using national averages will ignore significant spatial variation and squander the potential to look at subtle, very localized mechanisms determining crime and unemployment.

Given then a small spatial scale, it becomes important to consider the possibilities of spatial spill-overs of both crime and unemployment. This matters for two reasons. For one, it is well understood that ignoring spatial autocorrelation can bias estimation results (Anselin 1988). But maybe more importantly, examining the spatial dependencies can actually give us valuable insights in how crime and unemployment interact. There has been a lot of research showing that both indeed exhibit spatial dependencies. For crime, An early example is Morenoff et al. (2001), who show spatial clustering of murders in Chicago neighborhoods. Other examples for such studies, which mostly found very similar results, are Kershaw and Tseloni (2005), Breetzke (2008), Liu and Zhu (2017), Andresen and Malleson (2013) and many others. Baller et al. (2001) studied homicides in the US over a long time-frame (1960-90). Not only did they discover significant spatial dependence in the data, they also found the model to be unstable in both the time and the spatial dimension. For example, they found different signs for coefficients when looking at different subsets of the data, for example the South compared to the northern parts of the US, or different decades. For Europe, noteworthy studies include the one by Cracolici and Uberti (2009), Hooghe et al. (2010), Tarling and Dennis (2016). For Germany, one should also mention the studies by Oberwittler and Gerstner (2011) and Messner et al. (2013). Oberwittler and Gerstner (2011), who both find strong spatial dependencies. That unemployment also usually shows strong positive spatial autocorrelation is well known (see Elhorst 2003b for an overview) and, as for example Niebuhr (2003), this also holds in Germany.

In light of the previous research, I formulate the following three hypotheses:

H1: Crime and unemployment have a positive simultaneous relationship, leading to the

possibility of vicious or virtuous cycles.

H2: One reason crime can increase is unemployment is because crime is a dis-amenity, driving away business and/or skilled labor.

H3: Both crime and unemployment are autocorrelated across space. This gives the vicious cycle of crime and unemployment a spatial dimension, resulting in a feedback loop not only inside of a region, but spreading across regions.

3 Data

The panel data used covers all 401 German counties (*Kreise* and *kreisfreie Städte* in the German nomenclature) over the years 2004-2015. The crime data come courtesy of the German federal criminal police (*Bundeskriminalamt*, BKA) as part of their yearly published *Polizeiliche Kriminalstatistik* (PKS)¹. All other socio-economic variables were constructed from publicly available data sets published by the federal and state statistical offices (*Statistische Ämter des Bundes und der Länder*). The map data used for visualization and to construct spatial weight matrices for my regression model come courtesy of *Bundesamt für Kartographie und Geodäsie* (BKG).

Unfortunately, much of this data required a fair amount of cleaning up before it could be used. For one, several counties underwent restructuring in my time-frame, lowering the number of total counties from 439 in 2003 to 401 in 2015. In most cases, this did not present a major problem, as two counties were combined into one, making the homogenization of my data set straight forward. The exception to this is the area-reform of Saxony-Anhalt in 2007. Here, some counties were cut up and re-assembled in a somewhat arbitrary fashion, making it harder to homogenize observations before and after the reform. As in most cases no lower-level data was available, I instead combined these counties as well as I could, i.e. merging a county with another if most inhabitants of these two counties ended up the new one. This is of course a source of aggregation error, to control for which I have included a dummy for these years and counties in every regression. But since this only

¹The publicly available data sets only include county-level data for the years 2013 onwards. Therefore I am very grateful to the department IZ 33 of the BKA for giving me access to that data for the years 2003-2012.

affects 7 counties over a 3-4 year time-span, I am confident that this is only a minor source of measurement-error². Also, as my regression model requires a perfectly balanced panel, some imputation had to be done where there was data missing. Overall, imputations were only necessary for five variables, and at most 8.33% of the observations had to be imputed, in the other cases far less. For an overview of what data had to be imputed and how those imputations were performed see Table 8 in the appendix. A range of covariates of crime and unemployment were included in the analysis in order to control for omitted variable bias and to further examine the channels through which crime and unemployment interact. An overview of all variables used and their descriptive statistics can be found in Table 1. Due to endogeneity issues, not all variables could be included in every specification of my model, for details on what covariates were used specifically see the notes under the estimation results.

	Mean	Std. dev.	Min.	Max.
<i>Street crime (log.)</i>	7.152487	.5436428	5.624676	8.667933
<i>Spatial lag: Street crime (log.)</i>	7.175277	.4010877	6.084411	8.286187
<i>Unemployment rate (log.)</i>	1.27724	.5030557	-4.438844	2.682135
<i>Spatial lag: Unemployment rate (log.)</i>	1.286064	.422586	.1389454	2.561723
<i>GDP p.c. (log.)</i>	10.12649	.3601162	9.252127	11.7235
<i>Real GDP growth (log)</i>	4.616458	.0418016	4.194755	5.009751
<i>Rent per m²</i>	1.744469	.1865015	1.26976	2.625393
<i>Blanchard-Katz instrument</i>	2.201843	.155096	1.612988	2.69066
<i>Exposure to oil shocks</i>	2.761355	.6270735	-1.302094	4.391074
<i>Government employees p.c. (log.)</i>	6.883763	.5381489	5.293982	8.549983
<i>Share of males 10-18 (log.)</i>	1.407968	.1890886	.753388	1.790764
<i>Disposable household income (log.)</i>	9.850103	.243384	-4.60517	10.61307
<i>DE-domains p.c. (log.)</i>	9.102219	1.980222	-4.60517	11.85277
<i>Clearance rate street crime (log.)</i>	3.010253	.2812972	1.463568	4.157049
<i>Population density (log.)</i>	5.617081	1.084441	3.590776	8.44838
<i>Divorce rate (log.)</i>	10.69009	.2603059	9.107629	11.58924
	$N = 401$	$T = 12$	$N \times T = 4812$	

TABLE 1. DESCRIPTIVE STATISTICS

3.1 Crime: Data and Instruments

The main measure of crime used is ‘street crime’, which consists of all kinds of crime taking place in a public space³. This is done for several reasons. Firstly, in this study

²This is also supported by the fact that when a dummy variable for those counties in question was included in my regressions, the corresponding parameter estimate was always very small and never significant.

³For an exact definition see Bundeskriminalamt (2017a, pp. 7–8).

I am not interested in a specific type of crime, but rather crime in general. However, the total number of crimes committed is not a good measure to use, as it includes many violations that, while against the law, would not usually be perceived as crimes, such as fare dodging or crossing the border without permission, making it "almost useless", as Entorf and Spengler (2002, p. 7) put it. Therefore "street crime" arguably serves as a better proxy for the perceived level of crime in an area, as it aggregates many forms of crime people actually care about, such as pick-pocketing, muggings, assault etc. This also matters as the amenity-framework of job location implies that the perception of safety rather than the actual number of crimes drive the location of jobs, because a perceived risk of becoming a victim of a crime is the disamenity people or firms react to (Willis 1999). Secondly, as Birkel et al. 2014 find, most types of crime included in the street-crime measure have fairly low under-reporting rates. It is well known that police recorded crime rates under-reports the actual crime numbers. That in itself would not matter, if the share of crimes reported were homogeneous over time and space. Yet Tarling and Morris (2010) show that crime under-reporting can undergo serious fluctuations over time, as in between 1991 and 2008 people became more likely to report violent and less likely to report property crimes. And while Gove et al. (1985) argue that these fluctuations are mostly due to changing perceptions of how serious a crime is, MacDonald (2001) for instance finds that unemployed people are less likely to report burglaries, implying that likelihood of someone reporting a burglary and their socio-economic status are not statistically independent. Thus it is important to minimize this possibility of endogeneity by using crimes with low under-reporting bias. To further alleviate this problem, I will also use other types of crime with different levels of underreporting in my robustness checks. Also, as Willis (1999) points out, calculating the crime rate as crimes per 100,000 inhabitants as it is usually done might over - or underestimate the prevalence of crime, as the resident population is not equal to the pool of potential victims, especially in areas with many transients, for example popular tourist destinations or large cities with many potential victims commuting from the surrounding counties. I however found it to make no difference in my estimation results.

When estimating the impact of crime on unemployment, the crime rates need to be instrumented. In order to allow me to test whether the instruments used are indeed exogenous,

as required for unbiased results, the model needs to be over-identified, i.e. each endogenous variable needs to be instrumented with at least two instruments (see Sargan 1958). The first instrument for street crime used is the number of tenured government employees per capita. In Germany, almost all government jobs concerning law and order are tenured, so this can be seen as a proxy for how well staffed police and the justice system is in a certain area. I did not use the number of police officers, as this data is not readily available, and also Levitt (1995) found that it might be endogenous, as politicians increase the number of police officers in response to a rise in crime. Using the number of tenured employees instead gives me a measure on how well staffed a local government is in general, which is less likely to be endogenous.

The second instrument, the clearance rate, of street crime, can be interpreted within the Becker-Ehrlich framework as the perceived likelihood of getting caught. However, the clearance rates might have their own endogeneity issues, as they are calculated as solved crimes per reported crime, and are therefore affected by the same under-reporting issue mentioned before. For example, the average clearance rate of drug crimes in my data is 95.6%, which clearly in no way reflects the actual clearance rate, nor the probability of getting caught. Due to this under-reporting problem it is even possible that a more effective justice system could lower the clearance rates, as more crimes are uncovered that went unreported before. To deal with this, I use the clearance rate of street crime, which, as discussed earlier, has a low underreporting bias.

As alternative instruments, I also tried the share of young men in the community, defined as men between 10 and 18 per capita, and a shift-share instrument of crime. It is well known that most crimes, and especially the types of crime included in the 'street-crime'-measure, are overwhelmingly committed by young men (Glaeser and Sacerdote 1999; Entorf and Spengler 2000). That this is also the case in Germany is supported by the aforementioned survey among inmates by Entorf (2008), which reported that 89% of inmates are male, with an average age of 33. It stands to reason that this instrument is also exogenous, as it is mostly determined by birth rate patterns decades earlier as well as the parents decision where to live. The cut-off of 18 is chosen because there is a chance that men over 18 move in response to unemployment shocks, making their location endogenous again. The shift-

share instrument is constructed by multiplying the one-year-lag of the crime variable with its national growth rate, analogously to how the Blanchard-Katz instrument is calculated (see next section). As both the time lag and the national change in crime are very likely exogenous, this instrument should be so as well.

For relevance of these instruments, the correlations reported in Table 9 in the appendix give a first impression, although more sophisticated post-estimation methods were also used (see section 4).

3.2 Unemployment: Data and Instruments

The main measure of unemployment used are the official unemployment numbers as reported by the *Bundesagentur für Arbeit*. However, since I am dealing with a small geographic scale, this measure might not perfectly encapsulate the effect I am trying to measure, since it does not account for commuting, as Willis (1999) points out. If crime lowers employment in a specific city, it is possible that the unemployment rate there will only increase slightly, with most people being laid off living in the surrounding counties and commuting into the city. I therefore also check the effect increased crime numbers have on the employment rate in a given county, i.e the number of people per capita that actually work there. As can be seen from Figure 3, there can be substantial differences between those two numbers, with large cities often exhibiting both high unemployment and high employment rates. The same data source also reports sector specific employment figures, which allows me to test whether the employment-crime relationship is homogeneous across industries. The sectors for which employment data is available are the low skill service sector, such as retail, hospitality and transport, the high-skill services sector, like finance, insurance or consulting as well as manufacturing and lastly agriculture, including fishing⁴.

For the IV-estimations, again two instruments are needed. These were taken from the vast literature on regional unemployment (see Elhorst 2003a and Mameli et al. 2014 for an overview). The first instrument is a shift-share instrument first proposed by Blanchard and Katz (1992), the national employment growth rates in each industry multiplied by

⁴For details on how these sectors are defined, see Statistische Ämter der Länder und des Bundes 2017c.

the regional shares of employment in each industry in the previous year. This is inspired by the ‘industry-mix’-explanation popular in the regional unemployment literature, which seeks to explain differences in unemployment levels across regions by differences in the industrial base. In essence, the argument is that different industries are affected differently by structural change, and therefore regions with a lot of manufacturing for instance might exhibit high unemployment, while regions with a large share of employees in professional services for example do not (see Taylor and Bradley 1997 or Niebuhr 2003 for example). It seems fairly likely to me that this instrument is indeed exogenous, as it relies on the national employment growth, which almost certainly is independent of individual growth rates in a single county, and the time lag of employment shares, which are exogenous almost by definition. Yet some have questioned how relevant it is. For instance Martin (1997) found that unemployment growth can vary widely even among regions with the same industrial make-up. More recently, however, Autor et al. (2013) have shown industry mix to significantly impact changes in unemployment, as those local labor markets with industries heavily exposed to competition from China fared far worse than those with less exposure. The second variable used as an instrument for unemployment is the time lag of GDP p.c., which is exogenous by construction, and should also be relevant.

As an alternative instrument, I use the share of jobs in manufacturing in the previous year, multiplied by the Brent oil-price. This is an idea taken from Raphael and Winter-Ebmer (2001), who argue that regions where a lot of people work in manufacturing are more susceptible to oil-price shocks, both of which should be exogenous in my set-up. For the relevance of all these variables, see again Table 9 in the appendix.

3.3 Data Overview

A visual inspection of the most relevant variables, crime rates and unemployment rates, already give us some interesting insights. First, looking at simple scatterplots (Figure 2), crime and unemployment seem to have a positive relationship, and both seem positively spatially auto-correlated. Further, GDP growth and crime are seemingly negatively related, although the relationship is not as strong. This so far supports the hypothesis derived

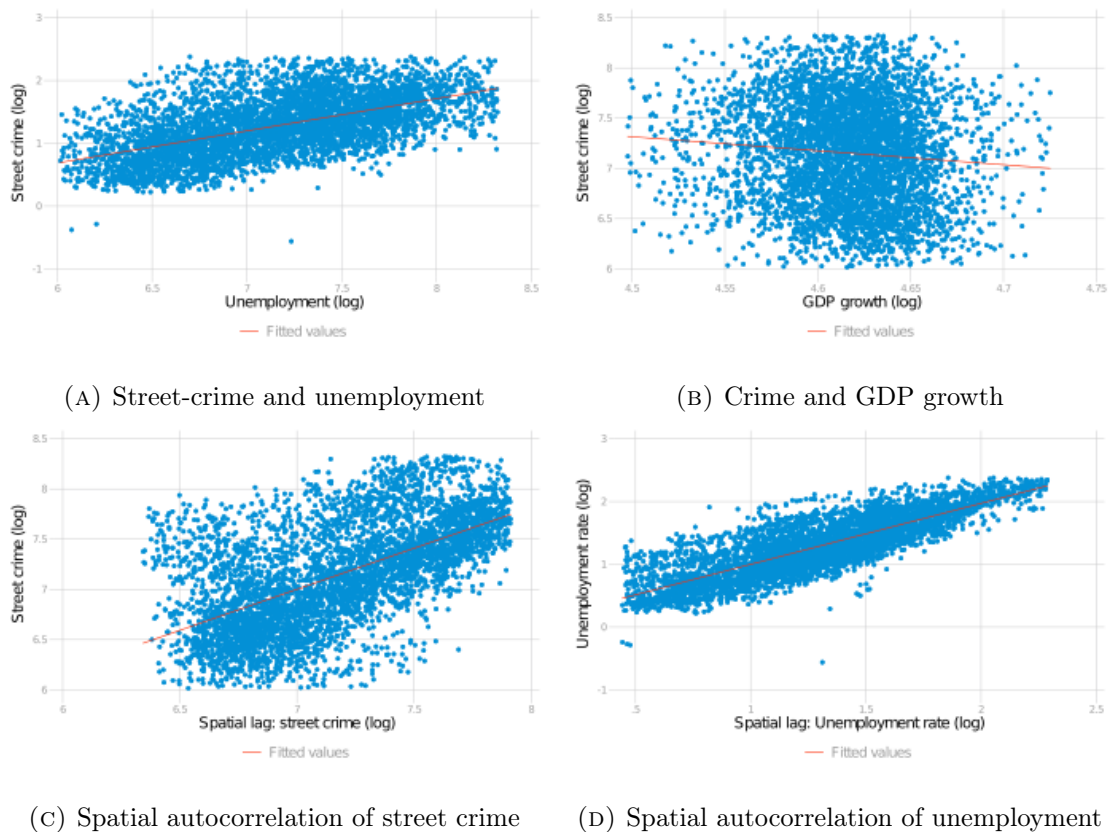


FIGURE 2. SCATTERPLOTS

Note: All variables are logarithmized, outliers have been removed for legibility.

from the literature, but we cannot say what the casual link between all these variables looks like. Considering the maps in Figure 3, a similar picture emerges. Especially unemployment and crime rates form distinct clusters, which is a clear sign of positive spatial autocorrelation (see LeSage and Pace 2009). Also, these clusters are very similar to each other. Both crime and unemployment are very high in the Rhine-Ruhr area as well as around Hamburg and Berlin, while a large cluster of low crime and low unemployment exists in the south. Comparing this with the spatial distribution of employment in low-skill sectors, we see some similarities, but more differences. Both have very high values in the large cities, but low-skill employment is far less clustered than unemployment. One cause for this might be that unemployment is measured at the place of residency, while employment is measured at the place of work. This could explain why employment exhibits many instances of negative spatial autocorrelation, i.e. cities with very high employment rates, surrounded by counties with very low rates, as people commute from the more rural areas to the cities for work. The spatial clustering of GDP growth is less distinct, and

does not clearly align with any of the spatial patterns found before, except for a large high-growth cluster in the south. Overall, this is again what one would expect to see if a bi-directional relationship between crime and unemployment existed while spatial spillovers were present, though other explanations are also possible.

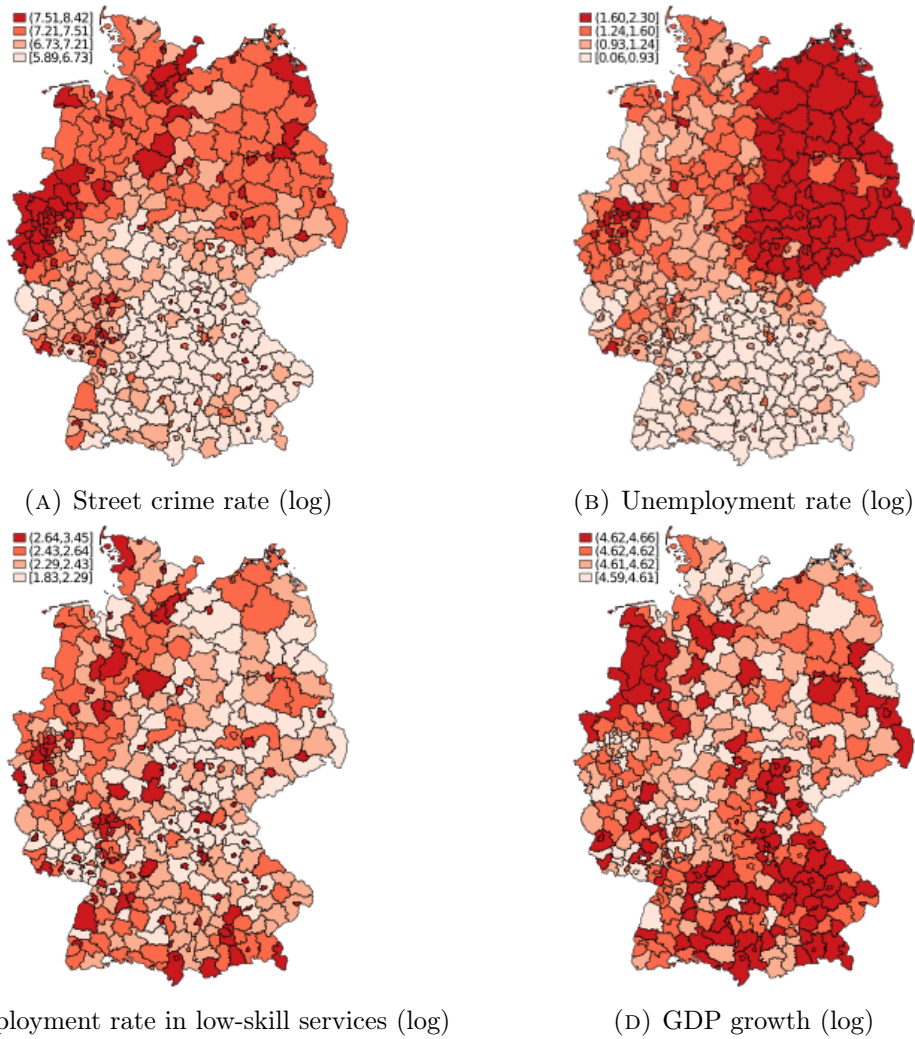


FIGURE 3. MAPS OF CRIME AND UNEMPLOYMENT

Note: All variables logarithmized and averaged over time. The base map-material comes courtesy of the BKG.

When looking at crime and unemployment in the time dimension, no clear patterns or trends emerge at first (see Figure 4 in the appendix). But when subtracting the panel average from each equation, a general downward-trend in both unemployment and crime rates becomes visible (Fig. 5), suggesting that there exist a common time trend which needs to be controlled for. Also, the effects of the financial crisis are again visible when doing so, but the effects are not as distinct as when looking at individual counties (Table

6 in the appendix).

4 Methods

Given the data and the hypothesis discussed in 2, let us now discuss how this shapes my model of the data generating process, and how such a model can be estimated. I will also discuss a range of specification tests used to check whether the assumptions made are sensible.

The hypothesis that unemployment and crime simultaneously affect each other (**H1**) implies a data generating process that looks like this, stacked over all cross-sectional elements:

$$u_t = \alpha_1 c_t + \alpha_2 u_{t-1} + X_t^u \alpha. \quad (4.1)$$

$$c_t = \beta_1 u_t + \beta_2 c_{t-1} + X_t^c \beta. \quad (4.2)$$

Here u_t, c_t are $N \times 1$ vectors of the unemployment and crime rates respectively and X_t^u, X_t^c are $M \times N$ matrices of time-variant exogenous variables. However, if **H3** holds, i.e. if unemployment and crime are spatially correlated with themselves, this should be modelled explicitly as an AR process. A so called SARAR model (see Kelejian and Prucha 1998) would look like this, where λ is the AR-parameter:

$$u_t = \lambda_u W u_t + \alpha_1 c_t + \alpha_2 u_{t-1} + X_t^u \alpha + e_t^u \quad (4.3)$$

$$c_t = \lambda_c W c_t + \beta_1 u_t + \beta_2 c_{t-1} + X_t^c \beta + e_t^c \quad (4.4)$$

This makes it necessary to define a $N \times N$ spatial weighting matrix W , the construction of which I will discuss later in this section. Given what we already know, the error terms e_t^u and e_t^c should include, besides an idiosyncratic part $\varepsilon_{it} \sim IID(0, \sigma_\varepsilon^2)$, a cross-sectional fixed effects term ϑ to control for latent variables, as well as time fixed effects ψ to control for the common time trends found in the data (Fig. 5). In addition, given the hypothesized spatial AR-process, it seems likely that the error terms are also correlated over space. So the error term of either equation can be described as follows, with $j \in N$ being the other

counties, w_{ij} being the the ij -th element of the spatial weight matrix W and ρ being the spatial autocorrelation coefficient:

$$e_{it}^k = \rho^k \sum_{j=1}^N w_{ij} e_{jt}^k + \vartheta_i^k + \psi_i^k + \varepsilon_{it}^k, \quad k \in \{u, c\} \quad (4.5)$$

This hypothetical data generating process already presents a number of identification challenges to solve. First, for the fixed-effects term, the data is *within*-transformed, i.e. the cross-sectional average of each variable is subtracted from each observation. Further, crime and unemployment are assumed to be endogenous, which I deal with using a two-stage least squares (2SLS) estimation with the instruments discussed before. In order to test for exogeneity of these instruments, it is beneficial if the first stage regressions are over-identified, which is why I use two instruments each. However, these are not the only endogenous variables, so are by construction the spatial and temporal lags of the dependent variables (see Anselin 1988 and Arellano and Bond 1991). To deal with the spatial lag being endogenous, Kelejian and Prucha (1998) proposed a 2SLS estimation procedure, which involves instrumenting the lag it with the spatial lags of all exogenous variables. Analogously, I instrument the time lags of the dependent variables with the time-lags of all exogenous ones. Taken together, this closely resembles the estimation procedure proposed by Fingleton and Le Gallo (2008). From their paper it follows that one should further include the time-and spatial lags of the instruments for crime and unemployment in order to minimize the risk the residuals are correlated with these. Now, following Kelejian and Prucha (1998), the spatial autocorrelation in the error term is dealt with by first estimating the autocorrelation coefficient ρ using GMM. This is done using the moments Mutl and Pfaffermayr (2011) have derived for the fixed-effects case. Next, a Cochrane-Orcutt type transformation of the data is performed, subtracting the spatial lag multiplied with the estimate of ρ of each variable and thus getting rid of any latent spatial autocorrelation. Overall, the estimation strategy I derived from this looks as follows⁵:

1. First, all variables are within transformed to account for the FE-term.
2. Next, an initial 2SLS estimate of the model is calculated.

⁵All estimations and tests are done in Stata 15 (StataCorp 2017).

3. The residuals of this initial 2SLS regression are then used to estimate the spatial autocorrelation coefficient ρ using GMM.
4. The data is Cochrane-Orcutt transformed using $\hat{\rho}$.
5. Lastly, the model is now estimated again using the same 2SLS procedure from before. Here heteroskedasticity- and autocorrelation-robust (HAC) standard errors are used in order to get viable test statistics.

With this procedure it should be possible to get an unbiased and consistent estimate of how crime effects unemployment and the other way around, assuming the model is not seriously mis-specified.

	Distance cut-off	Queen contiguity	Inverse distance
Distance cut-off	1	-	-
Queen contiguity	.7348	1	-
Inverse distance	.7791	.6362	1

TABLE 2. MC-RESULTS: AVERAGE CORRELATION OF SPATIAL LAGS

In theory, misspecifying the spatial weight matrix W could lead to such a misspecified model, as that matrix contains a definition of neighborhood super-imposed on the model. However, LeSage and Pace (2014) show that, as long as the spatial lags created by different weight matrices are highly correlated, the estimates for the partial derivatives do not differ materially. To check whether this holds in my model, I calculated three of the most common weight matrix specifications⁶, queen contiguity (a), distance cut-off (b) and inverse distance (c), for all German counties. Next, a Monte-Carlo (MC) simulation with a thousand repetitions was run. In each repetition, a random vector y of size 401x1 was drawn from a standard-normal distribution. Then the spatial lags of this vector were calculated, $\tilde{y}_k = W_k y$ for $k \in \{a, b, c\}$, and then the Pearson correlation coefficient of the spatial lags. When taking the average across all repetitions, I got the results shown in Table 2. LeSage and Pace (2014, p. 225) suggest that a correlation coefficient of greater than .5 would mean that the resulting effect estimates are very similar, so it does not seem to matter which specification I choose to use a distance cut-off matrix. This is a $N \times N$ matrix of binary weights w_{ij} that turn zero if the distance between the centroid of county i

⁶All matrices are row-standardized. See Anselin (2002, pp. 256–260) for specification details.

and that of county j is greater than some cut-off r , and one else. Lastly, this matrix is row standardized by dividing each weight with the sum of weights in that row, $\tilde{w}_{ij} = \frac{w_{ij}}{\sum_{j=1}^N w_{ij}}$. For the cut-off point r I chose 58.42 kilometers, as this is the minimum distance which ensures that each county has at least one neighbor.

To further ensure the model is not significantly misspecified, a range of tests are deployed. As mentioned before, in order to give consistent estimates, it is important that the instruments are both exogenous and relevant, i.e. correlated with the variable to be instrumented. For the latter, simple correlations already suggest that the instruments chosen are indeed relevant (see Table 9 in the appendix). To further ensure relevance, the partial R^2 of the first-stage estimations can be checked, i.e. the part of the variation that is explained only by the excluded instruments. To then check whether these are truly exogenous, I use the well-known Sargan-Hansen test (Sargan 1958), which in essence tests whether the residuals are correlated with the instruments used. A potential bias due to omitted variables is somewhat mitigated by the inclusion of cross-sectional and time fixed effect, which control for all latent, time-invariant covariates as well as time trends which affect all counties homogeneously. In order to rule out omitted variable bias further, a range of covariates were included in the regressions, a detailed account of which can be found in the notes beneath the regression tables.

5 Results

Turning now to the results, the model as laid out above does not seem too misspecified. The main results of the base model are shown in Table 3, the specification test results in Table 4. As most partial R^2 -statistics are fairly high, I am satisfied that the instruments used are indeed relevant, which is also born out by joint F test of the excluded instruments in the first-stage regressions (not reported). The Hansen-J statistics further suggest that the instruments used are also exogenous, as they are far from significant. A range of robustness checks have been performed that largely confirm these results, they can be found along with the full estimation results of the base model in the appendix (Tables 10 and 11).

	(1) Unemployment	(2) Street crime
<i>Street crime</i>	.1719* (.071)	-
<i>Unemployment</i>	-	-.0625 (.085)
<i>Time lag: Unemployment</i>	.0672 (.074)	.0875* (.026)
<i>Time lag: Crime</i>	-	.2840 (0.103)
<i>Spatial lag: Dependent var.</i>	.9069*** (.053)	.7489*** (.000)
ρ	-.2567	-.1396
<i>Covariates included</i>	Yes	Yes
<i>Time dummies</i>	Yes	Yes
<i>Fixed effects</i>	Yes	Yes

NxT=4812. Robust std. errors in parenthesis. ***p< .001, **p< .01, *p< .05

TABLE 3. BASE 2SLS RESULTS

Note: All variables are logarithmized. Street crime, unemployment the time and the spatial lags of were instrumented. Covariates included in model (1) were the the time lag of disposable household income, the number of newly registered DE-domains p.c. and the high-school dropout rate. Covariates included in model (2) were the the lagged clearance rate of street crime, GDP per capita, the divorce rate and population density. Excluded instruments in model (1) were the clearance rate of street crime and the number of government employees per capita as well as the time- and spatial lags of all instruments. Excluded instruments in model (2) were the Blanchard-Katz instrument and the time lag of GDP p.c. as well as the time- and spatial lags of all instruments.

The coefficient estimates of the base model suggest that there indeed exists a bi-directional relationship between crime and unemployment, although a bit more complicated than hypothesized in **H1**. While crime does increase unemployment, and higher unemployment

also increases crime, the latter effect seems to be delayed by one period. Interestingly, the contemporaneous coefficient is even negative, but not significant. This might support the Cantor-Land hypothesis that, at first, a higher unemployment rate *lowers* the crime rate, because the change in routine makes unemployed people less likely to become victims, while after some time increasing crime rates due to a motivational effect. However, since the data is not very granular in the time dimensions, this has to be seen as a rough estimate of the time dynamics at play. It seems likely that repeating this exercise with quarterly, monthly or even daily data would result in more exact and easier to interpret estimates.

	(1) Unemployment		(2) Street-crime	
<i>Avg. Direct effect of crime</i>	.1720		-	
<i>Avg. Indirect effect of crime</i>	.0119		-	
<i>Avg. Direct effect of 1 yr. lag unemployment</i>	-		.1013	
<i>Avg. Indirect effect of 1 yr. lag unemployment</i>	-		.2361	
	Statistic	p-value	Statistic	p-value
<i>1st stage: Partial R² endogenous var.</i>	.1655	-	.2433	-
<i>1st stage: Partial R² spatial lag</i>	.4933	-	.1879	-
<i>1st stage: Partial R² time lag unemp.</i>	.1801	-	.2199	-
<i>1st stage: Partial R² time lag crime</i>	-	-	.0726	-
<i>Hansen J</i>	10.457	.3148	13.615	.1913

TABLE 4. MARGINAL EFFECTS & SPECIFICATION TESTS OF BASE 2SLS ESTIMATION

It should be noted here that the reported coefficients cannot directly be interpreted as marginal effects, due to the implied feedback loops from spatial dependence. Therefore, the average direct and indirect effects of crime and unemployment respectively are reported in Table 4. They can be interpreted as the effect a change in one variable would have on the dependent variable in the county directly, and indirectly on a neighboring county⁷. It seems that the effect of crime on unemployment mostly localized, a rise in crime does not really effect unemployment in neighboring counties. Yet unemployment does have a spill-over effect, on average the impact on crime in the neighboring counties is even larger than in the county itself. This might be because unemployment has both a motivational effect, but also makes the region poorer, so it becomes profitable to cross county borders. In general, however, these results support both hypothesis **H1** and **H3**, there seems to exist a bi-directional relationship between crime and unemployment, and there is evidence

⁷For details on why the estimated cannot be interpreted as marginal effects and how these are calculated, see LeSage and Pace (2009, p. 34).

for spill-over effects that could in theory cause such a viscous cycle to spread across regions.

It might now be interesting to further pinpoint the effects found in the base model. For that, I first estimated the same model with different types of employment as the dependent variable (see Table 5). Interestingly, the hypothesized effect of crime is only significant when regressing on employment in the low-skill service sector, i.e. retail, hospitality and personal services. The effect on high-skill employment, i.e. finance, insurance and other professional services, is of similar magnitude, but just insignificant, while there seems to exist almost no effect on employment manufacturing or agriculture. This gives some insight into how this relationship might work. Employment in businesses that rely heavily on foot-traffic or at least require customers to physically visit the place of business, seems to be affected the worst by crime, and employment in sectors where the customer almost never visits the business the least.

	(1) Low-skill services	(2) High-skill services	(3) Manufacturing	(4) Agriculture
<i>Street crime</i>	-0.0778** (0.0273)	-0.0957 (0.0654)	-0.0317 (0.0189)	0.0628 (0.0747)
<i>Time lag: Employment</i>	-0.0074 (0.0491)	-0.0719 (0.0717)	0.831*** (0.0298)	0.816*** (0.167)
<i>Spatial lag: Employment</i>	0.264** (0.0999)	0.570*** (0.0861)	0.117* (0.0501)	0.0309 (0.240)
<i>Covariates included</i>	Yes	Yes	Yes	Yes
<i>Time dummies</i>	Yes	Yes	Yes	Yes
<i>Fixed effects</i>	Yes	Yes	Yes	Yes

NxT=4812. Robust std. errors in parenthesis. ***p< .001, **p< .01, *p< .05

TABLE 5. 2SLS ESTIMATES: EMPLOYMENT

Note: All variables are logarithmized. Street crime and the time and the spatial lags of employment were instrumented. Covariates included were the the time lag of disposable household income and the high-school dropout rate. Excluded instruments were the clearance rate of street crime, the share of males between 12 and 18 as well as the time- and spatial lags of all instruments.

Also, notably, the spatial spill-overs are much smaller than in the base model, and close to zero for manufacturing and agriculture. This is in line what with the visual evidence from the maps in Figure 3, where employment seemed to be much less clustered than unemployment. Other than that, these results are qualitatively similar to what was found in the base model, further suggesting a suitable specification was found.

Next, it would be interesting to see whether the effect of unemployment on crime is limited to the street crime measure. The results from the baseline model applied to five different measures of crime are reported in Table 6.

	(1) Assault	(2) Vandalism	(3) Drug crime	(4) Theft out of cars	(5) Burglaries
<i>Unemployment</i>	-0.0870 (0.0507)	-0.0815* (0.0403)	-0.0464 (0.0823)	-0.535*** (0.130)	-0.427* (0.182)
<i>Time lag: Unemployment</i>	0.142* (0.0638)	0.122* (0.0543)	0.0914 (0.104)	0.100 (0.120)	0.187 (0.164)
<i>Spatial lag: Dependent variable</i>	0.369* (0.175)	0.839*** (0.199)	0.633*** (0.151)	0.0136 (0.158)	0.215 (0.241)
<i>Time lag: Dependent variable</i>	-0.0347 (0.0203)	0.226 (0.263)	0.344** (0.106)	0.687*** (0.107)	0.823*** (0.246)
<i>Covariates included</i>	Yes	Yes	Yes	Yes	Yes
<i>Time dummies</i>	Yes	Yes	Yes	Yes	Yes
<i>Fixed effects</i>	Yes	Yes	Yes	Yes	Yes

NxT=4812. Robust std. errors in parenthesis. ***p< .001, **p< .01, *p< .05

TABLE 6. 2SLS ESTIMATES: CRIME

Note: All variables are logarithmized. Unemployment as well as the time and the spatial lags of crime and unemployment were instrumented. Covariates included were the lagged clearance rate of street crime, the divorce rate and population density. Excluded instruments in model were the Blanchard-Katz instrument and the time lag of GDP p.c. as well as the time- and spatial lags of all instruments.

Here, the relationship between crime and unemployment seems to differ by crime type. Regressing on the rates of assault, drug crime and to some degree vandalism results in fairly similar estimates. Contemporaneously, higher unemployment seems to lower the crime rate, although now this effect is sometimes significant. Then, with a delay, unemployment has the hypothesized positive impact, out-sizing the initial negative one. The same behavior, however, is not apparent when treating the rate of burglaries and theft from cars as a dependent variable. Here, unemployment has initially a very large negative impact on these crime rates, and the delayed positive effect is much smaller and not significant. This is not very surprising, however. Theft and especially burglaries are usually crimes committed out of a purely financial motive, and often by organized criminals that operate not only across county, but often across national borders. It therefore seems plausible that these crimes are committed in especially affluent regions, while higher unemployment would make a region less enticing for criminals. This might also explain why the latter two types of crime show very little spatial clustering compared to the first three, precisely because

these crimes are less dependent on where a criminal lives. Considering the possibility of an underreporting bias mentioned before, it seems however prudent to not give the estimation results for vandalism and drug crimes too much weight, as these have the lowest reporting rates out of all crimes considered.

Lastly, it is still unclear exactly how crime affects unemployment. We know that the effect seems to be especially large for businesses that depend on customers physically visiting them. Previous literature suggests that this might be because crime incurs cost to businesses, or because crime is a disamenity, driving away customers and even skilled employees. To further examine this, I slightly modified my model to estimate the effect of crime on the per m^2 rent as a proxy for the amenities a region offers and on GDP-growth as a measure of change in economic activity in an area. The same general set-up as in the base model was used, as of course time and spatial lags are exogenous by construction, and it seems plausible that both rents and GDP-growth are endogenous to crime as well. For example, lower rents might attract a different clientele more likely to commit crimes, or lower GDP-growth could lead to more unemployment, which is very likely endogenous as we saw before. Therefore both were again instrumented, leading to the estimation results reported in Table 7.

	(1) Rent	(2) GDP-growth
<i>Street crime</i>	-0.197*** (0.0741)	-0.211*** (0.0279)
<i>Spatial lag: Dependent variable</i>	0.00710 (0.0743)	0.0101 (0.156)
<i>Time lag: Dependent variable</i>	0.871*** (0.103)	-0.617*** (0.132)
<i>N x T</i>	4010	4812
<i>Covariates included</i>	Yes	Yes
<i>Time dummies</i>	Yes	Yes
<i>Fixed effects</i>	Yes	Yes
Robust std. errors in parenthesis. ***p < .001, **p < .01, *p < .05		

TABLE 7. EFFECT OF CRIME ON RENTS AND GDP-GROWTH

Note: All variables are logarithmized. Street crime as well as the time and the spatial lags of Rent per m^2 and GDP growth were instrumented. Covariates included in model (1) were population density, GDP per capita and the share of non-citizens. Covariates in model (2) were the lag of GDP p.c., the exposure to oil shocks and the number of newly registered DE-domains p.c. Excluded instruments in both models were the clearance rate of street crime, the share of males between 12 and 18 as well as the time- and spatial lags of all instruments.

As we can see, crime affects both rents and GDP-growth similarly, both are significantly lowered by an increase in crime. This means for one that crime does indeed increase unemployment in an area because it lowers economic activity. It also suggests that the disamenity explanation is at least part of the reason for this, as crime ridden counties seem a lot less desirable a place to live and, by extension, to be in. However, this effect seems to be very localized again, as neither rents nor GDP-growth seem to exhibit significant spatial autocorrelation. This is largely in line with previous literature, and can be seen as some evidence for the validity of *H2*.

6 Discussion

The estimation results were able to generally confirm the hypothesis derived from the literature. Crime is driven by unemployment, as most well-known theories of crime such as the Becker-Ehrlich model and the social disorganization theory would predict. To a degree, Cantor and Land (1985) were also validated, as it seems unemployment only increases crime after a delay, while the initial impact is negative, although not always significant. And not all types of crime were identically impacted, this pattern could only be observed with street crime, assault and vandalism. Burglaries and theft from cars on the other hand drastically decrease when unemployment rises, likely because these crimes are purely financially motivated and committed by criminals organized across county borders. And as the disamenity framework suggests, unemployment is also impacted by crime rates. A further investigation revealed that this effect is especially large when it comes to employment in the low-skill service industry, while other sectors such as manufacturing or agriculture do not seem to be affected at all. The fact that rents are also negatively impacted by crime suggests that the disamenity framework does indeed have some explanatory power. So there is reason to believe crime and unemployment have a bi-directional relationship, which of course means they might re-enforce each other. And while the effect of crime is largely localized, higher unemployment in one county does increase crime in both the county itself as well as neighboring ones, meaning that a possible vicious cycle could spread across county lines.

In terms of future research, it might be beneficial to repeat this study with more granular data in the space and especially the time dimension. I suspect that the spatial effects found in this paper only imperfectly describe all of the spatial dynamics present, it is for instance well known that both crime and unemployment vary a lot within cities, which is averaged out in this work due to data-limitations. And I feel especially the time dynamics are likely to be more complicated than shown, it might for instance be interesting to find when (and how) exactly unemployment stops to deter criminals and instead motivates them, something I again could not do due to the data I had available. Also, as mentioned, police recorded crime statistics have many drawbacks, most notably that they often suffer from an underreporting-bias, so it might be beneficial to repeat some of this analysis using survey data.

Non-the-less, the policy implications of the results that are stark. Firstly, policy makers should be aware of the possibility of vicious cycles. Crime and unemployment reinforcing each other might spin out of control, with higher unemployment breeding more crime, making people and business leave the area if they can, leading to more unemployment. This seems especially relevant today, since the share of low-skill service jobs significantly increased over the last decade, and these jobs are especially threatened by higher crime rates. On the other hand, good policy might also trigger a virtuous cycle, with lower crime rates and more employment reinforcing each other. That also means policies regarding poverty and unemployment should be considered together with policing for a holistic approach. Slashing funding for re-training or assistance with job-applications while increasing the policing budget might not have the desired effect. Further, as both crime and unemployment seem to spill over into neighboring counties, a purely local strategy for fighting crime or getting people to work seems inadvisable.

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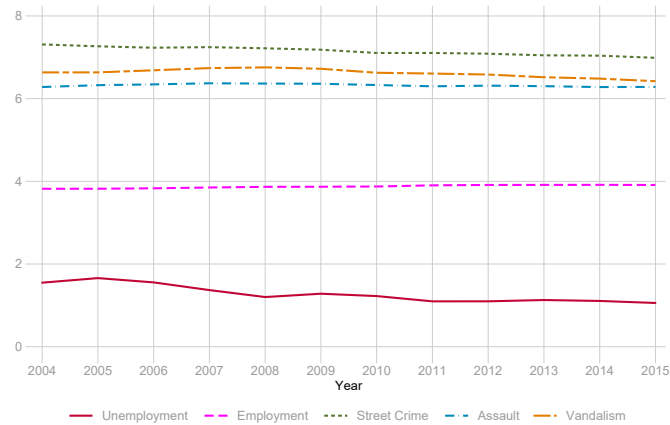
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Appendix

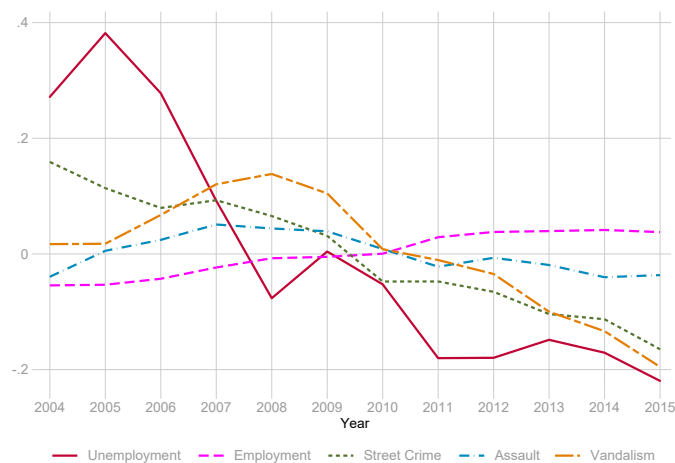
Variable	Share of obs. imputed	Exogenous vars. used	Estimation technique
Assault rates	8.33%	1 yr. lag of assault rate, pop. density and clearance rate	OLS, White std. errors
GDP	7.69%	1 yr. lag of GDP	OLS, White std. errors
Unemployment	0.29%	GDP p.c. and lag of unemployment	OLS, White std. errors
Drop out rate	2.42%	GDP p.c. and lag of drop out rate	OLS, White std. errors
State employees	1.07%	Population density, 1 year lag of state employees	OLS, White std. errors

TABLE 8. IMPUTATIONS USED TO CREATE BALANCED PANEL



Note: Averaged over counties.

FIGURE 4. TIME SERIES OF EMPLOYMENT AND CRIME VARIABLES



Note: Within-demeaned, averaged over counties.

FIGURE 5. TIME SERIES OF CENTERED EMPLOYMENT AND CRIME VARIABLES



FIGURE 6. CRIME AND UNEMPLOYMENT IN VARIOUS COUNTIES DURING THE FINANCIAL CRISIS

Note: Standardized with base-year 2007.

TABLE 9. CORRELATION OF EXCLUDED INSTRUMENTS WITH INSTRUMENTED VARIABLES

	Street crime	Unemployment	Spatial lag: Street crime	Spatial lag: Unemployment	Time lag: Street crime	Time lag: Unemployment
Clearance rate street crime	-0.0337*					
Gov. employees p.c.	-0.101***					
Shift-share instrument	0.159***					
Share of males 10-18	0.479***					
Time lag: GDP p.c.		-0.697***				
Blanchard-Katz instr.		-0.473***				
Exposure to oil price shocks		-0.534***				
Spatial lag: Clearance rate street crime			-0.107***			
Spatial lag: Gov. employees p.c.			-0.214***			
Spatial lag: Divorce rate			0.668***			
Spatial lag: Population density			0.364***			
Spatial & time lag: Disp. household income				-0.612***		
Spatial lag: DE-domains p.c.				-0.390***		
Spatial lag: Highschool dropout rate				0.714***		
Time lag: Clearance rate street crime					-0.138***	
Time lag: Divorce rate					0.584***	
Time lag: Population density					0.246***	
2 yr. time lag: Disp. household income						-0.277***
Time lag: DE-domains p.c.						-0.298***
Time lag: Highschool dropout rate						0.501***

$N \times T = 4812$. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All variables are logarithmized and *within*-demeaned.

TABLE 10. ROBUSTNESS CHECKS: EFFECT OF UNEMPLOYMENT ON CRIME

	(1)	(2)		(3)		(4)		(5)		(6)		(7)
	Full model	No time dummies		No time lags		No spatial effects		Spatial error model		Alternative instruments		No covariates
<i>Unemployment</i>	-0.0626 (0.0364)	-0.0525 (0.0352)	0.00913 (0.0301)	-0.0773 (0.0662)	-0.0805 (0.0600)	-0.0563 (0.0938)	-0.0309 (0.0585)					
<i>Time lag: unemployment</i>	0.0875* (0.0393)	0.0863* (0.0399)		0.152* (0.0667)	0.154** (0.0592)	0.104 (0.0897)	0.0125 (0.0665)					
<i>Spatial lag: Street crime</i>	0.749*** (0.150)	0.770*** (0.131)	1.031*** (0.0774)			0.700*** (0.172)	0.918* (0.391)					
<i>Time lag: Street crime</i>	0.284 (0.174)	0.280 (0.164)		1.090*** (0.105)	1.128*** (0.107)	0.310 (0.193)	0.0231 (0.474)					
<i>Government employees p.c.</i>	-0.0106 (0.0152)	-0.00859 (0.0157)	-0.0101 (0.0201)	-0.00371 (0.0189)	-0.00312 (0.0166)	-0.00856 (0.0164)						
<i>Population density</i>	-0.0983 (0.0692)	-0.0881 (0.0676)	-0.0913 (0.0911)	-0.000164 (0.0947)	-0.0458 (0.0821)	-0.0660 (0.0743)						
<i>Divorce rate</i>	-0.0799 (0.0583)	-0.0708 (0.0534)	-0.0477 (0.0639)	-0.0928 (0.0799)	-0.122 (0.0780)	-0.0474 (0.0586)						
<i>Clearance rate</i>	-0.0436*** (0.00952)	-0.0435*** (0.00938)	-0.0400*** (0.0101)	-0.0600*** (0.0107)	-0.0589*** (0.0146)	-0.0465*** (0.0102)						
<i>Outlier: dep. var.</i>	-0.0198* (0.00887)	-0.0194* (0.00888)	-0.0146 (0.0106)	-0.0339*** (0.00828)	-0.0333** (0.0102)	-0.0213* (0.00914)	-0.182*** (0.0349)					
<i>Outlier: instruments</i>	0.00324 (0.00515)	0.00403 (0.00533)	0.00135 (0.00641)	0.0175* (0.00710)	0.0184* (0.00807)	0.00490 (0.00636)	-0.00252 (0.00964)					
<i>County-reform dummy</i>	0.00237 (0.0106)	0.00255 (0.0105)	0.00173 (0.0139)	0.00615 (0.0157)	0.00585 (0.0119)	0.00235 (0.0104)	0.00450 (0.0126)					
<i>Time dummies</i>	Yes	No	Yes	Yes	Yes	Yes	Yes					
<i>Fixed effects</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes					
<i>N</i>	4812	4812	4812	4812	4812	4812	4812					

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All variables are logarithmized. Unemployment the time and the spatial lags of crime and unemployment were instrumented. Excluded instruments in all but model (6) were the Blanchard-Katz instrument and the time lag of GDP p.c. as well as the time- and spatial lags of all instruments. Excluded instruments in model (6) were exposure to oil shocks and the time lag of GDP p.c.

TABLE 11. ROBUSTNESS CHECKS: EFFECT OF CRIME ON UNEMPLOYMENT

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full model	No time dummies	No time lags	No spatial effects	Spatial error model	Alternative instruments	Employment	No covariates
<i>Street-crime</i>	0.172* (0.0711)	0.236** (0.0738)	0.167 (0.118)	0.623*** (0.125)	0.526*** (0.0885)	0.151 (0.0812)	0.0527 (0.0330)	0.0742 (0.0764)
<i>Spatial lag: Unemployment</i>	0.907*** (0.0533)	0.907*** (0.0642)	0.941*** (0.0369)			0.947*** (0.0734)	0.0605 (0.110)	1.061** (0.364)
<i>Time lag: Unemployment_t</i>	0.0672 (0.0740)	-0.0279 (0.0553)		0.886*** (0.0932)	0.945*** (0.0534)	0.0146 (0.0846)	-0.150*** (0.0237)	0.292 (0.323)
<i>Disp. household income p.c.</i>	-0.281*** (0.0803)	-0.230** (0.0825)	-0.357*** (0.0788)	-0.0412 (0.114)	-0.0189 (0.110)	-0.322*** (0.0837)	0.569*** (0.0898)	
<i>DE-domains p.c.</i>	0.000120 (0.00139)	-0.00187 (0.00152)	-0.00143 (0.00138)	0.00477* (0.00190)	0.00520** (0.00174)	0.000232 (0.00161)	-0.0000279 (0.000418)	
<i>High-school drop-out rate</i>	-0.0194 (0.0315)	-0.0103 (0.0308)	-0.0172 (0.0352)	-0.0506*** (0.0142)	-0.0517 (0.0301)	-0.0185 (0.0334)	-0.00384 (0.00241)	
<i>Outlier dummy</i>	-0.0480** (0.0161)	-0.0400* (0.0157)	-0.0634** (0.0219)	-0.0439*** (0.00972)	-0.0256 (0.0181)	-0.0429* (0.0167)	-0.00200 (0.00240)	-0.0899* (0.0389)
<i>County-reform dummy</i>	0.00455 (0.0155)	0.00663 (0.0169)	0.00392 (0.0164)	0.0104 (0.0246)	0.00241 (0.0206)	0.00484 (0.0168)	0.0000470 (0.00451)	-0.0150 (0.0126)
<i>Time dummies</i>	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fixed effects</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All variables are logarithmized. Street crime and the time and the spatial lags of the dependent variable were instrumented. Excluded instruments in all but model (6) were the clearance rate of street crime, and the share of government employees p.c. as well as the time- and spatial lags of all instruments. Excluded instruments in model (6) were the share of males between 12 and 18, the shift-share instrument of crime as well as the time- and spatial lags of all instruments.