Sex Crime, Murder, and Broadband Internet Expansion - Evidence for German Municipalities

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December 28, 2016

Abstract

This paper studies the effects of the introduction of a new mass medium on criminal activity in Germany. The paper asks the question whether high-speed internet leads to higher/lower sex crime offences and murder. I use unique German data on criminal offences and broadband internet measured on the municipality level to shed light on the question. In order to address endogeneity in broadband internet availability, I follow Falck et al. (2014) and exploit technical peculiarities on the regional level that determine the roll-out of high-speed internet. In contrast to findings for Norway (Bhuller et al., 2013), this paper documents heterogeneous effects within and across (spillover effects) municipalities. While within municipalities the substitution effect dominates for overall sex crime and especially for sexual child abuse, I find strong and positive spillover effects to neighbouring municipalities that are driven by rape and child abuse. The effects on murder increase under the instrumental variable approach but remain insignificant. Overall, the estimated net effects might stem from indirect effects referred to differences in reporting crime, a matching effect, and a direct effect of higher and intensive exposure to extreme and violent media consumption. After investigating the potential mechanism, I do not find any evidence in favor of a reporting or matching effect. This suggests that the estimated net effect is most likely a result of increased consumption of extreme media.

JEL Classification: K42, H40, L96, C26
Keywords: Crime, Broadband Internet, Media

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1 Introduction

During the last 10 to 15 years access to the internet has significantly reduced all kinds of market frictions. The internet makes the transmission of information cheaper and more easily accessible. This higher transmission of information has a strong impact on society and social interaction through social networking, forums and messaging.[1] However, there are only a few studies investigating the effects of (mass) media technologies on adverse side effects. Mastrorocco and Minale (2016) show for the case of Italy that exposure to crime through television shape individual perception and concerns about crime. Card and Dahl (forthcoming) provide evidence of the effect of professional football games on individual behaviour. They show that emotional cues provided by local NFL football games cause higher family violence. As shown in psychological laboratory experiments, the internet reduces pecuniary and non-pecuniary costs of violent and extreme pornography which increases the propensity to commit sex crimes (Donnerstein et al., 1987 and Allen et al., 1995). However, Zillmann and Bryant (1982) find no effect or even a reduction in sexual aggression after exposure to pornography. While laboratory experiments provide interesting insights into exposure and commitment, the reaction of crime consumption on aggression in controlled experiments might be different compared to private settings. By using field data on regional units, this study is mostly related to Kendall (2007) and Bhuller et al. (2013). After controlling for area fixed-effects and explanatory variables on the state-level using US data, Kendall (2007) finds a negative effect of internet on rape incidence. In contrast to most lab studies, the author concludes that online pornography and rape are substitutes. In a recent study using Norwegian data, Bhuller et al. (2013) find a positive and substantial effect of online uses and sexual crime which come primarily from rape. Child abuse does not react to broadband internet. In the empirical strategy, the authors account for time-constant and time-variant unobserved effects and observable characteristics on the municipality level.

This study uses data on the German municipality level to investigate the effect of the introduction of high-speed internet on criminal activity. By doing so, I provide evidence on consumption externalities of the internet that are unlikely to be internalized. The basic idea behind the relationship between internet usage and criminal behaviour is through higher and easier exposure of violent and extreme media input such as pornography violence. This higher exposure might affect individuals behaviour which becomes visible

[1] Among economists, the focus of interest is mainly on efficiency gains in terms of market competitiveness (Brown and Goolsbee, 2000), trade and FDI (Freund and Weinhold, 2004 and Choi, 2003) and hard economic outcome variables such as inflation and GDP growth (Choi and Yi, 2009). More recently, social scientists start to focus on how human behaviour is affected by higher internet exposure. Kolko (2010) investigates the effect of broadband adoption on online and offline activities. Broadband adoption leads to lower time spend on playing video games but not on activities like reading magazines or watching TV. Falck et al. (2014) using data on German municipalities relate the expansion of broadband internet to voter turnout and TV consumption. The authors find a negative effect of internet availability and voter turnout, which they related to a crowding-out of TV consumption.

on the regional level through reported crime rates. Most of the previous studies focus on sex crime. This paper extents the literature by additionally analysing the effects on crime against life such as murder. The basic idea behind internet and crime against life is rather similar to sex crimes. The consumption of violent media might lead to aggressive behavior with the result of higher offences. Therefore, the analysis adds to some extent to an ongoing discussion whether e.g. shooter games might have adverse side effects. Although shooter games can be played offline, the internet provides a way of interactive communication while playing.[2]

The empirical analysis starts by estimating the net effects of internet use on sex crime including all sex crime, sexual child abuse, rape and crime against life on the municipality level. Endogenous selection might play a crucial role in the context of the use of the internet and crime incidences. Beside a potential different behaviour of individuals in lab experiments, individuals select, based on unobservables, into the use of online information. After accounting for municipality fixed-effects and observable characteristics, there might be still time-varying unobserved factors that affect the crime rate and the usage of the internet jointly. To overcome the omitted variable bias, I use similar to Falck et al. (2014) exogenous variation in internet availability by exploiting technical peculiarities of the traditional public switched telephone network which affects the availability of internet. The roll-out of broadband internet in Germany at the early 2000s was based on existing infrastructure. The structure of the public switched telephone network was determined in the 1960s when the goal was to provide telephone service in West Germany. While the location and the allocation of the infrastructure (distribution frames) was irrelevant for the quality of telephony on the household level, the location for the DSL roll-out matters for the availability of broadband. Households in municipalities with a distance above 4,200 meters to the next distribution frame cannot access DSL. Connecting these households requires costly infrastructure projects. This situation defines a quasi-natural experiment where I identify the effects of the introduction of a new mass medium on criminal activity. In the empirical strategy I focus on the years between 2004 and 2008 that define the DSL-period in Germany.

After controlling for pre-existing crime levels and observable municipality characteristics, I find that a 1% point increase in the DSL subscription rate leads to an increase in overall sex crime by on average 1.5 crime cases per 10,000 inhabitants. Higher DSL subscription shows positive but insignificant coefficients for sexual child abuse (0.4 crimes per 10,000 inhabitants), rape (0.18 crimes per 10,000 inhabitants) and for crime against life (0.04 crimes per 10,000 inhabitants). Controlling additionally for time-varying unobserved effects that affect both DSL subscription and crime rates, the results show that overall

[2] Frostling-Henningsson (2009), Jansz and Tanis (2007) and Yee (2006) study the motives and characteristics of first-person shooter game players. They find that beside a connecting motive, primarily young men want to try out behaviour that is *impossible* in real life. However, Ferguson (2008) and more recently Cunningham et al. (2016) do not find any causal link between violent video games and violent crime.

sex crime increases by 0.9 cases per 10,000 inhabitants compared to the OLS estimate. The point estimate for sexual child abuse becomes negative and insignificant, whereas the effect on rape increases strongly accompanied with higher standard errors. The last and preferred specification accounts for fixed-municipality effects and time-varying unobservables. On average, the effect of high-speed internet on overall sex crime becomes negative and insignificant. This overall substitution effect is mostly driven by sexual child abuse. The reverse of the sign of overall sex crime is shown the be the results of heterogeneous effects with respect to the initial crime level. Among crime against life, the point estimate increases ten-fold but remains insignificant.

As a further extension of the empirical analysis I estimate regional spillover effects by calculating crime rates in neighbouring municipalities. This is motivated by the Crime Pattern Theory and the journey-to-crime literature. The Crime Pattern Theory states that offenders in general move within familiar nodes along consistent paths (Brantingham and Brantingham, 2008). There exists a rather large empirical literature focussing on the activity space of offenders. Overall, the distance traveled to crime sites varies by classification and age.[3] Based mainly on data from the US, the average travel distance among sexual assault and violent crime is about 2.0 miles (Bichler et al., 2011). Conducting a meta-analysis, Rossmo (2005) find that over time offenders travel on average farther in more recent years. The fact that the average size of the German municipalities corresponds to a radius of 1.9 miles (3.1 km) (Falck et al., 2014) suggest that spillover effect are likely to occur in neighbouring municipalities. Results show that there is a positive and significant effect of broadband internet expansion on all sex crime in neighbourhood municipalities that is mainly driven by sexual abuse against children and rape. The effects on crime against life are low and not significant after controlling for fixed-effects and time-varying unobserved factors.

Decomposing the net effect shows that the introduction of the new mass medium - DSL - explains almost 50% of the increase in overall sex crime. In a hypothetical situation without broadband internet, overall sex crime rates would have been 9.2% lower. However, further results suggest that this is mostly driven by an increase in rape and especially the distribution and the possession of pornographic material. The increase in illegal pornographic material that is with over 90% child-related provides a potential explanation of the substitution effect for sexual abuse against children. Comparing the results to the Norwegian case, this study suggest a much stronger connection between internet and sex crime.[4]

[3]Phillips (1980) and Snook (2004) find that older offenders travel farther away from home than younger offenders. Among adults the distance to property crime is shorter compared to violent crime, whereas the opposite is true for younger individuals.

[4]The main reason for the differences in magnitude might be the empirical design. While Bhuller et al. (2013) identify the effect based yearly variation, this paper uses variation over two different periods and a longer time interval.

The estimated net effects may stem from three possible mechanisms that might be in place especially for body-related offences such as sex crime and crime against life. Beside the direct effect of high-speed internet that comes from higher exposure or better opportunities to consume violent media input, there are two other mechanisms that may drive the results. Following Bhuller et al. (2013) there might be a matching effect. On the one hand, the internet makes the search process more efficient and reduces uncertainty and information constraints. This mechanism can increase the number of matches between offenders and victims. Moreover, the internet may expand an individual's network which might increase the probability of a match. On the other hand, spending time online decreases the probability of meeting other individuals and committing a crime. While the net effect is not clear, I further investigate the effect on all crimes other than sex crime or crime against life. If individuals spend more time at home, than this should be visible by observing a reduction for all crime rates. The results do not show any effect on overall crime. Bauernschuster et al. (2014) show that broadband internet access at home does not affect the amount of time spend at home vs. meeting friends and going to the cinema and/or restaurants. This result provides suggestive evidence that the probability of a match between victims and offenders might not be influenced by broadband internet access. Although the investigation of the full matching effect seems not to drive the empirical results, there remains some uncertainty left. The main uncertainty comes from the fact that individuals expand their network and search actively for potential victims. Investigating this mechanism is out of the scope of this paper but would add substantial knowledge in understanding the full effect.

A further possible mechanism might stem from differences in reporting. This is especially important for sex crimes as underreporting is a common concern (Tjaden and Thoennes, 1998). The internet might decrease the costs of reporting a (sex) crime. It is in fact possible that the internet provides a platform for victims to communicate with others (other victims or support groups) in an anonymous way which increases the likelihood of reporting the crime. Reporting criminal offences via filling in online forms in the early DSL period was to some extent possible for selected Federal States. Robustness results show, however, that this is not driving the relationship between broadband internet and considered crime. The empirical analysis tries further to investigate the reporting effect by analyzing detection rates. It should be noted that analyzing detection rates is based on the assumption that lower costs of reporting should result in weaker sex crime cases on average. If this is true, the detection rate should go up as weaker cases have a higher probability of being declared. Empirical results show that higher broadband access does not affect detection rates.

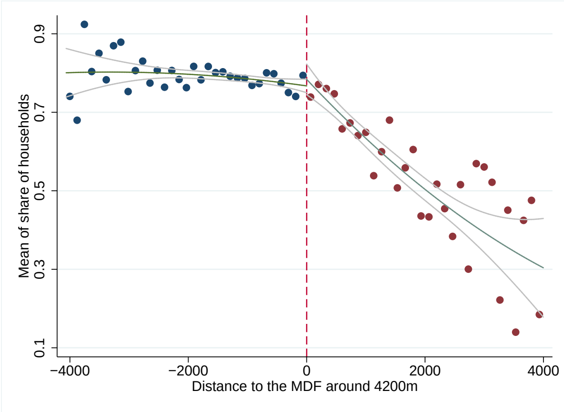
The remainder of the paper is structured as follows. Section 2 explains the source of identification, before section 3 presents the empirical strategy. Section 4 describes the data sources and provides descriptive statistics for the two defined periods. Section 5 presents the net results of the effect of broadband expansion on the different criminal offences. This section further presents robustness and placebo tests. Section 6 investigates the external validity of the results by comparing developments of criminal activities over time with selected municipalities and municipalities that cannot be used for identifying the effect given the instrumental variable strategy. In section 7, I discuss the possible mechanisms that might drive the net effect of reported crime. Section 8 summarizes and concludes.

2 Identification

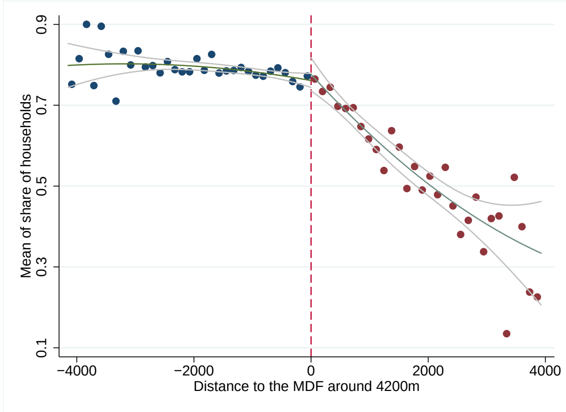
Identifying the effects of internet availability on criminal offences suffers from selection bias. Regions with high-speed internet access are on average different in many aspects. These regions typically are higher agglomerated, have a higher share of skilled individuals and higher per capita income indicating that the composition of the areas are different. These characteristics are correlated with the willingness to pay for high-speed internet which is also plausible to have an effect on crime. By simply comparing crime rates for two different high-speed internet levels, I would not be able to estimate the true causal effect. As a result, a simple regression across municipalities of DSL availability on crime would be potentially downward biased.

To overcome the omitted-variable bias, I will make use of regional peculiarities of the traditional public switched telephone network (PSTN), which affects the possibility to provide DSL in certain municipalities. As described in Falck et al. (2014) and Steinmetz and Elias (1979) early DSL availability required copper wires between the households and the main distribution frame (MDF). The implementation of the new technology was done through the regional PSTN. The structure of the PSTN was determined in 1960s when the goal was to provide telephone service in West Germany. In order to host a MDF, buildings were required with the routs for the cable ducts fixed. While it is the case that in high density areas MDFs are always placed, less agglomerated areas typically share one MDF. The crucial point is that the length of the copper wires did not affect the quality of the telephone services while for DSL connection the distance does matter. There exist a critical value of 4,200 meters. It is not feasible for regions that are more than the critical value apart from the next MDF to use DSL via a copper wire. The only way to make DSL available is by replacing the copper wire with other material such as fiber wire. However, the construction of fiber wire lines induces high infrastructure investments, thus, takes time and is costly.

These technical peculiarities provide a quasi-experimental situation for less agglomerated municipalities without an own MDF where the distance from the regional center of each municipality to the MDF can be used as an instrument for DSL availability. I use this situation for West German municipalities that are connected to a MDF located in another municipality and where no closer MDF is available. To illustrate the DSL availability



(1-A) DSL 2005 - geographic



(1-B) DSL 2005 - population

Notes: The figures plots the fraction of households with access to DSL on the distance to the next MDF for the year 2005 using a linear quadratic fit. The left panel uses the geographic centroid of the municipality whereas the right panel uses the population weighted center. The figures use all available West German municipalities without an own main distribution frame.

Figure 1: Share of Households with DSL Access

rates on the household level for different distances, Figure 1 plots the mean of the share of households that have access to DSL for the year 2005 for distances below and above the critical value of 4,200 meters. For illustrative purpose, I set 4,200 meters to zero. The left panel describes the relation using the geographic centroid of the municipality whereas the right panel uses the population weighted center. Municipalities with relatively short distances to the next MDF (left to the vertical line) show a rather constant fraction of about 80% of households with DSL access. If the distance becomes more than 4,200 meters, the relationship becomes strongly negative - the share of households with DSL access decreases sharply. A similar picture emerges on the right hand side of the figure by using the population weighted geographic center. Using this alternative distance measure results in slightly higher DSL access rates with the shape of the predicted values remaining the same.[5]

3 Empirical Strategy

The start of the empirical investigation of whether broadband internet access leads to different crime offences is by first looking at the simple cross-section of crime offences of municipality i at time t. In the cross-sectional analysis, I focus on the years between 2005 to 2008 as this period is defined to be the DSL period in Germany. Thus, I regress each of the crime variables on the share of households with home internet access in municipality

[5]See appendix B for a graphical illustration of the distribution of treated and non-treated municipalities across space.

i, a vector of covariates X_{it} and time-fixed effects:

$$crime_{it} = \beta_0 + \beta_1 DSL_{it} + X'_{it}\beta_2 + \lambda_t + \epsilon_{it} \tag{1}$$

where the comparison is between municipalities without an own MDF but that share the same MDF (fixed-MDF effects) and differ in their distance to that MDF.

The empirical model stated in equation (1) might be subject to endogeneity issues. If, for example, individuals in municipality i buy broadband internet because of the desire to engage in body-related criminal activity. Moreover, innovative and open-minded regions might be more willing to pay for broadband internet which is potentially correlated with crime offences. In order to address these selection concerns, I first use panel information and account for pre-existing levels of crime offences. By including pre-existing crime levels, I account for time-invariant unobserved effects that relate to broadband internet and crime. A potential candidate is the year 2001 (see for a similar strategy Bauernschuster et al., 2014) as broadband internet was just at the beginning to expand. Thus, I augment the OLS model in equation (1) by $\beta_3 crime_{i,2001}$. The true β_1 is identified under the assumption that between the years 2005-2008 and 2001 there are no time-variant unobserved effects that affect both, broadband internet and crime rates.

In order to account for potential time-variant unobserved effects that are correlated with both, the crime rate and DSL subscription rate at the municipality level I follow an instrumental variable approach. To overcome the potential source of endogeneity, I use as an instrument the traditional public switched telephone network (PSTN) that affects the probability of DSL subscriptions at the municipality level. The two-stage model can be written as:

$$crime_{it} = \beta_0 + \beta_1 \hat{DSL}_{it} + X'_{it}\beta_2 + \beta_3 crime_{i,2001} + \lambda_t + \epsilon_{it} \tag{2}$$

$$DSL_{it} = \gamma_0 + \gamma_1 PSTN_i + X'_{it}\gamma_2 + \gamma_3 crime_{i,2001} + \lambda_t + \psi_{it} \tag{3}$$

for $t = 2005, ..., 2008$. *crime* measures the crime level per 10,000 inhabitants in municipality i at time t. In the first stage, PSTN is a dummy variable that takes the value *1* if a municipalities' distance is above 4,200 meters from the next MDF and zero otherwise. This IV strategy identifies local average treatment effects for the compliant municipalities. For the main specification, the distance measure is between the geographic centroid and the MDF. Robustness estimates are shown based on the population weighted center.[6]

In a further step of the empirical approach, I account for municipality fixed-effects by comparing crime rates before the DSL era (defined between 1995-1999) with crime rates during the DSL era. Given data availability constraints, I focus on the years between 1996

[6]For comparison purpose with the IV models, the OLS specification in equation (1) and the augmented model with the crime rates in the year 2001 are estimated on the set of municipalities without an own MDF but that share the same MDF.

to 1999 for the pre-DSL period and 2005 to 2008 for the DSL period. This specification is a first difference model comparing the two defined periods and municipalities that share a MDF but differ in their distance to the MDF. The model can be written as:

$$\Delta crime_i = \beta_0 + \beta_1 \Delta D\hat{S}L_i + \Delta X_i' \beta_2 + \lambda_t + \epsilon_i \tag{4}$$

$\Delta crime$ measures the change in the crime rates between the pre-DSL and the DSL period. This first difference model is equivalent to a fixed-effects regression model as I pool differences between particular pre-DSL and DSL years and control for time-fixed effects. Given that DSL availability is zero in the pre-DSL period, equation (4) regresses the actual level of households with DSL on the change in the crime rates. ΔX_i is a vector of characteristics at the municipality level and ϵ_i is an idiosyncratic error term. In the first-difference specification I use *9-years* differences and connect one pre-DSL year to one DSL year. Thus, I estimate the differences between the pairs of 1996-2005, ..., 1999-2008 and control for time fixed-effects and observable characteristics in the regressions.

The first difference model accounts for unobserved time-constant effects at the municipality level. By using the same IV approach, the first stage can be written as;

$$\Delta DSL_i = \gamma_0 + \gamma_1 PSTN_i + \Delta X_i' \gamma_2 + \lambda_t + \psi_i \tag{5}$$

where PSTN is a dummy variable that takes the value *1* if a municipalities' distance is above 4,200 meters from the next MDF and zero otherwise.

4 Data and Descriptive Statistics

Data. The paper uses variation on the German municipality level that comes from different sources. The German Federal Criminal Office (*Bundeskriminalamt*) provides time series data for several crime categories and regional units such as municipality, county or federal state level that is delivered by the German State Criminal Offices (*Landeskriminalamt*). On the municipality level, however, they provide data only from 2009 onwards. Before 2009 the German State Offices summarized criminal offences on the aggregate county and federal state level. Retrieving crime statistics before 2009 turned out to be difficult and for some states impossible especially for statistics two decades ago. Moreover, some states have data going back to 1995 but on an aggregated criminal offences level. For Rhineland-Palatinate, the earliest available year for crime statistics is 2001. Given data availability restrictions, I am able to use information on criminal activity on the western German municipality level for four Federal States, namely Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony.[7] West Germany in total consist of 8,157 munici-

[7] Overall sex crime and crime against life is available for all four Federal States. Sexual child abuse is missing for municipalities in Rhineland-Palatinate and rape is missing for municipalities in Rhineland-Palatinate and Bavaria. See the data appendix for an overview of available information.

palities (in 2008 boundaries). The four Federal State make 77% (6,306) of all municipalities in West Germany. Under the instrumental variable strategy, I am able to use a sample of municipalities within the four Federal States equal to 2,689. By comparing the sample size of 2,689 municipalities without an own MDF to all West German municipalities without an own MDF (3,333), these municipalities cover about 81% of all available municipalities. According to the crime categories I focus on overall sexual crime, sexual abuse against children, rape and crime against life. Sex crime consist of several sub-categories of §174 StGB (criminal code) to §184 StGB including for example sexual abuse and rape. Crime against life summarizes murder under §211 StGB as well as illegal abortion under §218 StGB to §219 StGB.

Broadband internet availability in Germany can be measured on the municipality level as the share of households having access to high-speed internet. The original data are from the broadband atlas (*Breitbandatlas Deutschland*) published by the Federal Ministry of Economics and Technology (2009).[8] The telecommunication operators self-report covered households with a minimum data transfer rate of 384 kb/s and the self-reported data is available between 2005 onwards for all German municipalities in 2008 territories. I will make use and concentrate on digital subscriber line technology (DSL) availability as this is the dominant technology in Germany. The diffusion of high-speed internet in Germany started in 2000/01. Within the period between 2002 and 2008 broadband connections increased from 3.2 million DSL lines to almost 23 million lines (Bundesnetzagentur, 2012). I follow the literature and define two periods. The DSL period covers the years between 2004 to 2008 whereas the pre-DSL period covers the years between 1995 to 1999 (Falck et al., 2014). By comparing the DSL period with a pre-DSL period one identifies the effect of the introduction of a new mass medium on selected criminal activities.

In addition to crime and internet information I exploit further regional characteristics on the municipality level such population shares (age cells, female share in age-groups, share of foreigners in age-groups), regional net migration rate, unemployment rate, average real wage level, the educational level, police density, firm density and occupational shares within the regional unit and the share of individuals attending labor market programs. The data are provided by the German Statistical Office and the Research Center at the IAB (see Table A.1 in the Appendix). Some variables for the pre-DSL and DSL period are also available from Falck et al. (2014).

Descriptive Statistics. Despite a rapid expansion of high-speed internet there are differences in socio-demographic characteristics of internet users. On average the fraction of individuals using the internet increased within five years from about 37% at the beginning of the new century to 55% in 2005. Based on the (N)onliner Atlas (2005) young (more

[8] The established Breitbandatlas is one feature of the joint project between politics and investors to increase the access rate of households in Germany (Bundesagentur für Wirtschaft und Technologie, 2009).

Table 1: Descriptive Statistics

	pre-DSL period (1)	DSL period (2)	Difference (2)-(1) (3)
Panel A: Crime rates (per 10,000)			
All sex crimes	3.513	4.255	0.742
	(6.857)	(7.165)	(9.449)
Sex child abuse	0.953	1.051	0.098
	(2.614)	(2.665)	(3.697)
Rape	0.325	0.664	0.338
	(1.160)	(1.742)	(2.076)
Crime against life	0.242	0.168	-0.073
	(1.417)	(0.875)	(1.648)
Panel B: Broadband availability			
DSL (share of households)	0	74.63	74.63
	(0)	(21.11)	(21.11)
Panel C: Selected regional information			
Population	5,514	5,580	49.19
	(23,156)	(23,938)	(1000.4)
Female population share	50.27	50.57	-0.038
	(1.737)	(4.362)	(0.914)
Population share aged 18-65	65.74	62.58	-3.443
	(2.757)	(5.026)	(4.902)
Population share aged >65	16.53	18.90	2.312
	(3.297)	(3.515)	(1.627)
Unemployment rate	3.983	4.343	0.603
	(1.716)	(2.155)	(1.586)
Net migration rate	0.509	-0.077	-0.587
	(1.774)	(1.584)	(2.314)
Number of municipalities	6,211	6,211	6,211

Notes: The table reports descriptive statistics for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. The number of observations is less than 6,306 due to missing values in crime rates for 45 municipalities. 47 municipalities from Rhineland-Palatinate are excluded in the analysis with an average population size of 289. These small municipalities induces large outliers and drive the results to extreme values. Column (1) reports mean and standard deviation for the pre-DSL period defined for the years 1996 to 1999. Column (2) reports mean and standard deviation for the DSL period defined for the years 2005 to 2008. Column (3) reports the change between the two defined periods. DSL availability refers to the year 2005. Source for selected regional information is reported in the appendix.

than 80% below 30 years) and better educated (more than 80% among university graduates) individuals use the internet intensively. According to occupations, the data show that especially white-collar workers (75%) are among the internet users, whereas only 52% of unemployed individuals use the internet at the time of the interview. Although the empirical analysis is on the regional level, these numbers provide useful insights into the main user pool.

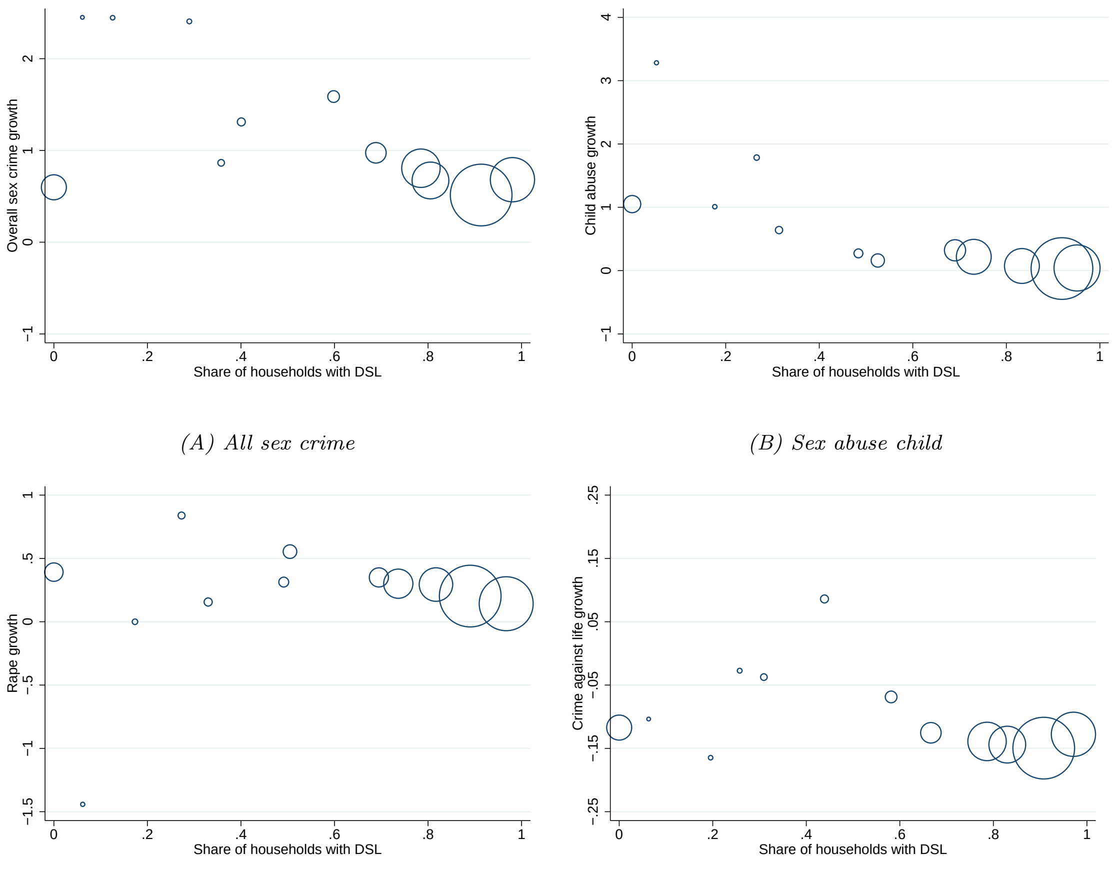
Table 1 shows descriptive statistics (mean and standard deviation in parentheses) of crime rates, DSL availability and selected regional characteristics for the two defined periods. According to crime rates, I define the variables in terms of crime per 10,000 inhabitants. In total, there are 3.5 overall sex crimes per 10,000 inhabitants in the pre-DSL period. This number increases to about 4.3 cases per 10,000 inhabitants in the DSL period. Sexual abuse against children in turn increased slightly between the two periods from less than 1 case per 10,000 inhabitants to 1.05 cases and accounts for about 25% of all sex crimes. Rape doubled between the two periods from 0.33 cases to 0.66 cases per 10,000 inhabitants and accounts for 16% of overall sex crime. By using more detailed data from 2009 for all available German municipalities, I find that the main categories among all sex crime are rape that accounts for about 15%, total sexual abuse 45% including sexual child abuse (23%) and the distribution of pornographic material 24%. This suggest that the information used in this paper are representative for Germany. Crime against life shows a slightly decreasing pattern over time with 0.24 cases per 10,000 inhabitants in the pre-DSL period and 0.17 cases in the period between 2005 and 2008.[9]

The second panel of Table 1 reports the fraction of households having access to DSL in West Germany. In the pre-DSL period there is by definition no household with DSL. In 2005, 74.6% of all households have a DSL subscription. This number increased from 2005 to 2008 by almost 15% points. Panel C reports selected characteristics on the municipality level. It shows that the population is aging, the average unemployment rate increased and on average, the considered municipalities experienced out-migration.

Graphical Evidence. Figure 2 plots the graphical relationship between DSL growth rate and the growth of the four crime categories between the pre-DSL and the DSL period controlling for year and MDF fixed effects. For the purpose of visualization, I present the graphs in 0.1 bins until 0.8 and between 0.8 to 1 I define 0.05 bins and calculate the average change in the crime rates within the bins. The reason is because at higher DSL rates the density is higher and based on Figure 1 the average DSL rate below the threshold of 4,200 meters is about 0.85. The size of the cycle captures the number of municipalities within the defined DSL bins.

For overall sex crime (Panel A) and sexual child abuse (Panel B) the figure shows a negative relationship between DSL and crime growth. Higher DSL rates are associated with lower crime growth which is slightly more pronounced for sexual child abuse. The graphical relation for rape and crime against life is less clear. It is important to note that with the empirical strategy stated above, I compare municipalities that are connected to the same main distribution frame but differ in their distance to the frame. Thus, the empirical strategy connects municipalities to the right in the Figure 1 with municipalities to the left. For example, the unweighted average in Panel B of Figure 2 is about 1.1

[9]See appendix B for a graphical illustration of the crime and DSL rates on the regional level.



(A) All sex crime

(B) Sex abuse child

(B) Rape

(D) Crime against life

Notes: The figure plots graphically the relationship between DSL growth rate and the change in crime rates from the pre-DSL to the DSL period conditional on year and MDF fixed effects. The size of the circles depend on the number of municipalities within the respective DSL bins.

Figure 2: Growth in DSL and crime rates

for municipalities below the threshold of 4,200 meters. Comparing the number with on average 0 growth for municipalities above the threshold, we obtain a difference of -1.1 child abuse cases.

5 Results

5.1 Baseline Estimation

The analysis of the effect of broadband internet on criminal offences uses an instrumental variable strategy based on the geographic centroid. As the variation comes from the municipality level, I cluster standard errors on the municipality level. Table 2 shows the baseline results. Each presented coefficient corresponds to a single regression. The table starts by presenting the results from OLS regressions for the years between 2005 to 2008 followed by augmenting the regression with pre-existing crime levels from the year 2001 and additionally accounting for time-varying unobserved effects. In a last step, the *IV + FD* results account for fixed-municipality effects. In this case, the dependent variables are the changes in crime rates per 10,000 inhabitants between the pre-DSL and DSL period. The first row shows the estimates for a linear regression. Conditional on covariates, there is

Table 2: Estimation results of internet availability on crime

	All sex crime		Sex child abuse	Rape	Crime against life
	(1)	(2)	(3)	(4)	(5)
OLS	1.658***	1.497***	0.430	0.158	0.039
	(0.488)	(0.473)	(0.269)	(0.286)	(0.027)
OLS + crime level$_{2001}$	1.658***	1.451***	0.430	0.148	0.038
	(0.488)	(0.458)	(0.268)	(0.281)	(0.027)
IV + crime level$_{2001}$	2.869**	2.354*	-1.252	2.068	0.045
	(1.351)	(1.331)	(1.950)	(2.297)	(0.114)
IV + FD	-1.620	-2.120	-3.676*	0.256	0.691
	(2.335)	(2.390)	(2.225)	(2.879)	(0.701)
F-test (first stage)	211.9	209.4	35.1	13.0	209.4
Observations	10,736	10,736	4,421	2,208	10,736
Number of MDFs	707	707	425	204	707
Municipalities	2,689	2,689	1,106	557	2,689
Control variables	No	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleinbergen-Paap F-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

a positive association between broadband internet and selected crime categories, statistical significant for overall sex crime. This association decreases slightly after controlling for pre-existing crime levels measured in 2001. In terms of magnitude, it shows that a 1% point increase in DSL increases overall crime by about 1.5 cases per 10,000 inhabitants. By instrumenting the DSL rate ($IV + crime\ level_{2001}$), I find that simple OLS estimates are downward biased for all sex crime, rape and crime against life. For sexual child abuse, the OLS estimate turns out to be upward biased. However, the IV model increases the standard errors strongly, resulting in insignificant effects for sexual child abuse, rape and crime against life. The last estimation results additionally control for fixed-municipality effects by taking differences between the DSL-period and the pre-DSL period. The point estimates for overall sex crime and child abuse decrease further, indicating a potential substitution effect between broadband internet and sex crime. In contrast, the effect on crime against life increases strongly but still remains insignificant, whereas rape becomes close to zero. The lower part of the table reports information statistics. Conditional on MDF-by-year fixed effects and control variables, municipalities above the threshold have on average a 5% to 15% lower DSL rates (depending on the sample) with a F-statistic ranging between 13 to 211. Thus, concerns about weak identification issues do not apply in this setting.

The applied strategy suggest that the relationship between crime and DSL is constant. Figure 1 shows that depending on the distance to the next MDF, the exposure to DSL is a continuous function of the distance before and after the threshold. Given that the DSL rate is rather constant for municipalities close to a MDF, one can interpret this level as the counterfactual DSL rate. By taking the difference between the counterfactual DSL rate and the predicted DSL rate based on the distance one obtains a measure of exposure to DSL. In fact, it could be the case that an increase in DSL availability is different for municipalities rather close but treated compared to municipalities rather far away from the threshold and treated. Thus, I narrow the set of municipalities that are located 2,000 meters around the threshold. This strategy provides insights into the variation that identifies the effect of DSL on crime and makes sure that high-speed internet access is technologically viable. Table 3 presents the results. According to the level specification (IV + *crime level*$_{2001}$), the Table shows that the point estimates increase in absolute terms for overall sex crime, rape and crime against life. The coefficient for child abuse turns out to be at the same level. The IV and fixed-effects model shows a stronger substitution effect for overall sex crime and child abuse and, although not significant, a stronger positive effect for crime against life. The point estimate of DSL on rape turns negative and not significant at any level.

Table 3: Estimation results for municipalities less than 2,000 meters around the threshold

	All sex crime		Sex child abuse	Rape	Crime against life
	(1)	(2)	(3)	(4)	(5)
IV + crime level$_{2001}$	3.288*	3.332*	-3.807	5.735	0.070
	(1.901)	(1.961)	(4.336)	(3.905)	(0.170)
IV + FD	-5.112	-5.975*	-9.541*	-0.197	1.179
	(3.408)	(3.526)	(5.346)	(4.863)	(1.078)
Control variables	No	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Heterogeneous Effects with respect to the Initial Crime Level. The baseline estimation table shows two interesting facts that are worth to investigate further. The first is the sign flip after accounting for fixed-municipality effects by running a first difference model. The second is the rather high coefficient for sexual child abuse. The sign flip among overall sex crime indicates heterogeneous effects depending on the initial crime level. Thus, for both categories the consideration of a subset of municipalities might provide further insights. In order to have sufficiently large numbers of observation (especially to have a strong enough first stage), I define *high* crime regions as being in the upper decile of the crime distribution and run for the two subset of municipalities the same regressions. The average overall sex crime rate in the high crime regions (upper decile) is 22 cases per 10,000 inhabitants. Among sexual child abuse, the average is slightly below 8 cases per 10,000 inhabitants. The table shows that the average effects reported in Table 2 are heterogeneous with respect

Table 4: IV + FD estimation results for high and low crime municipalities in the pre-DSL period

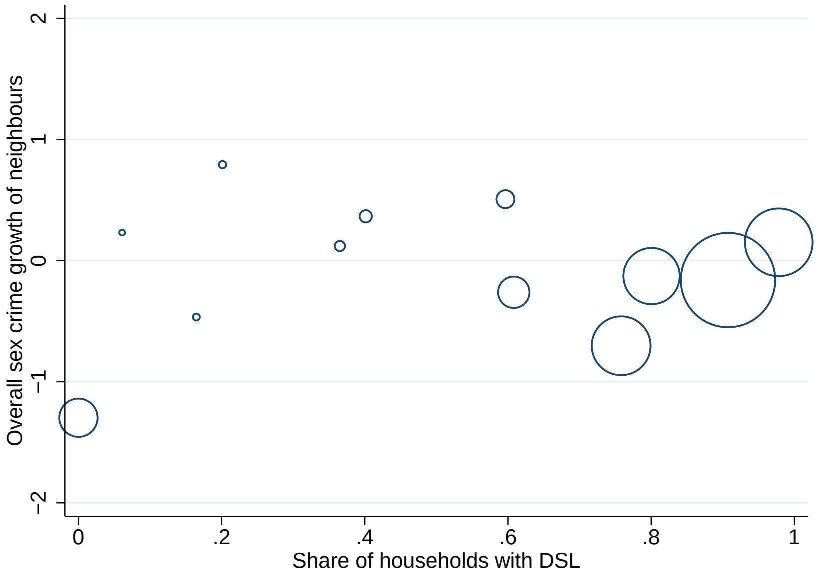
	All sex crime (1)	Sex child abuse (2)
upper decile	-7.484*	-8.961**
	(4.130)	(3.934)
below upper decile	2.496*	1.937
	(1.505)	(1.897)
Control variables	Yes	Yes

Notes: The table reports regression for two sub-samples of municipalities depending on the pre-DSL crime level. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. For the upper decile sub-sample it appears to be the case that municipalities are excluded in the regression because of a missing partner municipality with the same MDF (see empirical specification). In this case I search for all possible partner in the other sub-sample. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. Standard errors are heteroskedasticity robust and clustered at the municipality level. Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

to the initial crime level. DSL leads to a strong substitution effect in *high*-crime regions that is primarily driven by the substitution effect for sexual child abuse. The lower panel includes only municipalities with rather low crime levels in the pre-DSL period. Among them, I find that DSL significantly leads to higher overall sex crime. This heterogeneous effect with respect to the initial crime level explains most likely the sign flip in the baseline results table.

5.2 Spillover Effects

The situation in Germany with its technical peculiarities allows to estimate possible spillover effects on the regional level. This is motivated by the Crime Pattern Theory and the journey-to-crime literature. The Crime Pattern Theory states that offenders in general move within familiar nodes along consistent paths (Brantingham and Brantingham, 2008). There exists a rather large empirical literature focussing on the activity space of offenders. Based mainly on data from the US, Pyle et al. (1974) and Canter and Larkin



Notes: The figure plots graphically the relationship between DSL growth rate in municipality i and the change in crime rates of neighbourhood municipalities $j \neq i$ from the pre-DSL to the DSL period conditional on year and MDF fixed effects. The size of the circles depend on the number of municipalities within the respective DSL bins.

Figure 3: Growth in DSL and overall sex crime rates in neighbourhood municipalities

(1993) report a mean residence-to-crime distance of 1.34 to 1.53 miles. LeBeau (1987*a*) and LeBeau (1987*b*) find a mean distance of 3.5 miles. Using police data between 1998 and 2002 from Dallas (Texas, US) Ackerman and Rossmo (2015) find a mean residence-to-crime distance of 5.3 miles. Moreover, Rossmo (2005) shows that over time offenders travel on average farther in more recent years. The fact that the average size of the German municipalities corresponds to a radius of 1.9 miles (3.1 km) (Falck et al., 2014) suggest that spillover effect are likely to occur in neighbouring municipalities. Moreover, it is likely that committing a crime in a neighbourhood municipality deceases the probability of being known by other people and thus the likelihood of potential stigma effects to itself and one's relatives. From an economic point of view, moving away from the home municipality to commit a crime is associated with monetary costs and if exposure leads to (immediate) aggressive behaviour, the time interval to move corresponds to opportunity costs. This trade-off would suggest a response in criminal offences in neighbouring municipalities.

Before turning to the regression results, Figure 3 plots graphically the relation between the change in crime rates in neighbouring municipalities $j \neq i$ and and DSL share of municipality i conditional on year and MDF fixed effects. Although insignificant, the associated regression coefficient is 0.457 indicating a positive correlation between DSL in municipality i and crime in neighbourhood municipalities. Table 5 presents the results for all categories and controls for further observable variables. The first three regression results estimate the relation during the DSL period. The last specification (IV + FD) takes first differences in combination with IV. The OLS results show that there exist a

Table 5: Estimation results of internet availability on neighbourhood crime

	All sex crime		Sex child abuse	Rape	Crime against life
	(1)	(2)	(3)	(4)	(5)
OLS	1.005***	0.955***	-0.056	0.291***	0.032*
	(0.219)	(0.218)	(0.112)	(0.102)	(0.018)
OLS + crime level$_{2001}$	1.005***	0.954***	-0.052	0.292***	0.032*
	(0.219)	(0.218)	(0.112)	(0.103)	(0.018)
IV + crime level$_{2001}$	3.138***	3.011***	2.013***	1.756**	0.163***
	(0.562)	(0.574)	(1.953)	(0.860)	(0.051)
IV + FD	2.648*	2.558*	1.310*	1.547*	0.092
	(1.485)	(1.506)	(0.796)	(0.859)	(0.218)
F-test (first stage)	211.9	209.4	35.1	13.0	209.4
Observations	10,736	10,736	4,421	2,208	10,736
Number of MDFs	707	707	425	204	707
Municipalities	2,689	2,689	1,106	557	2,689
Control variables	No	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleinbergen-Paap F-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

positive association between broadband internet and overall sex crime, rape and crime against life. After accounting for time-varying unobserved effects, all point estimates increase in magnitude and are highly significant for all four categories. By applying the fixed-effects strategy, overall sex crime, sexual child abuse and rape remain significant, whereas crime against life remains positive, sizeable in magnitude but insignificant. In terms of magnitude, a 1% point increase in DSL in municipality i leads to an increase in all sex crimes by on average 2.5 cases per 10,000 inhabitants in neighbouring regions. This effect seems to be mostly driven by sexual child abuse and rape with 1.3 and 1.5 cases per 10,000 inhabitants, respectively. It suggest that on average, the negative effect reported in Table 2 is to some extent substituted and there exist a positive net effect for rape offences.[10]

Number of Neighbours. The spillover analysis so far calculates the average number of crime rates in all neighbouring municipalities. Figure C.1 shows that the average number of neighbours is 5.6 per municipality. There are about 165 municipalities with a rather low number of neighbours below 3. Calculating averages based on small numbers increases the probability that the regression coefficients could be affected by specific outcomes from one neighbouring municipality. In order to reassure that the results are not driven by municipalities with just one neighbouring municipality that might be an outlier, Table C.1 in the Appendix shows the results by conditioning on at least 3 neighbours using the last empirical specification by taking first differences and instrumenting DSL. The results are quantitatively similar and significantly not different. This provides evidence that the results are not driven by specific realizations of a single municipality.

5.3 Robustness Analysis

This section presents two basic robustness checks on the causal link between DSL and crime. First, instead of using the geographic centroid for estimating the distance to the next MDF, I use the population weighted center. A further concern relates to the fact that the basic specification takes multiple differences (including time-fixed effects). In

Table 6: IV + FD estimation results - robustness checks

	All sex crime (1)	Sex child abuse (2)	Rape (3)	Crime against life (4)
Panel A: within municipalities				
Population center	-1.443	-3.618*	0.362	0.527
	(2.347)	(2.134)	(2.594)	(0.650)
Average crime	-2.140	-3.687*	0.391	0.653
	(2.422)	(2.174)	(2.785)	(0.715)
Panel B: neighbourhood crime (spillover)				
Population center	1.480	1.354*	1.473*	-0.186
	(1.466)	(0.800)	(0.758)	(0.203)
Average crime	2.260	1.399*	1.556*	0.106
	(1.505)	(0.782)	(0.847)	(0.221)
Control variables	Yes	Yes	Yes	

Notes: The table reports regression results of robustness specifications for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Panel A shows specification within municipalities. Panel B shows specifications on neighbourhood crime. Standard errors are heteroskedasticity robust and clustered at the municipality level. Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

[10] I do not find any heterogeneous spillover effects with respect to the initial crime level. This is also mirrored by the fact that the coefficients do not change strongly across the different empirical specifications.

order to exclude the possibility that the results are driven by specific pre- and post-DSL crime outcomes, I collapse the observations to one observation per period by calculating the average crime rates.[11]

Table 6 reports the *IV + FD* estimation results of DSL expansion on crime distinguishing within municipalities (Panel A) and the effects on neighbourhood crime (Panel B). Panel A shows consistent results in terms of point estimates by taking the population weighted center to calculate the distance to the next MDF. The point estimates for overall sex crime, however, decrease slightly. By averaging over the defined periods provides evidence that the results are not driven by specific crime-year combinations. The effects on reported crime in neighbourhood municipalities shows again consistent results by using the population weighted center instead of the geographic centroid and averaging over the two periods. The standard errors for overall sex crime, however, increase slightly resulting in insignificant point estimates with a t-statistic of 1.50.

5.4 Placebo Test

An ideal placebo test in this empirical framework would be to compare outcomes in the pre-DSL period with outcomes in the late 1980s to test whether treated and non-treated municipalities perform different during these time periods. Due to data availability constraints for the outcome variables, this is not possible. Instead I test whether treated and non-treated municipalities show differences within the defined pre-DSL period. Thus, I run first difference specifications between the years 1999 and 1996 to test whether treated and non-treated municipalities have different growth rates. This specifications can be seen as reduced form regressions as I include the treatment dummy on the right-hand-side and test whether the treatment dummy has a significant effect. The tests generate reliability for a common pre-treatment trend.

Table 7 shows the results by testing the effect of the treatment dummy on crime within municipalities and for neighbourhood crime rates. The first difference between 1999 and 1996 in Panel A shows that treated and non-treated municipalities had similar

[11] I perform several additional robustness checks that are available upon request. First, I calculate crime rates per 10,000 inhabitants based on pre-DSL period population information. Even though the regressions control for net migration, this robustness analysis holds the denominator fix. A further robustness check is related to fact that the first year with crime information for municipalities in Rhineland-Palatinate is 2001. The concern relates to the fact that the IV + FD specification takes *9-years* differences which is not possible for municipalities in this Federal State. Thus, I only include one observation for municipalities in Rhineland-Palatinate namely the *7-year* difference between 2008 and 2001 and merge *7-years* differences for the remaining municipalities. These correspond to the pairs (1998-2005) and (1999-2006). The results are consistent with the findings above in terms of coefficient size. Due to the strong reduction in the sample size, the coefficients (being at the same level) for sexual child abuse have a t-value of 1.2 and 1.5 in Panel A and B. Although all specifications control for the net migration rate, a further concern might be selected migration based on DSL availability. By running an IV regression of DSL on net migration given further controls, I obtain a non-significant coefficient of -0.013 with a standard error of 0.008. Falck et al. (2014) further shows that only 3 out of 30 coefficients of the municipality characteristics are correlated with the instrument.

Table 7: Estimation results on growth rates between 1999 and 1996 - placebo test

	All sex crime (1)	Sex child abuse (2)	Rape (3)	Crime against life (4)
Panel A: within municipalities				
treatment dummy	0.282	0.184	0.493*	-0.135
	(0.499)	(0.203)	(0.293)	(0.086)
Panel B: neighbourhood crime (spillover)				
treatment dummy	-0.302	-0.146	-0.060	0.015
	(0.194)	(0.103)	(0.078)	(0.040)
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of placebo specifications for the sample of Bavaria, Baden-Wuerttemberg and Lower Saxony. The explanatory variable of interest in the regression is the treatment dummy indicating whether the distance to the next MDF is above 4,200 meters (=1) or below (=0). Due to data availability constrains, the regressions on the changes do not include municipalities from Rhineland-Palatinate. Crime rates are calculated per 10,000 inhabitants. Panel A shows specification within municipalities. Panel B shows specifications on neighbourhood crime. Standard errors are heteroskedasticity robust and clustered at the municipality level. Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

developments with respect to all four crime categories with a slightly diverging trend for rape cases. Although treated and non-treated municipalities start at different levels with respect to overall sex crime, the common trend assumption is justified for the within municipalities specifications. The pre-growth rates for neighbourhood crimes in Panel B do not show any significant effect. The results indicate that in most of the cases the developments in the pre-DSL period are not different for treated and non-treated municipalities.[12]

6 External Validity

One concern of the analysis might be the fact that the used municipalities in the IV-sample are too different and the results not transferable to more agglomerated municipalities. It is indeed the case that the selected municipalities are different in their regional composition. However, in order to generate intuition about the transferability of the results, Figure B.7 plots the development of the reported crime rates per 10,000 inhabitants among the four analysed categories for the two defined periods distinguishing between selected municipalities under the instrumental variable approach and all remaining municipalities. The left figures show the developments between 1996 to 1999 and the right figures the development between 2005 to 2008. What becomes immediately visible, although at lower actual

[12]See Figures B.6 and B.7 for a graphical visualization of pre-DSL crime developments distinguishing treated and non-treated municipalities as well as within municipality crime rates and crime rates for neighbourhood municipalities. By regressing the treatment dummy on actual crime rates between 1996 and 1999 shows that both groups do not differ in their level for child abuse and crime against life. Among overall sex crime treated municipalities, however, have a lower level of crime in the pre-DSL period.

levels for selected municipalities under the instrumental variable approach, is the strong co-movement of the crime rates. The graphical analysis shows rather similar patterns among German municipalities. Additionally, Table 8 shows the means and the standard

Table 8: Differences in outcomes among municipalities

	Selected Municipalities (1)	All other Municipalities (2)	Difference Test (p-value) (3)
Δ Sex crime	0.732	0.746	0.907
	(0.085)	(0.083)	
Δ Sex child abuse	0.072	0.110	0.516
	(0.041)	(0.040)	
Δ Rape	0.284	0.379	0.043
	(0.042)	(0.026)	
Δ Crime against life	-0.110	-0.026	0.000
	(0.016)	(0.013)	

Notes: The table reports the mean and the standard deviation of the dependent variables in the empirical approach for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Column (1) reports mean and standard deviation (in parenthesis) for the municipalities selected under the instrumental variable approach. Column (2) reports mean and standard deviation (in parentheses) of the remaining municipalities. Column (3) reports the p-values of a difference in means test.

deviations of the growth rates between the two periods. Column (1) shows the differences in crime rates between the DSL and the pre-DSL period for the selected municipalities that are used under the IV approach. Column (2) reports the differences in crime rates between the DSL and pre-DSL period for the remaining municipalities. Column (3) reports the p-value of a standard t-test. It appears to be the case that the selected municipalities do not experienced a significantly lower increase in all sex crimes and child abuse between the DSL and the pre-DSL period but a highly significant reduction among crime against life. Differences in rape growth rates are significant at the 5% level. Figure B.5 in the Appendix shows the distributions among selected municipalities. The density plot for all sex crimes show slightly fatter tails compared to the remaining municipalities indicating higher dynamics among the selected municipalities. The same is true for the remaining categories but to a lesser extent.

7 Mechanisms

In order to generate insights into the mechanism behind broadband internet and criminal activity, this section tries to differentiate the net effect into a direct effect and two indirect effects. The direct effect stems from higher exposure to extreme media input that affects aggressive behavior and becomes visible through reported crime rates. However, the net effect might be driven by two indirect effects (Bhuller et al., 2013). The first indirect effect corresponds to a reporting effect, whereas the second effect relates to a matching effect.

Reporting Effect. Regarding the reporting effect, it is well known that especially sex crime is prone to an underreporting. It is possible that the internet leads to e.g. higher reported sex crime without increasing the number of sex crimes. Following Bhuller et al. (2013) this might be the case given the fact that the costs of reporting a crime decrease in the internet period. One way might be through facilitating contact with support groups. For some

Table 9: IV + FD estimation results excluding Baden-Wurttemberg and Lower-Saxony

	All sex crime (1)	Sex child abuse (2)	Crime against life (3)
Panel A: within municipalities			
Δ DSL	-2.087	-8.456**	0.699
	(2.488)	(3.837)	(0.749)
Panel B: neighbourhood crime (spillover)			
Δ DSL	2.677*	2.484**	0.122
	(1.602)	(1.156)	(0.228)
F-test (first stage)	199.8	22.6	199.8
Observations	8,508	2,193	8,508
Number of MDFs	502	220	502
Municipalities	2,132	549	2,132
Control variables	Yes	Yes	Yes

Notes: The table reports regression results for the sample of Bavaria and Rhineland-Palatinate. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The *F*-test of excluded instruments refers to the Kleinbergen-Paap *F*-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

German Federal States it is possible to report an offence online. In 2003, Brandenburg started to implement so called "online guards" followed by Mecklenburg-Vorpommern, Hesse and Berlin in 2005. Lower Saxony and Rhinland-Westphalia adopted the "online guards" in 2007. Although most States have online opportunities for self-information and first contact possibilities, it is not always possible to report an offences online. Still today it is not possible to report offences online in Bavaria, Rhinland Palatinate, Turingia, Saarland and Bremen.[13] In order to investigate whether reported crime rates are influenced by lower reporting cost, I exclude the States where online reporting is possible (these are Lower-Saxony and Baden-Wuttemberg).[14] For representative purpose, Table 9 presents *IV + FD* estimation results based on Table 2 and Table 5. Due to data availability constrains, rape cannot be analyzed. The table shows that the direction of the coefficients

[13]The online page http://www.online-strafanzeige.de/ shows for every Federal State the possibility for reporting offences online with a link to the specific police departments.

[14]After consulting the Federal Police of Baden-Wurttemberg they made clear that although there is the possibility of reporting and first contacting, the online possibility does not substitute the traditional way of reporting crime offences.

do not differ by excluding the two States where lower reporting cost are most likely to be present. The results suggest that this way of faster and easier reporting through police opportunities do not drive the findings primarily. Interestingly, the point estimate for sexual child abuse increases strongly and becomes significance at the 5% level. The strong increase in absolute terms might in fact suggest that a reporting effect is present and the true effect shown in Table 2 presents an upper bound.

A further way of investigating the reporting effect is by analysing detection rates. This follows the assumption that lower cost of reporting by e.g. meeting with other victims and/or gathering information online that lead to an increase in reporting criminal offences such as sex crimes, results in weaker cases on average. If that is true the result would be that detection rates increase as weaker cases have a higher probability of being detected. The results are shown in Table 10. The table shows that neither within the municipality

Table 10: IV + FD estimation results analyzing detection rates

	All sex crime (1)	Sex child abuse (2)	Rape (3)	Crime against life (4)
Panel A: within municipalities				
Δ DSL	0.023	-0.055	-0.026	-0.006
	(0.034)	(0.060)	(0.054)	(0.005)
F-test (first stage)	203.5	25.6	11.5	206.1
Observations	6,240	2,720	1,694	9,838
Number of MDFs	618	365	189	682
Municipalities	2,502	984	526	2,639
Panel B: neighbourhood crime (spillover)				
Δ DSL	0.001	0.061	0.017	0.005
	(0.031)	(0.038)	0.018	(0.005)
F-test (first stage)	201.1	29.4	12.6	209.9
Observations	6,635	3,241	1,839	10,164
Number of MDFs	653	403	203	701
Municipalities	2,575	1,062	555	2,677
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for detection rates for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Detection rates are calculated in percent. In the case of zero criminal activity I assume a zero change between the two periods. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleinbergen-Paap F-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

specifications (Panel A) nor in the specification on neighbourhood crime (Panel B), DSL does not lead to higher detection rates. Following the stated assumptions, it indicates that a potential reporting effect that might drive higher reported crime rates is not the mechanism behind the net effect.

Matching Effect. Following Bhuller et al. (2013) the internet makes the search process more efficient and reduces uncertainty and information constraints. This mechanism can increase the number of matches ("meetings") between offenders and victims. Moreover, the internet may expand an individuals' network which might increase the probability of a match. On the other hand, spending time online decreases the probability of meeting other individuals and committing a crime. While the net effect is not clear, I investigate the matching effect by analysing total crime rates other than sex crime and crime against life. If individuals spend more time at home, than this should be visible by observing a reduction for all crime rates. Table 11 presents IV estimates for the baseline specification within municipalities and among neighbourhood regions. Irrespectively of the specified

Table 11: IV + FD estimation results analyzing all crime rates

	within municipalities (1)	neighbourhood crime (spillover) (2)
Δ DSL	50.04	22.49
	(122.9)	(45.62)
F-test (first stage)	209.4	209.4
Observations	10,736	10,736
Number of MDFs	707	707
Municipalities	2,689	2,689
Control variables	Yes	Yes

Notes: The table reports regression results for all crime rate excluding sex crime and crime against life for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleinbergen-Paap F-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

model, higher broadband internet does not affect the total number of reported crimes. This is indirect evidence that time spend at home does not drive the results. One should note that this result does not intent that time spend at home did not change at all over the pre-DSL and DSL period. It only indicates that treated and non-treated municipalities do not act differently. The "time spend at home" argument is also supported by findings provided by Bauernschuster et al. (2014). The authors show that home internet access within the DSL period does not affect the way and intensity people meet with friends and visit for example cinemas and restaurants or bars. Given that the behavior of going out and meeting with friends is unaffected, the probability of a match between an offender and a victim might not change due to broadband internet. The authors further show that high-speed internet has a positive effect on the number of out-of-school activities for children aged between 7-16. This might even increase the number of matches indicating again the reported effect being an upper bound. Although the investigation of the full matching effect seems not to drive extensively the empirical results there remains some uncertainty left. This uncertainty is present given the unexplained channel that offenders might search more efficiently online for a potential victim that results in "*better*" matches. Hanson and Morton-Bourgon (2005) provide evidence that the internet is used among adult offenders to meet primarily 13- through 15-years-old teenager. This part of the matching effect cannot be addressed in this paper.

Direct Effect through Illegal Pornographic Material. For a subset of municipalities, the data provide information of the distribution and possession of illegal pornographic material. Detailed information from Lower Saxony shows that in over 90% of the cases, illegal pornographic material is related to children and has a clear child-content. A potential rise in illegal pornography might explain the strong substitution effect for sexual child abuse. The German State Criminal Offices of Baden-Wuettemberg and Lower Saxony provide information on illegal pornographic material in general. Panel A of the table shows that

Table 12: Estimation results analyzing illegal pornographic material

	OLS (1)	OLS + $crime_{2001}$ (2)	IV + $crime_{2001}$ (3)	IV + FD (4)
Panel A: illegal pornographic material				
within municipalities	0.626*	0.614*	2.702	3.730
	(0.362)	(0.364)	(2.532)	(2.510)
spillover	0.094	0.078	0.803	0.780
	(0.160)	(0.157)	(0.895)	(0.833)
Panel B: interaction with child abuse				
within municipalities	0.012	0.024	2.012	-0.800
	(0.224)	(0.225)	(2.564)	(3.045)
F-test (first stage)	-	-	14.4	13.0
Observations	2,208	2,208	2,208	2,208
Number of MDFs	204	204	204	204
Municipalities	557	557	557	557
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of DSL on illegal pornographic material for the sample of Baden-Wuerttemberg and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleinbergen-Paap F-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

DSL is related to pornography within the municipalities. Even in the preferred empirical specification with fixed-effects, the results point to a causal relation (t-value: 1.52). The table further documents rather low and not significant (at any level) spillover effects. Although at weak significance levels, the results provide evidence for a potential mechanism that explains the substitution effect for sexual child abuse. This is the case given the high share of child-related offences within this sex crime category.

Although the analysis of illegal pornographic material provides a potential explanation of the substitution effect among sexual child abuse, the internet might also induce an indirect effect at the side of offenders. The internet does not only provide a way for victims to get into contact with e.g. other victims and support groups. It is also possible for potential perpetrators to contact anonymously support groups or other individuals with similar "tastes". This indirect effect might lead to a reduction in crime cases. To my knowledge, there is no data set that allows to get closer to this potential explanation. In fact, it could be the case that the coefficients are driven by different municipalities. If the "contact" argument is correct, then we should not observe e.g. an increase in illegal pornographic material and simultaneously a decrease in child abuse. However, a first simple correlation analysis shows a negative association between illegal pornographic material and sexual child abuse. A further way to investigate this is to change the outcome variable by the interaction between sexual child abuse and illegal pornographic material. If the rise in pornography drives the substitution effect, than we would expect a close to zero coefficient of DSL on the interaction effect as both effect substitute each other. Panel B of Table 12 reports the effect of DSL on the interaction between illegal pornographic material and sexual child abuse. In all model specifications, the effects are lower and not significant at any level. In the preferred specification including municipality fixed-effects (column (4)) the coefficient is negative and rather close to zero compared to the point estimates of -3.676 (Table 2) and 3.730 in Panel A of Table 12. This result supports the hypothesis that pornography drives the substitution effect.

8 Discussion and Conclusions

Does high-speed internet lead to higher criminal activity? Using unique German data on the regional level, this paper documents heterogeneous effects between broadband internet and (sex) crime within and between (spillover effects) municipalities. Overall sex crime and sexual child abuse point to a substitution effect, whereas rape and murder does not significantly respond to higher broadband internet. Despite the substitution effect, the results show a positive effect of DSL availability on crime offences in neighbouring municipalities that comes primarily from child abuse and rape. By assuming that there are no higher order spillover effects, Table 13 reports the estimated coefficients of DSL on crime including illegal pornography offences. The table indicates that the net effect of DSL on overall sex crime is positive. The rather high coefficient of pornography might explain the overall substitution effect for sexual abuse against children. This is confirmed by the available observations for Lower Saxony where over 90% of pornography offences have a child content and among them about 50% belong to possessing child pornography.

Table 13: Summary of DSL effects

	All sex crime (1)	Sex child abuse (2)	Rape (3)	Pornography (4)
within municipality	-2.120	-3.676	0.256	3.730
spillover	2.558	1.310	1.547	0.780
Sum (net effect)	0.438	-2.366	1.803	4.510

Notes: The table summarizes regression results for various sex crime offences for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants.

Adding up the coefficients results in a remaining part of -4.3. This suggest that general sexual abuse not related to children, which is the last sizable category where no data are available, also decreases. By assuming that the results can be generalized to all less agglomerated municipalities, one can evaluate the contribution of the introduction of the new technology on sex crime. During the two time periods, sex crime rates in the (IV-)sample increased by 0.732 cases per 10,000 inhabitants and DSL availability increased by about 80 percentage points. Multiplying the net DSL effect with the average increase in DSL availability implies that the DSL expansion explains almost 50% of the *increase* in overall sex crime. This indicates that overall sex crime would have been about 9.2% (the actual level in the DSL period for the IV-sample is 3.814 overall sex crime cases per 10,000 inhabitants) lower compared to the counterfactual situation without DSL.

Identifying the effects of internet availability on criminal offences suffers from selection bias. Regions with high-speed internet access are on average different in many aspects. These regions typically are higher agglomerated, have a higher share of skilled individuals and higher per capita income indicating that the composition of the areas is different. These characteristics are correlated with the willingness to pay for high-speed internet which is also plausible to have an effect on crime. To overcome the omitted-variable bias, I follow Falck et al. (2014) and exploit regional peculiarities of the traditional public switched telephone network (PSTN), which affects the possibility to provide DSL in certain municipalities. The implementation of the new technology was done through the regional PSTN. The structure of the PSTN was determined in the 1960s when the goal was to provide telephone service in West Germany. These technical peculiarities provide a quasi-experimental situation for less agglomerated municipalities without an own distribution frame where the distance from the regional center of each municipality to the distribution frame can be used as an instrument for DSL availability. Thus, I identify the effect of the introduction of a new mass medium on crime rates.

The estimated net effect might be driven by different mechanisms. Beside a direct effect coming from increased consumption of extreme and violent media such as pornography, the internet provides the opportunity to communicate and contact other people more efficiently which decreases the cost of reporting a crime. This reporting effect might lead to an increase in reported (sex) crimes without increasing the actual number of crimes. Moreover, the internet makes the search process more efficient and reduces uncertainty and information constraints. This mechanism can increase the number of matches between offenders and victims. In addition, the internet may expand an individuals' network which might increase the probability of a match. While spending time online decreases the probability of meeting other individuals and committing a crime, a direct online search might increase the probability of a match. After investigating the potential mechanisms, I find that the estimated net effect most likely represents a direct effect of increased extreme media consumption. This is further supported by the observation that illegal pornographic material response strongly to broadband internet. The child-related content of this category provides a plausible explanation of the documented substitution effect. However, the net effect of rape contributes positively to the overall effect indicating that broadband internet has adverse side effects.

The paper contributes to the discussion of adverse side effects of broadband internet. It suggests that extreme media consumption should be more effectively monitored and controlled by the government in order to prevent body-related crime offences, especially rape. Although there is evidence of a substitution effect for sexual child abuse, increased child-related pornographic material is per se an adverse side effect to the society overall. However, there is some uncertainty left with respect to the importance of the matching effect. Although I do not find support for different time allocation that could influence criminal activity, further research needs to be done to shed light on more efficient online search for potential victims. If the internet increases the "*quality*" of a match, controlling and monitoring extreme media consumption might not be the right policy.

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Appendix

Appendix A: Detailed Data Description

Table A.1: Variables definition

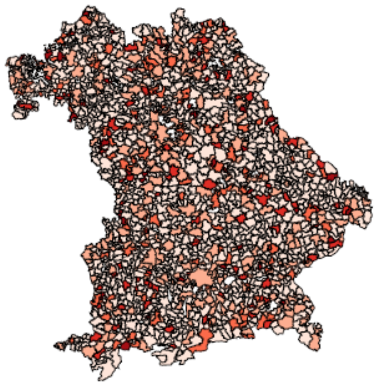
Crime Variables	Description
All sex crime	Number of reported sexual offences defined as in the German Criminal Code §174 StGB to §184 StGB, including rape, sexual abuse, sexual abuse against children and the distribution of pornographic products committed in year t in municipality i. The number is divided by the population size and multiplied by 10,000. Source: Federal Criminal Crime Offices (Landeskriminalamt) Availability: Bavaria, Rhineland-Palatinate, Lower Saxony, Baden-Wuettemberg
Child abuse	Number of reported sexual offences defined as in the German Criminal Code §176 StGB, §176a StGB to §176b StGB committed in year t in municipality i. The number is divided by the population size and multiplied by 10,000. Source: Federal Criminal Crime Offices (Landeskriminalamt) Availability: Bavaria, Lower Saxony, Baden-Wuettemberg
Rape	Number of reported sexual offences defined as in the German Criminal Code §177 StGB (Abs. 2, 3, 4), and §178 StGB committed in year t in municipality i. The number is divided by the population size and multiplied by 10,000. Source: Federal Criminal Crime Offices (Landeskriminalamt) Availability: Lower Saxony, Baden-Wuettemberg
Pornographic material	Number of reported sexual offences defined as in the German Criminal Code §184 StGB a-d committed in year t in municipality i. The number is divided by the population size and multiplied by 10,000. Source: Federal Criminal Crime Offices (Landeskriminalamt) Availability: Lower Saxony, Baden-Wuettemberg
Crime against life	Number of reported sexual offences defined as in the German Criminal Code §211 StGB, and §218 StGB to §219 StGB committed in year t in municipality i. The number is divided by the population size and multiplied by 10,000. Source: Federal Criminal Crime Offices (Landeskriminalamt) Availability: Bavaria, Rhineland-Palatinate, Lower Saxony, Baden-Wuettemberg
Internet Variables	
Broadband internet	Fraction of households in municipality i at time t with a subscription to DSL defined by an access speed of 384 kb/s or above. Documented numbers start in 2005. Source: Breitbandatlas Deutschland Availability: all German municipalities
Treatment	Equals 1 for municipalities with a distance of more than 4,200 meters to the next main distribution frame (MDF). The distance is calculated using the geographic centroid and the population weighted center. Source: Falck et al. (2014) Availability: all German municipalities
Control Variables	
Female population share	Fraction of females in municipality i belonging to the age groups 20-29, 30-39, 40-49, and 50 or above. The pre-DSL fractions are calculated for the years 1996 and 1999 based on administrative data provided by the Federal Employment Agency. Source: Falck et al. (2014) Availability: all German municipalities

Table A.1 continued: Variables definition

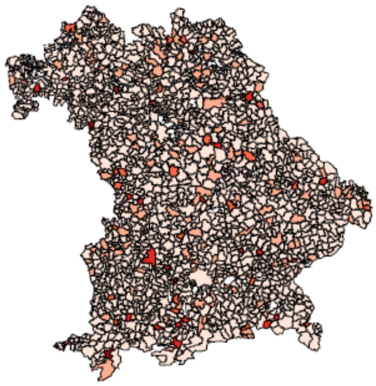
Control Variables	Description
Population aged 18-65	Fraction of the population aged between 18 and 65 years in municipality i for the pre-DSL and the DSL period. The pre-DSL fraction refers to the year 2001. Source: Falck et al. (2014) Availability: all German municipalities
Population aged > 65	Fraction of the population aged above 65 years in municipality i for the pre-DSL and the DSL period. The pre-DSL fraction refers to the year 2001. Source: Falck et al. (2014) Availability: all German municipalities
Net migration	Net migration rate in municipality i for the pre-DSL and the DSL period. The pre-DSL fraction refers to the year 2001. Source: Falck et al. (2014) Availability: all German municipalities
Unemployment rate	Unemployment rate in municipality i for the pre-DSL and the DSL period. The pre-DSL fraction refers to the year 2001. Source: Falck et al. (2014) Availability: all German municipalities
Police density	Number of police officers in county i for the pre-DSL and the DSL period divided by the population in municipality i. The pre-DSL fraction refers to the year 1999. Source: Federal Statistical Offices Availability: Bavaria, Rhineland-Palatinate, Lower Saxony, Baden-Wuettemberg
Firm density	Number of establishments in municipality i for the pre-DSL and the DSL period divided by the population. The pre-DSL fraction refers to the year 1999. Source: Federal Employment Agency Availability: all German municipalities
Wage	Average real daily wage in municipality i calculated among full-time employees. The pre-DSL fractions are calculated for the years 1996 and 1999 based on administrative data provided by the Federal Employment Agency. Source: Federal Employment Agency Availability: all German municipalities
Occupation	Occupational shares in municipality i calculated for the categories production, salary, sale, clerical and service (ref. agrar sector) The pre-DSL fraction refers to the year 1999. Source: Federal Employment Agency Availability: all German municipalities
Foreigners	Fraction of foreigners in municipality i belonging to the age groups 20-29, 30-39, 40-49, and 50 or above. The pre-DSL fraction refers to the year 1999 and is calculated based on administrative data provided by the Federal Employment Agency. Source: Federal Employment Agency Availability: all German municipalities
Program participation	Fraction of individual in municipality i being in a public sponsored labor market program. The pre-DSL fractions are calculated for the years 1996 and 1999 based on administrative data provided by the Federal Employment Agency. Source: Federal Employment Agency Availability: all German municipalities

Appendix B: Additional Descriptive Results

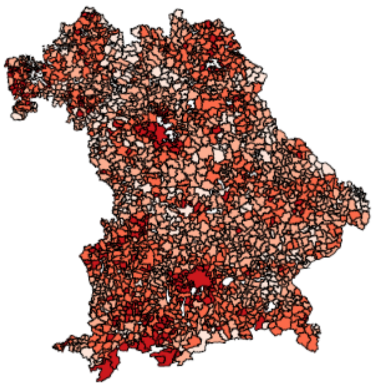
(A) All sex crime



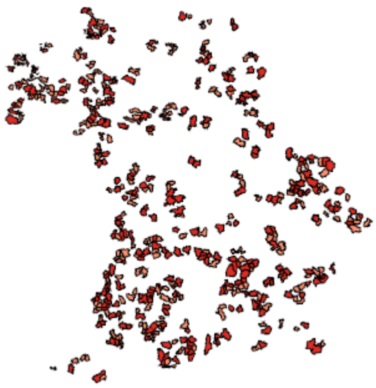
(B) Sex abuse child



(C) Crime against life



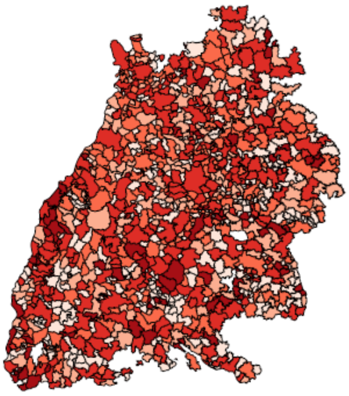
(D) DSL



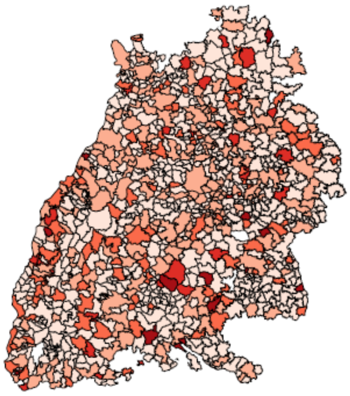
(E) Treated and non-treated municipalities

Notes: The sub-figures A, B, C plot the geographical distribution of the dependent and available crime variables for Bavaria measured for simplicity in 2005. The darker the calor, the more cases per 10,000 inhabitants. The categories are 0 to 1, 1 to 3, 3 to 5, 5 to 10 and more than 10 cases per 10,000 inhabitants. Sub-figure (D) plots the share of households with broadband internet (DSL) connection. The categories are 0-60% (light), 61-80%, 81-90% and 91-100% (dark). Sub-figure (E) shows treated (dark) and non-treated (light) municipalities used in the empirical section. White areas indicate missing values.

Figure B.1: Geographical distribution of crime and DSL growth rates and treated/non-treated municipalities for the Federal State Bavaria

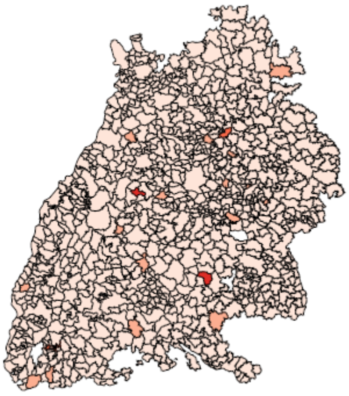


(A) All sex crime

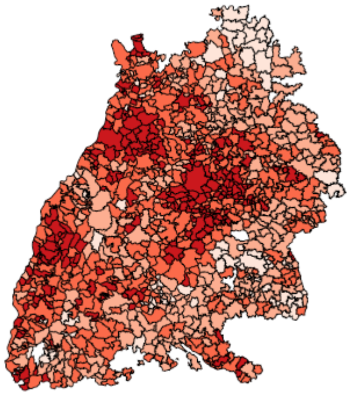


(B) Sex abuse child

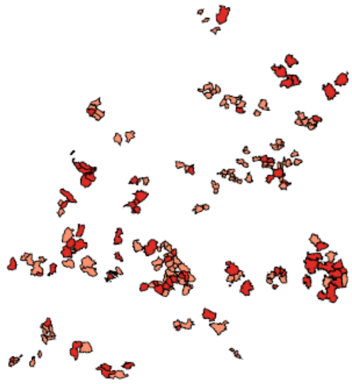
(C) Rape



(D) Crime against life



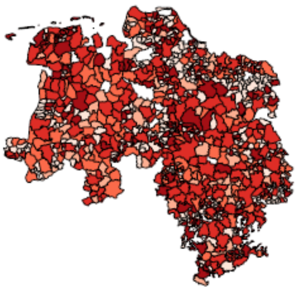
(E) DSL



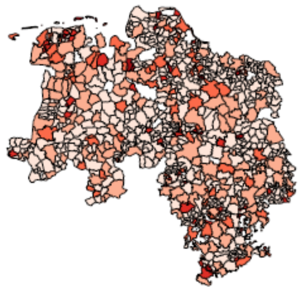
(F) Treated and non-treated municipalities

Notes: The sub-figures A, B, C, D plot the geographical distribution of the dependent and available crime variables for Baden-Wuttemberg measured for simplicity in 2005. The darker the calor, the more cases per 10,000 inhabitants. The categories are 0 to 1, 1 to 3, 3 to 5, 5 to 10 and more than 10 cases per 10,000 inhabitants. Sub-figure (E) plots the share of households with broadband internet (DSL) connection. The categories are 0-60% (light), 61-80%, 81-90% and 91-100% (dark). Sub-figure (F) shows treated (dark) and non-treated (light) municipalities used in the empirical section. White areas indicate missing values.

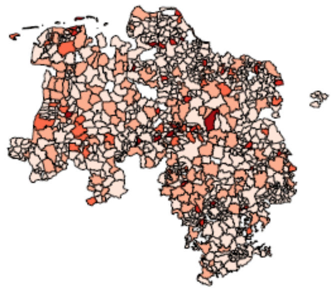
Figure B.2: Geographical distribution of crime and DSL growth rates and treated/non-treated municipalities for the Federal State Baden-Wuttemberg



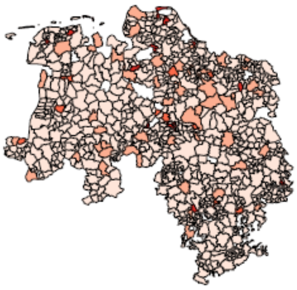
(A) All sex crime



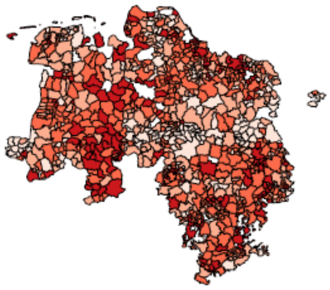
(B) Sex abuse child



(C) Rape



(D) Crime against life

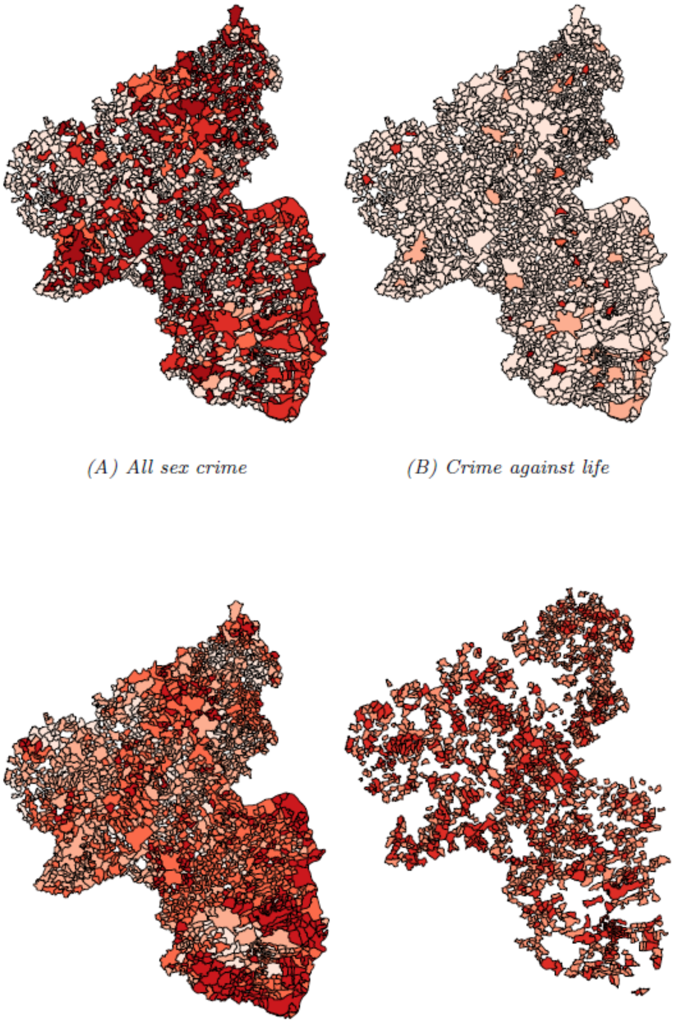


(E) DSL

(F) Treated and non-treated municipalities

Notes: The sub-figures A, B, C, D plot the geographical distribution of the dependent and available crime variables for Lower Saxony measured for simplicity in 2005. The darker the calor, the more cases per 10,000 inhabitants. The categories are 0 to 1, 1 to 3, 3 to 5, 5 to 10 and more than 10 cases per 10,000 inhabitants. Sub-figure (E) plots the share of households with broadband internet (DSL) connection. The categories are 0-60% (light), 61-80%, 81-90% and 91-100% (dark). Sub-figure (F) shows treated (dark) and non-treated (light) municipalities used in the empirical section. White areas indicate missing values.

Figure B.3: Geographical distribution of crime and DSL growth rates and treated/non-treated municipalities for the Federal State Lower Saxony

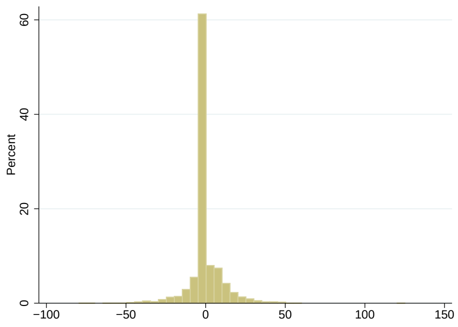


(A) All sex crime *(B) Crime against life*

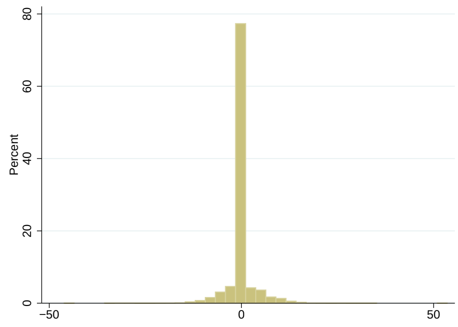
(C) DSL *(D) Treated and non-treated municipalities*

Notes: The sub-figures A, B plot the geographical distribution of the dependent and available crime variables for Rhineland-Palatinate measured for simplicity in 2005. The darker the calor, the more cases per 10,000 inhabitants. The categories are 0 to 1, 1 to 3, 3 to 5, 5 to 10 and more than 10 cases per 10,000 inhabitants. Sub-figure (C) plots the share of households with broadband internet (DSL) connection. The categories are 0-60% (light), 61-80%, 81-90% and 91-100% (dark). Sub-figure (D) shows treated (dark) and non-treated (light) municipalities used in the empirical section. White areas indicate missing values.

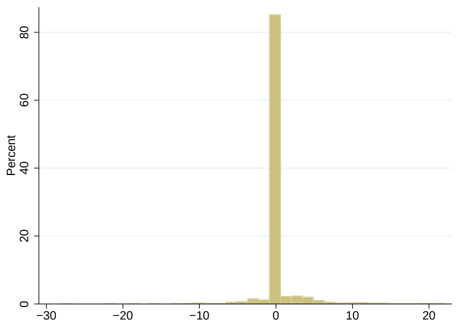
Figure B.4: Geographical distribution of crime and DSL growth rates and treated/non-treated municipalities for the Federal State Rhineland-Palatinate



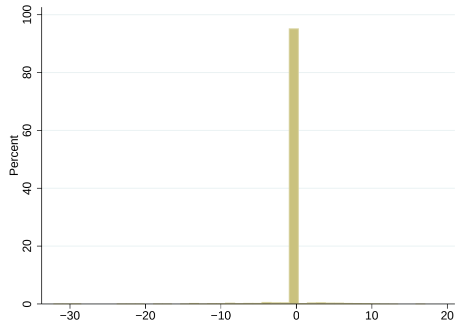
(1) All sex crime



(2) Sex abuse child



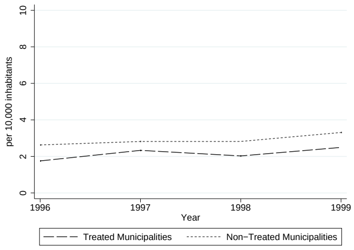
(3) Rape



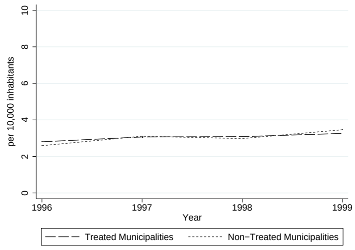
(4) Crime against life

Notes: The figure shows the distribution for the change in crime rates from the pre-DSL to the DSL period. The DSL period corresponds to the years 2005 to 2008 whereas the reference year for the pre-DSL period covers the years between 1996 to 1999. Selected municipalities correspond to municipalities used in the empirical section without an own MDF.

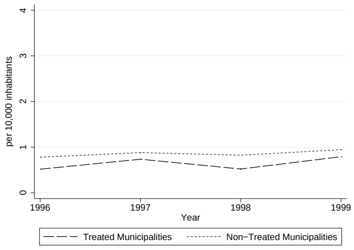
Figure B.5: Density plots among crime categories for selected municipalities in the empirical analysis



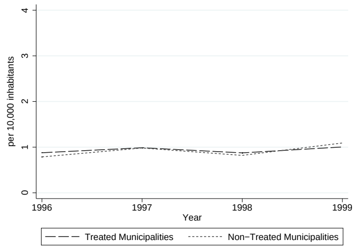
(1-A) All sex crime - within municipality



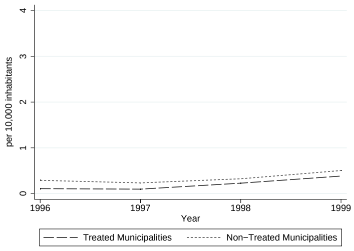
(1-B) All sex crime - spillover



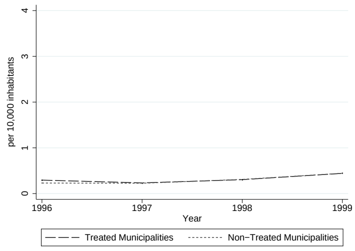
(2-A) Sexual child abuse - within municipality



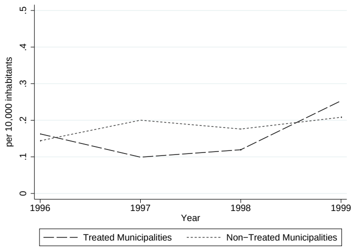
(2-B) Sexual child abuse - spillover



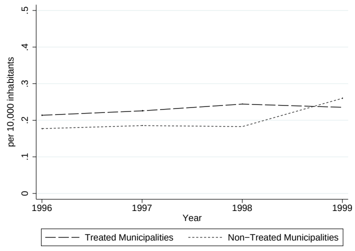
(3-A) Rape - within municipality



(3-B) Rape - spillover



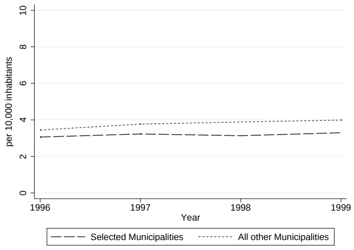
(4-A) Crime against life - within municipality



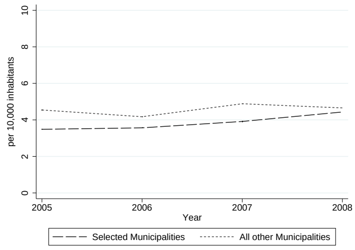
(4-B) Crime against life - spillover

Notes: The figure shows the development of different crime rates per 10,000 inhabitants for the pre-DSL (1996-1999) distinguishing between treated and none-treated municipalities as well as within municipalities crime rates and crime rates in the neighbourhood regions.

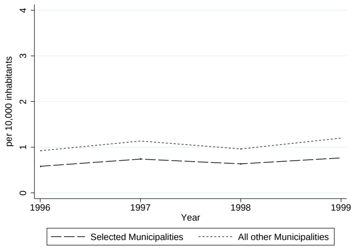
Figure B.6: Pre-DSL crime level development for treated and non-treated municipalities in the IV-sample



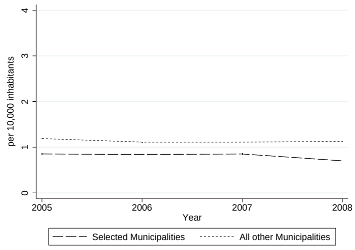
(1-A) All sex crime 1996-1999



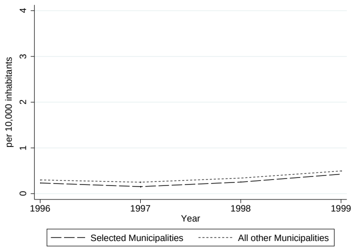
(1-B) All sex crime 2005-2008



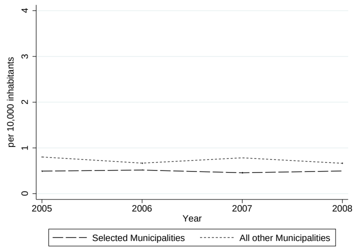
(2-A) Sexual child abuse 1996-1999



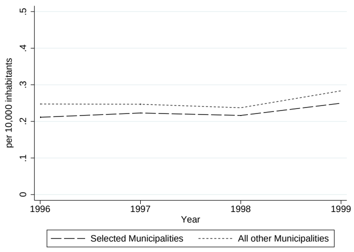
(2-B) Sexual child abuse 2005-2008



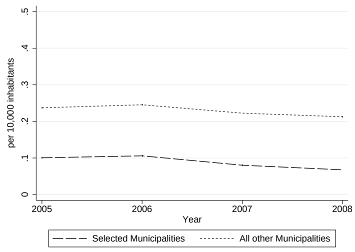
(3-A) Rape 1996-1999



(3-B) Rape 2005-2008



(4-A) Crime against life 1996-1999

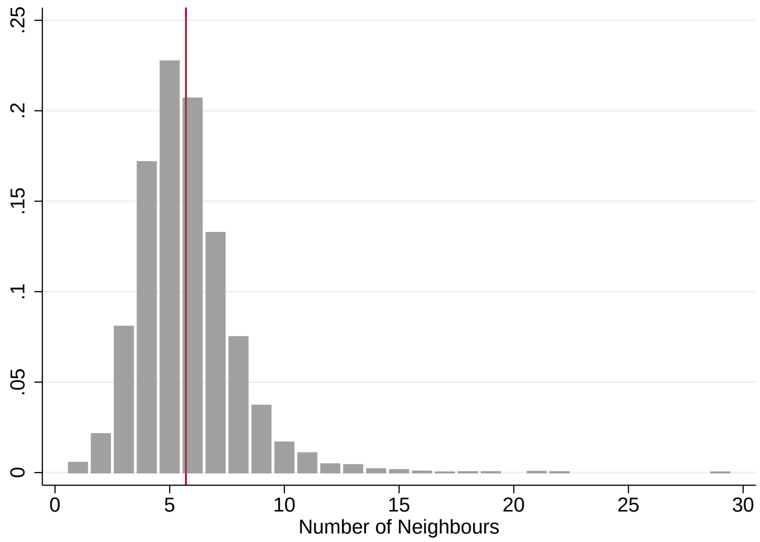


(4-B) Crime against life 2005-2008

Notes: The figure shows the development of different crime rates per 10,000 inhabitants for the pre-DSL (1996-1999) and the DSL (2005-2008) period. Selected municipalities correspond to municipalities used under the IV-approach, whereas all other municipalities correspond to the remaining municipalities.

Figure B.7: Crime level development for selected (IV-sample) and remaining municipalities

Appendix C: Neighbourhood Information



Notes: The figure plots the distribution of the number of neighbours used in the analysis on spillover effects to calculate for every municipality i crime rates in neighbouring municipalities $j \neq i$. The red line shows the mean of 5.6 neighbours per municipality. In total, there are 19 islands with no neighbouring municipality.

Figure C.1: Distribution of neighbourhood links

Table C.1: Estimation results of internet availability on neighbourhood crime with 3 neighbours and more

	All sex crime		Sex child abuse	Rape	Crime against life
	(1)	(2)	(3)	(4)	(5)
IV + FD	2.624*	2.460	1.153	1.442*	0.112
	(1.533)	(1.545)	(0.828)	(0.868)	(0.223)
F-test (first stage)	204.0	203.3	35.7	12.3	203.3
Observations	10,361	10,361	4,213	2,132	10,361
Number of MDFs	682	682	406	198	682
Municipalities	2,595	2,595	1,054	538	2,595
Control variables	No	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restriction, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2001. The instrument refers to a threshold dummy indicating whether a municipalities' distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleinbergen-Paap F-statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. As a robustness check, I calculate standard errors at the MDF level (available upon request). Additional control variables not shown in Table 1 are: skill level, shares of females and foreigners in 4 age-groups, real daily wage level, firm density, occupational structure and public program participation rates.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.