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The Internet Effects on Sex Crime and Murder – Evidence from the Broadband Internet Expansion in Germany

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Abstract

This paper studies the effects of the introduction of a new mass medium on criminal activity in Germany. The paper asks the question of 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 at 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 at 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 a substitution effect of internet and child sex abuse and no effect on rape incidences. The effects on murder increase under the instrumental variable approach however remain insignificant. Overall, the estimated net effects might stem from indirect effects related to differences in reporting crime, a matching effect, and a direct effect of higher and more intensive exposure to extreme and violent media consumption. After investigating the potential channel, I do find some evidence in favor of a reporting effect suggesting that the direct consumption effect is even stronger. Further investigation of the development of illegal pornographic material suggests that the direct consumption channel does play a significant role in explaining the substitution effect.

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

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[*]Full address of correspondence: André Nolte, Centre for European Economic Research, Department of Labour Markets, Human Resources and Social Policy, L 7, 1, 68161 Mannheim, Germany, E-Mail: nolte@zew.de. I would like to thank the Federal Criminal Offices and Statistical Offices of Bavaria, Baden-Wuerttemberg, Lower Saxony and Rhineland-Palatinate for providing criminal and police data. I would also like to thank Andrea Weber, Andreas Peichl, Antonio Ciccone, Francois Laisney, Sebastian Siegloch, Oliver Falck, Johannes Voget, Laura Pohlan, Arne J. Warnke and conference participants at the ASSA meetings in Chicago, the Spring Meeting for Young Economists in Halle, the Labour Market Seminar at ZEW in Mannheim and the Ph.D. Workshop on Digitization at CESifo for helpful comments and suggestions.

1 Introduction

Over the last 10 to 15 years, access to the internet has significantly reduced a variety of market frictions. The internet makes the transmission of information cheaper and more easily accessible. This higher transmission of information has had a profound impact on society and social interaction through social networking, forums and messaging.[1] However, there are only a few studies which investigate the effects of (mass) media technologies on adverse side effects. Mastrorocco and Minale (2016) show that, in the case of Italy, exposure to crime through television shapes individual perceptions and concerns about crime. Card and Dahl (2011) provide evidence of the effect of professional football games on individual behavior. They show that emotional cues provided by local NFL football games lead to higher family violence. More recently, Lindo et al. (forthcoming) show that football-game-induced emotions increase rape victimization especially among 17-24 year old women. As shown in psychological laboratory experiments, the internet reduces the 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 and, in some cases, even a reduction in sexual aggression after exposure to pornography. While laboratory experiments provide interesting insights into exposure and commitment, the effect of extreme media consumption on aggression in controlled experiments might be different compared to private settings (Levitt and List, 2007, Dahl and DellaVigna, 2009). In its use of field data on the county and municipality level, this study is most closely related to Kendall (2007) and Bhuller et al. (2013). After controlling for area-fixed effects and explanatory variables at the state level using US data, Kendall (2007) finds a negative effect of internet availability 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 that internet usage has a positive and substantial effect on sexual crime which primarily consists of rape. Child abuse does not react to broadband internet. In the empirical

[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 have begun to focus on how human behavior is affected by higher internet exposure. Kolko (2010) investigates the effect of broadband adoption on online and offline activities. Broadband adoption leads to less time spent on playing video games but not on activities like reading magazines or watching TV. Using data on the German municipality level, Falck et al. (2014) 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.

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 link between internet usage and criminal behavior is that the internet provides greater and easier exposure to violent and extreme media input such as violent pornography. This greater exposure might affect individuals' behavior which becomes visible on the regional level through reported crime rates. Most of the previous studies focus on sex crime. This paper extents the literature by also analyzing the effects on crime against life such as murder. The basic idea behind internet and homicide is rather similar to sex crimes. The consumption of violent media might increase or decrease aggressive behavior resulting in a different level of offences. Therefore, the analysis to some extent adds to an ongoing discussion as to whether e.g. shooter games might have adverse side effects. Although shooter games can be played offline, the internet allows interactive communication while playing.[2]

The empirical analysis starts by estimating the net effects of internet availability on sex crime including all sex crime, child sex abuse, rape and homicide on the municipality level. Endogenous selection might play a crucial role in the context of the use of the internet and crime incidences. Beside individuals potentially exhibiting different behavior 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 still be time-varying unobserved factors that jointly affect the crime rate and internet usage. 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 internet availability. The roll-out of broadband internet in Germany in 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) had no impact on

[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 behavior 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.

the the quality of telephony at the household level, the location for the DSL roll-out is significant for the availability of broadband. Households in municipalities located at a distance of more than 4,200 meters from the next distribution frame cannot access DSL. Connecting these households requires costly infrastructure projects. This situation defines a quasi-natural experiment in which I identify the effects of the introduction of a new mass medium on criminal activity.

After controlling for municipality-fixed effects, MDF-by-year fixed effects, observable municipality characteristics and instrumenting the internet variable, I find that a 1% point increase in the internet availability rate leads to a *decrease* in overall sex crime by on average -0.037 crime cases per 10,000 inhabitants. This overall substitution effect is driven by child sex abuse (-0.059). High-speed internet does not show any effect on rape and homicide. Varying the empirical specification with respect to the defined instrument and by sub-samples provides significant coefficients of DSL on child sex abuse ranging between -0.023 and -0.10. Further robustness and placebo tests suggest a causal interpretation.

The estimated net effects may stem from three possible mechanisms that might be in place, especially in the case of body-related offences such as sex crime and homicide. Besides the direct effect of high-speed internet that comes from higher exposure or better opportunities to consume violent media content, 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 homicide. If individuals spend more time at home, then this should be observable in an overall reduction in the crime rates. The results do not show any effect on overall crime. Moreover, I do not find any effects on other crime rates that are correlated with sex crime and homicide. 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. The main uncertainty stems from the fact that individuals expand their network and actively search for potential victims. Investigating this mechanism is beyond the scope of this paper but would add substantial insight into understanding the full matching mechanism. However, if this type of a matching effect is present, the true substitution effect between internet and child abuses coming from a direct consumption channel is likely to be higher compared to the documented net 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, 2000). 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) anonymously 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 in some Federal States. Robustness results suggest that there is some weak evidence of a positive reporting effect indicating the the true consumption effect might be even stronger. The empirical analysis attempts to further investigate the reporting effect by analyzing detection rates. 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.

As a last step, the paper provides evidence on illegal pornographic material and broadband internet. Higher internet availability increases illegal pornography cases which is shown to be strongly related to child abuses. This provides direct evidence of the consumption channel. Moreover, focusing on the composition of the offenders suggests that the consumption channel is a substitute among potential offenders with a relationship to the family of the children who would have been abused a child in the absence of the introduction of the new mass media.

The remainder of the paper is structured as follows. Section 2 describes the data sources and provides descriptive statistics for a defined pre-DSL and DSL period. Section 3 explains the source of identification, before section 4 presents the empirical strategy. Section 5 discusses the sample selection and provides first graphical evidence on the relation between broadband internet and crime. Section 6 presents the net results of the effect of broadband expansion on the different criminal offences as well as further robustness and placebo tests. Section 7 investigates the external validity of the results by comparing the development of criminal activity over time in selected municipalities and municipalities that cannot be used for identifying the effect given the instrumental variable strategy. In section 8, I discuss the possible mechanisms that might drive the net effect of reported crime. Section 9 summarizes and concludes.

2 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 recorded data on criminal offences on the aggregate county and federal state level. Retrieving crime statistics before 2009 proved to be difficult and for some states impossible, especially for statistics from two decades ago. Moreover, some states have data going back to 1996 but on an aggregated criminal offences level. For Rhineland-Palatinate, the earliest available year for crime statistics is 2002. Due to 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. Data on overall sex crime and homicide is available for all four Federal States. Child sex abuse is missing for municipalities in Rhineland-Palatinate and rape is missing for municipalities in Rhineland-Palatinate and Bavaria. See Table A.1 in Appendix A for an overview of available information. Western Germany in total consists of 8,157 municipalities (in 2008 boundaries). These four Federal States make up 77% (6,306) of all municipalities in West Germany. Due to missing values in the outcome variable the number of municipalities is reduced to 6,253.

According to the crime categories I focus on overall sexual crime, sexual abuse against children, rape and homicide (crime against life). Sex crime consists of several sub-categories listed in §174 StGB to §184 StGB (criminal code) including for example sexual abuse and rape. Homicide 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 in which high-speed internet is available. The original data are from the broadband atlas (*Breitbandatlas Deutschland*) published by the Federal Ministry of Economics and Technology (2009).[3] The telecommunication operators self-report covered households with a minimum data transfer rate of 384 kb/s and the self-reported data is available from 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 2005 to 2008 while the pre-DSL period covers the years between 1996 to 1999 (Falck et al., 2014). By comparing the DSL period with a pre-DSL period it is possible to identify 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 as population share (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, industry 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. Moreover, I capture the economic dynamics of the region by using information provided by the Mannheimer Firm Panel. This includes the number of firm entries and exits as well total firm sales (see Table A.1 in Appendix A for a detailed overview of the variables). Some variables for the pre-DSL and DSL period are also available from Falck et al. (2014).

Descriptive Statistics. Despite the rapid expansion of high-speed internet there are differences in the 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 millennium to 55% in 2005. Based on the (N)onliner Atlas (2005) young (more than 80% under 30 years of age) and better educated (more than 80% of university graduates) individuals used the internet intensively. According to occupations, the data show that especially white-collar workers (75%) were internet users, whereas only 52% of unemployed individuals used the internet at the time of the interview. Although the empirical analysis is at the regional level, these numbers provide useful insights into the main user pool.

[3]The established Breitbandatlas is one feature of a 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	4.200	4.261	0.061
	(13.66)	(7.258)	(15.29)
Child sex abuse	0.961	1.050	0.088
	(2.671)	(2.664)	(3.737)
Rape	0.325	0.665	0.339
	(1.160)	(1.744)	(2.078)
Homicide	0.242	0.167	-0.073
	(1.409)	(0.872)	(1.642)
Panel B: Broadband availability			
DSL (share of households)	0	83.89	83.89
	(0)	(19.73)	(19.73)
Panel C: Selected regional information			
Female population share	50.27	50.57	0.298
	(1.754)	(4.353)	(4.071)
Population share aged 18-65	65.73	62.59	-3.140
	(2.771)	(5.020)	(4.758)
Population share aged >65	16.55	18.91	2.364
	(3.310)	(3.528)	(1.583)
Unemployment rate	3.979	4.342	0.363
	(1.726)	(2.159)	(1.756)
Net migration rate	0.506	-0.078	-0.584
	(1.786)	(1.607)	(2.342)
Number of municipalities	6,253	6,253	6,253

Notes: The table reports descriptive statistics for the sample of Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Column (1) reports mean and standard deviation for the pre-DSL period defined as the years 1996 to 1999. Column (2) reports mean and standard deviation for the DSL period defined as 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 Appendix A.

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 4.1 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 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 which accounts for about 15%, total sexual abuse (45%) including child sex abuse (23%) and the distribution of pornographic material (24%). This suggests that the information used in this paper is representative for Germany. Homicide 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.[4]

The second panel of Table 1 reports the fraction of households with access to DSL in West Germany. In the pre-DSL period there are by definition no households with DSL. On average, 84% of all households have DSL available. This number increased from 2005 to 2008 by almost 15 percentage points. Panel C reports selected characteristics on the municipality level. It shows that during the pre-DSL and the DSL period the population is aging, the average unemployment rate increased and, on average, the observed municipalities experienced out-migration. Table B.1 in Appendix B shows further descriptive statistics. The table shows an increase (decrease) in high-skilled (low-skilled) individuals, higher real daily wages and more firms per head (firm density). Moreover, it provides evidence that the economy becomes more service-oriented and less production-intensive. The share of individuals on active labor market programs (ALMP) increases over time. Based on information from the German Statistical Office, the police density decreased slightly between the two periods.

3 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 income per capita, indicating that the composition of these 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. By simply comparing crime rates for two different high-speed internet levels, I would not

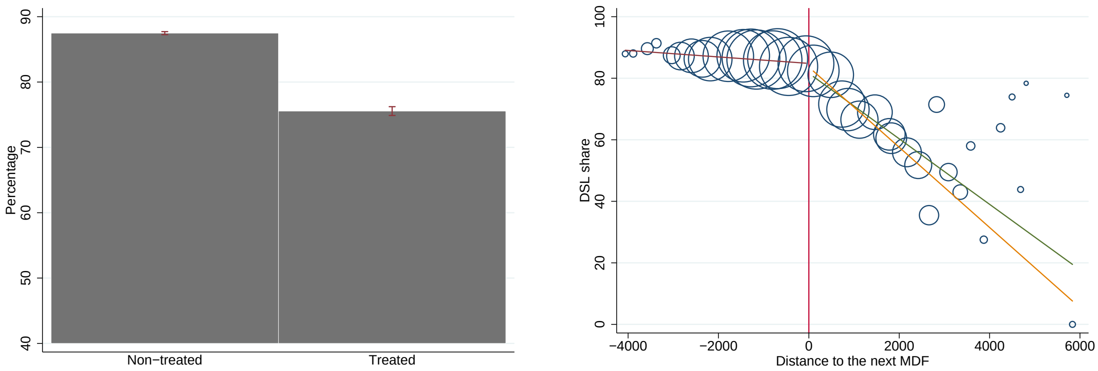
[4]See Figures B.1 to B.4 in Appendix B for a graphical illustration of the crime and DSL rates on the regional level.

be able to estimate the true causal effect. As a result, a simple regression analysis across municipalities of DSL availability on crime would be potentially 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 relied on copper wires to connect households to 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 across 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 MDFs are always placed in high density areas, less agglomerated areas typically share one MDF. Crucially, the length of the copper wires did not affect the quality of the telephone services whereas for DSL connections this distance does matter. It is not feasible for regions that are more than the critical value of 4,200 meters away 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 requires high levels of investment in infrastructure, thus, takes time and is costly.

These technical peculiarities create a quasi-experimental situation for less agglomerated municipalities during the years between 2005 and 2008 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. In particular, treated municipalities are municipalities without an own MDF and with a distance to the next MDF of more than 4,200 meters. Moreover, it is required for treated municipalities that there is no closer MDF available where the municipality could have been connected to. Untreated municipalities are municipalities without an own MDF but with a distance to the next MDF of less than 4,200 meters. To illustrate the DSL availability rates at the household level, Panel (A) of Figure 1 plots the mean of the share of households that have access to DSL in the years between 2005 and 2008 at distances below (non-treated) and above (treated) the critical value of 4,200 meters. Municipalities with relatively short distances to the next MDF show a rather constant fraction of about 86% of households with DSL availability. If the distance exceeds 4,200 meters, the relationship becomes strongly negative - the share of households with DSL availability decreases sharply.[5] A similar picture emerges in Panel (B).

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



(A) DSL by treatment *(B) DSL by distance*

Notes: Panel (A) plots the fraction of households with access to DSL for treated and non-treated municipalities between 2005 and 2008 for West Germany. 95% confidence intervals on top of each bar in Panel (A). Panel (B) plots the share of households with DSL on the distance to the next main distribution frame for all municipalities used under the instrumental variable approach. For representative purpose 4,200 meters are set to zero. The size of the circles in Panel (B) correspond to the number of municipalities within 250 meter bins.

Figure 1: Share of households with DSL availability

Municipalities below the threshold of 4,200 meters (left) show a constant DSL fraction, whereas the fraction decreases between 4,200 meters to 6,200 meters steadily. After 2,000 meters away from the threshold the DSL rate varies greatly between the municipalities. For some municipalities the DSL rate is at a similar level as compared to municipalities just above the threshold. This observation for municipalities rather far away from the threshold (above 6,200 meters) might generate some concerns about the exclusion restriction in the IV setting.

4 Empirical Strategy

As part of the empirical investigation of whether broadband internet leads to different crime offences I begin 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 as the DSL period in Germany.[6] Thus, I regress each of the crime variables on the share of households with home internet access in municipality i, a vector of covariates X_{it} and time-fixed effects λ_t:

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

[6] Information of broadband internet is available from 2005 onwards which restricts the analysis to this year. Moreover, the change in covered households with broadband internet between the start in 2000/01 and the defined DSL period was very rapidly and slowed down thereafter.

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

In a further step in this empirical approach, I account for municipality-fixed effects by comparing crime rates before the DSL era (defined between 1996-1999) with crime rates during the DSL era. 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:

$$\Delta crime_{it} = \beta_0 + \beta_1 \Delta DSL_{it} + \Delta X'_{it}\beta_2 + (\lambda_t \times MDF_i) + \epsilon_{it} \tag{2}$$

where index t indicates that multiple differences are estimated per municipality. $\Delta crime$ 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 (2) regresses the actual level of households with DSL on the change in the crime rates. ΔX is a vector of characteristics at the municipality level and ϵ is an idiosyncratic error term. In the first-difference specification I use *9-year* 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 λ_t and observable characteristics X_{it} in the regressions. MDF_i captures main distribution frame fixed effects, thus, comparing municipalities that are connected to the same main distribution frame. Moreover, I allow for heterogeneous trends within MDF regional units by interacting the MDF-fixed effects with time-fixed effects.

Even after controlling for municipality-fixed effects there might still be endogeneity issues. If, for example, individuals in municipality i buy broadband internet out of a desire to engage or not to engage in violent 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 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 first stage can be written as:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 PSTN_i + \Delta X'_{it}\gamma_2 + (\lambda_t \times MDF_i) + \psi_{it} \tag{3}$$

In the first stage, PSTN is a dummy variable that takes the value *1* if a municipality's distance is above 4,200 meters from the next MDF and zero otherwise. Specifically, I calculate the distance between the geographic centroid and the main distribution frame.[7] This empirical model identifies the effect of the introduction of broadband internet by comparing crime rates with a defined pre-period. The model does not identify changes in broadband internet within the municipality in the DSL period. Moreover, the use of a dummy variable as an instrument identifies local average treatment effects for the compliant municipalities.

5 Sample Selection and Graphical Evidence

Sample selection. Under the described instrumental variable strategy (only municipalities without an own MDF and no closer MDF available), I am able to use a sample of municipalities within the four Federal States equal to 2,691. Comparing the sample size of 2,691 municipalities without an own MDF to all Western German municipalities without an own MDF (3,333), these municipalities cover about 80% of all available municipalities. However, some municipalities in particular from Rhineland-Palatinate are relatively small. As crime rates are weighted by the population, small municipalities may have relatively large crime rates vis-à-vis large changes between the two periods. As shown in Figure B.5 in Appendix B there exists high variation of the change in sex crime rates between municipalities. The first percentile of the change in overall sex crime is -33 cases per 10,000 inhabitants and the 99th percentile is 32 cases per 10,000 inhabitants. In order to prevent that estimation results are biased by large outliers due to the local size of the municipality and to increase the representativeness of the sample, I exclude 257 municipalities (251 from Rhineland-Palatinate and 6 from Bavaria) with a population size of less than 200 inhabitants from the sample. There are further 22 municipalities from Rhineland-Palatinate with an average change of more than -100 overall sex crime cases per 10,000 inhabitants between the two periods. I exclude these outliers from the sample which reduces the sample mainly for overall sex crime and homicide, because Rhineland-Palatinate does not provide information on child sex abuse and

[7] For the purpose of comparison with the IV models, the OLS specifications are estimated on the set of municipalities that fulfil the requirements for the IV approach (no own MDF, no closer MDF available).

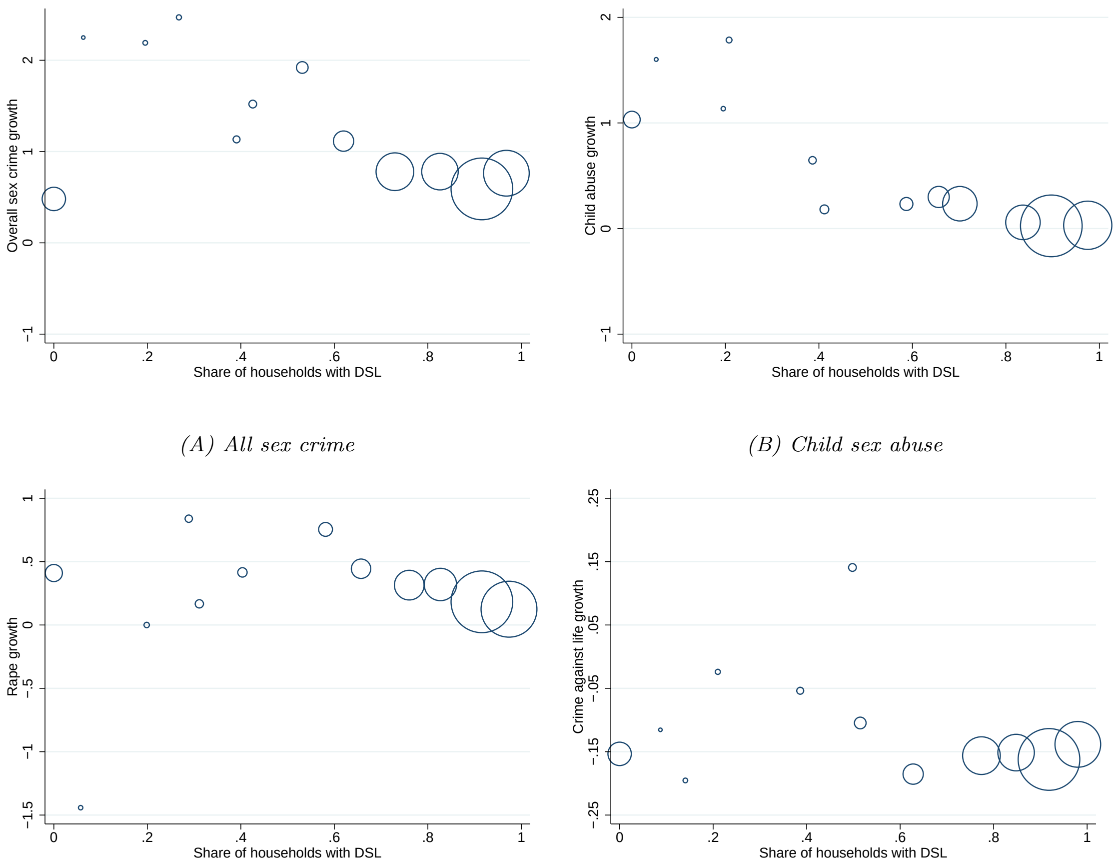
rape.

Moreover, Panel (B) of Figure 1 shows the average DSL rate over the distance to the main distribution frame. Between the threshold of 4,200 meters and roughly 2,000 meters away from the threshold, the DSL share is monotone downward-sloping. After 2,000 meters away from the threshold the variance increases strongly. There are some municipalities with large distances and a relatively high DSL share. Reasons for this observation might be special investment programs and initiative. In terms of the validity of the instrument, however, it might be suggestive evidence for the violation of the exclusion restriction which might bias the estimated coefficients. Therefore, I exclude all observations that are above the green line in Panel (B) of Figure 1 and provide the results on the full sample in Appendix C.[8] In fact, I provide empirical evidence for the violation of the assumption (exclusion restriction) by changing the IV strategy (see Section 6.2). Overall, this leads to a final sample of 2,311 municipalities.

Graphical evidence. Figure 2 plots the graphical relationship between the DSL growth rate and the growth of the four crime categories between the pre-DSL and DSL period controlling for year-by-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 the bins have a size of 0.05 and calculate the average change in the crime rates within the bins. This 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 circles captures the number of municipalities within the defined DSL bins.

For overall sex crime (Panel A) and child sex abuse (Panel B) the figure shows a negative relationship between DSL and crime growth. Higher DSL rates are associated with lower growth in crime rates, a trend which is slightly more pronounced in the case of child sexual abuse. The graphical relation in the case of rape and homicide is less clear and suggests a zero correlation.

[8] I introduce a step function and exclude municipalities with a distance to the next MDF between 6,200 and 7,700 meters and a DSL share above 60% as well as municipalities with a distance above 7,700 meters with a DSL share of more than 50%. This leads to the exclusion of 151 municipalities.



(A) All sex crime

(B) Child sex abuse

(B) Rape

(D) Homicide

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

6 Results

6.1 Baseline Estimation

The analysis of the effect of broadband internet on criminal offences uses an instrumental variable strategy based on the geographic centroid for the municipality to the main distribution frame. 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, then accounts for municipality-fixed effect - *OLS + FD* - by estimating first differences between the DSL period and the pre-DSL period. In this case, the dependent variables are the changes in crime rates per 10,000 inhabitants between the pre-DSL and DSL period. In a last step, the *IV + FD* results account for fixed-municipality effects and instruments the DSL variable with a dummy indicating a distance to the main distribution frame of more than 4,200 meters. Conditional on covariates, there

Table 2: Estimation results of internet availability on crime

	All sex crime (1)	All sex crime (2)	Child sex abuse (3)	Rape (4)	Homicide (5)
OLS	0.016***	0.014***	0.002	-0.0005	0.0005
	(0.006)	(0.006)	(0.003)	(0.003)	(0.0003)
OLS + FD	0.003	0.004	-0.002	-0.001	-0.003**
	(0.010)	(0.010)	(0.003)	(0.004)	(0.001)
IV + FD	-0.033	-0.037	-0.059**	0.007	0.012
	(0.030)	(0.031)	(0.029)	(0.043)	(0.009)
First stage coef. γ_1	-12.92***	-12.40***	-4.809***	-3.489***	-12.40***
	(1.030)	(1.001)	(0.875)	(1.082)	(1.001)
F-Statistic (first stage)	157.4	153.3	30.2	10.4	153.3
Observations	9,223	9,223	3,880	1,996	9,223
Number of MDFs	652	652	376	186	652
Municipalities	2,311	2,311	970	505	2,311
Control variables	No	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipality's in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

is a positive association between broadband internet and selected crime categories, statistically significant for overall sex crime. In terms of magnitude, it shows that a 10% point increase in DSL - which is on average the difference between treated and non-treated municipalities - increases overall sex crime by about 0.14 cases per 10,000 inhabitants. Accounting for municipality-fixed effects by estimating first differences the table shows that the correlation vanishes for all sex crimes and child abuse, whereas homicide shows a significant negative coefficient. This model, which is not directly comparable to the OLS model, identifies long-term shifts in crime rates that are associated with the introduction of DSL. The last estimation results control for fixed-municipality effects by taking differences between the DSL period and the pre-DSL period and instruments the DSL variable. The point estimates for overall sex crime and child abuse decrease strongly, indicating a substitution effect between broadband internet and sex crime. This substitution effect is driven by sexual abuse against children. A 10% point increase in DSL decreases child abuses by about 0.59 cases per 10,000 inhabitants. In contrast, the effects on rape and homicide increase but become insignificant. 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 3.5% to 13% lower DSL rates (depending on the sample) with a F-Statistic ranging between 10 to 160. Thus, concerns about weak identification issues do not apply in this setting.

Sensitivity analysis - bandwidth around the threshold. This subsection varies the distance around the threshold which has a couple of implications. Narrowing the bandwidth provides insights into the variation that identifies the effect DSL has on crime and ensures that high-speed internet access is technologically viable. It further generates a set of more equal municipalities with respect to observable characteristics (see Table B.2 in Appendix B for standard t-tests). For simplicity, I narrow the set of municipalities to less than 2,000 and 3,000 meters around the threshold. Narrowing the threshold has two further implications. It allows to a greater extent for the terminology of lucky (non-treated) and unlucky (treated) municipalities and additional serves as a robustness check with respect to the observed outliers from Panel (B) of Figure 1. Table 3 presents the results. Narrowing the set to munic-

Table 3: IV+ FD estimation results - sensitivity analysis

	All sex crime		Child sex abuse		Rape		Homicide	
	2,000m	3,000m	2,000m	3,000m	2,000m	3,000m	2,000m	3,000m
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ DSL	-0.084*	-0.053	-0.104*	-0.056*	0.025	-0.006	0.020	0.011
	(0.047)	(0.034)	(0.058)	(0.032)	(0.056)	(0.039)	(0.014)	(0.010)
F-Statistic (first stage)	88.6	138.4	15.5	30.9	7.13	11.3	88.6	138.4
Number of MDFs	588	683	333	408	166	195	588	683
Municipalities	1,932	2,373	847	1,049	438	529	1,932	2,373
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's 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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

ipalities closer to the threshold shows a stronger substitution effect for overall sex crime that is driven by child abuse. The point estimate for the set of municipalities with a distance of 2,000 meters around the threshold increases twofold in absolute terms and stays significant at the 10% level. The table shows similar movements, although not significant and accompanied by relatively large standard errors, for rape and homicide. The set of municipalities with 3,000 meters around the threshold - which includes some outliers - shows similar estimates compared to the baseline results without outliers.

The comparison of the results with the full sample in Table C.1 in Appendix C provides some interesting insights. First, the point estimates for all empirical specifications are slightly lower compared to the baseline results in Table 2. This might hint to an attenuation bias, e.g. a correlation of the instrument with the error term. Second, excluding municipalities with relatively large distances increases the documented substitution effect gradually.

6.2 Robustness Analysis - Empirical Specification

This section presents four basic robustness checks on the causal link between DSL and crime. I provide estimation results on the full sample in Appendix C. As a first robustness check, I use the population weighted center instead of using the geographic centroid for estimating the distance to the next MDF. A further concern is the fact that the basic specification takes multiple stacked differences (including time-fixed effects). In order to exclude the possibility that the results are driven by

Table 4: IV + FD estimation results - robustness checks

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Population center	-0.036	-0.048*	0.020	0.008
	(0.030)	(0.027)	(0.035)	(0.008)
Average crime per period	-0.034	-0.053*	0.011	0.012
	(0.031)	(0.030)	(0.044)	(0.009)
Population 500 +	-0.017	-0.077**	0.012	0.026
	(0.048)	(0.033)	(0.052)	(0.019)
Years 2005/06	-0.058*	-0.031	0.045	0.010
	(0.034)	(0.036)	(0.064)	(0.009)
Years 2007/08	-0.013	-0.081**	-0.022	0.013
	(0.037)	(0.038)	(0.047)	(0.009)
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of robustness specifications for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Results using all municipalities are shown in Table C.2. Crime rates are calculated per 10,000 inhabitants. The DSL variable takes values between 0 and 100. Standard errors are heteroskedasticity robust and clustered at the municipality level. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

specific connections between pre- and DSL period crime outcomes, I collapse the observations to one observation per period (pre-DSL and DSL) by calculating the average crime rates and then taking the difference. A third robustness check relates to small populations for some municipalities. This might induce large outliers just by chance (see Kahneman, 2011, page 109 ff). As a fourth check, I use only the outcomes from 2005/06 to see whether the results differ across the defined period.[9] It might be the case that the distribution of extreme or violent media might take some time which is plausible given the early stage of the broadband internet period.[10]

Table 4 reports the *IV + FD* estimation results of DSL expansion on the crime rates. Taking the population weighted center to calculate the distance to the next MDF shows that the child abuse coefficient decreases to -0.048 and becomes significant at the 10% level. Averaging first over the defined periods provides evidence that the results are not driven by specific crime-year combinations. The negative and significant child abuse coefficient decreases slightly. Concentrating on municipalities with at least 500 inhabitants changes the results. All point estimates increase in absolute terms and point - although still not significant - to a positive effect on rape and homicide. Child sex abuse becomes even more negative and more precisely estimated. This provides evidence that relatively small municipalities among the less agglomerated municipalities attenuate the effects towards zero. Using only data from 2005/06 again gives different results. The negative and significant effect for child sex abuse reduces to -0.031 and becomes insignificant. This indicates that the effect is larger in absolute terms for the years 2007/08. In fact, the coefficient is twice as large for the second half of the DSL period. As I will point out in the mechanism section, the rather large difference is connected to the distribution of illegal pornographic material. Pornographic material provides a potential explanation for the substitution effect among child abuse and DSL as both react stronger in 2007/08.

[9] To reduce the possibility of outliers because of less observations per municipality, I first take the average over all pre-DSL years and than estimate the difference to every DSL year accordingly.

[10] 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 fixed. Moreover, I connect a random pre-period year to every DSL year. This is justified as the length of the first difference should not matter for identification of the effect. This generates a similar coefficient (-0.044*) for child abuse. 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 3 out of 30 coefficients of the municipality characteristics are correlated with the instrument.

6.3 Treatment Intensity

The technical peculiarities provide us with a way to construct an instrument for DSL availability. This instrument provides a parameter interpreted as a local average treatment effect. Technically one would expect a constant share of DSL availability below the technical threshold and a zero DSL share thereafter. The first one is observed as show in Figure 1. The data further show a monotone and decreasing relation between the distance and DSL. Municipalities slightly above the threshold of 4,200 meters are having almost as higher DSL shares as municipalities slightly below. Any change of the IV specification that tries to capture the observed distribution would be entirely data driven.[11] However, it might be informative to assess the validity of the instrument by changing the empirical specification.

Treatment intensity I - overidentification test. In order to address the question of validity of the instrument, I further decompose treated municipalities above 4,200 meters away from the next main distribution frame into two sub-categories to perform overidentification tests. To construct a further category, I divide the distance above the threshold at the mean distance among the treated municipalities. The mean municipality among the treated has a distance of 5,300 meters. Thus, I specify the first stage as:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 PSTN_{i,1} + \gamma_2 PSTN_{i,2} + \Delta X'_{it}\gamma_2 + (\lambda_t \times MDF_i) + \psi_{it} \tag{4}$$

where the first treatment dummy $PSTN_1$ captures municipalities at a distance between 4,200 and 5,300 meters. The second treatment dummy $PSTN_2$ captures all municipalities above 5,300 meters. Table C.3 in Appendix C shows the test statistics by splitting the treatment dummy into two categories. All selected crime categories do not show a significant test statistics (Hansen J-Statistic) providing evidence for the validity of the instruments for the sample. In terms of coefficient results, I find a slightly better fit for child sexual abuses. The coefficient of -0.059 documented in Table 2, however, reduces to -0.034.

Table C.4 in Appendix C shows a similar strategy using the full sample. However, instead of using two treatment dummies, the table reports the Hansen J-Statistic for three treatment dummies. The first cutoff stays at 5,300 meters, whereas the second cutoff and thus the third treatment dummy captures the distance after 6,200 meters. Overall, the test statistic increases considerably with significant results for all sex

[11] The situation does not allow for a regression kink design because there is no policy rule that would lead to the observe relationship between the distance and the DSL share.

crime and child sex abuse. This indicates that municipalities with longer distances cause the invalidity of the used instruments. The Hansen J-Statistic decrease further if e.g. only municipalities with a distance of less than 2,000 meters around the threshold are used. All in all, it provides evidences that the third treatment dummy causes the correlation and drives the coefficient towards zero. Moreover, the result further justifies the empirical approach by excluding the outliers and/or narrowing the bandwidth around the threshold.

Treatment intensity II - continuous instrument. The analysis so far uses a dummy variable indicating whether a municipality is treated or not. Panel (B) of Figure 1 shows that the treatment intensity increases with higher distances. Thus, I specify as a further data-driven robustness check the first stage as:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 PSTN_i * distance_i + \Delta X'_{it}\gamma_2 + (\lambda_t \times MDF_i) + \psi_{it} \qquad (5)$$

where $PSTN$ takes the value *1* if a municipality is located more than 4,200 meters away from the MDF and zero otherwise. The treatment dummy is interacted with the actual distance centered at the threshold. This allows the presence of different treatment intensities among the treated municipalities. Given the discussion of the instrument, this specification uses all municipalities with a distance of less than 2,000 meters around the threshold. Table C.5 in Appendix C provides the estimation results. The F-Statistic of the first stage are larger compared to the specification in Table 3. The coefficient for child sexual abuse, however, reduces from -0.10 to -0.023 and becomes significant at the 5% level. Rape and homicide do not react significantly.

6.4 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 differently 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 exhibit 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. These 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 5 shows the results by testing the effect of the treatment dummy on crime. The first difference between 1999 and 1996 shows that treated and non-treated municipalities had similar developments in all four crime categories. Although treated and non-treated municipalities start at different crime levels, the common trend assumption is justified for the empirical specifications for all models. The results suggest a causal interpretation of the coefficients of DSL on the selected crime categories. On a graphical basis, Figures B.6 in Appendix B provides a visualization of pre-DSL crime developments distinguishing treated and non-treated municipalities. The development for all sex crime and the two sub-categories show indeed parallel trends. Although the change in the outcome between 1999 and 1996 is not significant for homicides, Panel (4) of Figure B.6 shows that the development for homicide seems to be different between treated and non-treated municipalities.

Table 5: Estimation results on growth rates between 1999 and 1996 - placebo test

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
treatment dummy	0.389	0.142	0.685	-0.077
	(0.697)	(0.290)	(0.431)	(0.122)
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of placebo specifications for the sample from Bavaria, Baden-Wuerttemberg and Lower Saxony. Results using all municipalities are shown in Table C.6. 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. Robust standard errors in parenthesis. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

7 External Validity

One concern of the analysis might be the fact that the municipalities used in the IV-sample are too different and the results therefore not transferable to more agglomerated municipalities. It is indeed the case that the selected municipalities differ in their regional composition. The local average treatment interpretation naturally limits the generalizability of the results. However, in order to determine the transferability of the results, Figure B.7 in Appendix B plots the development of the reported crime rates per 10,000 inhabitants among the four analyzed categories for the two defined periods distinguishing between selected municipalities under the instrumental variable approach and all remaining municipalities. The figures on the

Table 6: Differences in outcomes among municipalities

	Selected Municipalities (1)	All other Municipalities (2)	Difference Test (p-value) (3)
Δ All sex crime	-0.501	0.758	0.000
	(0.160)	(0.085)	
Δ Child sex abuse	0.072	0.101	0.628
	(0.041)	(0.041)	
Δ Rape	0.299	0.371	0.122
	(0.043)	(0.025)	
Δ Homicide	-0.119	-0.031	0.000
	(0.017)	(0.012)	

Notes: The table reports the mean and 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 parentheses) 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.

left show the developments between 1996 and 1999 and the figures on the right the development between 2005 and 2008. What becomes immediately visible, although at lower actual levels for selected municipalities under the instrumental variable approach, is the strong co-movement of the crime rates. Thus, the graphical analysis shows fairly similar patterns among German municipalities. Additionally, Table 6 shows the means and the standard 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 experience a significant different change over time for all sex crime cases. According to child abuses and rape the changes are not significantly different, whereas municipalities in the IV sample experience a highly significant reduction in homicide. The different results among the sex crime categories compared to all sex crime are driven by Rhineland-Palatinate. Figure B.5 in Appendix B shows the full distribution of the changes in crime among selected municipalities. The density plot for all sex crimes show slightly fatter tails compared to the remaining municipalities (not shown), indicating higher dynamics among the selected municipalities. The same is to a lesser extent true for the remaining categories.

8 Mechanisms

In order to gain insight 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 which affects individual behavior and becomes observable in 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.

8.1 Reporting Effect

Regarding the reporting effect, it is well known that sex crime in particular is prone to underreporting. It is possible that the internet leads to e.g. higher rates of reported sex crime without increasing the actual number of sex crimes. Following Bhuller et al. (2013) this might be the case given the fact that the costs of reporting a crime have decreased in the internet period. One way this might happen is through facilitating contact with support groups.[12] In some German Federal States

Table 7: IV + FD estimation results excluding Lower-Saxony

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	-0.040 (0.031)	-0.086*** (0.030)	-0.064 (0.047)	0.012 (0.009)
F-Statistic (first stage)	155.4	29.6	12.2	155.4
Observations	8,132	2,796	934	8,132
Number of MDFs	556	280	91	556
Municipalities	2,042	703	241	2,042
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample from Bavaria, Baden-Wuerttemberg and Rhineland-Palatinate. Results using all municipalities are shown in Table C.7. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

it is possible to report an offence online. In 2003, Brandenburg began implementing

[12] The crime statistics in Germany are organized such that offences are reported in the reporting year and not retrospectively to the year of occurrence.

"online guards" followed by Mecklenburg-Vorpommern, Hesse and Berlin in 2005. Lower Saxony and Rhineland-Westphalia also adopted "online guards" in 2007. Although most States offer residents the opportunity to inform themselves about crime and to get in contact with law enforcement, it is not always possible to report an offence online. Even today it is not possible to report offences online in Bavaria, Rhineland-Palatinate, Thuringia, Saarland and Bremen.[13] In order to investigate whether reported crime rates are influenced by a lower cost of reporting, I exclude municipalities from Lower-Saxony where online reporting was possible during the time period.[14] Table 7 shows that the direction of the coefficients do not differ when municipalities from Lower Saxony - where lower reporting cost are most likely to be present - are excluded. In fact, estimates from column (1) and column (4) are at the same level. The point estimate for child sexual abuse increases considerable in absolute terms. However, the point estimate is not significantly different from -0.059 in Table 2 but becomes significant at the 1% level. The increase in absolute terms provides some evidence that a positive reporting effect might be present and the effect shown in Table 2 represents an upper bound. If that is the case, the consumption channel and thus the substitution effect are even more pronounced. What should be noted at this stage is the coefficient for the early years (2005/06) without Lower Saxony. Excluding Lower Saxony provides a coefficient equal to -0.047 and by focussing in addition on municipalities with at least 500 inhabitants gives a DSL coefficient of -0.052 that is marginal significant at the 10% level (t-value: 1.5). This provides evidence that effect is not entirely driven in later years (2007/08). This effect is also present by using the full sample. Results are reported in Table C.7 in Appendix C.

A further way to investigate the reporting effect is by analyzing detection rates. This follows the assumption that the lower cost of reporting by e.g. meeting with other victims and/or gathering information online leading 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 8. The table shows that DSL does not lead to higher detection rates. Following the assumption, the internet does not induces weaker cases. However, the coefficients for child abuse and rape are even negative which might have implications on the nature of the

[13]The online page http://www.online-strafanzeige.de/ which Federal States allow offences to be reported online with a link to the specific police departments.

[14]After consulting the Federal Police of Baden-Wuerttemberg they made clear that, although it is possible to report offences and contact law enforcement online, this option is no substitute for reporting criminal offences in the traditional way.

Table 8: IV + FD estimation results analyzing detection rates

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	0.048	-0.104	-0.106	-0.000
	(0.047)	(0.080)	(0.094)	(0.003)
F-Statistic (first stage)	136.9	22.8	10.2	153.4
Observations	5,128	2,374	1,518	8,420
Number of MDFs	578	323	171	630
Municipalities	2,158	862	474	2,267
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for detection rates for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Results using all municipalities are shown in Table C.8. Detection rates are calculated in percent. In the case of zero criminal activity in both periods, I assume a zero change between the two periods. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

composition of offenders (relation vs no relation to the family). This is addressed in subsection 8.4.

8.2 Matching Effect

According to 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 individual's network which might increase the probability of a match. On the other hand, if the internet displaces other activities with activities at home it might cause a reduction in sex crime and murder due to less personal contact. While the net effect is not clear, I investigate the matching effect by analyzing total crime rates other than sex crime and homicide.[15] If individuals spend more time at home, then this should be visible in an observable reduction across all crime rates. Table 9 presents IV estimates. The regression model shows that higher broadband internet does not affect the total number of reported crimes other than sex crime and homicide. This is indirect evidence that time spent at

[15]The indirect effect might be in place if the internet displaces other activities that are correlated with sex crime. This might be in line with Dahl and DellaVigna (2009) showing that violent crime reduces after larger theater audiences for violent movies. The reasons for the reduction are indirect because of the attendance but also because of a direct substitution away from criminal behavior.

home does not drive the results. One should note that this result does not mean that the amount of time spent at home did not change at all over the pre-DSL and DSL period. It merely indicates that treated and non-treated municipalities do not behave differently. Column (2)-(4) also report estimation results for crime categories that are correlated with sex crime and murder. If an indirect mechanism drives the results for child abuses than it would be plausible to find similar effects for correlated crime categories. Again the introduction of broadband internet had no effect on other crime rates that are correlated with sex crime and homicide. It suggests that the channel works through consumption of extreme media. The "time

Table 9: IV + FD estimation results analyzing other crime rates

	All other crime (1)	Theft (2)	Arms-related offences (3)	Drug-related offence (4)
Δ DSL	-0.389	0.026	-0.018	-0.204
	(1.296)	(0.240)	(0.131)	(0.181)
F-Statistic (first stage)	153.3	152.0	10.4	153.3
Observations	9,221	8,171	1,996	19,223
Number of MDFs	652	557	186	652
Municipalities	2,311	2,048	505	2,311
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for all crime rate excluding sex crime and homicide, theft, arms-related offences, and drug-related offences for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Results using all municipalities are shown in Table C.9. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

spent 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 the frequency with which people meet with friends or go to cinemas and restaurants or bars. Given that social behavior such as going out and meeting with friends is unaffected, the probability of a match between an offender and a victim might not change as a result of broadband internet. The authors further show that high-speed internet has a positive effect on the number of out-of-school activities for children between the age of 7 and 16. This might even increase the number of matches indicating again that the reported negative effect for child abuse might be an upper bound in the baseline specification. Although the investigation of the full matching effect seems not to drive the empirical results to a great extent, there remains some uncertainty. 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 teenagers primarily between the ages of 13 and 15 years old. This part of the matching effect cannot be addressed in this paper. The presence of this matching effect, however, would again lead to an upward bias indicating a stronger consumption effect.

8.3 Direct Effect through Illegal Pornographic Material

For a subset of municipalities, the data provide information on the distribution and possession of illegal pornographic material. Detailed information from Lower Saxony shows that in over 90% of cases, illegal pornographic material has clear child-related content. A potential rise in illegal pornography might explain the strong substitution effect for child sex abuse. The German State Criminal Offices of Baden-Wuerttemberg and Lower Saxony provide information on illegal pornographic material in general. A row correlation (not shown in the table) shows that DSL is

Table 10: Estimation results analyzing illegal pornographic material

	OLS + FD	IV + FD			
	All (1)	All (2)	2,000m threshold (3)	All 07/08 (4)	2,000m threshold 07/08 (5)
Δ DSL	0.011***	0.056	0.063	0.104**	0.098*
	(0.004)	(0.040)	(0.054)	(0.048)	(0.059)
F-Statistic (first stage)		10.4	7.13	13.2	10.9
Observations	1,996	1,996	1,733	997	866
Number of MDFs	186	186	166	186	166
Municipalities	505	505	438	505	438
Control variables	Yes	Yes	Yes	Yes	Yes

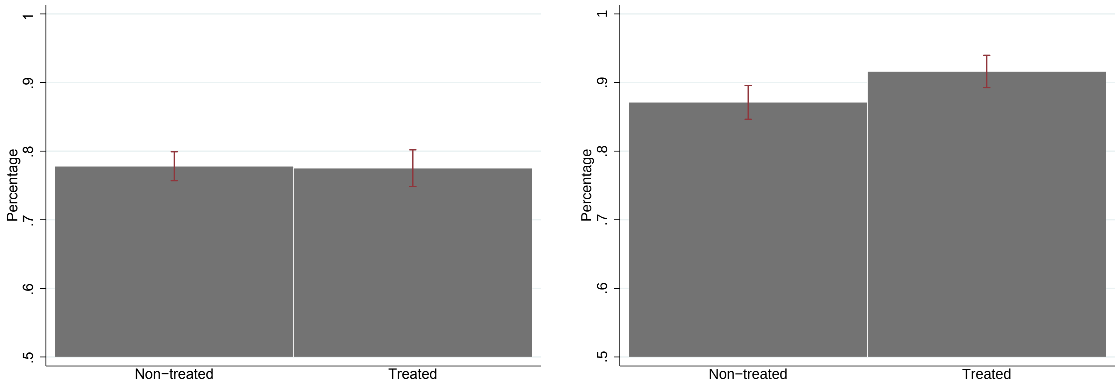
Notes: The table reports regression results of DSL on illegal pornographic material for the sample from Baden-Wuerttemberg and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

positively related to pornography. Table 10 presents in column (1) the long-term shift of illegal porn that is associated with the introduction of broadband internet. It shows that DSL increases the possession and distribution of such material. Column (2) to (5) estimate the same model but instrumenting the DSL variable for all municipalities and for municipalities with less than 2,000 meters around the threshold as well as for the later DSL years 2007/08. Focussing on the overall effect, the results point to a downward bias of the OLS coefficient and a positive causal relation (t-value: 1.40 in column (2)). The point estimate increases in column (3) for municipalities with less than 2,000 meters around the threshold. This is consistent with the documented findings above. Only focussing on the later years 2007/08 shows that illegal pornography response to DSL fairly strongly with a coefficient of 0.104 that is significant at the 5%-level (10% point increase in DSL increases illegal pornography cases by 1.2 cases per 10,000 inhabitants). The point estimate is at a similar level among municipalities with a distance of less than 2,000 meters around the threshold. The mechanism seems to be valid as supply and demand for pornography might shift to a new equilibrium within the DSL period. As demand and supply rise, individual behavior seems to adjust which becomes observable in a decrease in child sexual abuse cases. The results provide evidence for a potential mechanism that explains the substitution effect for child sex abuse.

Although the analysis of illegal pornographic material provides a potential explanation for the substitution effect on child sex abuse, the internet might also induce an indirect effect for offenders. The internet does not only provide a way for victims to come into contact with e.g. other victims and support groups. It is also possible for potential perpetrators to contact support groups or other individuals with similar "tastes" anonymously. This indirect effect might lead to a reduction in crime cases. To my knowledge, there is no data set that might allow to get closer to this potential explanation. In fact, it could be the case that the coefficients (negative effect on child abuse and positive effect on pornographic material) 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 a simultaneous decrease in child abuse at the regional unit. However, a first simple correlation analysis shows a negative association between illegal pornographic material and child sexual abuse. A further way to investigate this descriptively is by regressing the change in child abuse chases on the change in illegal pornography offences conditional on covariates and MDF-by-year-fixed effects. This results in a significant (10%-level) coefficient of -0.051. An increase in e.g. 10 illegal pornography cases decreases child sex abuse by about 0.5 offences per 10,000 inhabitants. The coefficient becomes -0.058 (10% significance level) by narrowing the set of municipalities to those with a distance of less than 2,000 meters around the threshold (see detailed results in Table C.10 in Appendix C). This finding is in line with the documented result of a stronger substitution effect among municipalities with a distance of less than 2,000 meters around the threshold and supports the hypothesis that pornography drives the substitution effect.

8.4 On the Composition of Offenders

Child victims are often abused by family members or relatives. Based on survey data among 223 imprisoned offenders in Germany, there exists some evidence that the offenders within this crime category are primarily related to the family of the child (Turner et al., 2014). In more than half the cases (53%) the offenders had abused children within their families, whereas 30% had abused children outside the family.[16] Using more representative data for the year 2008 published by the German Criminal Office shows that in 58% of all child sex abuses the offender had a relation to the childs' family (19% relatives, 30% personal acquaintance, 9% acquaintance). Based on the negative causal effect of DSL on child abuse, one hypothesis is that broadband internet is a substitute among offenders who would have been abused a child with a relationship to the family in the absence of the introduction of the new media. This hypothesis is difficult to analyze with the underlying data. However, one way to get closer to this statement is by going back to the analysis of detection rates. Table 8 reported a negative and insignificant coefficient of DSL on detection



(1-A) pre-DSL period *(1-B) DSL period*

Notes: The figures plot the detection rates for child sex abuse for treated and non-treated municipalities with a distance of less than 2,000 meters around the threshold. Panel (1-A) reports the detection rates for the pre-DSL period. Panel (1-B) reports the detection rates for the DSL period. Red bars indicate 90% confidence intervals.

Figure 3: Detection rates of child sex abuse cases by treatment and period

rates. In order to run the regression with a sufficient number of observation, I

[16]The study by Turner et al. (2014) may not be representative to child abuses in general but provides interesting results by showing socio-demographic characteristics. Moreover, non-reporting might be even more severe for child abuses within the family.

assumed a zero change between the two defined periods in the absence of any offence. However, it is often the case that there is one case in e.g. the DSL period only and not in the pre-DSL period which leads to the exclusion of the municipality in the regression. Figure 3 reports simple average detection rates of child abuse cases by period and treatment status. The average detection rates in the pre-DSL period have been similar between treated and non-treated municipalities and are slightly below 80%. Panel (1-B) shows that detection rates in general increased over time but the increase was significantly stronger among treated municipalities (p-value of a difference test in the pre-DSL (DSL) period is 0.905 (0.037)). This increase - which is not observed for other crime rates (see Figure C.1 in Appendix C) - might be a hint that in municipalities with higher DSL availability (non-treated) the pool of offenders is changing towards a higher fraction of offenders with no relation to the child's family. This holds under the assumption that reported child abuse cases have a higher probability of being declared if there exists a relation to the family. Therefore, it provides some suggestive evidence that the substitution effect is driven by offenders with a relationship to the potential children who would have abused the child without DSL.[17]

9 Discussion and Conclusions

Does high-speed internet lead to higher or lower rates of criminal activity? Using unique German data on the regional level, this paper documents a substitution effect of child sexual abuse and internet availability, whereas rape and murder do not significantly respond to higher availability of broadband internet. This result is robust to various empirical specifications and is higher in magnitude for municipalities rather close to the technical threshold.

Identifying the effects of internet availability on criminal offences suffers from selection bias. To overcome the omitted-variable bias, I follow Falck et al. (2014) and exploit regional peculiarities of the traditional public switched telephone net-

[17]Base on the debate about child abuses through catholic patronates/priests at the beginning of the 2000s in the US and around 2010 in Germany (Leygraf et al., 2012, Terry et al., 2011), one hypothesis might be that the negative coefficient is driven by municipalities with monasteries/cloisters (a map with the location of monasteries can be found at `https://www.orden.de/ordensleben/geistliche-landkarte/`). This might be the case because they are likely to be classified as having a relationship to the child. Table C.11 in Appendix C shows the results by excluding municipalities with at least one monastery. If these municipalities are driving the results, we would expect the negative coefficient to become closer to zero. In fact, the table shows that the coefficients are stable across categories suggesting that this particular group does not drive the results.

work (PSTN), which affects the capacity 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 and 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 results should be interpreted as medium- to long-term shifts in crime rates that are due to the new technology.

One remaining question is the different findings compared to Bhuller et al. (2013). The medium- to long-term perspective might be one aspect of the different documented results compared to findings for Norway. The set-up in Norway is based on yearly within-municipality variation by focussing on the first 9 years after the DSL introduction, whereas this study compares crime rates during a period when DSL was already implemented with a period when it was not. Moreover, the empirical strategies differ which might have implications on the group of compliers. In the set-up that underlies this study, compliers are less-agglomerated municipalities with low DSL shares because they are located relatively far away from the next distribution frame, whereas in Norway, the complier group consist of municipalities that use the internet because of the increase in coverage in the previous year. As the yearly growth in the coverage rate decrease, the relative weight of the compliers changes over time towards less agglomerated municipalities. Beside the different empirical strategies, the different effects might also be due to different responses at the individual level. It is possible that the distribution of "regular" or non-illegal pornographic material via e.g video tape was already high in Germany and the internet caused a substitution from tape to online consumption. If non-illegal pornography increases rape, this might explain the zero effect in Germany. The increase in illegal pornographic material in Germany provides a plausible mechanism for the reduction in child abuse. It would be interesting to study changes in illegal pornography in Norway to rationalize the documented zero effect on child abuse.

The estimated net effect might be driven by different mechanisms. Alongside a direct effect resulting 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 reduces 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 individual's 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 corresponds to a direct effect of increased extreme media consumption. In particular, the results suggest to some extent a positive reporting effect indicating that the substitution effect through the consumption channel is even stronger. The consumption channel is further supported by the observation that illegal pornographic material responds strongly to broadband internet and proves to be a potential explanation for the overall substitution effect for sexual abuse against children. This is confirmed by representative observations for Lower Saxony where over 90% of pornography offences involve child-related content and among them about 50% correspond to possessing child pornography. The data further suggest that the composition of the pool of offenders changed due to internet availability. One potential explanation is that the decrease in child cases appeared to be among offenders with a relation to the family who would have abused the child without the introduction of broadband internet. Following the assumption that child abuse cases with strangers have a lower probability of being declared, the data provide evidence for lower detection rates in high internet regions and therefore provide suggestive evidence on the background of such offences.

This paper contributes to the discussion of the adverse side effects of broadband internet. Although there is evidence of a substitution effect for child sex abuse, increased child-related pornographic material is per se an adverse side effect for society as a whole. The results suggest that at least some potential offenders do search for alternatives which provides scope for law enforcement/government to offer external psychological support before child-related content is consumed while simultaneously prosecuting individuals and organization who are producing and distributing illegal pornographic material. The results further suggest that external and psychological support might be most successful among potential offenders who have a relation to the family. In order to derive more comprehensive conclusions and policy recommendations it is necessary to study the internet effect beyond the introduction.

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Appendix of Online Publication

A Data Addendum

Table A.1: Definition of variables

Crime variables	Description
All sex crime	Number of reported sexual offences as defined 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-Wuerttemberg
Child sex abuse	Number of reported sexual offences as defined 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-Wuerttemberg
Rape	Number of reported sexual offences as defined 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-Wuerttemberg
Pornographic material	Number of reported sexual offences as defined 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-Wuerttemberg
Homicide	Number of reported crime against life offences as defined 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-Wuerttemberg
Internet variables	
Broadband internet	Fraction of households in municipality i at time t with technical availability of 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

Table A.1 continued: Definition of variables

Control variables	Description
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: Federal Employment Agency and Falck et al. (2014) Availability: all German municipalities
Population aged 18-65	Fraction of the population aged between 18 and 65 years in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Population aged > 65	Fraction of the population aged above 65 years in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Net migration	Net migration rate in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Unemployment rate	Unemployment rate in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Occupation	Occupational shares in municipality i at year t calculated for the categories agrar, production, salary, sale, clerical and service (ref. service sector). The pre-DSL fractions are calculated for the years 1996 to 1999 based on administrative data provided by the Federal Employment Agency.
	Source: Federal Employment Agency
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-Wuerttemberg
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 fractions are calculated for the years 1996 to 1999 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 involved in a publicly sponsored labor market program. The pre-DSL fractions are calculated for the years 1996 to 1999 based on administrative data provided by the Federal Employment Agency.
	Source: Federal Employment Agency Availability: all German municipalities

Table A.1 continued: Definition of variables

Control variables	Description
Industry	Industry shares in municipality i at year t calculated for the categories agrar/energy/mining, production, steel/metal/machinery, vehicle construction/apparatus engineering, consumer goods, food, construction, finishing trade, wholesale trade, retail trade, transport and communication, business services, household services, education/helth, organizations, public sector, else. Source: Federal Employment Agency
Skill level	Skill level in municipality i at year t. *Low skilled*: No degree/ high-school degree *Medium skilled*: Vocational training *High skilled*: Technical college degree or university degree. The skill level is also measured for the inflow-specific sample. Missing and inconsistent data on education are corrected according to the imputation procedure described in Fitzenberger et al. (2006). This procedure relies on the assumption that individuals cannot lose their educational degrees. Source: Federal Employment Agency
Real daily wage	Average real daily wage in municipality i at year t calculated among full-time employees. Gross daily wages are right-censored due to the upper social security contribution limit. To address this problem, we construct cells based on gender, year and region (East and West Germany). For each cell, a Tobit regression is estimated with log daily wages as the dependent variable and age, tenure, age squared, tenure squared, full-time dummy, two skill dummies, occupational, sectoral as well as regional (Federal State) dummies as explanatory variables. As described in Gartner (2005), right-censored observations are replaced by wages randomly drawn from a truncated normal distribution whose moments are constructed by the predicted values from the Tobit regressions and whose (lower) truncation point is given by the contribution limit to the social security system. After this imputation procedure, nominal wages are deflated by the CPI of the Federal Statistical Office Germany normalised to 1 in 2010. Source: Federal Employment Agency
Number of establishments	Number of establishments in municipality i at year t. Source: Federal Employment Agency
Size of establishments	Number of employees per establishment in municipality i at year t. Source: Federal Employment Agency
Number of female & low-qualified employee	Number of female and low-qualified employees per establishment in municipality i at year t. Source: Federal Employment Agency
Median establishment wage/age	Median wage/age at the establishment level based on employee information in municipality i at year t. Source: Federal Employment Agency
Number of entry firms	Number of firms entering the market in municipality i at year t. The pre-DSL fraction refers to the year 2000. Source: Mannheimer Firm Panel
Number of exit firms	Number of firms exiting the market in municipality i at year t. The pre-DSL fraction refers to the year 2000. Source: Mannheimer Firm Panel
Total sales	Total sales based on firm information in municipality i at year t. The pre-DSL fraction refers to the year 2000. Source: Mannheimer Firm Panel

B Additional Descriptive Results

Table B.1: Further descriptive statistics

	pre-DSL period (1)	DSL period (2)
Regional information		
Low-skilled	0.170	0.153
	(0.044)	(0.037)
Medium-skilled	0.777	0.777
	(0.047)	(0.047)
High-skilled	0.226	0.272
	(0.280)	(0.344)
Average real daily wage	97.047	101.142
	(11.236)	(18.055)
Police density	0.061	0.060
	(0.120)	(0.116)
Female population share 20-30	0.443	0.463
	(0.093)	(0.094)
Female population share 30-40	0.387	0.446
	(0.093)	(0.087)
Female population share 40-50	0.411	0.465
	(0.113)	(0.083)
Female population share 50-65	0.357	0.440
	(0.149)	(0.101)
Foreign population share 20-30	0.051	0.040
	(0.058)	(0.050)
Foreign population share 30-40	0.041	0.049
	(0.045)	(0.053)
Foreign population share 40-50	0.042	0.033
	(0.053)	(0.038)
Foreign population share 50-65	0.034	0.033
	(0.055)	(0.042)
Share of ALMP	0.002	0.007
	(0.007)	(0.008)
Regional occupational structure		
Agriculture	0.020	0.020
	(0.020)	(0.016)
Production	0.385	0.309
	(0.090)	(0.077)
Salary	0.107	0.115
	(0.041)	(0.038)
Sale	0.062	0.069
	(0.022)	(0.025)
Clerical	0.207	0.212
	(0.057)	(0.056)
Service	0.211	0.264
	(0.057)	(0.077)

Notes: The table reports descriptive statistics for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. 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. See Table A.1 for the source of the variables.

Table B.1 continued: Further descriptive statistics

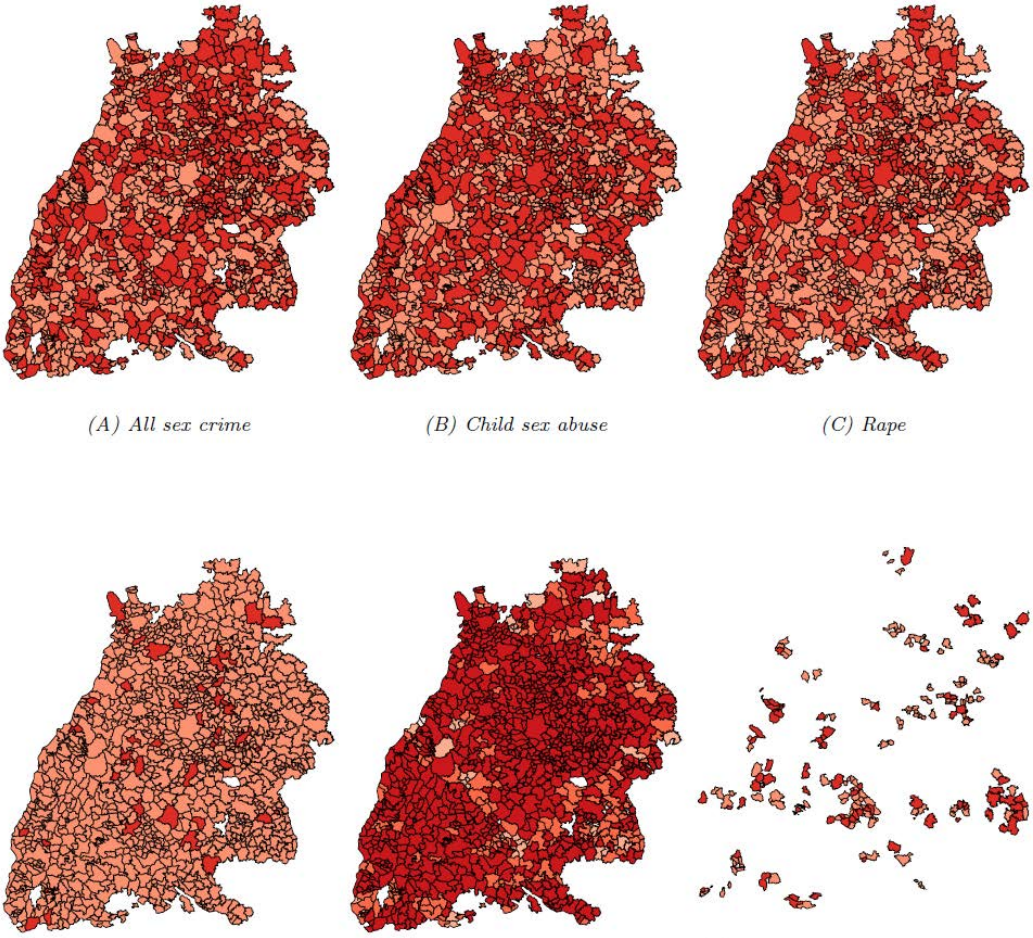
	pre-DSL period (1)	DSL period (2)
Firm information		
Number of establishments	35.414	50.169
	(45.807)	(62.898)
Establishment size	6.669	6.611
	(6.122)	(5.426)
Number of female employees	2.509	3.040
	(2.709)	(2.717)
Number of low qualified employees	1.655	1.175
	(2.196)	(1.589)
Median establishment wage	57.203	60.476
	(9.691)	(11.778)
Median establishment age	37.412	42.510
	(4.280)	(3.456)
Number of entry firms	3.431	3.161
	(4.627)	(4.178)
Number of exit firms	2.384	4.003
	(3.614)	(5.472)
Sales	35.369	82.685
	(380.464)	(863.031)
Industry composition		
Agriculture/Energy/Mining	0.030	0.027
	(0.024)	(0.020)
Production	0.073	0.055
	(0.054)	(0.040)
Steel/Metal/Machinery	0.103	0.094
	(0.065)	(0.062)
Vehicle construction/Engineering	0.049	0.044
	(0.052)	(0.046)
Consumer goods	0.061	0.046
	(0.044)	(0.032)
Food	0.037	0.035
	(0.026)	(0.022)
Construction	0.070	0.041
	(0.044)	(0.027)
Finishing trade	0.051	0.037
	(0.024)	(0.018)
Wholesale trade	0.051	0.049
	(0.026)	(0.023)
Retail trade	0.085	0.094
	(0.029)	(0.028)
Transport and communication	0.044	0.051
	(0.025)	(0.022)
Business services	0.081	0.103
	(0.035)	(0.039)
Household services	0.059	0.078
	(0.034)	(0.034)
Education/Health	0.114	0.131
	(0.043)	(0.042)
Organizations	0.015	0.020
	(0.013)	(0.013)
Public sector	0.053	0.054
	(0.026)	(0.023)

Notes: The table reports descriptive statistics for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. 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. See Table A.1 for the source of the variables.

Table B.2: Difference test by treatment status and sample

	Full sample				Less than 2,000 meters around the threshold			
	N	non-treat	treat	p-value	N	non-treat	treat	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population	2,462	1,380.862	1,450.658	0.247	1,932	1,320.267	1,382.552	0.307
Female population share	2,462	0.501	0.499	0.001	1,932	0.501	0.499	0.054
Population share aged 18-65	2,462	0.621	0.609	0.000	1,932	0.619	0.612	0.009
Population share > 65	2,462	0.190	0.186	0.004	1,932	0.191	0.187	0.020
Unemployment rate	2,462	0.041	0.038	0.000	1,932	0.040	0.039	0.041
Net migration rate	2,462	-0.001	-0.001	0.479	1,932	-0.001	-0.001	0.844
Low-skilled	2,462	0.154	0.150	0.031	1,932	0.153	0.152	0.718
Medium-skilled	2,462	0.771	0.781	0.000	1,932	0.773	0.780	0.002
High-skilled	2,462	0.498	0.483	0.441	1,932	0.499	0.502	0.904
Average real daily wage	2,462	99.922	97.844	0.005	1,932	99.421	97.506	0.018
Firm density	2,462	0.026	0.026	0.558	1,932	0.025	0.026	0.603
Police density	2,462	0.084	0.090	0.340	1,932	0.084	0.093	0.226
Female population share 20-30	2,462	0.466	0.466	0.993	1,932	0.465	0.466	0.773
Female population share 30-40	2,462	0.473	0.460	0.001	1,932	0.473	0.460	0.006
Female population share 40-50	2,462	0.486	0.489	0.389	1,932	0.486	0.489	0.478
Female population share 50-65	2,462	0.460	0.448	0.001	1,932	0.459	0.448	0.005
Foreign population share 20-30	2,462	0.032	0.026	0.001	1,932	0.030	0.026	0.058
Foreign population share 30-40	2,462	0.043	0.035	0.000	1,932	0.040	0.036	0.079
Foreign population share 40-50	2,462	0.025	0.021	0.001	1,932	0.023	0.021	0.226
Foreign population share 50-65	2,462	0.024	0.021	0.009	1,932	0.023	0.021	0.345
Share of ALMP	2,462	0.013	0.012	0.135	1,932	0.012	0.012	0.200
Occupational structure								
Agriculture	2,462	0.020	0.022	0.015	1,932	0.020	0.021	0.387
Production	2,462	0.305	0.317	0.000	1,932	0.307	0.317	0.007
Sale	2,462	0.070	0.068	0.001	1,932	0.070	0.067	0.002
Salary	2,462	0.117	0.112	0.009	1,932	0.116	0.113	0.082
Clerical	2,462	0.216	0.204	0.000	1,932	0.214	0.203	0.000
Service	2,462	0.266	0.273	0.027	1,932	0.266	0.274	0.031
Firm information								
Number of establishments	2,462	40.766	42.237	0.510	1,932	38.287	39.739	0.507
Establishment size	2,462	6.413	6.443	0.890	1,932	6.387	6.413	0.917
Number of female employees	2,462	2.943	2.951	0.939	1,932	2.944	2.935	0.936
Number of low qualified	2,462	1.058	1.090	0.629	1,932	1.060	1.107	0.533
Median establishment wage	2,462	60.111	61.237	0.038	1,932	60.039	61.016	0.108
Median establishment age	2,462	43.494	43.005	0.002	1,932	43.482	43.075	0.014
Number of entry firms	2,462	2.241	2.359	0.364	1,932	2.109	2.247	0.318
Number of exit firms	2,462	3.454	3.449	0.981	1,932	3.278	3.353	0.701
Sales	2,462	60.791	57.421	0.702	1,932	59.047	54.709	0.687
Sector composition								
Agriculture/Energy/Mining	2,442	0.026	0.029	0.003	1,917	0.026	0.028	0.072
Production	2,462	0.057	0.052	0.009	1,932	0.057	0.052	0.011
Steel/Metal/Machinery	2,462	0.095	0.097	0.386	1,932	0.095	0.099	0.192
Vehicle construction/Engineering	2,462	0.041	0.042	0.313	1,932	0.041	0.043	0.381
Consumer goods	2,462	0.042	0.043	0.668	1,932	0.043	0.042	0.398
Food	2,462	0.033	0.036	0.001	1,932	0.034	0.036	0.079
Construction	2,462	0.037	0.045	0.000	1,932	0.037	0.044	0.000
Finishing trade	2,462	0.035	0.037	0.033	1,932	0.035	0.037	0.039
Wholesale trade	2,462	0.050	0.047	0.001	1,932	0.050	0.047	0.002
Retail trade	2,462	0.095	0.091	0.002	1,932	0.095	0.091	0.012
Transport and communication	2,462	0.053	0.053	0.915	1,932	0.053	0.053	0.558
Business services	2,462	0.106	0.100	0.001	1,932	0.105	0.101	0.024
Household services	2,462	0.079	0.078	0.335	1,932	0.078	0.078	0.819
Education/Health	2,462	0.135	0.131	0.018	1,932	0.136	0.132	0.044
Organizations	2,462	0.020	0.020	0.593	1,932	0.020	0.021	0.535
Public sector	2,462	0.055	0.054	0.233	1,932	0.055	0.054	0.316

Notes: The table reports descriptive statistics for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony in 2008 by treatment status. Column (1)-(4) report the means and the p-value of a standard t-test for the full sample. Column (5)-(8) report the means and the p-value of a standard t-test for municipalities with distances of less than 2,000 meters around the threshold.

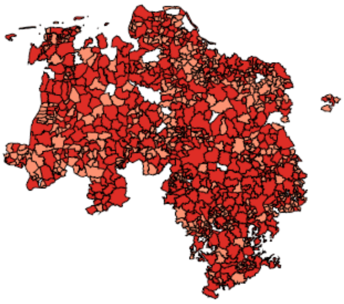


(A) All sex crime (B) Child sex abuse (C) Rape

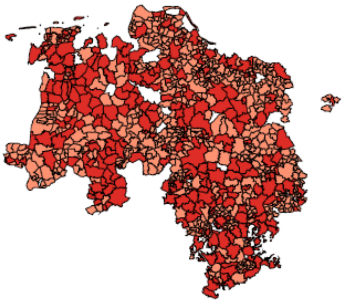
(D) Homicide (E) DSL (F) Treated and non-treated municipalities

Notes: Figures (A), (B), (C) and (D) plot the geographical distribution of the dependent crime variables (change in crime rate per 10,000 inhabitants between the pre-DSL and the DSL period) for Baden-Wuerttemberg. Dark (light) red correspond to a positive (negative) change per 10,000 inhabitants. 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). Figure (F) shows treated (dark) and non-treated (light) municipalities used in the empirical section. White areas indicate missing values.

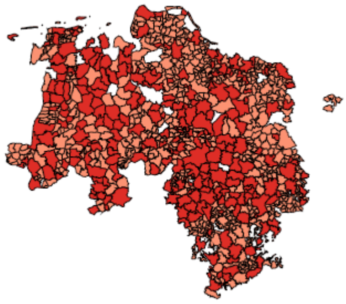
Figure B.1: Geographical distribution of crime and DSL growth rates and treated/non-treated municipalities for the Federal State of Baden-Wuerttemberg



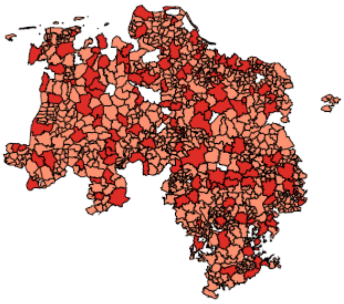
(A) All sex crime



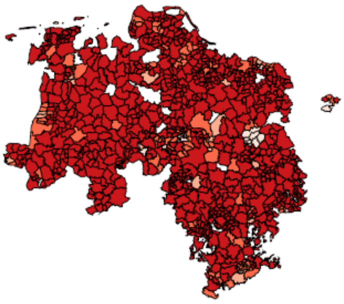
(B) Child sex abuse



(C) Rape



(D) Homicide

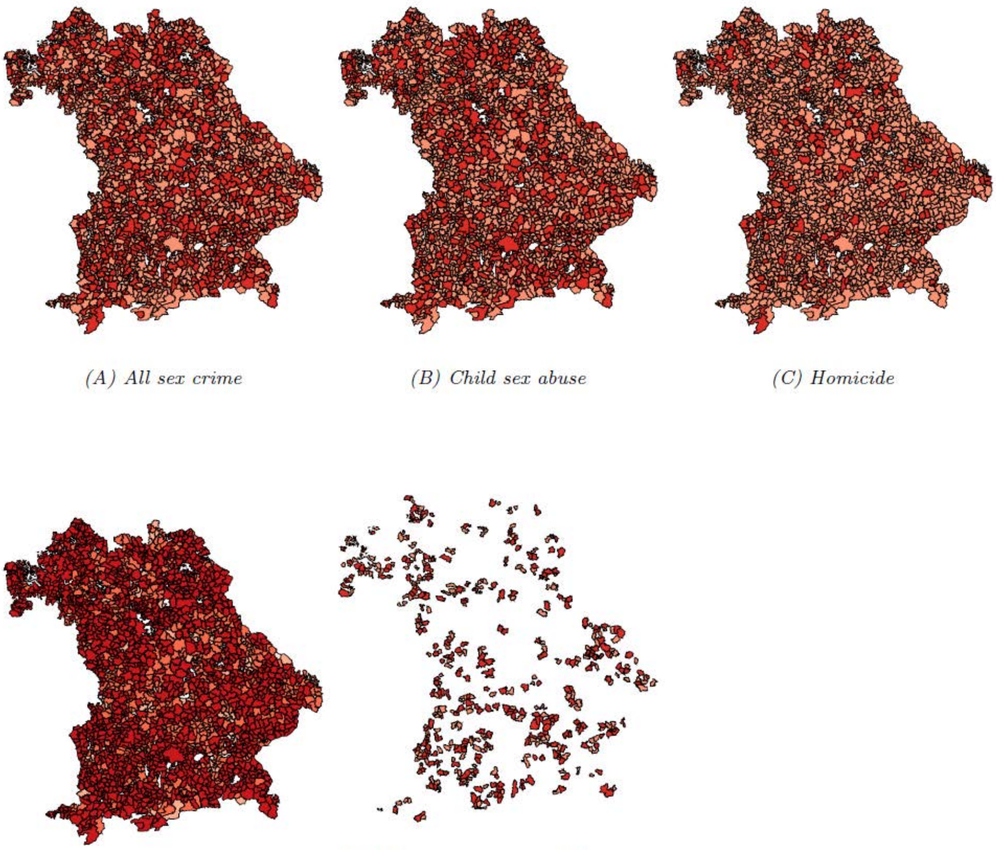


(E) DSL

(F) Treated and non-treated municipalities

Notes: Figures (A), (B), (C) and (D) plot the geographical distribution of the dependent crime variables (change in crime rate per 10,000 inhabitants between the pre-DSL and the DSL period) for Lower Saxony. Dark (light) red correspond to a positive (negative) change per 10,000 inhabitants. 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). 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 of Lower Saxony

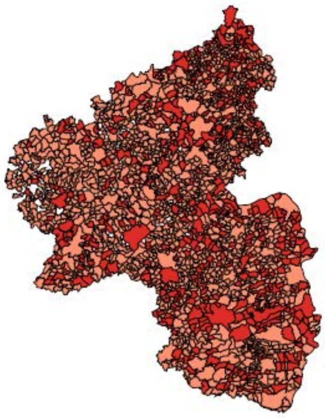


(A) All sex crime *(B) Child sex abuse* *(C) Homicide*

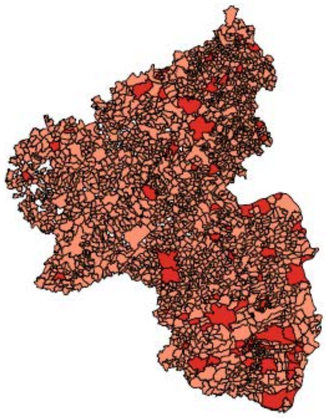
(D) DSL *(E) Treated and non-treated municipalities*

Notes: Figures (A), (B) and (C) plot the geographical distribution of the dependent crime variables (change in crime rate per 10,000 inhabitants between the pre-DSL and the DSL period) for Bavaria. Dark (light) red correspond to a positive (negative) change per 10,000 inhabitants. 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). Figure (E) 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 of Bavaria

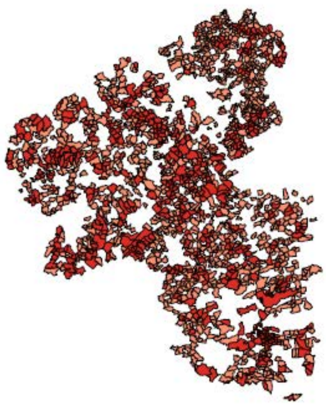


(A) All sex crime



(B) Homicide

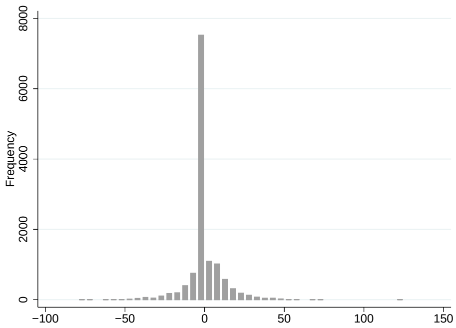
(C) DSL



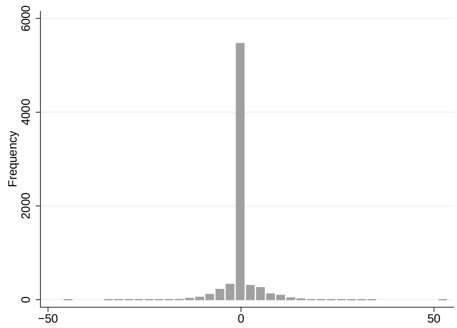
(D) Treated and non-treated municipalities

Notes: Figures (A) and (B) plot the geographical distribution of the dependent crime variables (change in crime rate per 10,000 inhabitants between the pre-DSL and the DSL period) for Rhineland-Palatinate. Dark (light) red correspond to a positive (negative) change per 10,000 inhabitants. 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). Figure (D) shows treated (dark) and non-treated (light) municipalities used in the empirical section. White areas indicate missing values.

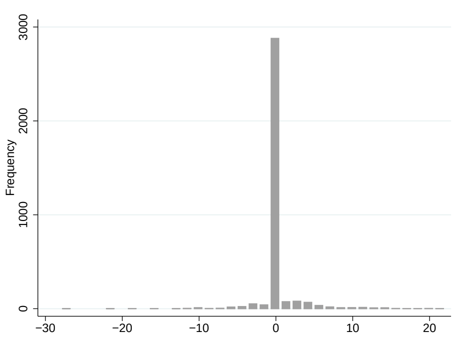
Figure B.4: Geographical distribution of crime and DSL growth rates and treated/non-treated municipalities for the Federal State of Rhineland-Palatinate



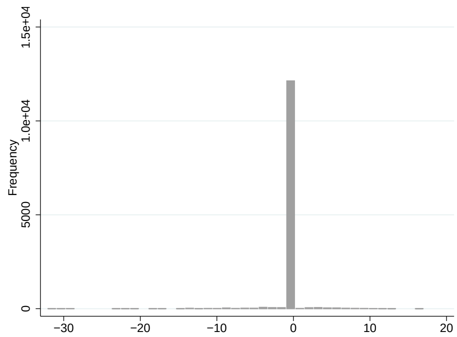
(1) All sex crime



(2) Child sex abuse



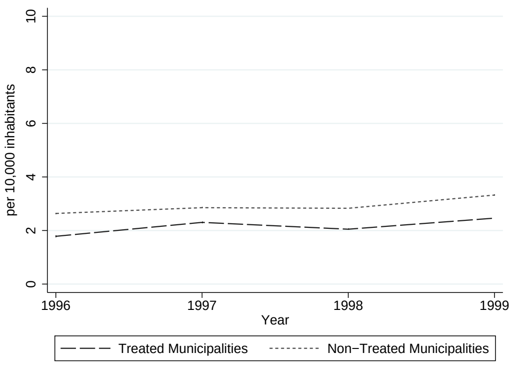
(3) Rape



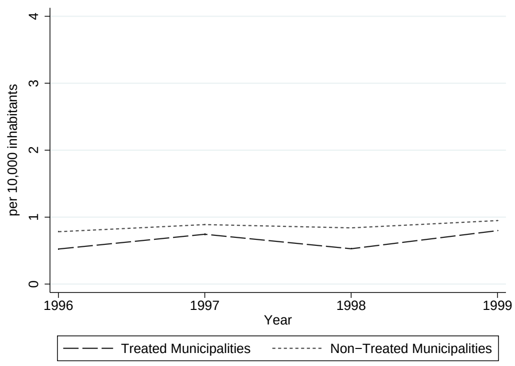
(4) Homicide

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 pre-DSL period covers the years between 1996 to 1999.

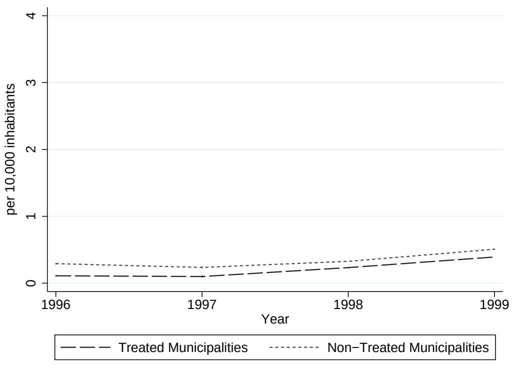
Figure B.5: Density plots among crime categories for selected municipalities in the empirical analysis



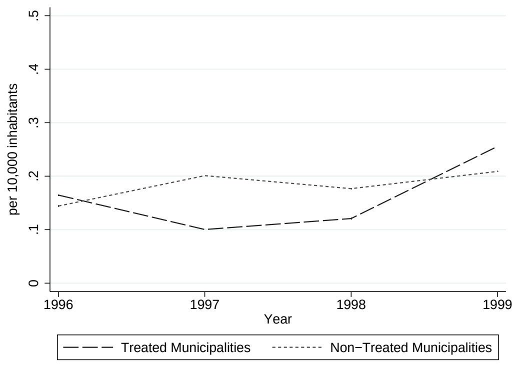
(1) All sex crime



(2) Child sex abuse



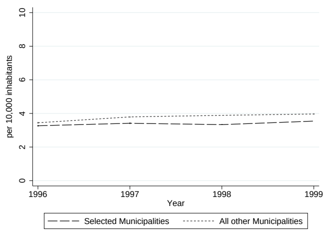
(3) Rape



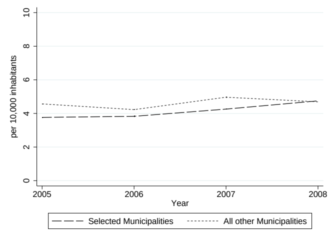
(4) Homicide

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.

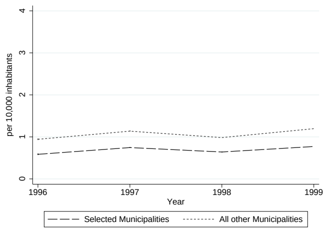
Figure B.6: Pre-DSL crime level development for treated and non-treated municipalities in the IV-sample



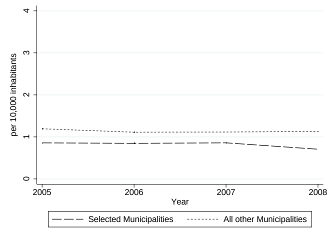
(1-A) All sex crime 1996-1999



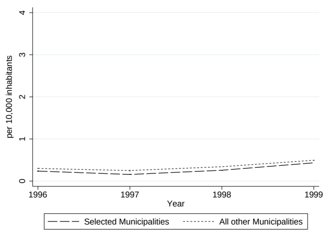
(1-B) All sex crime 2005-2008



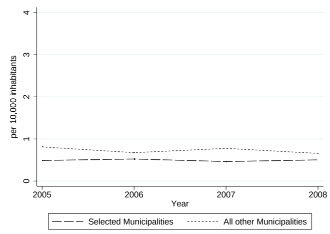
(2-A) Child sex abuse 1996-1999



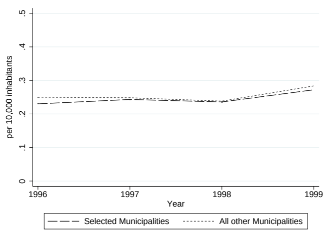
(2-B) Child sex abuse 2005-2008



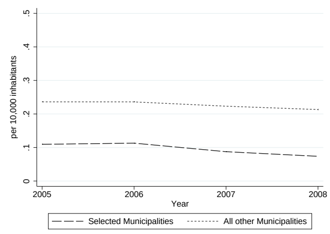
(3-A) Rape 1996-1999



(3-B) Rape 2005-2008



(4-A) Homicide 1996-1999



(4-B) Homicide 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

C Additional Econometric Results

Table C.1: Estimation results of internet availability on crime, full sample

	All sex crime		Child sex abuse	Rape	Homicide
	(1)	(2)	(3)	(4)	(5)
OLS	0.016***	0.014***	0.004*	0.001	0.0004
	(0.005)	(0.006)	(0.002)	(0.003)	(0.0003)
OLS + FD	0.004	0.004	0.0004	0.002	-0.003*
	(0.009)	(0.009)	(0.003)	(0.003)	(0.001)
IV + FD	-0.031	-0.037	-0.043*	0.009	0.011
	(0.030)	(0.031)	(0.026)	(0.038)	(0.009)
F-Statistic (first stage)	161.9	157	35.9	12.0	157.4
Observations	9,825	9,825	4,384	2,172	9,825
Number of MDFs	699	699	423	202	699
Municipalities	2,462	2,462	1,097	549	2,462
Control variables	No	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table C.2: IV + FD estimation results - robustness checks, full sample

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Population center	-0.032	-0.032	0.015	0.008
	(0.030)	(0.024)	(0.031)	(0.008)
Average crime per period	-0.036	-0.036*	0.012	0.012
	(0.032)	(0.025)	(0.037)	(0.009)
Population 500 +	-0.015	-0.054*	0.014	0.025
	(0.048)	(0.029)	(0.048)	(0.018)
Years 2005/06	-0.058*	-0.026	0.003	0.011
	(0.033)	(0.031)	(0.051)	(0.009)
Years 2007/08	-0.012	-0.071*	0.025	0.012
	(0.038)	(0.042)	(0.053)	(0.010)
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of robustness specifications for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. The DSL variable takes values between 0 and 100. Standard errors are heteroskedasticity robust and clustered at the municipality level. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table C.3: Test for overidentification

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	-0.006	-0.034**	-0.024	-0.002
	(0.020)	(0.015)	(0.033)	(0.004)
First stage coef. γ_1	-4.555***	-1.067	-1.919**	-4.555***
	(0.909)	(0.741)	(0.937)	(0.909)
First stage coef. γ_2	-29.47***	-12.81***	-7.539***	-29.47***
	(2.046)	(2.123)	(2.548)	(2.046)
Hansen J-Statistic	2.407	1.125	0.925	2.286
(p-value)	(0.120)	(0.288)	(0.336)	(0.130)

Notes: The table reports regression results and the Hansen J-Statistic with its p-value of the Chi-sq distribution in parenthesis. The test statistic is based on robust variance-covariance matrix clustered at the municipality level. The categories for the two treatment dummies are based on distance categories above the threshold distance of 4,200 meters. By setting the threshold distance equal to zero, the first treatment dummy captures the distances between 0 to 1,100 meters and the second treatment dummy all municipalities with distances above 1,100 meters.

Table C.4: Test for overidentification - full sample

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	0.009	-0.007	-0.012	-0.000
	(0.020)	(0.011)	(0.024)	(0.004)
First stage coef. γ_1	-4.681***	-0.927	-1.724*	-4.681***
	(0.898)	(0.755)	(1.023)	(0.898)
First stage coef. γ_2	-18.08***	-6.208***	-5.805**	-18.08***
	(2.038)	(1.691)	(2.419)	(2.038)
First stage coef. γ_3	-30.43***	-14.17***	-9.601**	-30.43***
	(2.557)	(2.400)	(3.907)	(2.557)
Hansen J-Statistic	5.973	7.217	1.215	2.405
(p-value)	(0.050)	(0.027)	(0.544)	(0.300)

Notes: The table reports regression results and the Hansen J-Statistic with its p-value in parenthesis of the Chi-sq distribution. The test statistic is based on robust variance-covariance matrix clustered at the municipality level. The categories for the three treatment dummies are based on distance categories above the threshold distance of 4,200 meters. By setting the threshold distance equal to zero, the first treatment dummy captures the distances between 0 to 1,100 meters and the second treatment dummy captures the distance between 1,100 to 2,000 meters and the third treatment dummy captures all municipalities with distances above 2,100 meters.

Table C.5: IV + FD estimation results - treatment intensity

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	0.009	-0.023**	-0.022	0.002
	(0.020)	(0.010)	(0.024)	(0.003)
First stage coef. γ_1	-0.019***	-0.010***	-0.006***	-0.015***
	(0.001)	(0.001)	(0.002)	(0.001)
F-Statistic	493.5	60.5	15.1	493.5

Notes: The table reports regression results and the coefficient γ_1 from equation 5 for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony using municipalities with less than 2,000 meters around the threshold. Crime rates are calculated per 10,000 inhabitants. The DSL variable takes values between 0 and 100. Standard errors are heteroskedasticity robust and clustered at the municipality level. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table C.6: Estimation results on growth rates between 1999 and 1996 - placebo test, full sample

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
treatment dummy	0.354	0.167	0.568	-0.060
	(0.614)	(0.264)	(0.386)	(0.107)
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of placebo specifications for the sample from 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. Robust standard errors in parenthesis. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table C.7: IV + FD estimation results excluding Lower-Saxony - full sample

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	-0.040	-0.066**	-0.058	0.012
	(0.032)	(0.026)	(0.052)	(0.009)
F-Statistic (first stage)	154.4	33.0	9.4	154.4
Observations	8,712	3,248	1,036	8,712
Number of MDFs	597	321	100	597
Municipalities	2,178	813	265	2,178
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for the sample from Bavaria, Baden-Wuerttemberg and Rhineland-Palatinate. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table C.8: IV + FD estimation results analyzing detection rates - full sample

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	0.033	-0.088	-0.090	-0.008
	(0.048)	(0.073)	(0.081)	(0.006)
F-Statistic (first stage)	143.2	25.8	11.8	153.4
Observations	5,387	2,681	1,660	8,934
Number of MDFs	610	363	187	674
Municipalities	2,276	975	518	2,412
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for detection rates for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Detection rates are calculated in percent. In the case of zero criminal activity in both periods, I assume a zero change between the two periods. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table C.9: IV + FD estimation results analyzing other crime rates - full sample

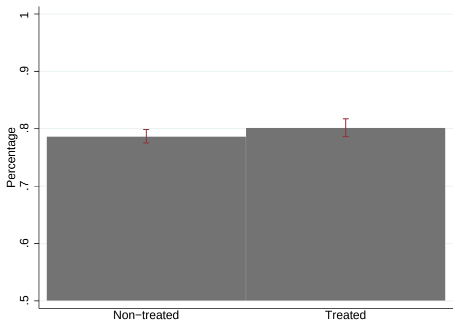
	All other crime (1)	Theft (2)	Arms-related offences (3)	Drug-related offence (4)
Δ DSL	-0.019	0.054	0.005	-0.202
	(1.284)	(0.241)	(0.114)	(0.185)
F-Statistic (first stage)	157.4	154.4	12.0	157.4
Observations	9,827	8,691	2,172	9,827
Number of MDFs	699	597	202	699
Municipalities	2,462	2,178	549	2,462
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results for all crime rate excluding sex crime and homicide, theft, extortion, and drug-related offences for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. Due to data availability restrictions, the pre-DSL crime rates for municipalities in Rhineland-Palatinate refer to the year 2002. The DSL variable takes values between 0 and 100. The instrument refers to a threshold dummy indicating whether a municipality's distance to the next MDF is above 4,200 meters. The *F*-test of excluded instruments refers to the Kleibergen-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). Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

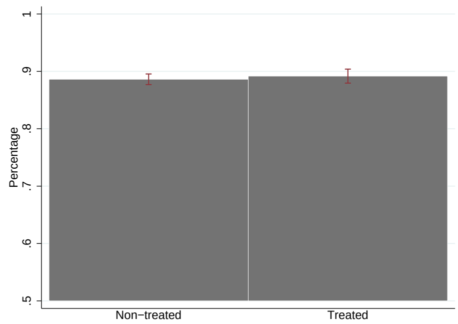
Table C.10: Estimation results of child sex abuse on illegal pornographic material

	All		2,000 meters around the threshold	
	(1)	(2)	(3)	(4)
Δ illegal porn	-0.045	-0.051*	-0.057*	-0.058**
	(0.029)	(0.027)	(0.031)	(0.029)
Municipalities	522	522	466	466
Control variables	No	Yes	No	Yes

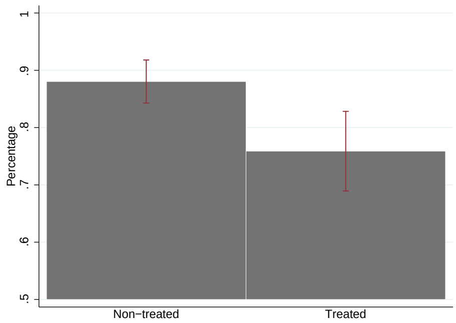
Notes: The table reports OLS regression results for the sample from Baden-Wuerttemberg and Lower Saxony. The dependent variable is the change in child sex abuse calculated per 10,000 inhabitants. The variable of interest is the change in illegal pornographic material cases. Standard errors are heteroskedasticity robust and clustered at the municipality level. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.



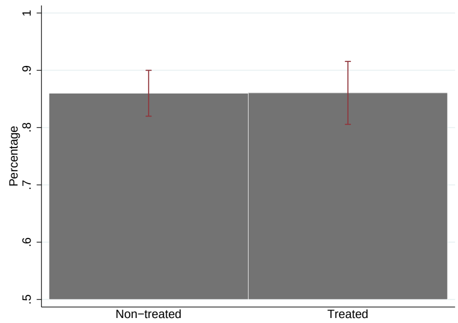
(1-A) pre-DSL period - all sex crime



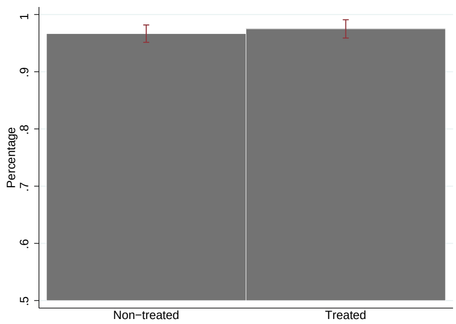
(1-B) DSL period - all sex crime



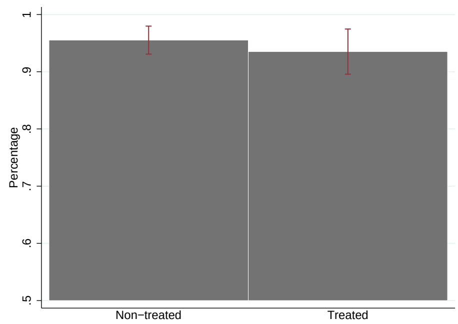
(2-A) pre-DSL period - rape



(2-B) DSL period - rape



(3-A) pre-DSL period - homicide



(3-B) DSL period - homicide

Notes: The figures plot the detection rates for all sex crime (Panel 1), rape (Panel 2) and homicide (Panel 3) for treated and non-treated municipalities with a distance of less than 2,000 meters around the threshold. Panels (A) show the detections rates for the pre-DSL period. Panels (B) show the detection rates for the DSL period. Red bars on top indicate 90% confidence intervals. In Panel (1), the p-value of a difference test in the pre-DSL (DSL) period is 0.187 (0.592). In Panel (2), the p-value of a difference test in the pre-DSL (DSL) period is 0.007 (0.987). In Panel (3), the p-value of a difference test in the pre-DSL (DSL) period is 0.549 (0.451).

Figure C.1: Detection rates by treatment and period, remaining crime categories

Table C.11: IV + FD estimation results - excluding municipalities with monasteries

	All sex crime (1)	Child sex abuse (2)	Rape (3)	Homicide (4)
Δ DSL	-0.036	-0.060**	0.011	0.012
	(0.031)	(0.029)	(0.042)	(0.009)
F-Statistic (first stage)	154.7	30.2	10.3	154.7
Observations	9,135	3,816	1,972	9,135
Number of MDFs	644	370	184	644
Municipalities	2,289	955	499	2,289
Control variables	Yes	Yes	Yes	Yes

Notes: The table reports regression results of robustness specifications without municipalities where a monastery is located for the sample from Bavaria, Baden-Wuerttemberg, Rhineland-Palatinate and Lower Saxony. Crime rates are calculated per 10,000 inhabitants. The DSL variable takes values between 0 and 100. Standard errors are heteroskedasticity robust and clustered at the municipality level. Control variables are: age structure, unemployment rate, net migration rate, skill level, share of females and foreigners in four age-groups, real daily wage level, police density, occupational and industry structure, firm density, firm entry and exit, total sales and public program participation rates. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.