

Internet Pornography: Catharsis or Catalyst for Sexual Crime?

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Abstract

Does pornography trigger sex crime? We use Norwegian registry data on crime and internet adoption to shed new light on this question. A public program with limited funding rolled out broadband access points in 2000–2008, and provides plausibly exogenous variation in internet use. Our 2SLS estimates suggest that internet usage is associated with a substantial increase in reported incidences of rape and other sex crimes. We present a conceptual framework that highlights three mechanisms for how internet use may affect reported sex crime, namely a reporting effect, a matching effect on potential offenders and victims, and a direct effect on crime propensity. Results indicate that the direct effect dominates, plausibly as a result of increased consumption of pornography.

Keywords: Pornography, rape, sex crimes, internet, broadband, instrumental variables

JEL codes: J13, H40, I28, D31

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1 Introduction

The debate about the adverse effects of pornography is an old one. In particular, the impact on sex related crimes such as rape and child sex abuse have been a subject of fierce controversy. The rapid growth of the internet with its vast content of extreme sexual content at our finger tips, has triggered renewed interest in these issues among policymakers and researchers alike.

There are several theories that relate the consumption of pornographic material to sexual crimes. One theory argues that pornography increases the likelihood of sex crimes because it triggers sexual arousal and aggression, degrades women or children to objects, and affects social and individual norms. A second, opposing, theory highlights the potential cathartic effects of pornography consumption, because it leads to sexual relief thereby potentially offsetting sexual aggression. While the former hypothesis was dismissed by the US presidential Johnson Committee report in 1970, the Reagan-appointed Meese Commission concluded 16 years later that “*substantial exposure to sexually violent materials as described here bears a causal relationship to antisocial acts of sexual violence*” (Donnerstein et al., 1987).

There is little causal evidence that supports these theories and – as discussed in detail below – much of what we know about the relationship between pornography and sex crime is either circumstantial or anecdotal. The most systematic evidence comes from psychology, where much effort has been devoted to laboratory experiments that study how subjects (typically male college students) respond to the exposure to pornography. Although the vast majority of these laboratory experiments seem to support a positive effect of pornography on sexual aggression (Hald et al., 2010), it is not clear whether this actually translates into higher crime rates in the field.¹

In this paper, we use a unique Norwegian data set to examine the relationship between internet usage, pornography, and sex crime. Any observational study that directly relates observed consumption, whether it is of internet or pornography, to sex crime will need to address whether the effects run from consumption to crime or vice versa, and whether some omitted factor may be causing both crime and consumption. It is therefore important to rely on plausibly exogenous supply shocks for identification. To achieve this, we exploit a public program introduced by the Norwegian government in the late 1990s aimed at ensuring broadband internet access at a reasonable price throughout the country. Because of limited funding, access to broadband internet was progressively rolled out, so that the necessary infrastructure (access points) was established in different municipalities at different times.

¹See for example the related discussion in Dahl and Dellavigna (2009) in the context of movie violence, but also Levitt and List (2007a,b) more broadly on lab experiments.

We use this spatial and temporal variation in the availability of broadband internet across municipalities in an instrumental variables setup. Specifically, we instrument the fraction of households with an internet subscription, using the fraction of households that are covered by broadband internet. We show that our instrument is unrelated to municipality characteristics such as the poverty rate, immigrant population share, baseline sex crime rate, and police density.

Our 2SLS estimates show that internet usage is associated with a substantial increase in rape incidence and other types of sex crime. These estimated impacts capture the net relationship between internet usage and sex related crime which are of interest in and of itself. To further interpret the findings, we present a simple conceptual framework that highlights three mechanisms through which internet usage may affect reported sex crime: a reporting effect, a matching effect of potential sex offenders and victims, and a direct effect on the propensity for crime itself.

Internet usage might increase reported crime, without necessarily causing an increase in actual crime rates. Although it is not possible to report crime to the police on the internet, internet usage may induce people to report crimes that they would otherwise not have reported. We find no evidence of changes in reported crime relative to convictions, which would be expected if marginal reports are less strong, as seems reasonable. Nor do we find changes in the time elapsed between the crime and the report.

Next, we try to distinguish between the two other main channels through which internet usage may affect sex crime. First, there may be a matching effect where the positive relationship between internet usage and sex crime reflects that it is easier for sex offenders to meet their potential victims via chat rooms than through alternative activities. Second, internet usage can have a direct effect on sex crime propensity by exposing more people to sexually violent material. Broadband internet creates fast access to sexually explicit material, especially movie clips and live video feeds. High-speed internet also offers accessibility, affordability and anonymity to its users and, in the case of Norway, implied an abrupt slackening in the constraints on supply of sexually explicit material which had been banned for decades.

Our results suggest that the direct effect dominates. First of all, we find no effect on other types of crime, where internet usage might have an indirect effect through displacing alternative activities but most likely little if any direct effect. Second, we exploit that the direct effect of internet usage is likely to vary systematically with access to non-internet pornography. Consistent with this hypothesis, we find weaker effect of internet usage on sex crime in border areas where historically pornography was more easily available.

This evidence suggests that the positive effect of internet usage on sex crimes and rape is driven by a direct effect of increased exposure to pornographic material. This is consistent with many previous laboratory studies which support that pornography and sex crime are complements, and is in line with the well-known feminist slogan: “*Pornography is the theory, and rape is the practice*” (Morgan, 1978).

To support the validity of our results, we pay close attention to the possibility that people living in areas with better access to broadband internet are inherently different, and could be more inclined to commit sex related crime to start out with. First of all, our fixed effects specification controls for permanent differences in crime rates between municipalities, as well as for common time-specific changes in crime rates, such as a secular nationwide trend. Our estimates are also robust to the inclusion and exclusion of a large set of controls.

We also run several specification checks. In particular, our results are supported by the fact that we find no effects of two different placebo-tests. In the first placebo-test, we pretend that the internet expansion took place in the period before the actual roll-out. In the second placebo-test, we regress sex crime rate in a given year on internet usage in a subsequent year. If our instrument is correlated with unobserved time-varying municipality characteristics that affect sex crimes, we would expect significant effects of internet usage in the placebo-tests. Nevertheless, to allow the municipalities to follow different secular trends, we add municipality-specific (linear and quadratic) time trends. It’s heartening to find that our estimates are very similar when including these trends. In the spirit of Duflo (2001), we also show that the positive effects of internet usage on sex crime are robust to interacting the year fixed effects with baseline municipality characteristics. To ensure that our results are not driven by secular changes between urban and rural areas coinciding with the internet expansion, we drop the five big cities from our analysis; the estimates barely move. We also take several steps to make sure that our results are not driven by outlier observations.

Our research is related to a growing literature in economics about the effects of the internet.² Closely related to the our paper is the study by Kendall (2007). Using US state-level panel data from 1998–2003, he regresses the fraction of households with an internet subscription on rape incidences. In contrast to our findings, Kendall reports a negative association between internet use and rape. Our study differs from Kendall (2007) in a number of respects. First of all, the institutional settings differ in important ways. Unlike the US, Norway has a homogeneous population with a

²According to Freund and Weinhold (2004) and Choi (2003), the internet has a positive effect on bilateral trade and foreign direct investment, respectively. Yi and Choi (2005) and Choi and Hoon Yi (2009) found that the Internet lowers the inflation rate and increases economic growth.

unified legal system and law enforcement practices across the country. Furthermore, Norway has a strict equalization program in the grant system for local governments, resulting in very similar spending (per capita) on public services, such as police. These features of the Norwegian setting make it possible to arrive at estimates that are potentially less biased than those presented in Kendall (2007). Second, our identification strategy differs from that used by Kendall. While he assumes that internet usage is uncorrelated with unobserved time-varying determinants of crime, we relax this assumption by exploiting plausibly exogenous supply shocks to instrument for internet usage. It should finally be noted that the arrival of the internet most likely implied a much stronger shock to the availability of pornography in Norway compared to the US. While pornography was de facto legalized and readily available in most of the US, a legal ban on pornography was in place in Norway. Access to pornography was therefore severely limited in Norway before the arrival of the internet, and was largely restricted to smuggling across the border.

The remainder of the paper is structured as follows. Section 2 discusses previous research on pornography and sex related crimes, and highlights the role of the internet in the consumption of pornography. Section 3 describes our data, before Section 4 describes the expansion of broadband internet, and Section 5 discusses our empirical strategy. Section 6 reports our main findings as well as the results from a number of robustness checks, while section 7 outlines a conceptual framework to interpret the estimates, and uses it to investigate potential mechanisms. We conclude our paper with a discussion of our findings in relation to the policy debate on censorship of pornography in general, and internet pornography in particular.

2 Background information

Pornography and rape

An extensive theoretical literature postulates a causal relationship between exposure to pornography and sexually related crimes.

On the one hand, pornography can be a complement to rape, sexual harassment and sexual assault for a variety of reasons; It may create arousal that is not completely relieved (Posner, 1992), or sexual excitement that spills over into preexisting aggression, so-called excitation transfer, in which case pornography reinforces existing aggression (Zillman, 1971). Pornography may also instill beliefs that women are more promiscuous, thereby decrease the perceived cost of sexual advances while increasing the expected benefits (Dworkin, 1981). Moreover, pornography may convey misogynistic beliefs, condoning or even encouraging rape (see e.g. Mackinnon, 1995).

Pornography can also be a substitute for sexual crimes if it relieves sexual arousal, either directly or as a complement to masturbation (Posner, 1992), deflecting men from seeking potentially criminal outlets. Exposure to sexually arousing media could also shift the attention of the viewer, causing aggression to decrease (Donnerstein et al., 1975). Consumption of pornography may therefore have a cathartic effect, reducing sexual crime rates in societies with easy access to pornography.

Since the 1960s, a large body of research has been devoted to investigating the relationship between exposure to pornography and sexual aggression against women, yet no real consensus has been reached. In the field, law enforcement officers point to the pervasive use of pornography among sexual offenders to support a link between the two.³ In observational studies, sexual crime rates are found to have fallen or been stable as pornography has become rampant, suggesting no detrimental effect (Kutchinsky, 1991; Posner, 1992; Mocan and Bali, 2005), whereas correlations across US states suggest a positive association between consumption of pornography and sex crime rates (Baron and Straus, 1984).⁴

Most pervasively, both sides of the debate on pornography and rape have been fed by a large literature on the laboratory effects of exposure to pornography.⁵ Several laboratory experiments point towards increased sexual aggression among subjects exposed to pornography; Erectile responses to viewing non-consensual sex are stronger among convicted rapists (Abel et al., 1977; Barbaree et al., 1979; Quinsey et al., 1981), and sexual arousal to simulated rapes grows with repeated exposure (Marshall et al., 1991). Some studies have shown that pre-existing aggression can intensify after viewing pornography (e.g. Zillman, 1971), in particular violent pornography (Donnerstein and L., 1981; Donnerstein et al., 1987), while misogynistic beliefs and acceptance of rape myths gain a firmer foothold (Malamuth and Check, 1981; Zillman and Bryant, 1984).

Some laboratory experiments also point in the opposite direction, suggesting no effect of exposure to pornography, or, in some cases, even a reduction in sexual aggression among subjects; Zillman and Bryant (1984) report less aggression among exposed subjects after viewing 36 sexual films over a 6 week period than among control

³Gebert (2003), for instance, reports several cases in which he draws connections between rape and consumption of pornography. Similarly, a study of 100 survivors at a rape crisis center revealed that 28 percent reported that their abuser used pornography and that pornography was imitated during the abusive incident for 12 percent of the women (Bergen and Bogle, 2000).

⁴Further, Winick and Evans (1996) study a temporary freeze on pornography prosecutions in four US states, finding no increase in the number of arrests for rape, prostitution and sex crimes compared to other US states, while Wongsurawat (2006) finds lower rape rates with higher subscription rates to Penthouse Magazine, using availability of post office boxes as an instrument.

⁵For an indepth survey of the psychological literature, see Donnerstein et al. (1987), also Fisher and Barak (2001); Manning (2005); Diamond (2009); Hald et al. (2010).

subjects who had seen only non-arousing films (see also Donnerstein et al., 1975). In the same vein, Linz (1985) finds no effect on misogynistic attitudes or acceptance of rape myths after a massive exposure to feature-length hard-core pornographic films.

Even if the vast majority of laboratory experiments seem to point to a positive effect of pornography on sexual aggression (Hald et al., 2010), in particular when it concerns sexually violent material (Donnerstein et al., 1987), evidence from such studies has some important limitations when applied in the field. First, lab subjects, even when not self-selected, are most often a select group linked to universities, or at best chosen to be representative of the entire population. If users of pornography are a selected group, potentially underrepresented at universities, then the overall average effect of exposure to pornography among lab subjects may not give a good indication of the effect of increased access. Second, consuming pornography in a controlled and supervised setting such as in a laboratory, is likely to be quite different from consuming it in more private settings. And third, responses in terms of inflating a blood pressure cushion or administering electric shocks may not be good proxies for actual sexually charged criminal activity.

In summary, as emphasized by Posner (1992, p. 367): “*Given these conflicting tugs, the question whether the net effect of pornography is to increase the incidence of rape, and if so significantly, is an empirical one.*” In the current paper, we aim to provide field evidence on the causal effects on sexual crimes of increased access to pornography through internet usage.

Pornography and the internet

Distributors of pornography are among the earliest and most innovative entrepreneurs on the internet, and adult entertainment has a large presence on the internet. In 1998, about 20–30,000 pornographic sites were available online, and revenues in the late 1990s already stood at 700 million USD (Stack et al., 2004). By 2006, the US online adult entertainment industry controlled about 12% of all internet sites (Ropelato, 2006), and pocketed 2.8 billion USD in revenue, putting it in second place as the highest-grossing sector in adult entertainment, rivaling video sales and rentals for first place, and in continuing strong growth (Edelmann, 2009).⁶ Moreover pornographic materials abound on peer-to-peer internet networks, where e.g. for a period 73% of all movie searches were for pornographic films on the once dominant downloading engine Kazaa (Kendall, 2007).

Internet pornography has quickly become such an important channel of adult

⁶For an indepth look at consumption patterns of adult entertainment in the US, see Edelmann (2009).

entertainment due to its *'Triple-A Engine' effect of Accessibility, Affordability, and Anonymity* (Manning, 2005). While there are social barriers of entry to bookstores and movie theatres, anonymous access to internet pornography from home significantly reduces the threat of social deterrents. The arrival of broadband internet also meant that distributors could offer explicit imaging of all kinds without excessive waiting times, in particular for movies. As an example, a quality 5-minute video clip of 35MB would require about 1.5 hours to load on a 56Kbps dial-up line, compared to just 5 minutes on a 1Mbps DSL broadband connection, and just seconds on high-speed broadband connections. Accordingly, Edelman (2009) reports that “[a]s of June 2008, broadband users outnumber narrowband users 18 to 1 at sites that comScore (2008) classifies as adult”.

In 2008, 36% of internet users visited at least one adult website per month, on average almost 8 sites per month (Edelman, 2009). Meanwhile, 69% of men and 20% of women in a Swedish survey reported that they regularly viewed erotic pictures and/or films on the internet (Cooper et al., 2003). In a survey of students in New England, Sabina et al. (2008) found that 93% of men and 62% of women had been exposed to pornography before turning 18 years old, while only 3% and 17% had never been exposed to pornography at all.

An important characteristic of internet pornography consumption is ready access to extreme and deviant forms of pornography. Sabina et al. (2008) found that over 30% of surveyed students had seen depictions of sex between people and animals, 20% portrayals of rape or sexual violence, and 15% sexual pictures of children. Indeed, the willingness to view extreme and deviant forms of pornography seems to be non-negligible: Even in the at least semi-public setting of an experiment, Fisher and Barak (2001) report that 8%, 4%, and 3% of subjects chose to view sexual movies including animals, violence and children, respectively, compared to a combined 22% who chose to view nonviolent female sexual insatiability or common sexual acts, and only 8% who chose to view a control film (*Saturday Night Live*).⁷

3 Data

Crime data

Our crime data come from administrative police registers containing complete records of (i) reported crime for each municipality over the period 1993–2008, and (ii) individual criminal charges for every resident over the period 1993–2004. The dataset

⁷Note that the remaining 55% chose not to view anything, so the share of subjects viewing extreme pornography is more than doubled when considering only those who chose to view a film in the first place.

contains all serious crimes, but also misdemeanors like drunk driving, excessive speeding or shop lifting. A “reported crime” is defined as a crime reported to or recorded by the police. A person is registered as “charged” if, at the end of the investigation, they are considered to be the perpetrator of a reported crime by police and prosecutor (independent of potential indictments, prosecutions or convictions). Statistics Norway has constructed sub-categories of crime and we rely on these definitions to construct crime categories that correspond to those used by the US FBI (see Appendix A, Table A1).⁸ An advantage of our data, is that it excludes reports that are dismissed by police and prosecutor, because they are likely to be fraudulent or concern acts that are not criminal.⁹

A problem for any empirical study of crime is the difficulty in measuring criminal activity. Typically, measures are constructed from either self-reported survey data or from registered crimes. Self-reports of criminal activity should be interpreted cautiously since they are often impossible to validate and since there are incentives to misreport (MacDonald, 2002; Kirk, 2006). In particular, truthful self-reporting is less likely among subjects with an extensive criminal record compared to subjects with little or no criminal history (Hinderlang et al., 1981). A key advantage of our register data is that “reported crimes” are cleanly identified. Moreover, the register data have the advantage that offenders cannot choose not to be registered, unlike voluntary surveys where they may decline to participate.

A disadvantage of register data is that it excludes crimes which are not reported or not recorded by the police. Moreover, criminal charges require that the police identifies the offender. In line with previous studies, our main analysis focuses on reported crime rather than charges. This is in part because we do not have information on criminal charges after 2004, but also because the evidential burden is often difficult in these cases; which is mirrored in the fact that only in a quarter of cases are charges brought. Also, rape offenders are unidentified in about a tenth of cases even after the investigation (Stene, 2001). It should be noted that we get qualitatively the same and quantitatively quite similar results when looking at criminal charges instead of reported crime; the former estimates are, however, less precise (see Table A3 in the Appendix).

⁸Throughout this paper, our definition of sex crimes excludes crimes related to the distribution of pornography. Results are unchanged when these are included in the definition of sex crimes.

⁹In 1997, this concerned about 9% of reports on rape and other sexual activity, and about 7% on child sexual abuse (Stene, 2001).

Internet data

Our internet data contain complete records of the fraction of households that subscribe to or are covered by broadband internet, in every municipality over the period 2000–2008. Throughout this paper, broadband is defined as internet connections with download speed faster than 128 kbit/s.

The data on broadband coverage come from the Norwegian Ministry of Government Administration. The ministry monitors the coverage and usage of broadband internet, and the suppliers of broadband access to end-users are therefore required to file annual reports about their coverage rates to the Norwegian Telecommunications Authority. The coverage rates are based on information on the area signal range of the local access points and detailed information on the place of residence of households. In computing the coverage rates on the municipality level, it is taken into account that multiple suppliers may provide broadband access to households living in the same area, so that we avoid double counting.

For the period 2002–2008, the data on broadband subscriptions are from the quarterly Internet Survey performed by Statistics Norway, surveying all suppliers of broadband access to end-users. The survey contains information on the aggregate numbers of households with broadband subscriptions in each municipality. For the years 2000 and 2001, our data on broadband subscriptions come from the state-owned enterprise Telenor, which was the sole provider of broadband internet during this period.

We also have detailed information on media usage for a representative sample of individuals at age 9–79, provided by Statistics Norway’s Media User Survey. Each year, around 2700 individuals are asked detailed questions about their media usage, with a response rate above 70 percent. Importantly, the survey contains information on respondents age, sex, education, labor market status and some household characteristics, besides their internet usage and connection speed. This survey data is available for the period 2000–2008.

Sociodemographic data

Our sociodemographic data come from administrative registers provided by Statistics Norway. Specifically, we use a rich longitudinal database which covers every resident from 1993 to 2008. It contains individual demographic information (sex, age, immigrant status, country of origin, marital status, number of children), socio-economic data (years of education, income, employment status), and geographic identifiers for municipality of residence. The information on educational attainment is based on annual reports from Norwegian educational establishments, whereas the income

data and employment data are collected from tax records and other administrative registers. The household information is from the Central Population Register, which is updated annually by the local population registries and verified by the Norwegian Tax Authority. The coverage and reliability of Norwegian register data is considered to be exceptional, as illustrated by the fact that they received the highest rating in a data quality assessment conducted by Atkinson et al. (1995).

Summary statistics

In our main analysis, we use municipality-level data on crime rates, internet usage and coverage rates, and other sociodemographic variables expressed in per capita terms. We focus on the years 2000–2008, when broadband internet coverage went from virtually zero to almost 100 percent. Table 1 displays summary statistics for the key variables. Detailed descriptions of each of the variables are given in Appendix A, Table A1.

We use three different outcome variables, namely reported overall sex crime, reported rape and reported child sex abuse. We define the outcome variables in terms of crime rates per 100,000 inhabitants. The first panel of Table 1 displays the mean of crime rates across municipalities, with standard deviations in parentheses. Rape and child sex abuse are the two main categories of sex crimes, making up nearly 25 and 40 percent of all sex crimes, respectively. The remaining categories are e.g. procurement, prostitution and public exposé. It is also evident that the rate of sex crime is fairly stable until the year 2005, after which it declines slightly, largely driven by a decrease in child sex abuse.

The second panel of Table 2 shows means and standard deviations of internet coverage and usage rates, defined as shares of households in a given municipality at the beginning of each year. We can see that there is virtually no broadband usage nor coverage in 2000; However, all households with a telephone connection would have dial-up access to internet, but limited to a bitrate of less than 56 kbit/s. In 2008, mean broadband coverage is as high as 98 percent, whereas the mean user rate reaches almost 54 percent. We find the largest variation in coverage rates across municipalities around 2004, while the user rates vary the most during the last three years (see also Section 4 below).

The third panel of Table 1 displays a number of other sociodemographic variables, which are defined in Table A1 in Appendix A. Local unemployment, poverty, urban settlement (centrality) and immigrant population are all defined in per capita terms. Most of these variables are shown to be fairly stable over the period. There is, however, an increase in the immigrant population share during the period. It is also

evident that unemployment in Norway remained very low compared to most other European countries. We see that average years of education and police density were stable over the period, whereas the overall crime rate declined somewhat. It should finally be noted that the estimations below will also control for a number of other control variables, including population shares by age-group and gender, as well as the population shares of immigrants by age, gender, country of origin, and refugee status (see Appendix A, Table A1). For brevity, we omit summary statistics for these variables.

4 Expansion of broadband internet

During the 1990s, many OECD countries were planning the expansion of services related to information and communications technology (ICT). The new technology was seen as essential for retaining competitiveness and achieving high standards of living in a global economy. In particular, broadband internet was believed to reduce job-search costs, increase productivity in private enterprises and enable efficient provision of public services. In Norway, this manifested itself in the National Broadband Policy that was introduced by the Norwegian Parliament in May 1998 (St.meld.nr.38 (1997–1998)). There were two main goals: The first was to ensure that every household and private enterprise throughout the country had access to broadband at a reasonable and uniform price. The second was to ensure that the public sector quickly adopted broadband internet.

The federal government took several steps to reach these goals. First and foremost, it invested heavily in the necessary infrastructure. The investment in infrastructure was largely channeled through the state-owned telecom company Telenor, which was the sole supplier of broadband access to end-users in the early 2000s and continues to be the main supplier today. Moreover, virtually all broadband infrastructure was, and still is, owned and operated by Telenor.

Second, local governments were required to ensure access to broadband internet by 2005 to local public institutions, such as administrations, schools, and hospitals (St.meld.nr 49 (2002–2003)). To assist municipalities in rural areas, the federal government provided financial support through a funding program known as *Høykom*, which was initiated in 1999. Local governments could receive funds from this program by submitting a project plan that had to be evaluated by a program board with expert evaluations. While the criteria determining selection are somewhat unclear, the board stated that it did want to ensure broadband coverage throughout the country. Once approved, financial support was provided in the initial years of broadband

access, thus making it possible for public institutions to cover relatively high initial costs (Hansteen, 2005; Statskonsult, 2007).¹⁰

The transmission of broadband signals through fiber optic cables required installation of local access points. Over the period 2000–2008, such access points were progressively rolled out, generating considerable spatial and temporal variation in broadband coverage. The staged implementation of broadband was in part due to limited public funding, but also because Norway is a large and sparsely populated country. There are often long driving distances between the populated areas, which are mostly far apart or partitioned by mountains or the fjord-gashed shoreline.¹¹

Figure 2a displays the overall mean broadband coverage rate and the distribution of broadband coverage rates across municipalities at the start of each year between 2000 and 2008. There is considerable variation, both across municipalities and over time. In particular, the requirement of broadband internet access to public institution seems to have spilled over into increased coverage among households. This is mirrored by an increase in the average coverage rate from around 25 percent in 2003 to more than 85 percent by 2006.

By December 2000, broadband transmission centrals were installed in the cities of Oslo, Stavanger, Trondheim and Tromsø, as well as in a few neighboring municipalities of Oslo and Trondheim; however, less than one-third of the households were covered by a broadband service provider in each of these municipalities. Figure 1 shows geographic differences in the broadband coverage rates across municipalities between 2003 and 2006. The maps illustrate that for a large number of municipalities there was no broadband coverage at all before 2004, whereas most municipalities had achieved fairly high coverage rates after 2006. Moreover, there is considerable variation in coverage rates within the municipalities in these years. We find the largest dispersion in coverage rates across municipalities between 2004 and 2005. While almost complete broadband coverage was reached by 2008, more than 50 percent of households in nearly all municipalities were covered by a broadband provider two years earlier.

Figure 2b displays the distribution and averages of broadband user rates across

¹⁰During the period 1999–2005, the Høykom programme received more than 1000 such applications and co-funded nearly 400 projects, allocating a total of 50 million Euros (NOK 400 million), provided initially by the Ministry of Trade and Industry. From 2002, the Ministry of Education and Research co-financed another scheme *Høykom skole* dedicated to the education segment within the scope of the earlier program, as it opened for financial support for broadband infrastructure in public schools. There are virtually no private schools in Norway.

¹¹The Norwegian territory covers about 149,400 square miles, equivalent to the area of the US states of Delaware, District of Columbia, Pennsylvania, Maryland, South Carolina, Virginia and West Virginia combined. The country is dominated by mountainous or high terrain, as well as a rugged coastline stretching about 1,650 miles, broken by numerous fjords and thousands of island (making the coastline approximately 10 times longer if the length of the fjords were included).

municipalities between 2000 and 2008. Broadband usage increases over time, with most of the increase taking place after 2003, and reaching an average user rate around 55 percent by 2008. We find that there was a substantial increase in broadband usage after the initial expansion of broadband coverage. Moreover, there was considerable increase in broadband usage also after 2006, even though there was less of an increase in broadband coverage in these later years.

Figure 3 plots average broadband user rates from 2000 to 2008 according to gender and age group. Broadband usage increases during this period for each group, although we find considerable heterogeneity across age groups. More than 60 percent of the respondents below age 45 use broadband internet by 2005, whereas this is true for only around 40 percent of those aged 45–66 and about 10 percent of those aged 67–79. From 2005 to 2008, we find that average usage among the youngest age groups flattens out around 80 percent. However, broadband usage continues to increase up to 70 percent for those aged 45–66, and increases by a factor of roughly three for those aged 67–79. Moreover, in all age groups and almost all years, we find that male respondents report higher user rates than female respondents.

5 Empirical strategy

We employ two approaches to estimate the net effect of internet usage on sex crime. Our first approach is a fixed effects specification, controlling for permanent differences in crime rates between municipalities, as well as for common time-specific changes in crime rates, such as a secular nationwide trend. Our second approach implements two-stage least squares (2SLS), using the spatial and temporal variation in the availability of broadband internet across municipalities to instrument for internet usage.

Our empirical approaches are summarized by the following two equations.

$$c_{kt}^* = \delta i_{kt} + x'_{kt} \beta + \alpha_k + \tau_t + \epsilon_{kt} \quad (1)$$

$$i_{kt} = \phi z_{kt-1} + x'_{kt} \lambda + \gamma_k + \theta_t + \eta_{kt} \quad (2)$$

where c^* is the reported sex crime rate per 100,000 inhabitants, i is the fraction of households with broadband internet subscriptions (at the beginning of the year), z is the fraction of households with access to broadband internet (at the beginning of the year), and x is a set of time-varying covariates (measured at the municipality level in per capita terms). In all cases, the subscript k denotes municipality and the subscript t denotes year. Both equations include a full set of municipality indicators and year indicators. The standard errors are always clustered at the municipality level and

robust to heteroskedasticity. The fixed effects approach is given by Equation (1), and can be interpreted as giving a difference-in-differences estimate of the effect of internet usage on sex crime. In the 2SLS approach, equation (2) is the first stage and equation (1) is the second stage. In our main analysis, the sample consists of all municipalities over the period 2000–2008.

In both approaches, we report results with and without a large a set of time-varying sociodemographic controls, to check that the estimated effect is not driven by time-varying observable factors. For example, younger individuals are more likely to use internet and are also more often involved in a sex crime. If some municipalities experience growth in the number of young inhabitants, then this could increase both internet usage and sex crimes, which would bias the fixed effects estimates. We therefore include detailed controls for demographic characteristics. The control variables are listed in Table A1, and includes population shares by age group, gender, immigrant status, centrality of residence, education level, income level, poverty rate, and unemployment rate. Another concern is that public funding to broadband infrastructure across municipalities might be correlated with the amount of resources allocated to the police, which could bias our estimates. We therefore include the number of police officers per capita as a control variable. Finally, there may be some underlying trend in crime that is correlated with the expansion of broadband internet. To address this concern, we also add the total crime rate (excluding sex crimes) as a control variable.

The fixed effects specification resembles closely the approach of Kendall (2007). If unobservable determinants of crime are fixed at the municipality level, then these will be controlled for through the municipality indicators α_k , just like common time shocks are absorbed by the year indicators τ_t . The identifying assumption is that the sex crime rates in different municipalities follow a common underlying trend, in the absence of broadband internet usage. Several features of the Norwegian setting make it possible to arrive at fixed effects estimates that are potentially less biased than those presented in Kendall (2007). Unlike the US, Norway has a homogenous population spread out over more than 400 geographically distinct municipalities. Importantly, the municipalities are subject to a common institutional and judicial infrastructure, and they have very similar spending on local public services.¹² The legal system and law, including criminal law, are determined nationally, and the

¹²There are strict federal provisions for minimum standards of different local public services, and considerable ear-marked grants-in-aid from the central government. The federal government also determines the tax rate (except for an optional residential tax capped at one percent) and the tax base of the income tax. Also, the federal government uses equalization transfers to redistribute income from rich to poor municipalities, such that local differences in revenues are largely offset (Løken, 2009).

27 police districts and 67 district courts do not in general share borders with a municipality. Budgetary, administrative and strategic responsibility for the police and the courts rest nationally with the Police Directorate, the National Courts Administration, and the Ministry of Justice and Police.

Although these institutional features increase the confidence in our fixed effects estimates, the assumption of a common underlying time trend in sex crime across municipalities might be too strong. On the one hand, individuals who are more likely to commit a sex crime may also be more likely to use the internet, because they want to consume internet pornography. This will generate upward bias in the fixed effects estimates. On the other hand, potential sex offenders may be more likely to use internet to consume pornography for sexual relief in areas where there are fewer potential victims or more strict policing. This may lead to a downward bias in the fixed effects estimates. These examples illustrate that the fixed effects estimates may suffer from omitted variables bias, but also that the direction of the bias is unclear.

Although our vector of controls x should take into account some of the confounding factors, it is far from clear that it is sufficient. While we can only control for observable demand side characteristics, it is widely documented that observables explain only a small portion of the observed variation in crime (Glaeser et al., 1996). Randomizing internet use is obviously not feasible in our application: We cannot in practice force internet use onto people. One can, however, think of a social experiment which randomizes internet access at the municipality level. The randomization breaks the correlation between access and unobserved determinants of crime. Comparing internet usage in municipalities with and without internet access, would give the effect of internet access on internet usage. Comparing crime rates between these municipalities would give the effect of internet access on crime. Taking the ratio between the two would give a Wald estimate of internet use on crime, using random access to internet as an instrumental variable.

The intention of our 2SLS approach is to mimick this ideal experiment. Our source of exogenous variation comes from the staged installation of broadband infrastructure, generating spatial and temporal variation in broadband coverage rates. Section (4) describes this plausibly exogenous supply shock. For each municipality and every year, we instrument the fraction of households with broadband internet subscriptions, using the fraction of households that are covered by broadband infrastructure in the previous year. This 2SLS approach may then identify the average effect on sex crime for the subpopulation that is induced to use internet because of the increase in coverage in the previous year. Following Imbens and Angrist (1994), we refer to this subpopulation as compliers, and to the 2SLS estimate as the local average treatment

effect (LATE).

A benefit of studying the expansion of internet compared to, for example, investigating legalization of pornography in some country, is that the effect on access to pornographic content is likely to be quite instantaneous; When an internet connection is installed, pornographic content is immediately and readily available to the user. In contrast, legalization will tend to affect access only with a significant lag, given the time needed for entrepreneurs to establish networks for distribution and marketing, and raise awareness about their presence.

Because we control for municipality fixed effects and time-varying observables, it is not necessary that the timing of the internet expansion is completely unrelated to municipality characteristics. It is useful, however, to understand the determinants of the timing of the expansion across municipalities. To examine this issue further, we estimate the following equation

$$\Delta z_{kt} = m'_{k,2000}\xi + \gamma_k + [\theta_t \times m_{k,2000}]' \psi + \chi_{kt} \quad (3)$$

where $\Delta z_{kt} = z_{kt} - z_{kt-1}$ and $m_{k,2000}$ includes municipality-level information from year 2000 on average years of education, unemployment rate, poverty rate, number of policemen per capita, immigrant population share, share of population residing in a densely populated locality (a centrality indicator), baseline sex crime rate, and industry composition.

Figure 4 plots the estimated coefficients from the vector ψ (and the associated 95-percent confidence intervals). Consistent with the discussion in Section 4, our results indicate that broadband expansion is positively related to centrality, educational attainment and population size until the beginning of 2003. From 2004 onwards, there appears to be no systematic relationship between the timing of the broadband expansion and these variables. But more importantly, the timing of the expansion does not seem to be correlated with background variables such as immigrant population share, local unemployment rate, poverty rate, police density, or (baseline) sex crime. Although it is reassuring that the broadband internet expansion is uncorrelated with such risk factors of sexual crime (ADD REFERENCE), we cannot rule out that our instrument is correlated with time-varying unobservable determinants of sex crime which would bias our 2SLS estimates. To further increase our confidence in the 2SLS approach, we therefore run several specification checks, presented after the main results.

6 Main results

Fixed effect estimates

We begin by estimating equation (1). The first column in Table (2) shows the results from the specification with only municipality and year indicators. The first row suggests that when the internet usage rate increases by 1 percentage point, the overall crime rate increases on average by 0.589 crimes per 100,000 inhabitants. The next two rows report the estimates for rape and child sex abuse. We see that a 1 percentage point increase in internet usage is associated with an increase in rapes of 0.14 and an increase in child sex abuse of 0.21, per 100,000 inhabitants. The effect on the overall crime rate is the most precisely estimated, but all coefficients are statistically significant at the 10 percent level.

In the next three columns we start adding controls variables to see to what extent the fixed effects estimates of internet usage on sex crime can instead be attributed to time-varying observable factors. The second column adds demographic controls to the fixed effect specification. The estimated effects are quite similar. This conforms well to intuition, reflecting the homogenous Norwegian population. A second potential threat to the estimates are changes in the costs of committing crimes that are correlated with internet usage. It is therefore reassuring to see that our estimates barely move when adding a control for the number of policemen per capita in the municipality. A third worry is that there is some underlying trend in crime that happens to be correlated with internet usage. To mitigate this concern, the last column of Table 2 adds the total crime rate (excluding sex crimes) as a control. Again the estimates barely move.

The fixed effects effects suggest a positive relationship between internet usage and sex crime, which is statistically and economically significant. However, a major concern is that people select into internet usage based on time-varying unobservables correlated with sex crime. For example, the fixed effects estimates will be upward biased if individuals who are more likely to commit a sex crime are also more likely to use internet, because they want to consume internet pornography. To address this concern, we turn attention to the 2SLS-estimates

2SLS estimates

Table 3 reports results from the 2SLS approach, given by equations (1) and (2). The first column shows the estimates from the specification with only municipality fixed effects and time dummies. The first stage instruments the user rate in a municipality in year t with the coverage rate at the beginning of the year. The first stage coefficient

on the coverage rate is about 0.13. Since the instrument ranges from 0 (nobody covered) to 1 (everybody covered), we can directly interpret this first stage coefficient as the size of the complier group. For example, it implies that an increase in internet coverage of 10 percentage points induces (an additional) 1.3 percent of the population to use internet within the next year. We can also see that the first stage is strong, with an F-statistic around 323, which means that we do not need to worry about a weak instrument.

Turning to the second stage results in the first column we see that when the internet usage rate increases by 1 percentage point, the overall crime rate increases on average by 1.3 crimes per 100,000 inhabitants. The next two rows report the 2SLS estimates for rape and child sex abuse. We find that a 1 percentage point increase in internet usage causes an increase in rapes of 0.45 and an increase in child sex abuse of 0.65, per 100,000 inhabitants.

In the next three columns we start adding control variables, to see to what extent the 2SLS estimates of internet usage on sex crime can instead be attributed to time-varying observable factors. The second column adds demographic controls, which does not affect the first stage estimate, nor the estimated effects on overall sex crime rate and rapes. The effect of internet use on child sex abuse drops by about 25% but remains substantial, although it is no longer significant at the 10 percent level. Column (3) also includes a control for police density, before Column (4) finally also controls for other types of crime. The inclusion of these controls barely moves our estimates.

Fixed effects vs. 2SLS estimates

As discussed above, our 2SLS estimates should be interpreted as the LATE for the subgroup that was induced to use internet because of the increase in coverage in the previous year. When comparing these estimates to those produced by the fixed effects approach, it is important to keep in mind that the complier group from which we are now drawing our inference might not be representative of the population at large. To learn about the characteristics of the complier group, we take advantage of the survey data on individuals' internet use in the period 2000–2008. Using these data, we estimate the first stage separately by gender–age groups. The first column of Table 6 reports the subgroup estimates for the first stage coefficient. We can see that broadband access has a stronger effect on the young than the old, and that effects are also stronger among males than among females.

The characteristics of the complier group may also help in understanding why the 2SLS estimates are larger than the fixed effects estimates. Multiplying the

coefficients from this first stage estimation with the user rate and population share of the gender–age groups reported in columns 2 and 3 of the table, we get the distribution of compliers as a fraction of the number of internet users. Dividing by the total fraction of compliers in the population of users then gives the share of the gender–age groups in the group of compliers. These shares are reported in column 4 of Table 6, and show that the complier group is quite different from the overall population. While 29% of the population are males aged 16–44, 49% of the complier group are males in this age group. When including females, as much as 85% of the compliers are aged 16–44, compared to just 57% in the population at large. Further, while as expected 50% of the population are males, this is true for 60% of compliers.

Finally, columns 5–7 of Table 6 reports the probability of being charged with a sex crime for the gender–age groups. While sex crimes on the one hand are almost uniquely committed by males, young males are much more likely to be offenders than older males. Males aged 16–24 are for instance more than twice as likely to be charged with a sex crime than men 45–66. Taking the distribution of probabilities of sex crime charges, and taking a weighted average using the complier shares as weights, the overall probability of sex crimes, rapes and child sexual abuse are indeed just under twice that of the general population.¹³ It should not be surprising, then, to find that the LATE estimated in our 2SLS approach is larger than the estimates from the fixed effects approach,¹⁴

Economic significance

To put the estimates into perspective, we calculate counterfactual crime rates by subtracting the LATE effect – the sex crimes estimated to have been caused by the internet use of compliers – from the actual crime rate. The LATE effect can be calculated by multiplying the size of the complier group among internet users (the usage rate times the first stage) times the estimated effect.

Figure 5 shows the actual time trends for our various outcomes, and the predicted counterfactual time trends based on our IV-estimates. Panel (a) reports these for all sex crimes. Until 2004 the two trends coincide. During this period the broadband internet usage was low and the complier group therefore very small. From 2004 onwards internet usage quickly increased. We see a divergence between the observed sex crime rate and the counterfactual which predicts the crime level that would

¹³Since our complier group is estimated from the media use survey, we compare the reweighted crime rates with crime rates calculated correspondingly, but using the population shares from this survey as weights.

¹⁴It should also be noted that measurement error in the user rate would attribute to attenuation bias in the fixed effect estimates, but not affect the estimates from the 2SLS-approach. This could also help explain the larger size of the latter estimates.

have prevailed without internet use. By 2008, roughly 10 sex crimes per 100,000 inhabitants would have been avoided if broadband internet had not been introduced.

Panels (b) and (c) show similar trends and counterfactuals for rape and child sex abuse for the 2000 to 2008 period. Rape increased until 2002, after which it was relatively stable. The predicted rate shows that without the broadband internet expansion rape would have decreased and would have been back to the 2000-level by the end of our observation period. As for the overall crime rate, predicted rape is significantly lower than the observed level. Panel (c) shows the results for child sex abuse. Here we see an even larger predicted decrease although we lack precision to firmly reject equality with observed levels of child sex abuse.

Specification checks

We just saw that our estimates are robust to the inclusion of a wide range of controls. We now report results from additional specification checks to further increase our confidence in the 2SLS results.

We first perform subsample analyses to investigate the sensitivity of our specification to outliers. First, we verify whether our results are driven by secular changes between urban and rural areas that may have coincided with the internet expansion. When we drop the five large cities (Oslo, Bergen, Trondheim, Stavanger and Kristiansand) from our analysis, the estimates barely move. We further check that our estimates are not driven by observations with extreme values for sex crimes or zeros, neither of which is the case. Results are reported in Table A2 in the appendix.

A second set of robustness checks address the validity of our identification strategy. An important requirement for our approach to be valid is that the internet expansion does not coincide with existing trends in crime rates. As a first check for this possibility we estimate municipality-specific trends using data covering the period prior to the internet expansion, namely 1992–2000. For each municipality we obtain a slope estimate $\hat{\nu}_k$. We then extrapolate pre-expansion time trends in our specification (both 1st and 2nd stage) as follows

$$c_{kt}^* = \delta i_{kt} + x'_{kt}\beta + \alpha_k + \lambda_1 \hat{\nu}_k t + \lambda_2 \hat{\nu}_k t^2 + \tau_t + \epsilon_{kt} \quad (4)$$

which will take into account any variation in our instrument that coincides with pre-existing trends in the outcome. Columns (5) and (6) in Table 3 report the estimates based on these specifications, which are nearly identical to our baseline estimates.

A second and related test, following Duflo (2001), consists of interacting baseline

(year 2000) covariates with a time trend:

$$c_{kt}^* = \delta i_{kt} + x'_{kt} \beta + \alpha_k + t \sum_j \lambda_j x_{(k,2000),j} + \tau_t + \epsilon_{kt} \quad (5)$$

or time dummies:

$$c_{kt}^* = \delta i_{kt} + x'_{kt} \beta + \alpha_k + \tau_t \sum_j \lambda_j x_{(k,2000),j} + \epsilon_{kt} \quad (6)$$

The idea here is that violations of the common trend assumption embedded in our IV-strategy will be picked up by these interactions. The final two columns in Table 3 report the estimates based on equations (5) and (6). Again we find that these are close to our baseline estimates.

We also performed two placebo tests. The first consists in replacing current crime rates and control variables with these rates lagged eight years, while maintaining current internet use and coverage. In this pre-reform placebo test, we pretend that the internet expansion took place in the period before the actual roll-out. Since there was no broadband internet expansion during the period 1992–2000, significant estimates would suggest that the instrument is correlated with underlying municipality-specific trends in sex crime. Table 4 reports the results using these pre-reform outcomes. First note that, as expected, the first stage is not affected and only changes slightly because of the time shift of the regressors. In none of the three second stages do we find an impact of internet use on sex crime. This reinforces our confidence that we are indeed estimating the effect of internet use in Table 3, and are not picking up differential secular trends in sex crime.

The second placebo test consists in verifying that future internet use does not affect current sex crimes. If so, then we would worry that there is some confounding variable causing both sex crimes and internet use. We regress the sex crime rate in year t on internet usage in the following year $t + 1$. We instrument internet use in year $t + 1$ with the coverage rate in the same year. We control for the coverage rate in years t and $t - 1$ to ensure that we are indeed estimating the effect of next year's internet usage through future variation in our instrument.¹⁵ The final column of Table 4 shows the estimates of this placebo test. It is reassuring to find that future internet usage does not affect the rates of overall sex crimes, rape or child sex abuse.

¹⁵Since the coverage rate is cumulative, not controlling for coverage rates in the previous year would raise the concern that we are incorporating variation that is prior to the sex crime rate, defeating our purpose of performing a placebo test. It turns out, however, that the estimates without the control for coverage rates in the previous year are very similar and never statistically significant (t -statistic never exceeds 1).

7 Mechanisms

This section presents a conceptual framework that we use to explore different mechanisms by which internet usage may affected reported crime.

7.1 Conceptual framework

For a crime to happen, we need (at least) two individuals meeting each other. If λ is the rate (say per year) at which people meet and N the populations size, then $M = \lambda N$ is the number of matches of potential sex offenders and victims. Let $p = Pr(\text{crime}|\text{match})$ denote the probability that a match leads to a crime. Then, the number of comitted crimes equals $C = p \cdot M$, which in per capita terms is

$$c = p \cdot \lambda$$

where $c = C/N$ is the crime rate per capita. This setup can be justified using, for example, an underlying poisson process where matches arrive at a given rate, as is standard in the job-search literature.

We are interested in how internet usage (per capita), $i = I/N$, affects the crime rate. In our data we only observe reported crimes (per capita) c^* which relate to comitted crimes as follows

$$c^* = q \cdot c \tag{7}$$

where $q = Pr(\text{report}|\text{crime})$.

To see how internet usage may affect reported crimes, and how this relates to committed crime, taking the total derivative of (7) with respect to i we get

$$\frac{dc^*}{di} = q \frac{dc}{di} + c \frac{dq}{di} = q \cdot \left(\lambda \frac{\partial p}{\partial i} + p \frac{\partial \lambda}{\partial i} \right) + c \frac{dq}{di} \tag{8}$$

which highlights three different channels through which internet usage can affect reported crimes.

The first equality shows that we need to distinguish between the effect on committed crime and the effect on reporting. Although it is not possible to report crime on the internet, internet usage may, for instance, induce people to report crimes that they would otherwise not have reported. Below, we examine the impact of internet usage on reporting behavior.

The second equality decomposes the effect on crime into two channels: internet usage may affect the likelihood of a match between two individuals becoming a crime $\partial p/\partial i$, and it may also increase the matching rate $\partial \lambda/\partial i$. The first channel can be

thought of as the direct effect of internet usage through exposing more people to sexually violent material. As discussed above, theory makes conflicting predictions about the sign of $\partial p/\partial i$, depending on whether internet pornography is a complement or substitute to sexual related crime. The second channel represents the indirect of internet usage, as people are drawn away from alternative activities (both during and after usage) and their associated levels with sexual crime. A first possibility is that internet usage displaces activities that have a higher matching rate. This can be the case, for example, if internet usage leads people to stay more at home and to have less face-to-face interactions. A second possibility is that the aftermath of internet usage is more dangerous than the alternative activity. This can occur, for example, if it is easier for sex offenders to meet their potential victims via online chat rooms than through alternative activities. Below, we try to distinguish between these two different channels through which internet usage may affect sex crime.

7.2 *Reporting behaviour*

Internet usage might increase reported crime, without necessarily causing an increase in actual crime rates. To investigate this we first consider charges brought by police and prosecutor, relative to reported crimes. We expect that, everything else equal, crimes are more likely to be reported the stronger is the victims belief that charges will be made. Although it is not possible to report crime to the police on the internet, internet usage may induce people to report crimes that they would otherwise not have reported, for example by creating contact points with victims groups. However, it seems hard to argue that it should affect the likelihood of police charges based on evidence. If internet usage causes an increase in reporting, then we would expect a drop in the ratio of charges to convictions as internet became more common. Figure 6 shows that conviction rates are quite stable between 1998 and 2004 (the last year for which we have data on charges) for sex crimes, rapes and child sexual abuse alike, giving no indication of an increase in the number of reports. In line with these results, we find no significant effects on the charges-to-reports ratio when regressing this on internet usage, using our IV-approach, including all controls. Results are reported in Table 7. We have also estimated the effect of internet usage on conviction rates relative to reported crime rates for the years 2000–2004, finding no evidence of an effect.

We next consider the time from the reported crime was allegedly committed to the time of the report itself. Substantial changes in the timing of reports would suggest changes in reporting behavior, that could in principle also have an effect on the likelihood of charges. For instance, if the crime is more recent at the time of the

report, then witnesses and other evidence may be more forthcoming. In principle, therefore, changes in reporting may not be reflected in Figure 6. To address this, we again use our IV-approach from above, including all controls, to estimate the effect of internet usage on the time between the crime and the report (measured in years). Again, results reported in Table 8 reveal no evidence of changes in reporting behavior.

7.3 Direct and indirect effect of internet usage

If there is no effect of internet usage on reporting, we still need to distinguish whether our estimated effects are driven by an indirect effect increasing the likelihood of matches, or by a direct effect on the propensity for sex crime itself. For instance, internet use might displace alternative activities (both during and after usage) that are more or less associated with sexual crime. If activities on the internet imply that people stay more at home, then this should affect all crimes, not only sex crimes. However, we find no effect of internet usage on other, non-sex related, crimes.

On the other hand, the positive relationship between internet usage and sex crime might reflect that it is easier for sex offenders to meet their potential victims on the internet, for instance via chat rooms, than through alternative activities. To test this hypothesis, we exploit that the direct effect of internet usage is likely to vary systematically with access to non-internet pornography. In particular, municipalities on the border with mainly Sweden, but also Finland or Russia, could more easily take advantage of the liberal laws governing pornography in these countries. Meanwhile, the effect on matching over the internet should be the same across border and non-border areas. A difference in the effect of internet usage between these two areas should therefore reflect an effect on crime propensity, not matching. Results from separate estimations using our fixed effects approach and our 2SLS approach, including all controls, are reported in Table 9. Indeed, the effects of internet usage on sex crime are weaker in border areas than in non-border areas. While effects compared to our baseline estimates are somewhat larger in non-border areas, they are cut almost in half in border areas. This suggests that the direct effect on crime propensity is the dominating factor behind our results. In comparison, there is little if any evidence of differential effects of internet usage on sex crime along other observable population characteristics (see Table A4 in the Appendix).

8 Conclusion

Can a government legitimately prohibit its citizens from viewing pornography, or would this be an unjustified violation of basic freedoms?¹⁶ This question lies at the heart of a fierce political and judicial debate, raising fundamental issues about when, and on what grounds, the government is justified in using its coercive powers to limit the freedom of individuals. Traditionally, social conservatives want to censor pornography because it is inherently immoral and obscene, while social liberals defend the freedom of adults to consume pornography in private, invoking freedom of speech (Dworkin, 1985).¹⁷ Since the late 1960s, a strong feminist movement has joined in the call for censoring pornography (e.g. Dworkin, 1981; Mackinnon, 1995). Rather than a focus on the obscenity and immorality of pornography, however, feminists have argued that pornography harms women, both directly in its production, and indirectly, by propagating subordination of women in general and sexualized violence against women (and children) in particular. In the latter, liberal defenders of pornography would agree that there could be a legitimate case for censorship if pornography can reliably be shown to cause significant harm to people other than those who voluntarily consume it (or participate in it).¹⁸ The determining question for social liberals is therefore whether, and to what extent, exposure to pornography really causes harm to third parties, e.g. by propagating rape, child abuse or other forms of sexually related crime.¹⁹

In this paper, we use unique Norwegian data on crime and internet adoption to shed new light on this question. A public program with limited funding rolled out broadband access points in 2000–2008, and provides plausibly exogenous variation in

¹⁶Clearly defining pornography is a task even the US Supreme court has struggled with: Justice Potter Stewart famously wrote in his concurrence to *Jacobellis v. Ohio* (1964), that “*perhaps I could never succeed in intelligibly [defining ‘hard-core pornography’]. But I know it when I see it, and the motion picture involved in this case is not that*”. In the current paper, we do not take a firm stand on the types of pornography from which we are drawing our inference. However, our estimates should be expected to reflect in particular the effect of violent and extreme pornography, since these are the types of pornography for which the cost of access is likely to be affected the most by access to internet.

¹⁷For a thorough and balanced discussion of this issue, see West (2008).

¹⁸Indeed, this is the underpinning of the central liberal doctrine for justifying interference with civil liberties against mature and unimpaired subjects, the so-called *harm principle* (see Mill, 1859).

¹⁹An original take on the debate can be found in the 1991 dissent to *Barnes v. Glen Theatre, Inc.*, where Justice White (with Justices Marshall, Blackmun and Stevens concurring) argues that Indiana’s ban on nude dancing violates the US constitution precisely because it seeks to mute the communicative aspects of the dance, to deter “prostitution, sexual assaults, criminal activity, degradation of women, and other activities which break down family structure”. In his concurrence, Justice Souter uses this precise argument to argue the constitutionality of the nudity ban. Source: <http://caselaw.lp.findlaw.com/scripts/getcase.pl?court=US&vol=501&invol=560>. Reading date: 2010/11/10.

internet use. Our 2SLS estimates suggest that internet usage is associated with a substantial increase in reported incidences of rape and other sex crimes. We present a conceptual framework that reveals three mechanisms for how internet use may affect reported sex crime, namely a reporting effect, a matching effect on potential offenders and victims, and a direct effect on crime propensity. Results indicate that the direct effect dominates, plausibly as a result of increased consumption of pornography.

Our results speak to the policy debate on censorship of internet pornography. Since the mid-1990s, the arrival of the internet, with its abundant and readily available supply of violent and exotic pornography, has reinvigorated the debate. In 1996, the US congress passed the Communications Decency Act in an effort to regulate pornographic material on the Internet, while the Children's Internet Protection Act was passed in 2001. In 2005, the US Attorney General formed the Obscenity Prosecution Task Force to pursue in particular producers of extreme pornography. The task force has since successfully litigated cases against publishers of extreme pornography, for instance the 2008 case against Paul Little who was sentenced to 46 months in prison and a \$1.4 million fine for distribution of obscene material. The opposition to pornography is hardly restricted to the US. In China, internet pornography was banned in 2002, and the production of pornographic movies was banned in 2008. While possession of pornography may give prison terms up to 3 years long, large distributors of pornography may face execution.

While our results indicate that the case for censorship of pornography because of its harms on third parties may have merit, caution is in order. In the first place, though the evidence suggests that internet use increases the propensity for sexual crime, we cannot completely rule out matching or reporting effects. Second, though internet is the main channel of pornography, it could be that there are other aspects of internet's content that drives our result.

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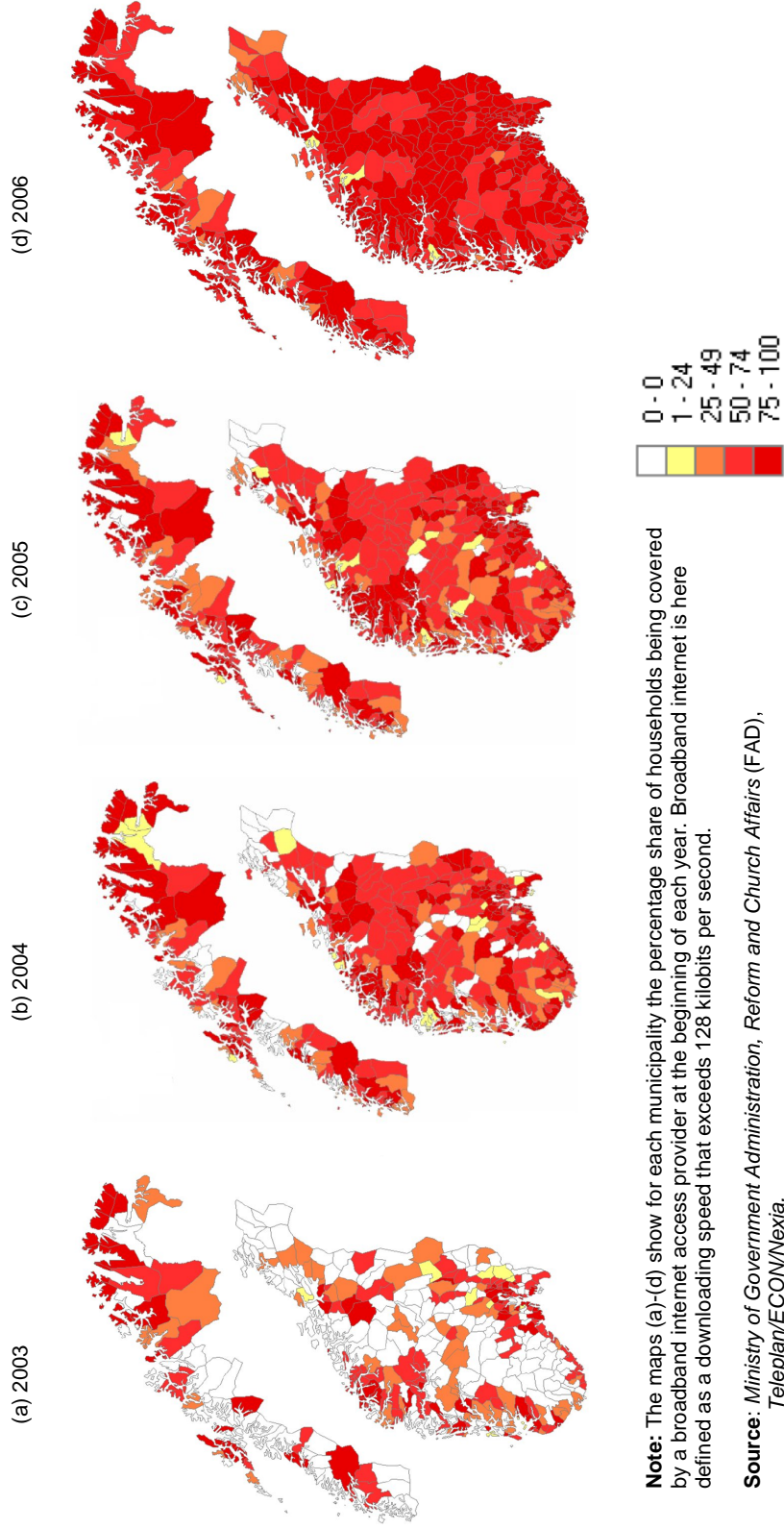
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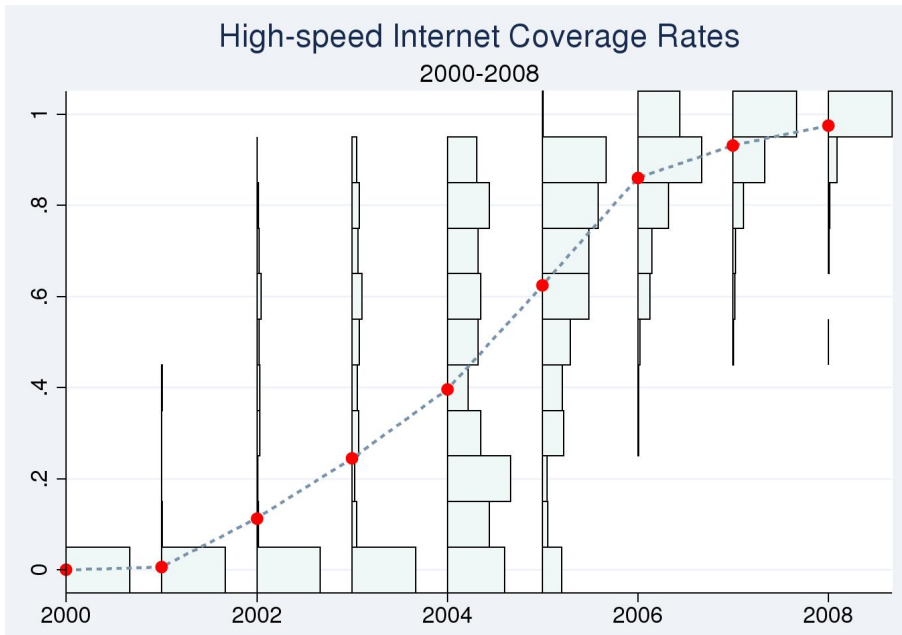
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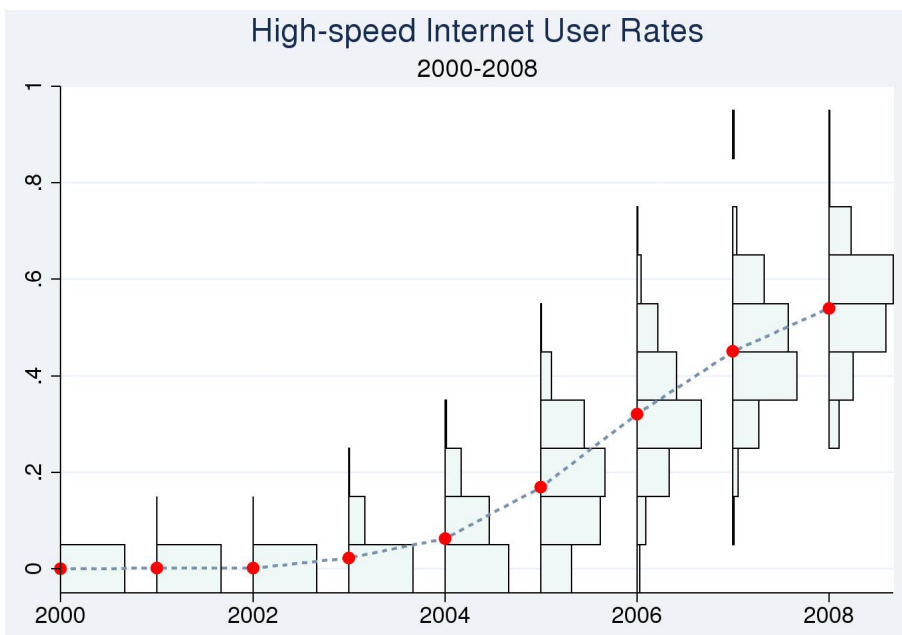
Note: The maps (a)-(d) show for each municipality the percentage share of households being covered by a broadband internet access provider at the beginning of each year. Broadband internet is here defined as a downloading speed that exceeds 128 kilobits per second.

Source: *Ministry of Government Administration, Reform and Church Affairs (FAD), Teleplan/ECON/Nexia.*

Figure 1. Broadband internet expansion across Norway



(a) Coverage rates



(b) User rates

Figure 2. Internet user rates, averages and distribution across municipalities, 2000–2008

Note: Figures show the overall mean and distribution of broadband user rates (figure a) and coverage rates (figure b) across municipalities for each year during the period 2000 – 2008. Data on internet user rates from Internet Survey provided by Statistics Norway. Data on broadband coverage rates provided by the Ministry of Government Administration, see details in Table A1 and section 3.

Table 1. Descriptive statistics

	Overall	2000	2002	2004	2006	2008
<i>Crime rates (per 100,000)</i>						
All sex crimes	50.44 (56.22)	46.25 (49.01)	53.46 (59.18)	53.84 (58.30)	48.61 (51.05)	41.41 (46.77)
Rapes	12.11 (19.98)	10.04 (18.19)	12.94 (23.38)	12.67 (18.93)	11.34 (17.11)	12.47 (20.44)
Child abuse	21.05 (37.72)	19.35 (31.07)	23.13 (39.86)	22.20 (38.09)	20.98 (36.99)	14.33 (29.25)
<i>Internet usage and coverage (percent)</i>						
User rate	17.39 (21.35)	0.00 (0.00)	0.08 (0.60)	6.26 (7.00)	32.13 (11.73)	53.95 (10.27)
Coverage rate	46.19 (42.05)	0.00 (0.00)	11.43 (22.97)	39.76 (30.01)	85.95 (12.14)	97.58 (4.57)
<i>Control variables</i>						
Local unemployment rate	1.49 (1.02)	3.14 (1.59)	1.40 (0.63)	1.68 (0.70)	1.12 (0.57)	0.76 (0.42)
Poverty rate	3.90 (1.18)	3.55 (1.02)	3.99 (1.05)	4.19 (1.08)	4.06 (1.30)	2.89 (0.95)
Urban settlement	49.79 (27.70)	48.53 (27.59)	49.82 (27.42)	49.85 (27.93)	50.04 (27.93)	50.92 (27.79)
Average years of education	12.57 (10.45)	12.38 (10.43)	12.43 (10.42)	12.56 (10.42)	12.69 (10.43)	12.86 (10.43)
Immigrant population share	4.81 (2.81)	3.86 (2.45)	4.23 (2.53)	4.72 (2.63)	5.19 (2.78)	6.26 (3.27)
Police density	1.22 (0.82)	1.21 (0.83)	1.26 (0.84)	1.23 (0.83)	1.20 (0.81)	1.20 (0.82)
Overall crime rate	5029.21 (2733.31)	5337.08 (2729.49)	5319.85 (2812.28)	5083.11 (2842.38)	5011.08 (2788.94)	4401.77 (2518.50)

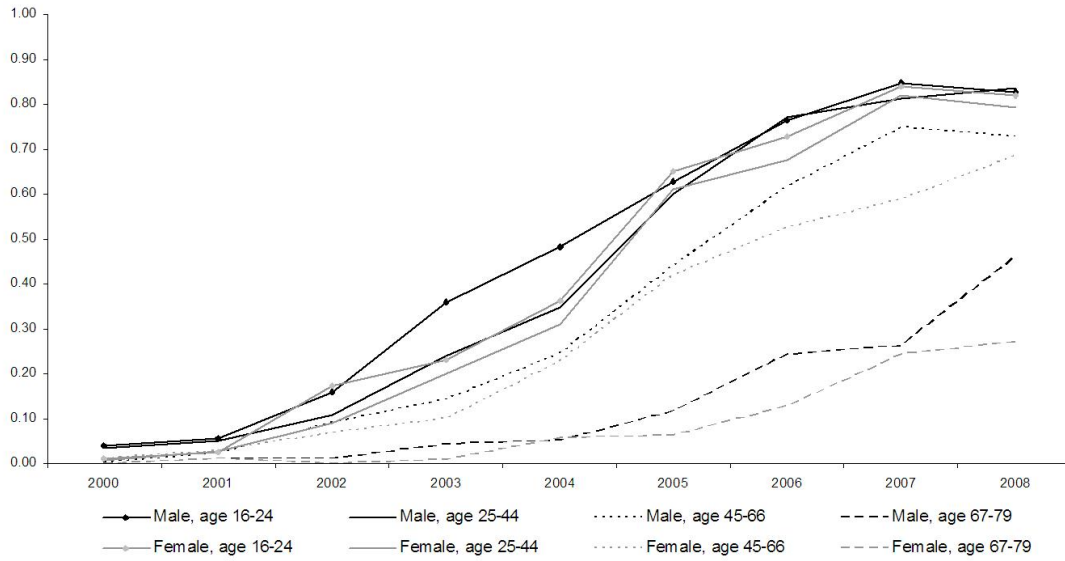


Figure 3. Broadband user rates by gender and age, 2000 – 2008

Sources: Media User Survey provided by Statistics Norway for the period 2000 – 2008. Broadband usage is here defined as having an internet connection with speed higher than 128 kbit/s.

Table 2. Fixed effects estimates of internet use on sex crime

All sex crimes	58.9 (18.4) ^{***}	65.3 (20.1) ^{***}	65.0 (20.1) ^{***}	66.8 (19.8) ^{***}
Rape	13.9 (7.0) ^{**}	11.9 (7.1) [*]	11.9 (7.1) [*]	12.1 (7.0) [*]
Child sex abuse	21.3 (12.1) [*]	22.8 (13.3) [*]	22.7 (13.3) [*]	23.7 (13.1) [*]
Demographic controls		✓	✓	✓
Police density			✓	✓
Other crimes				✓

Note: Crime rates are calculated per 100,000 persons using data on reported crimes for each municipalities over the period 2001 – 2008. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 8 years = 3376 observations. All regressions include municipality fixed effects and year dummies. Detailed description of control variables is given in Table A1.

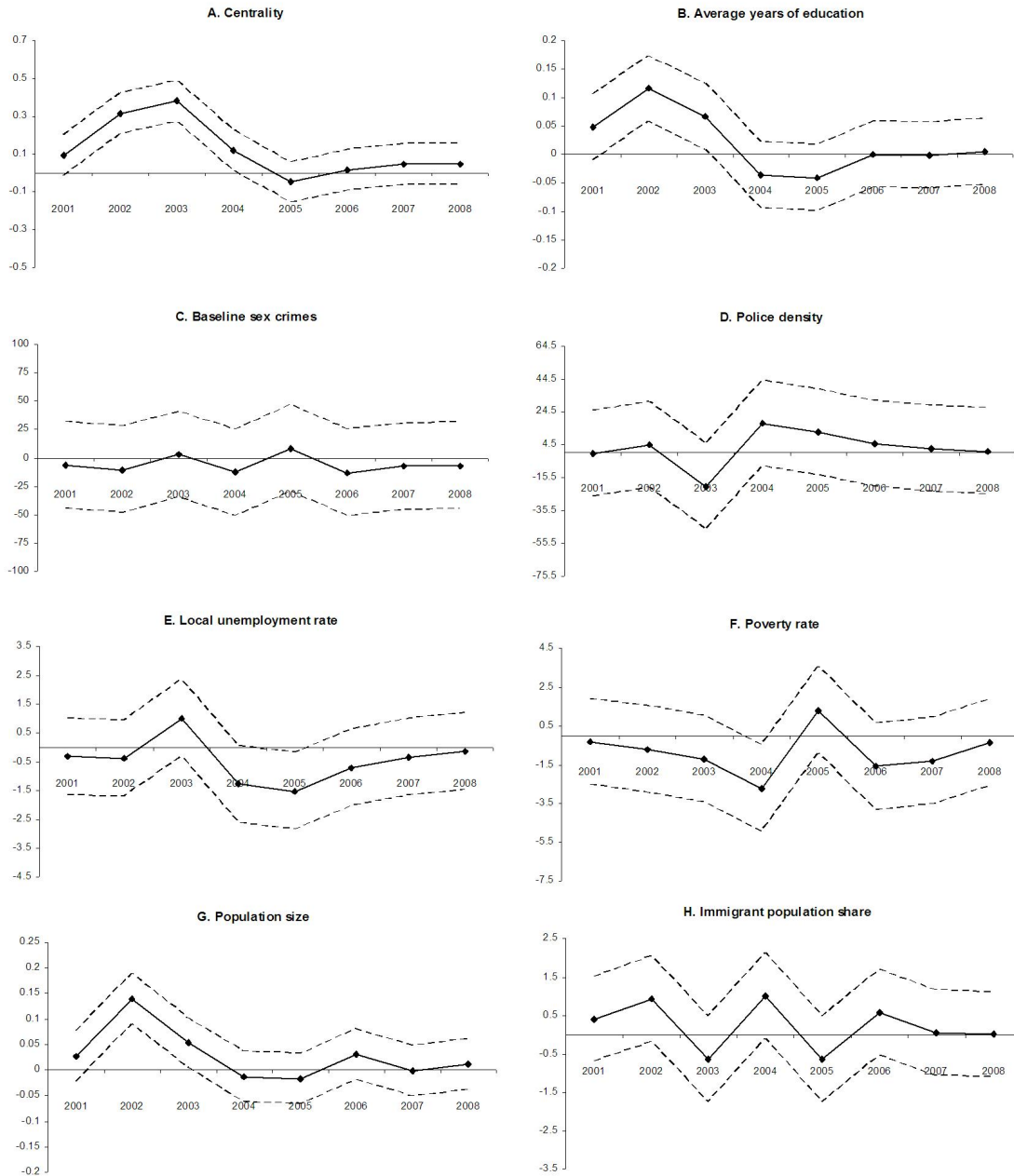
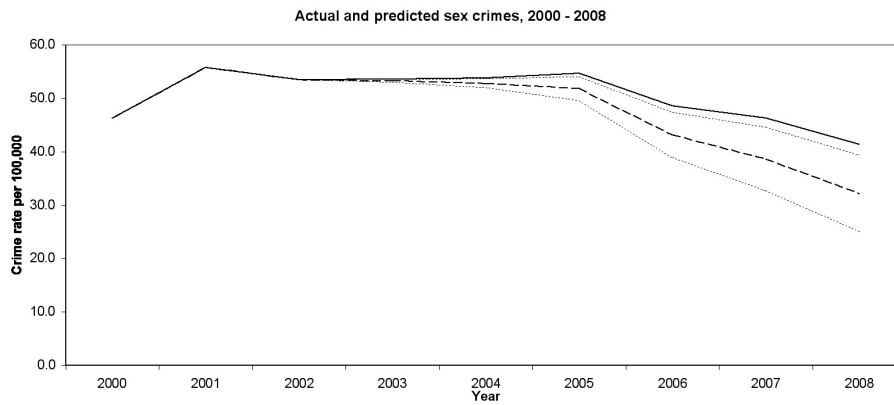
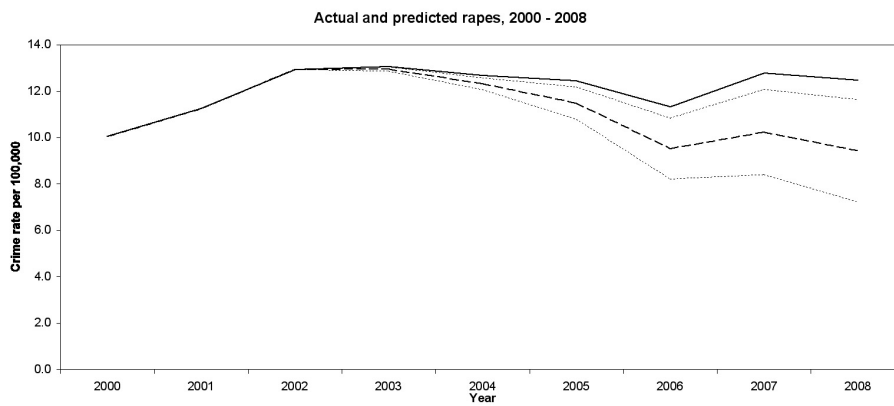


Figure 4. Broadband expansion regressed on baseline municipality characteristics, 2000 – 2008

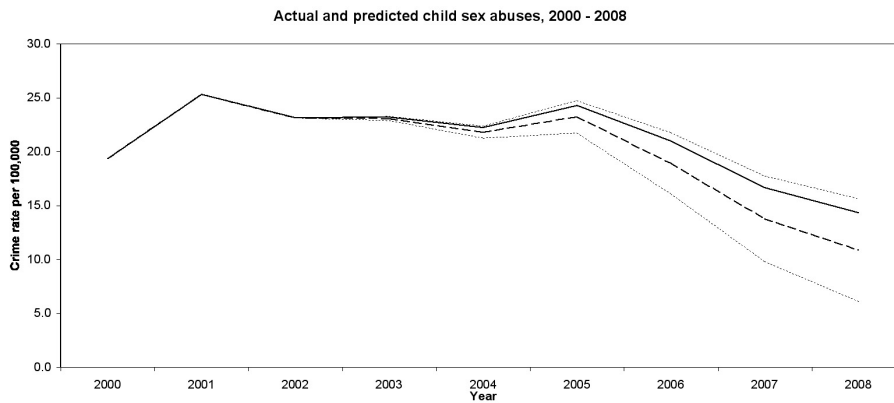
Sources: Data on broadband coverage rates at the municipality level provided by the Ministry of Government Administration, remaining data from Statistics Norway, see details in Table A1 and section 3. See discussion in section 5 on expansion plots.



(a) All sex crimes



(b) Rapes



(c) Child sex abuse

Figure 5. Actual and predicted time trends

Sources: Data on reported crimes and internet user rates, both provided by Statistics Norway at the municipality level. Data on broadband coverage rates at the municipality level provided by the Ministry of Government Administration, see details in Table A1 and section 3 on variable definitions. The solid lines show actual trends in sex crimes over the period 2000 – 2008, whereas the dark dotted lines show the predicted trend in sex crimes in the case on no broadband expansion (see discussion in section 5 on calculation of predicted trends). The grey lines confidence intervals for the predicted trends at a significance level of 95 percent.

Table 3. Instrumental Variables estimates of internet use on sex crime

	Baseline specifications			Pre-reform trend		Covariate Interactions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All sex crimes	129.9 (51.1)**	130.8 (51.6)**	130.6 (51.6)**	133.6 (51.7)**	133.6 (51.7)**	134.0 (51.6)**	121.0 (56.6)**	182.7 (97.7)*
Rape	44.9 (15.8)**	43.5 (16.0)**	43.4 (16.0)**	44.1 (16.0)**	43.3 (16.0)**	44.6 (16.0)**	45.1 (17.8)**	82.0 (30.6)**
Child sex abuse	65.1 (32.5)**	49.1 (34.5)	49.0 (34.5)	50.4 (34.5)	50.3 (34.5)	50.8 (34.60)	42.5 (38.1)	90.0 (63.5)
First-stage: $Coverage_{k,t-1}$	0.131 (0.01)**	0.128 (0.01)**	0.128 (0.01)**	0.128 (0.01)**	0.128 (0.01)**	0.128 (0.01)**	0.118 (0.01)**	0.098 (0.01)**
F-value (instrument)	323.2	322.3	322.2	325.3	324.7	327.5	348.6	118.2
Demographic controls		✓	✓	✓	✓	✓	✓	✓
Police density			✓	✓	✓	✓	✓	✓
Other crimes				✓	✓	✓	✓	✓

Note. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 8 years = 3376 observations. All regressions include municipality fixed effects and year dummies.

Table 4. Placebo tests

	Baseline	Pre-reform <i>outcomes</i>	Next year <i>usage</i>
All sex crimes	133.6 ^{***} (51.7)	-21.7 (55.0)	-98.8 (55.0)
Rapes	44.1 ^{***} (16.0)	9.4 (15.5)	-18.1 (51.5)
Child abuse	50.4 (34.52)	-62.9 (47.1)	37.0 (80.0)
First-stage: $Coverage_{k,t-1}$	0.128 ^{***} (0.01)	0.133 ^{***} (0.01)	
$Coverage_{k,t}$			0.054 ^{***} (0.01)

Note: Crime rates are calculated per 100,000 persons using data on reported crimes for each municipalities. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 8 years = 3376 observations. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. For the baseline estimations we use data on reported crimes over the period 2001 – 2008. In column (2) we display results from regressions where we use data on reported crimes and controls from the pre-broadband expansion period 1993 – 2000, while broadband user rates and coverage rates are still from the period 2001 – 2008. In column (3) we display results from regressions where we use user rates and coverage rates from the subsequent periods, controlling for coverage rates from the current and previous periods.

Table 5. Alternative outcomes

	Overall crime	Vandalism	Theft
Effect of internet usage	-422.1 (981.6)	-8.2 (133.6)	-372.0 (606.6)
Dependent mean	4933.8	333.4	2192.4

Note: Crime rates are calculated per 100,000 persons using data on reported crimes for each municipalities over the period 2001 – 2008. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 8 years = 3376 observations. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1.

Table 6. Compliance and sex crime rates by gender and age

	Compliance			Crime rates	
	<i>Pop.share Userrate</i>	<i>Firststage</i>	<i>Shareofcompliers</i>	<i>Allsexcrimes</i>	<i>Rape</i>
Male, age 16-24	9.8	0.53	0.35	24.6	71.3
Male, age 25-44	18.8	0.47	0.20	24.0	36.1
Male, age 45-66	15.9	0.38	0.14	11.0	18.4
Male, age 67-79	5.6	0.15	0.03	0.3	6.9
Female, age 16-24	8.9	0.49	0.35	20.0	1.2
Female, age 25-44	19.5	0.43	0.13	14.8	0.6
Female, age 45-66	19.5	0.34	0.08	5.4	0.2
Female, age 67-79	6.3	0.10	-0.03	0.0	0.0
Overall	100.0	0.39	0.16	100.0	16.2

Note: Population shares reported in column (1) are based on microdata for respondents from Media User Surveys for the period 2001 – 2008. Average broadband user rates by gender and age given in column (2) are calculated using the Media User Surveys. Also using the Media User Surveys, we perform separate estimations of the first stage equation (2) for each gender-age group. Each cell in column (3) contains the coefficient in a regression of broadband usage on the average coverage rate in the municipality, estimated separately for each gender-age group. All regressions include municipality fixed effects and year dummies. Column (4) reports the share of compliers in each gender-age group. The share of compliers is calculated as (first stage coefficient \times average user rate \times pop. share) divided by the sum of this factor over all gender-age groups. Crime rates given in columns (5)-(7) are based on criminal records data for all Norwegian residents aged 16-79 for the period 2001 – 2004, and are measured as criminal charges per 100,000 persons.

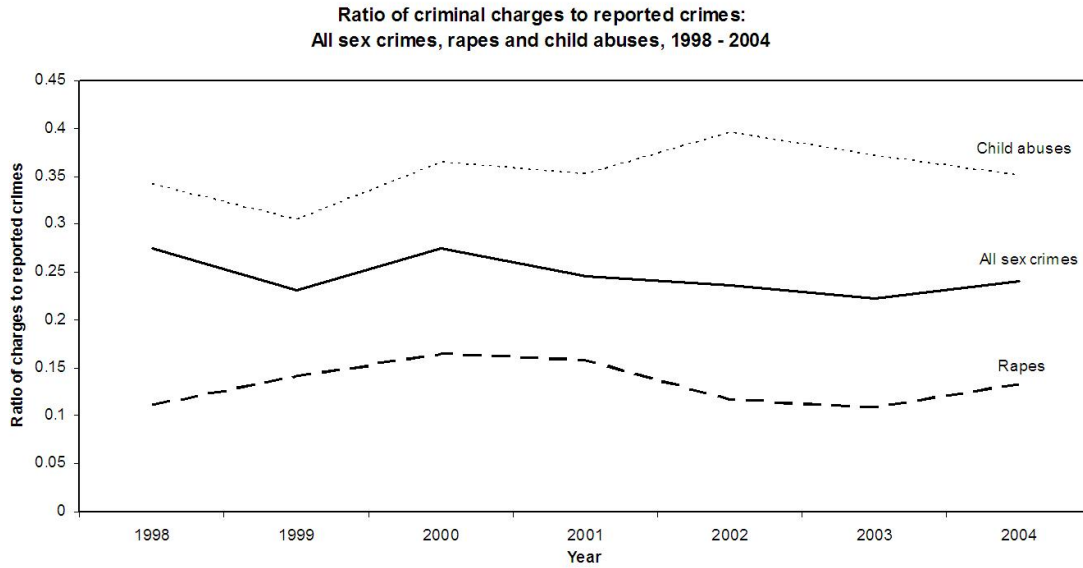


Figure 6. Ratio of charges to reported crimes, 1998-2004

Source: Data on individual crime records and municipality level reported crimes data provided by Statistics Norway over the period 1998 – 2004. For each crime category, we define the ratio of criminal charges to reported crimes as the number of reported crimes that lead to a criminal charge divided by the total number of reported crimes in each municipality over the period 2001 – 2004.

Table 7. Effect of internet usage on the ratio of criminal charges to reported crimes

	All sex crimes	Rape	Child sex abuse
Effect of internet usage	-0.11 (0.97)	-1.88 (1.38)	-0.07 (1.42)
Dependent mean	0.40	0.19	0.55

Note: For each crime category, we define the ratio of criminal charges to reported crimes as the number of reported crimes that lead to a criminal charge divided by the total number of reported crimes in each municipality over the period 2001 – 2004. We use data on criminal records for all Norwegian residents and data on reported crimes at the municipality for the period 2001 – 2004. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 4 years = 1688 observations. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1.

Table 8. Effect of internet usage on difference between reporting time and crime

	All sex crimes	Rape	Child sex abuse
Effect of internet usage	0.05 (0.65)	-0.06 (0.46)	-0.44 (0.83)
Dependent mean	0.39	0.14	0.46

Note: The dependent variable is the average distance in years between the time of reporting and the time of crime happening in a municipality in a given year, measured as the average number of days between the date of crime reporting and the date when crime was committed divided by 365. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 8 years = 3376 observations. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1.

Table 9. Border areas

	FE		IV	
	Border	Non-border	Border	Non-border
All sex crimes	62.1** (28.9)	75.2*** (24.4)	72.7 (66.8)	155.3** (67.0)
Rapes	-0.1 (13.1)	16.0* (8.2)	28.5 (24.9)	49.6** (20.7)
Child abuse	33.5** (14.2)	24.9 (16.6)	31.3 (39.3)	56.8 (44.7)
<u>First-stage:</u> <i>Coverage</i> _{k,t-1}			0.123*** (0.01)	0.128*** (0.01)
Municipalities	113	309	113	309

Note: Standard errors are heteroscedasticity robust and clustered at the municipality level. All regressions include municipality fixed effects and year dummies.

A Additional results

Table A1. Variable definitions

Variable	Description
<i>Sex crimes</i>	
All sex crimes	The number of reported alleged all sexual abuses mentioned in the Norwegian Penal Code §§191-203, including rape, attempted rape, sexual abuse of children, incest, procurement, prostitution, public exposé, and various other unapproved sexual abuses, committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.
Rapes	The number of reported rapes and attempted rapes as defined in the Norwegian Penal Code §192 that were allegedly committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.
Child abuses	The number of reported sexual abuse of children under 10, 14 or 16 years of age as defined in the Norwegian Penal Code §§195-7 that were allegedly committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.
<i>Internet variables</i>	
User rate t	Percentage share of households residing in a given municipality who have a paid broadband internet subscription, with access speed above 128 kilobits per second at the beginning of year t .
Coverage rate t	Percentage share of households residing in a given municipality who are being covered by a broadband internet provider, with access speed above 128 kilobits per second at the beginning of year t .
<i>Demographic controls</i>	
Age-group	Percentage shares of the population residing in a given municipality belonging to the age-groups 16-21, 22-24, 25-34, 35-44, 45-54, 55-66, and 67 or above at the beginning of year t .

Variable	Description
Sex	Percentage shares of the female population residing in a given municipality belonging to the age-groups 16-21, 22-24, 25-34, 35-44, 45-54, 55-66, and 67 or above at the beginning of year t.
Immigrants	Percentage shares of the immigrant, male-immigrant, non-western immigrant, non-western male-immigrant, refugee and male-refugee populations residing in a given municipality belonging to the age-groups 16-21, 22-24, 25-34, 35-44, 45-54, 55-66, and 67 or above at the beginning of year t.
Centrality	Percentage share of the population in a given municipality residing in a densely populated locality ay the beginning of year t.
Education	Average years of education among the age-group 16-59 residing in a given municipality at the beginning of year t.
Income	Average after-tax disposable income earned during year t by individuals aged 16-59 years residing in a given municipality.
Poverty	Percentage share of population having income below half of the median equivalent after-tax income in a given municipality, when the equivalent income is calculated using the OECD equivalence scale.
Unemployment	Percentage share of the population aged 16-59 residing in a given municipality that is registered as fully unemployed at beginning of year t.
Police density	Number of policemen in service in a given municipality at the beginning of year t, divided by the population size of the municipality and multiplied by 1,000.
<i>Other crimes</i>	
Overall crime rate	The total number of reported crimes (excluding sex crimes) allegedly committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.
Theft	The total number of thefts, including burglary (§ 147), larceny-theft (§ 257, §§ 261-262, § 258) and motor vehicle theft (§ 260) as defined in the Norwegian Penal Code § 147 and §§ 257-262 that were allegedly committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.

Variable	Description
Vandalism	The number of reported acts of vandalism as defined in the Norwegian Penal Code §§291-294 that were allegedly committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.

Table A2. Additional specification checks

	Baseline	No outliers	No zeros	No cities
All sex crimes	133.6*** (51.67)	130.4*** (49.3)	130.8** (55.7)	138.6*** (53.3)
Rapes	44.1*** (16.0)	41.4*** (15.6)	49.9* (26.6)	47.2*** (16.5)
Child abuse	50.4 (34.5)	42.4 (33.1)	39.1 (45.9)	51.9 (35.7)
<u>First-stage:</u>				
$Coverage_{k,t-1}$	0.128*** (0.01)	0.128*** (0.01)	0.101*** (0.01)	0.128*** (0.01)

Note: Crime rates are calculated per 100,000 persons using data on reported crimes for each municipalities over the period 2001 – 2008. Standard errors are heteroscedasticity robust and clustered at the municipality level. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. The baseline results are based on 422 municipalities \times 8 years = 3376 observations. In column (2), we check the robustness of our results to extreme values by dropping all the observations with a value of the dependent variable higher than its 99th percentile. In column (3), we drop all observations with zero values of the dependent variable, where we drop the 5 largest cities in Norway, i.e. Oslo, Bergen, Trondheim, Stavanger and Kristiansand, in column (4).

Table A3. Effects of internet use on sex crime charges

	All sex crime	Rape	Child sex abuse
Effect of internet usage	29.8 (29.3)	8.1 (7.2)	10.0 (20.9)
Dependent mean	24.0	2.7	13.6

Note: Results are based on IV estimations of equation (1) where we replace reported crime rates by criminal charge rates using data at the municipality level over the period 2001 – 2004. Standard errors are heteroscedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities \times 4 years = 1688 observations. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. Dependent mean are calculated as the unweighted means of criminal charge rates per 100,000 persons across municipalities over the period 2001 – 2004.

Table A4. Subsamples

	Overall crime			Male-female ratio			Centrality	
	<i>Baseline</i>	Common		Common	Low		<i>Common effect</i>	<i>Rural areas</i>
		<i>effect</i>	Low		<i>crimeareas</i>	<i>maleratio</i>		
All sex crimes	133.6*** (51.7)	133.4*** (49.3)	0.2 (10.0)	136.0** (52.8)	-5.1 (9.1)	135.2*** (45.7)	-1.8 (12.8)	
Rapes	44.1*** (16.0)	48.3*** (15.5)	-5.3 (4.2)	45.2*** (16.4)	-2.3 (3.9)	35.3** (13.9)	9.4* (4.8)	
Child abuse	50.4 (34.5)	44.6 (32.8)	7.4 (7.3)	52.6 (35.2)	-4.6 (6.7)	60.2* (30.4)	-10.6 (9.0)	

Note: Crime rates are calculated per 100,000 persons using data on reported crimes for each municipalities over the period 2001 – 2008. Standard errors are heteroscedasticity robust and clustered at the municipality level. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. For the subsample analysis, we divide municipalities into equally large subgroups based on the baseline 2000 median value across municipalities of overall crime rate (excluding sex crimes), male-female ratio and share on population living in densely populated areas (centrality indicator), respectively. The median overall crime rate in 2000 was 4,855 per 100,000, whereas the median male-female ratio was 0.5015 and the median share of densely residing population was 0.4835. Thereafter, we construct dummy indicator for high/low overall crime, high/low male-female ratio and urban/rural municipalities, and interact the subsample dummies with user rates in the second stage equation (1) and with coverage rates in the first stage (2). Thus, the results are based on IV estimations of equation (1) using data on 422 municipalities \times 8 years = 3376 observations. For the periodic analysis, we similarly construct dummies for the years 2001-2004 and 2005-2008 and interact the period dummies with user rates in the second stage and coverage rates in the first stage, still performing regressions based on the same number of observations.