Broadband Internet: An Information Superhighway to Sex Crime?*

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Abstract

Does internet use trigger sex crime? We use unique Norwegian data on crime and internet adoption to shed 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 instrumental variables estimates show that internet use is associated with a substantial increase in both reports, charges and convictions 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 sex crime propensity. To investigate the importance of these mechanisms, we use data on individual reporting behavior, police investigations, and criminal charges and convictions. None of the analyses we perform suggest that the positive relationship between internet use and sex crime is driven by changes in reporting behavior. Our findings suggest that the direct effect on sex crime propensity is positive and non-negligible, possibly as a result of increased consumption of pornography.

Keywords: Broadband internet, sex crime, rape, media
JEL codes: K42, H40, L96, C26

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1 Introduction

The internet is a virtual treasure trove of information, and it has enabled new forms of social interactions through instant messaging, internet forums, and social networking. These fundamental changes have led to a growing interest among researchers and policymakers about the internet's impact on society. Though economists have mostly focused on the efficiency enhancing effects of internet, there has been increasing political and public concern about potential adverse side effects of internet use.[1]

We use a unique Norwegian data set to study one particular kind of consumption externality: how internet use affects the prevalence of sex crime in general and rape and child sex abuse in particular. Evidence on these consumption externalities are of particular interest because they are unlikely to be internalized, and because they play an important role in the debate about parental control, and government censoring and monitoring of internet traffic. Yet, there is little if any causal evidence on the link between internet use and sex crime and – as discussed in detail below – most of what we know is circumstantial.

We begin by estimating the net impact of internet use on rates of overall sex crime, rape and child sex abuse. As a source of exogenous variation in internet use, we exploit a public program introduced by the Norwegian government in the late 1990s. The program aimed at ensuring broadband internet access at a reasonable price throughout the country.[2] 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. We use this spatial and temporal variation in the availability of broadband internet across municipalities in an instrumental variables (IV) setup: For each municipality and every year, we instrument the fraction of households with broadband internet subscription with the fraction of households that are covered by broadband infrastructure in the previous year. Our empirical approach – which controls for municipality-specific fixed effects

[1]Brown and Goolsbee (2002) examine whether the internet makes markets more competitive. Freund and Weinhold (2004) and Choi (2003) find that the internet has a positive effect on bilateral trade and foreign direct investment, while Yi and Choi (2005) and Choi and Yi (2009) suggest that the internet lowers the inflation rate and increases economic growth. Saint-Paul (2009) studies how internet search engines affect economic growth, while George (2008) and Ward (2007) examine how the internet affects the market for daily newspapers and local music stores, respectively. Gentzkow and Shapiro (2010) find that media consumption on the internet is somewhat more ideologically segregated than is consumption of traditional media, though less segregated than face-to-face interactions.

[2]Throughout this paper, we focus on broadband internet, defined as internet connections with download speeds that exceed 256 kbit/s. Before the expansion of broadband internet, all households with a telephone connection would have dial-up access to internet, but limited to a bitrate of less than 56 kbit/s. Broadband internet facilitated internet use without excessive waiting times.

(and time effects) – is motivated by two features of the broadband program. First, most of the supply and demand factors tend to vary little over time. Second, the timing of the roll-out is unlikely to co-vary with the key correlates of sex crime. We demonstrate that the data are consistent with these program features, and further challenge the validity of the instrument by probing the stability of the IV estimates to alternative specifications and by performing placebo tests, finding little cause for worry.

Our IV estimates show that internet use is associated with a substantial and statistically significant increase in reported sex crime: Overall, the estimates suggest that about 3.2 % of the total number of rapes and 2.5 % of the total number of sex crimes and child sex abuses that occurred between 2000 and 2008 would have been avoided if broadband internet had not been introduced. The difference peaks in 2006, when we estimate that the introduction of broadband internet accounts for roughly three out of 53 sex crimes. Our analysis also suggests that internet use in 2006 explains about one out of twelve rapes and one out of 23 child sex abuses. We further show that internet use affects not only the recording of sex crimes, but also has a significant and sizable effect on the number of charges and convictions.

Following Imbens and Angrist (1994), we interpret our IV estimates as the local average treatment effect (LATE) of internet use on sex crime for the sub-population of compliers that is induced to use internet because of the increase in coverage in the previous year. To learn about the characteristics of this complier group, we take advantage of survey data on individuals' broadband use and access. We find that there is an overrepresentation among compliers of individuals who are more likely to be sex offenders, most notably young males. This illustrates that the estimated effects pertain to a subgroup of early adopters of broadband internet which is relatively prone to sex crime, and that we need to be cautious in extrapolating these LATEs to the average effects of internet use for the entire population.

We further present a conceptual framework that highlights three distinct mechanisms. The first possible mechanism is that internet use only affects reporting behavior, so that both reports, charges and convictions of sex crimes increases without an increase in the number of sex crimes. Although it is not possible to report crime to the police on the internet, it could reduce the cost of reporting by providing information or facilitating contact with support groups. However, we find no evidence of changes in reported sex crime relative to charges or convictions, which would be expected if marginal reports are less strong. There are also no changes in the time elapsed between the crime and the report, suggesting again that reporting behavior was unaffected. Internet use could also affect reporting if it makes the investigation more effective: If the probability that a reported sex crime leads to a charge and conviction increases, more victims may report crimes. To assess this, we use information from police records about the grounds invoked to end an investigation. We find no evidence that internet increases the likelihood of identifying the perpetrator, nor that it lowers the likelihood of closing a case due to lack of evidence.

Next, we try to distinguish between the two other possible mechanisms. On the one hand, there may be a matching effect where the positive relationship between internet use and sex crime reflects that it is easier for sex offenders to meet their potential victims via chat rooms or other internet sites than through alternative activities. On the other hand, internet use may have a direct effect on the propensity for sex crime. The prime suspect here is the consumption of internet pornography, as argued, for instance, by the US Congress in the passing of the Communications Decency Act in 1996 and the Children's Internet Protection Act in 2001. The argument is that internet reduces the pecuniary and non-pecuniary costs of (violent and/or extreme) pornography, which many laboratory experiments suggest increases the propensity to commit sex crimes (Donnerstein et al., 1987; Allen et al., 1995).

Our results indicate that the direct effect is positive and substantial. First, we find no effect on other types of crime, such as vandalism or theft, where internet use might have an indirect effect through displacing alternative activities but should have little if any direct effect. Second, we exploit that the size of the direct effect through internet pornography should vary systematically with access to non-internet pornography. In particular, we find a weaker effect of internet use on sex crime in municipalities close to the national border: While a legal ban on distribution of explicit pornography was enforced in Norway, pornography was legal and readily available in Sweden, the neighboring country. Although we admittedly cannot rule out matching effects, the results suggest that the positive net impact of internet use on sex crime is driven, at least in part, by the direct effect of internet use.

Our paper complements the evidence on the effect of exposure to sexual media content. Perhaps the most systematic evidence comes from psychology, where much effort has been devoted to laboratory experiments that study how subjects respond to the exposure to pornographic material.[3] However, it is not clear whether this actually translates into higher crime rates in the field. The nature of both pornography consumption and expression of aggression is likely to be quite different

[3]Though some studies find no effect or a reduction in sexual aggression after exposure to pornography (see e.g. Zillman and Bryant, 1984), the majority of laboratory experiments do find that pornography increases sexual aggression, in particular as concerns violent pornography (Donnerstein et al., 1987; Allen et al., 1995).

in a controlled and supervised setting such as a laboratory (with no possibility of sexual relief), compared to more private settings.[4] To avoid the problems associated with laboratory experiments, some researchers have opted for observational data. A recent example is Kendall (2007). Using state-level panel data from the US, Kendall finds a negative association between internet subscription and rape incidences.[5] The hope in these observational studies is that any remaining bias is small after controlling for area fixed effects and observables. In contrast, we exploit a plausibly exogenous variation in internet use to deal with the standard problems of simultaneous causality and correlated unobservables. Our paper also complements existing studies in that we explore possible mechanisms through which internet use may affect sex crimes.

Our paper is also related to a broader literature in economics on the effect of media. Among others, Besley and Burgess (2002), Strömberg (2004), Gentzkow (2006), and DellaVigna and Kaplan (2007) provide evidence that media exposure affects political outcomes. Card and Dahl (2011) show that emotional cues provided by local NFL football games cause a spike in family violence. Relative to this set of papers that emphasizes the direct effect of media exposure, Gentzkow and Shapiro (2008) and Dahl and Dellavigna (2009) stress the indirect effect through time use. In Dahl and Dellavigna (2009), the substitution in activities induced by violent movies dominates the effect of content. This mechanism also operates in Gentzkow and Shapiro (2008), who show that the introduction of television in preschool years had positive effects on test scores for children of immigrants, who would otherwise have had less exposure to the English language. Our results suggest that the direct effect of internet use on the propensity for sex crime is likely to be important to understand its positive net impact on reported sex crime.

The paper unfolds as follows. Section 2 presents a conceptual framework to help structure our discussion of previous research related to the relationship between internet use and sex crime. 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. Section 7 investigates the alternative mechanisms through which internet use may affect reported sex crime, guided by our conceptual framework. Section 8

[4]See 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 laboratory experiments.

[5]Similarly, Baron and Straus (1984) study the circulation of eight pornographic magazines across US states. In line with our findings, they document a strong positive association, even when controlling for a number of possible confounders. Comparing the trend in rates of rape and assault between 1964 and 1984 in Denmark, Sweden, USA and West Germany, Kutchinsky (1991) finds that though access to pornography increased substantially over the period, rape rates did not grow compared to assault rates in any of the four countries.

summarizes and concludes with a discussion of policy implications.

2 Conceptual Framework

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

$$c = p \cdot \lambda$$

where $c = C/N$ is the sex crime rate. This setup can be justified using, for example, an underlying Poisson process where matches arrive at a given rate (Mortensen, 1988).

We are interested in how the internet user rate (per capita), i, affects the sex crime rate. In our data we observe reported sex crime rates c^* which relate to committed sex crime rates as follows

$$c^* = q \cdot c \tag{1}$$

where $q = Pr(report|crime)$.

We now discuss in turn how internet use may affect p, λ, and q, before linking the three channels together in a coherent framework.

The effect of internet on crime propensity

First, internet may affect the probability that a match leads to a sex crime, p. To fix ideas, let the propensity to commit a sex crime depend on a latent risk factor ρ, such that $p = p(\rho)$ where $\partial p/\partial \rho > 0$. Several theories link the consumption of pornographic material to the risk factor ρ. One theory is that pornography increases the risk of sex crimes because it triggers sexual arousal and aggression, degrades women or children to objects, and affects social and individual norms, i.e. $\partial \rho/\partial porn > 0$ (see e.g. Zillman, 1971; Dworkin, 1981; Mackinnon, 1995). An alternative theory emphasizes the potential cathartic effects of pornography consumption and argues that associated sexual relief (through masturbation) can decrease sexual aggression, i.e. $\partial \rho/\partial porn < 0$ (see e.g. Posner, 1992; Donnerstein et al., 1975).

In either case, internet use and the propensity to commit a sex crime can be linked as follows

$$\frac{\partial p}{\partial i} = \frac{\partial p}{\partial \rho}\frac{\partial \rho}{\partial porn}\frac{\partial porn}{\partial i}$$

Since internet reduces the pecuniary and non-pecuniary cost of pornography, we expect that $\partial porn/\partial i > 0$. Indeed, pornography abounds on the internet, and since its early infancy distributors of pornography have exploited internet's *'Triple-A Engine' effect* of *Accessibility*, *Affordability*, and *Anonymity* (Manning, 2005). In 2006, the US online adult entertainment industry controlled about 12 % of all internet sites (Ropelato, 2006), and pocketed 2.8 billion USD in revenue (Edelmann, 2009). Pornographic materials also abound on peer-to-peer networks, where for a period 73 % of all movie searches were for pornographic films on the once dominant downloading engine Kazaa (Kendall, 2007). The arrival of broadband internet further meant that distributors could offer explicit imaging of all kinds without excessive waiting times, in particular for movies. For example, downloading a 5-minute video clip of 35MB takes about 1.5 hours on a 56 kbps dial-up line compared to just 5 minutes on a 1 Mbps DSL broadband connection, and just seconds on high-speed broadband connections. Edelmann (2009) reports that "[a]s of June 2008, broadband users outnumber narrowband users 18 to 1 at sites that comScore (2008) classifies as adult".

Assuming that $\partial porn/\partial i > 0$, the sign of $\partial p/\partial i$ is informative of the sign of $\partial \rho/\partial porn$, and vice versa. However, there is little if any evidence on $\partial p/\partial i$, and most of what we know about $\partial \rho/\partial porn$ is either circumstantial or anecdotal. In the field, it has been argued that the pervasive use of pornography among sexual offenders indicates that $\partial \rho/\partial porn > 0$ (Gebert, 2003). The majority of laboratory experiments also find that pornography increases sexual aggression, in particular as concerns violent pornography (Donnerstein et al., 1987; Allen et al., 1995). However, it is not clear whether this actually translates into higher crime rates in the field. The nature of both pornography consumption and expression of aggression is likely to be quite different in a controlled and supervised setting such as a laboratory (with no possibility of sexual relief), compared to more private settings (see also Dahl and Dellavigna (2009); Levitt and List (2007a,b)).

The effect of internet on matching

The second channel through which internet can affect sex crimes is the rate at which potential offenders meet victims, λ. While there is no direct evidence on this channel, there is a related literature on how the internet may reduce uncertainty and frictions in the marriage market. Hitsch et al. (2010), for example, find that search frictions in online dating markets are very small, arguing that the matching rate depends on internet use.

Along the same lines, there are several ways in which the internet can increase the number of matches between potential offenders and victims. First, internet can reduce information constraints. For example, dating sites allow searches based on personal characteristics such as age, education and profession, but also on less readily observed dimensions such as sexual preferences, hobbies and religion. Internet may also expand the choice set, making matches available that were previously unavailable, perhaps due to a lack of connection in traditional social networks. Finally, the anonymity that internet offers can also expand the pool of people looking for a match: Stevenson and Wolfers (2007) cite data from Forrester Research suggesting that over a third of online personals are placed by someone who is currently married.

However, because more time spent online implies less time spent on other activities, internet can also cause a reduction in matching due to a incapacitation effect. There is also a potential substitution effect if social interaction through internet forums and social networks leads to less direct personal interaction.

The relationship between internet use and matching is therefore ambiguous. The relative weight of the various mechanisms will depend on the efficiency enhancing effect of the internet in the search process for mates, dates and friends. It will also depend on individual preferences and characteristics of the market participants. In light of the success of social networking, dating sites and chat rooms, there is surprisingly little evidence on these issues.

The effect of internet on crime reporting

Finally, the internet may affect the decision to report a sex crime and thus the reporting rate, q. Under-reporting of sex crime is widespread (Tjaden and Thoennes, 2000) and a common policy concern (see e.g. the September 14 Hearing of the Subcommittee on Crime and Drugs of the Judiciary Committee, US Senate). Because Norwegian police did not accept online crime reports, there is no direct link between internet use and the probability that a sex crime is reported. Government reports and historical accounts also suggest that online support and information for victims of sex crimes was limited in the period (NOU, 2008; Johnsen et al., 2010). However, internet may still decrease costs of reporting if it reduces the stigma of being the victim of a sex crime, for instance by facilitating contact with victims or support groups in informal online discussion rooms. Although we are not aware of any systematic studies of the relationship between crime reporting and internet use, we cannot rule out the possibility that $\partial q/\partial i > 0$.

Linking the channels together

We can now link the three channels together to relate reported sex crime to internet use as follows.

$$c^*(i,\rho) = q(i) \cdot c(i,\rho) = q(i) \cdot \lambda(i) \cdot p(\rho) \tag{2}$$

This shows first that committed sex crime is filtered through the probability to report, and second that the number of sex crimes depends on the matching between offenders and victims and on the propensity to commit a crime once matched.

To see how internet use may affect reported sex crimes, and how this relates to committed sex crime, we take the total derivative of (2) with respect to i:

$$\begin{aligned} \frac{dc^*}{di} &= c\frac{\partial q}{\partial i} + q\frac{\partial c}{\partial i} + q\frac{\partial c}{\partial \rho}\frac{d\rho}{di} \\ &= \underbrace{c\frac{\partial q}{\partial i}}_{\text{reporting}} + \underbrace{qp\frac{\partial \lambda}{\partial i}}_{\text{matching}} + \underbrace{q\lambda\frac{\partial p}{\partial \rho}\frac{d\rho}{di}}_{\text{direct effect}} \end{aligned} \tag{3}$$

Equation (3) highlights that the net effect of internet use on reported sex crime consists of three distinct components. The first term shows that we need to distinguish between the effect on committed sex crime and the effect on reporting through $\partial q/\partial i$. The following two terms decompose the effect on actual sex crime into two channels: the direct effect of internet use on the latent risk factor for sex crime, $d\rho/di$, and the effect of internet on the matching of potential victims and offenders, $\partial\lambda/\partial i$. In Section 7, we explore which of these three mechanisms are likely to be important for the estimated effect of internet use on reported sex crimes.

3 Data

Our analysis uses several data sources, which we can link through unique identifiers for each individual and every municipality.

Crime data

Our crime data come from administrative police registers containing complete records for the period 1993-2008 of (i) reported crime in each municipality and (ii) criminal charges and convictions for every resident. The data contain 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). A person is registered as "convicted" if at the end of the judicial process they are sanctioned to imprisonment, fines or a suspended sentence.

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. In particular, our data exclude reports that are dismissed by police and prosecutor, because they are likely to be fraudulent or concern acts that are not criminal.[6] 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 or convictions.

Statistics Norway has constructed sub-categories of crime and we rely on these definitions to construct crime categories that correspond closely to those used by the US FBI (see web appendix, Table A1). Our analysis focuses on three sex crime categories: overall reported sex crime, reported rape and reported child sex abuse. Rape and child sex abuse are the two main sub-categories of overall sex crime, making up nearly 25 and 45 %, respectively. Remaining sub-categories of overall sex crime include public exposé, indecency and other minor sexual offenses. It should be noted, however, that prostitution is legal in Norway, and is hence not included in the crime statistics. In addition, our definitions of sex crimes exclude crimes that are related to the distribution of pornography (though our results are unchanged when these are included).

To be concrete, the Norwegian Penal Code (§§ 191–208) defines a sexual act as the penetration of a body orifice by any object or body part. Rape is defined as (i) the instigation of a sexual act by violence or threat, or (ii) the performance of a sexual act on an individual that is incapacitated or otherwise unable to evade the act. In our data, completed rape under (i) and (ii) constitutes 81.3 % of all rapes,

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

attempted rape 13.5 %, and grossly negligent rape 1.1 %. The remaining 4.1 % are rapes committed with aggravating circumstances, such as those involving repeat offenders or group rapes, causing severe pain or defamation, or resulting in severe mental or bodily harm or death.[7] Child sex abuse is defined as (i) involving a child in the performance of a sexual act, or (ii) conspiring to promote sexually offensive behavior from a child, where a child is an individual aged below 16. In our data, 65.7 % of cases concern (i), 27.9 % of cases concern (ii), while 6.4 % of cases concern child incest. Under (i), 23.3 % of cases concern acts against children below 14, and 12.4 % acts against children below 10.

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 2000–2008. Throughout this paper, broadband is defined as internet connections with download speed that exceeds 256 kbit/s.

The data on broadband coverage come from the Norwegian Ministry of Government Administration. The ministry monitors the coverage and use 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 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. 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 number of households with broadband subscriptions in each municipality.

To characterize the sub-population that is induced by the program to subscribe to broadband internet, we also exploit detailed information on media use for a

[7] In nearly half the cases of rape reported at Oslo Police District during 2001–2010, there was no acquaintance between victim and offender prior to the incidence. In about 30 % of the cases, the offender turned out to be a friend, while in the remaining 20 % the offender was either a partner, an ex-partner, or a family member (Sætre and Grytdal, 2011). Moreover, one-third of the rapes happened at social events or parties, usually involving alcohol, while only one out of ten rapes were assaults. Unfortunately, we do not have data on offenders and victims in cases of sex crimes reported at other police districts.

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 use, with a response rate above 70 %. Importantly, the survey contains information on respondents age, sex, education, labor market status and some household characteristics, besides their internet use and connection speed. This survey data is available for the period 2000–2008.

Socio-demographic data

Our socio-demographic 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 use and coverage rates, and other socio-demographic variables expressed in per capita terms. We focus on the years 2000–2008, when broadband internet coverage went from virtually zero to almost 100 %. Table 1 displays summary statistics for the key variables across 422 municipalities. Detailed descriptions of each of the variables are given in the web appendix, Table A1.

Our main analysis uses 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 sex crime rates across municipalities, with standard deviations in parentheses. It is evident that the rate of sex crime is fairly stable, perhaps with a weakly increasing trend over the period. Note that sex crime rates in Norway are similar to those in other European countries. For instance, rape rates are almost identical to those in the UK, France and Germany, though somewhat lower than rates in the US (32 per 100,000) and Canada (78 per 100,000).[8]

The second panel of Table 1 shows means and standard deviations of internet coverage and user rates, defined as shares of households in a given municipality at the beginning of each year. We can see that there is no broadband use 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 %, whereas the mean user rate reaches almost 54 %. We find the largest variation in coverage rates across municipalities around 2004, while the user rates vary the most during the last three years. Section 4 describes in detail the spatial and temporal variation in internet use and coverage.

The third panel of Table 1 displays a number of other socio-demographic variables. 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. 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 web appendix, Tables A1 and A2). For brevity, we omit discussion of 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. The new technology was seen as essential for retaining competitiveness and achieving high standards of living in a global economy. 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)).[9] This section provides details about the program and describes the expansion of broadband internet.

[8]Source: Seventh United Nations Survey of Crime Trends and Operations of Criminal Justice Systems, covering the period 1998–2000.

[9]Public programs have been succesful in expanding internet also in other countries. For example, Goolsbee and Guryan (2006) show that the so-called E-Rate program had a major impact on internet access in public schools in the United States.

Table 1. Descriptive statistics

	Overall	2000	2002	2004	2006	2008
Crime rates (per 100,000)						
All sex crimes	54.5	47.6	54.0	55.7	53.2	57.5
	(60.4)	(52.3)	(60.2)	(59.9)	(56.6)	(60.2)
Rapes	12.8	10.1	13.3	13.4	11.8	14.7
	(20.6)	(18.2)	(23.6)	(19.8)	(17.5)	(21.7)
Child abuse	23.9	20.7	24.5	23.8	23.5	23.5
	(41.3)	(34.2)	(41.2)	(39.1)	(40.7)	(44.5)
Internet use and coverage						
User rate (%)	17.4	0.0	0.1	6.3	32.1	54.0
	(21.4)	(0.0)	(0.6)	(7.0)	(11.7)	(10.3)
Coverage rate (%)	46.2	0.0	11.4	39.8	86.0	97.6
	(42.1)	(0.0)	(23.0)	(30.0)	(12.1)	(4.6)
Control variables						
Local unemployment	1.5	3.1	1.4	1.7	1.1	0.8
rate (%)	(1.0)	(1.6)	(0.6)	(0.7)	(0.6)	(0.4)
Poverty rate (%)	3.9	3.6	4.0	4.2	4.1	2.9
	(1.2)	(1.0)	(1.1)	(1.1)	(1.3)	(1.0)
Urban settlement (%)	49.8	48.5	49.8	49.9	50.0	50.9
	(27.7)	(27.6)	(27.4)	(27.9)	(27.9)	(27.8)
Average years of	12.6	12.4	12.4	12.6	12.7	12.9
education	(1.5)	(1.4)	(1.4)	(1.4)	(1.4)	(1.4)
Immigrant population	4.8	3.9	4.2	4.7	5.2	6.3
share (%)	(2.8)	(2.5)	(2.5)	(2.6)	(2.8)	(3.3)
Police density	1.2	1.2	1.3	1.2	1.2	1.2
(per 1,000 cap.)	(0.8)	(0.8)	(0.8)	(0.8)	(0.8)	(0.8)
Overall crime rate	5029.2	5337.1	5319.9	5083.1	5011.1	4401.8
(per 100,000 cap.)	(2733.3)	(2729.5)	(2812.3)	(2842.4)	(2788.9)	(2518.5)

Note: Detailed descriptions of the variables are given in Table A1. Summary statistics for additional control variables are given in Table A2.

The program

The National Broadband Policy had 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 Norwegian 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. The stated aim was 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]

Supply and demand factors

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]

[10]During the period 1999–2005, the Høykom program 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.

[11]The Norwegian territory covers about 149,400 square miles, an area about the size of California or Germany, with around 13% and 6% of those regions' populations (in 2008), respectively. The country is dominated by mountainous or high terrain, as well as a rugged coastline stretching

The government reports describing the National Broadband Policy and the roll-out of broadband access points (see St.meld.nr. 38 (1997-1998); St.meld.nr. 49 (2002-2003); Ministry of Transport and Communications (2001, 2002)), suggest that the main *supply factors* determining the timing of roll-out are topographical features, and existing infrastructure (such as roads, tunnels, and railway routes), that slow down or speed up physical broadband expansion.[12] Based on the program accounts, we expect the *demand factors* that correlate with the timing of the roll-out to be related to public service provision, industry composition, income level and educational attainment, and the degree of urbanization in the municipality.

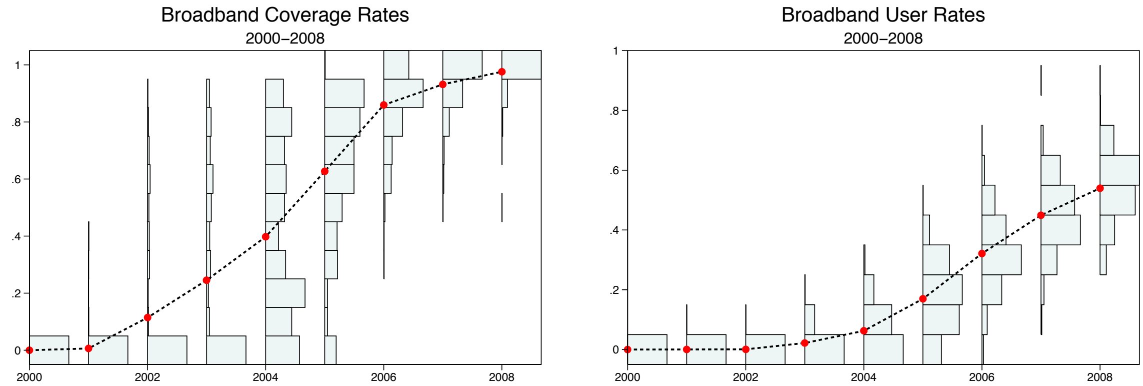
Descriptive statistics

Figure 1a 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 12 % in 2002 to 86 % 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 A1 in the web appendix shows geographic differences in the broadband coverage rates across municipalities in every year 2003–2007. 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 % of households in nearly all municipalities were covered by a broadband provider two years earlier.

about 1,650 miles, broken by numerous fjords and thousands of islands (making the coastline approximately 10 times longer if the length of the fjords were included). Because we control for municipality fixed effects, (unchanging) topographical features cannot be used as instruments for broadband internet coverage.

[12]The reason is that the transmission of broadband signals through fiber-optic cables required installation of local access points. In areas with challenging topography and landscapes, it was more difficult and expensive to install the local access points and the fiber-optic cables. Furthermore, the existing infrastructure mattered for the marginal costs of installing cables to extend the coverage of broadband within a municipality and to neighboring areas.



(a) Coverage rates **(b)** User rates

Figure 1. Internet user and coverage rates, averages and distribution across municipalities, 2000–2008

Note: Figures show the overall mean and distribution of broadband user rates (panel a) and coverage rates (panel b) across municipalities for each year during the period 2000–2008.

Figure 1b displays the distribution and averages of broadband user rates across municipalities between 2000 and 2008. Broadband use increases over time, with most of the increase taking place after 2003, and reaching an average user rate around 55 % by 2008. We find that there was a substantial increase in broadband use after the initial expansion of broadband coverage. Moreover, there was a considerable increase in broadband *use* also after 2006, even though there was less of an increase in broadband *coverage* in these later years.

5 Empirical strategy

Randomizing internet use is 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 coverage at the municipal level. The randomization breaks the correlation between internet coverage and unobserved determinants of sex crime. On the one hand, comparing sex crime rates between municipalities with and without internet coverage would give a reduced form estimate of the effect of internet coverage on sex crime. On the other hand, comparing internet use between these municipalities, would give a first stage estimate of the effect of internet coverage on internet use. Taking the ratio between the two would give an IV estimate of the effect of internet use on sex crime, using random variation in internet coverage as an instrumental variable for internet use.

The intention of our IV-approach is to mimic this hypothetical experiment. Our source of exogenous variation in internet use comes from the staged installation of broadband infrastructure, generating spatial and temporal variation in broadband coverage rates. Below, we describe how we use the roll-out of broadband coverage to estimate the effects of internet use on sex crimes.

IV model

For each municipality and every year, we instrument i_{kt}, the fraction of households with broadband internet subscriptions, with $z_{k,t-1}$, the fraction of households that are covered by broadband infrastructure in the previous year. The IV model is given by the following two-equation system, where (5) is the first stage and (4) is the second stage:

$$c^*_{kt} = \delta i_{kt} + x'_{kt}\beta + \alpha_k + \tau_t + \epsilon_{kt} \tag{4}$$

$$i_{kt} = \phi z_{kt-1} + x'_{kt}\lambda + \gamma_k + \theta_t + \eta_{kt} \tag{5}$$

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 α_k and year indicators τ_t. In our main analysis, the sample consists of all municipalities over the period 2000–2008. The standard errors are always clustered at the municipality level and robust to heteroskedasticity.

Unobservable determinants of sex crime that are fixed at the municipality level will be controlled for through the municipality indicators, just like common time shocks are absorbed by the year indicators. To check that the estimated effect is not driven by time-varying observable factors, we report results with and without a large set of time-varying controls. We will add two types of control variables. The first type is supply and demand factors that are expected to correlate with the broadband expansion; these variables are described in Section 4. The second type is factors that are likely to correlate with sex crime. To identify such factors, we relied on the existing literature on sex crime offenders, and on police reports that contain detailed information on the characteristics of victims, offenders and offenses. These sources suggests that demographics may be important: Offenders are predominantly young and male, and immigrants are overrepresented (Sætre and Grytdal, 2011); The access to voluntary sex partners may depend on the ratio of females to males, and on the ratio of married and cohabiting (Posner, 1992; Smith and Bennett, 1985).

In addition, the level of sex crime tends to correlate with socioeconomic attributes of the area, such as unemployment, poverty, overall crime, income and education (Blau and Blau, 1982; Smith and Bennett, 1985; Sætre and Grytdal, 2011). Lastly, the level of deterrence may affect the decision to offend (Posner, 1992).

The full set of control variables are listed and described in Table A1. They include population shares by age-group, gender and immigrant status; rates of divorced, students, poverty, welfare dependency, and unemployment; centrality of residence, education level, income level, industry composition, and public services by category; as well as police density and the total crime rate (excluding sex crimes) at the start of the year.[13]

Assessing the IV model

Our IV model – which controls for municipality-specific fixed effects (and time effects) – is motivated by two features of the program. First, most of the supply and demand factors tend to vary little over time. Second, the timing of the roll-out is unlikely to co-vary with key correlates of sex crime.

To investigate whether the data are consistent with these program features, we first regress broadband coverage rate on municipality fixed effects, time fixed effects, and time-varying supply and demand factors. We find that 86% of the variation in broadband coverage can be attributed to time-invariant municipality characteristics and common time effects, while less than 1% of the variation in broadband coverage can be attributed to time-varying supply and demand factors.

Second, we examine the relationship between the timing of broadband roll-out and baseline municipality characteristics. To this end, we estimate the following equation

$$\Delta z_{kt} = \gamma_k + [\theta_t \times m_{k,2000}]' \psi_t + \chi_{kt} \tag{6}$$

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 A2 plots the estimated coefficients from the vector ψ_t for every t (and the associated 95 % confidence intervals). Consistent with the discussion of supply and demand factors in Section 4, our results indicate that

[13] We allow for a flexible functional form by including a large set of dummies for different values of the controls listed in Table A1. Note that police resources and other reported crimes are potentially endogenous to sex crimes. It is therefore useful to include them separately, and comforting to note that estimates barely move when they are included.

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.

Third, we follow the method proposed by Altonji et al. (2005a) which uses selection on observables as a guide to selection on unobservables. We begin by examining the extent to which the instrument is correlated with the part of the outcome explained by the observables. To this end, we estimate $cov(x'_{kt}\beta, z_{kt-1}|\alpha_k, \tau_t)/var(z_{kt-1}|\alpha_k, \tau_t)$.[14] We find that this association is close to zero and insignificant, at -.012, .000, and .012 for all sex crime, rape, and child sex abuse, respectively. The correlation between $x'_{kt}\beta$ and z_{kt-1} is also very low, at $-.016$, .002, and .022 for all sex crime, rape, and child sex abuse, respectively (controlling for year and municipality fixed effects). These results are consistent with our identifying assumption that the timing of the roll-out is uncorrelated with unobserved time-varying factors of sex crime. To quantify the possible bias from selection on time-varying unobservables, we apply the method of Altonji et al. (2005a). The results from their test are reported and discussed in detail in Section A.2 in the web appendix. The test lacks sufficient power due to the difficulty of predicting variation in sex crime. However, if we were to interpret the noisy point estimates based on the full set of controls, they suggest that the time-varying unobservables (a) understate the effects of internet on overall sex crime, (b) have little impact on the estimated effect on rape, and (c) can potentially generate too large bias to draw conclusions about the effect on child sex abuse.

Although these three pieces of evidence broadly support our IV model, we will further challenge the validity of the instrument by probing the stability of the IV estimates to alternative specifications, and by performing placebo tests.

6 Net effect of internet use on sex crime

This section begins by providing a graphical depiction of how we use the roll-out of broadband coverage to estimate the effects of internet use on sex crimes, before turning to a more detailed regression-based analysis. Next, we take advantage of survey data on individuals' broadband subscription and accessibility to help us interpret the IV-estimates. We then consider the economic significance of our estimates by comparing the actual sex crime rate to the estimated counterfactual

[14] We follow Altonji et al. (2005b) and estimate $x'_{kt}\beta$ under the null-hypothesis of no effect of internet on sex crimes ($\delta = 0$ in Equation (4)).

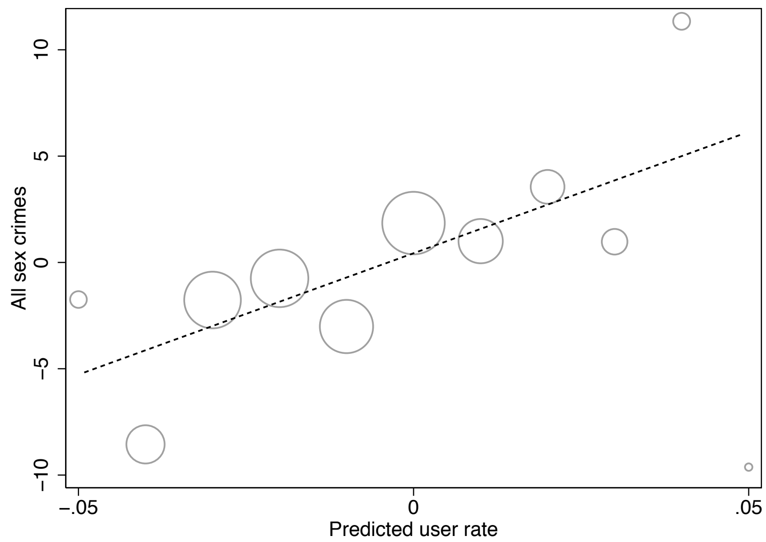


Figure 2. Predicted user rates and reported sex crime.
Note: Bins are based on the predicted user rates with bin size = 0.01. The size of the circle indicates the no. of municipalities in each bin. Predicted user rates are from the first stage (Equation (5)) after removing time and municipality fixed effects.

sex crime rate in the absence of the expansion of broadband internet. Lastly, we show that the estimated effects are remarkably robust to the usual robustness checks performed in IV studies.

Graphical evidence

Figures 2 and 3 provide a visual representation of our IV approach, after taking out municipality and year effects. Figure 2 draws a scatter plot of the predicted user rates from equation (5) against the overall sex crime rate. To reduce noise, we report the predicted user rates in bins of size .01, and let the size of the circle represent the number of municipalities in each bin. The figure shows a strong association between internet use and the sex crime rate, with a correlation of .12. It is also worth noting that the linear functional form gives a fairly good approximation of the observed relationship.

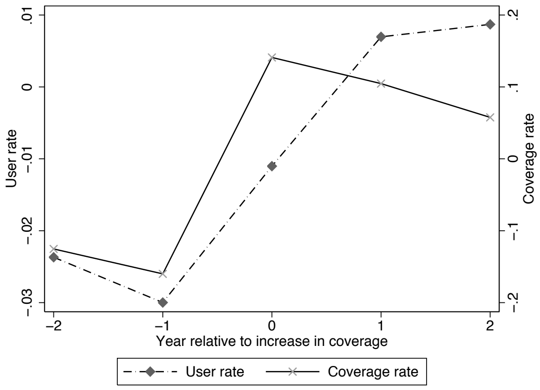
Figure 3 breaks down the observed relationship between internet use and the sex crime rate into the first stage and the reduced form. It is helpful to recenter the data such that both cause and effect occur at time zero: For the coverage rate, time zero represents the year with the strongest growth in broadband coverage rate; for the user rate and the sex crime rate, time zero represents the year after the strongest growth in broadband coverage rate. Panel (a) shows that the increase in coverage rate from time -1 to time zero is associated with a substantial rise in the user rate from time -1 to time zero. This suggests a strong first stage effect on internet use of the increase in internet coverage in the previous year. Panel (b) shows that the increase in coverage rate from time -1 to time zero is associated with a substantial rise in the sex crime rate from time -1 to time zero. This suggests a strong reduced form effect on sex crimes of the increase in internet coverage in the previous year. Similar pictures for rape and child sex abuse are shown in Panels (c) and (d) of Figure 3. In contrast, there is no evidence of changes in the set of time-varying socio-demographic controls from time -1 to time zero, as shown for police density or poverty rates in Panels (e) and (f) and for a large set of covariates in the web appendix, Figure A3.

Main results

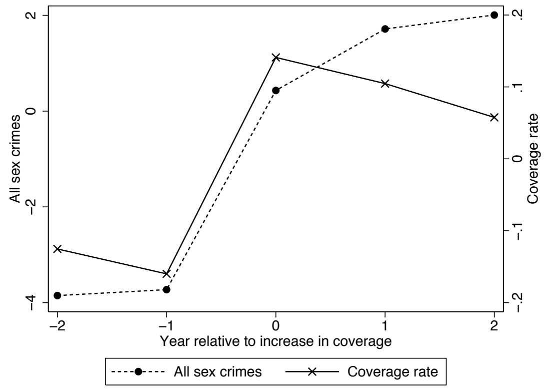
Table 2 reports results from the IV-model given by equations (4) and (5). Column 1 shows the estimates from the specification with only municipality fixed effects and time dummies. The first stage instruments the user rate in a municipality in year t with the coverage rate at the start of the year. The first stage coefficient on the coverage rate is about 0.13, which is also the size of the complier group since the instrument ranges from 0 (nobody covered) to 1 (everybody covered). It implies that a 10 percentage point increase in internet coverage induces (an additional) 1.3 % of the population to use internet within the next year. The first stage is strong, with an F-statistic around 323, which means weak instrument bias is not a concern.

Turning to the second stage results, column 1 shows that when the internet user rate increases by 1 percentage point, the overall sex crime rate increases by on average 1.15 crimes per 100,000 inhabitants. The next two rows report the IV-estimates for rape and child sex abuse. We find that a 1 percentage point increase in internet use causes an increase in rapes of 0.38 and an increase in child sex abuse of 0.62, per 100,000 inhabitants.

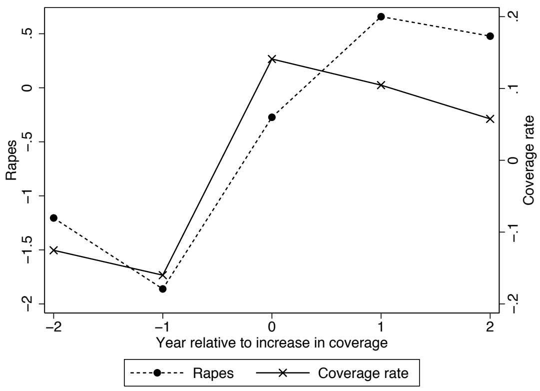
Columns 2–5 in Table 2 include control variables to see to what extent the IV-estimates of internet use on sex crime are sensitive to the inclusion of time-varying observable factors. We first add demographic controls in column 2, which does not affect the first stage estimate, nor the estimated effects on overall sex crime rate and rapes. Only the effect of internet use on child sex abuse drops by about 30 % but remains substantial, although it is no longer significant at the 10 %-level. Another potential threat to our estimates is changes in the cost of committing sex crimes that are correlated with the roll-out of broadband coverage. It is therefore reassuring to see in column 3 that our estimates barely move when we control for the number of policemen per capita in the municipality. A further worry is that there could



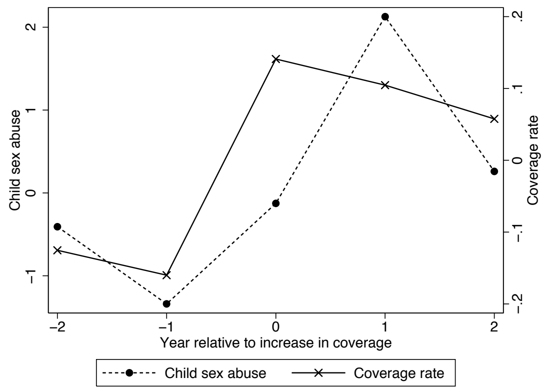
(a) First-stage



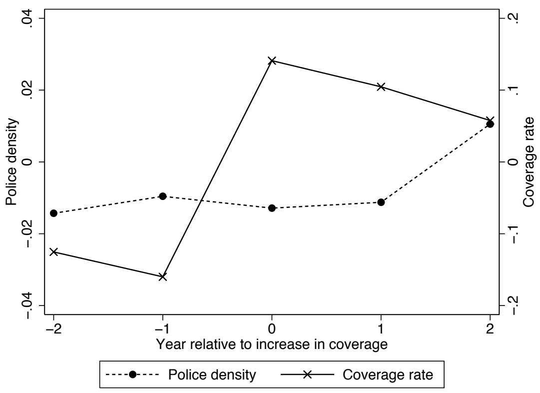
(b) Reduced form: All sex crimes



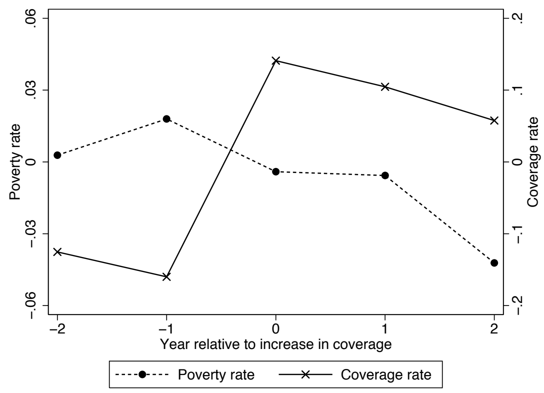
(c) Reduced form: Rape



(d) Reduced form: Child sex



(e) Reduced form: Police density



(f) Reduced form: Poverty rate

Figure 3. Graphical illustration of the IV approach.

Note: For each municipality, we recenter the data such that both cause and effect occur at time zero: For the coverage rate, time zero represents the year with the strongest growth in broadband coverage rate; for the user rate, the sex crime rates and the municipality characteristics, time zero represents the following year. Variables are residuals from a regression on time and municipality fixed effects. The range of the right y-axis is 1 standard deviation of broadband coverage. The range of the left y-axis is 1/10th standard deviation of the variable. The sample consists of the 413 municipalities (of 422) for whom we can construct a balanced sample of 2 years around 0.

Table 2. Instrumental variables estimates of internet use on sex crime

	Baseline specifications					Pre-reform trend		Covariate Interactions	
						Linear	Quadratic	Time trend	Time FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All sex crimes	1.152**	1.181**	1.180**	1.231**	1.389**	1.415**	1.424**	1.307**	1.473
	(0.518)	(0.537)	(0.537)	(0.536)	(0.562)	(0.565)	(0.562)	(0.607)	(1.017)
Rape	0.378**	0.384**	0.384**	0.394**	0.419**	0.421**	0.427***	0.427**	0.599*
	(0.163)	(0.165)	(0.165)	(0.165)	(0.168)	(0.169)	(0.169)	(0.183)	(0.314)
Child sex abuse	0.616*	0.436	0.436	0.459	0.583	0.603	0.632	0.515	0.747
	(0.355)	(0.373)	(0.373)	(0.373)	(0.391)	(0.396)	(0.397)	(0.424)	(0.671)
First-stage:									
$Coverage_{k,t-1}$	0.131***	0.128***	0.128***	0.128***	0.124***	0.124***	0.124***	0.116***	0.096***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)
F-value (instr.)	323.2	324.7	324.5	327.2	319.8	312.8	313.1	331.6	123.8
Demographics		✓	✓	✓	✓	✓	✓	✓	✓
Police density			✓	✓	✓	✓	✓	✓	✓
Other crimes				✓	✓	✓	✓	✓	✓
Broadband					✓	✓	✓	✓	✓

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the internet user rate. Regressions are based on 422 municipalities × 8 years = 3376 observations. All regressions include municipality fixed effects and year dummies. Controls are discussed in Section 3 and detailed in the web appendix, see Table A1. Standard errors are heteroskedasticity robust and clustered at the municipality level. * $p < 0.10$, ** < 0.05, *** $p < 0.01$.

be some secular trend in criminal activity that is correlated with the broadband expansion. To address this concern, column 4 adds the total crime rate (excluding sex crimes) as a control. Again the estimates barely move. Lastly, column 5 adds supply and demand factors that are expected to correlate with the broadband expansion. Again we find that the estimated effects are quite similar to our baseline estimates; if anything, the inclusion of time-varying demand and supply factors indicate that the baseline IV specification understates the positive effect of internet use on sex crime.

Characteristics of compliers

Following Imbens and Angrist (1994), our IV estimates should be interpreted as the local average treatment effect (LATE) of internet use on sex crime for the subpopulation of compliers that is induced to use internet because of the increase in coverage in the previous year. To learn about the characteristics of the complier group, we use our survey data on individual's internet use in the period 2000–2008 to estimate the first stage of the IV for different gender–age groups. We next compare the sex crime rates for the compliers to those in the population at large.

The first stage estimates for different gender–age groups are shown in the second column of Table 3. We see that the expansion of broadband internet has a stronger effect on the young than the old, and that effects are also stronger among males than among females. The proportion of the compliers in a given gender–age group are then calculated as the ratio of the first stage for that subgroup to the overall first stage, multiplied by the proportion of the population in the gender–age group (Angrist and Pischke, 2009). Column 1 displays the proportion of the population in each gender–age group, whereas column 3 shows the distribution of the compliers by gender and age. Column 4 displays the relative likelihood of an individual belonging to a particular gender–age group, in the complier group compared to the population at large. While 29 % of the population are males aged 16–44, more than 47 % of the compliers are males in this age-group. When including females, around 82 % of the compliers are aged 16–44, compared to just 57 % in the population at large. We also see that males are overrepresented among the compliers in every age group compared to the population at large.

Columns 5–7 of Table 3 report the probability of being charged with a sex crime for the different gender–age groups prior to the internet expansion (year 2000). While sex crimes 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 three times as likely to be charged with a sex crime than men aged 45–79. We calculate re-weighted sex crime rates to get a rough idea of the extent to which the complier group is more prone to sex offenses than the population at large. We simply take the weighted average of the sex crime probabilities by gender-age group using the complier shares as weights. We find that the re-weighted sex crime rate is 46% higher than the actual sex crime rate in the population at large. This illustrates that the estimated effects pertain to a subgroup of early adopters of broadband internet which is relatively prone to sex crime, and that we need to be cautious in extrapolating these LATEs to the average effects of internet use for the entire population.

Economic significance

To put the size of our IV estimates into perspective, we calculate the counterfactual sex crime rate that would have occurred in the absence of broadband expansion. The counterfactual sex crime rate is given by actual crime minus the predicted effect of internet use on sex crime among compliers. In each year, the predicted effect of internet use on sex crime is calculated as the percentage point increase in the coverage rate in the previous year multiplied by the first stage times the LATE (Angrist and Pischke, 2009). Overall, the estimates suggest that about about 3.2 % of the total number of rapes and 2.5 % of the total number of sex crimes and child sex abuses that occurred between 2000 and 2008 would have been avoided if broadband internet had not been introduced.

Figure 4 breaks down the overall picture and shows the actual time trends for our various outcomes, as well as the predicted counterfactual time trends based on our baseline IV estimates (cf. column 4 of Table 2). Panel (a) reports these for all sex crimes, whereas panels (b) and (c) focus on rape and child sex abuse. Until 2002, the actual and counterfactual trends coincide because of very low increases in coverage. From 2002, the coverage rates quickly increased. We then see a substantial difference between the actual and counterfactual sex crime rates. The difference peaks in 2006, when we estimate that roughly three out of 53 sex crimes per 100,000 inhabitants would have been avoided if broadband internet had not been introduced. Our analysis also suggests that internet use in 2006 explains about one out of twelve rapes and one out of 23 child sex abuses, per 100,000 inhabitants. This implies increases in 2006 of 6.5 %, 8.7 %, and 6 % in the odds of a reported sex crime, rape, and child sex abuse, respectively.

Robustness analysis

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

Table 3. Compliance and sex crime charges by gender and age

	Compliance				Crime charges		
	$P\left[X=x\right]$	First stage	$P\left[X=x \vert I_{1i}>I_{0i}\right]$	$\frac{P[X=x \vert I_{1i}>I_{0i}]}{P[X=x]}$	All sex crimes	Rape	Child sex abuse
Men							
16–24	0.098	0.371	0.226	2.310	66.73	13.35	44.49
25–44	0.188	0.208	0.244	1.295	44.50	7.98	16.11
45–79	0.215	0.083	0.111	0.517	20.15	2.63	7.38
Women							
16–24	0.089	0.339	0.187	2.111	1.10	0.19	0.56
25–44	0.195	0.130	0.158	0.809	0.56	0.00	0.19
45–79	0.215	0.055	0.074	0.342	0.18	0.00	0.14
Overall	1.000	0.161	1.000	1.000	19.48	3.39	9.09

Note: Column 1 reports the distribution of the population by gender and age, $P\left[X=x\right]$, based on microdata for respondents from Media User Surveys for the period 2001–2008. Also using the Media User Surveys, we perform separate estimations of the first stage equation (5) for each gender–age group. The first-stage coefficients are reported in column (2). All regressions include municipality fixed effects and year dummies. Column (3) reports the distribution of compliers by gender and age, $P\left[X=x|I_{1i}>I_{0i}\right]$, calculated as (first stage coefficient for the gender-age group × pop. share) divided by the overall first stage coefficient. Column (4) shows the relative likelihood of an individual belonging to a particular gender–age group, in the complier group compared to the population at large. Crime rates given in columns (5)–(7) are based on criminal records data for all Norwegian residents aged 16–79 for year 2000, and are measured as criminal charges per 100,000 persons.

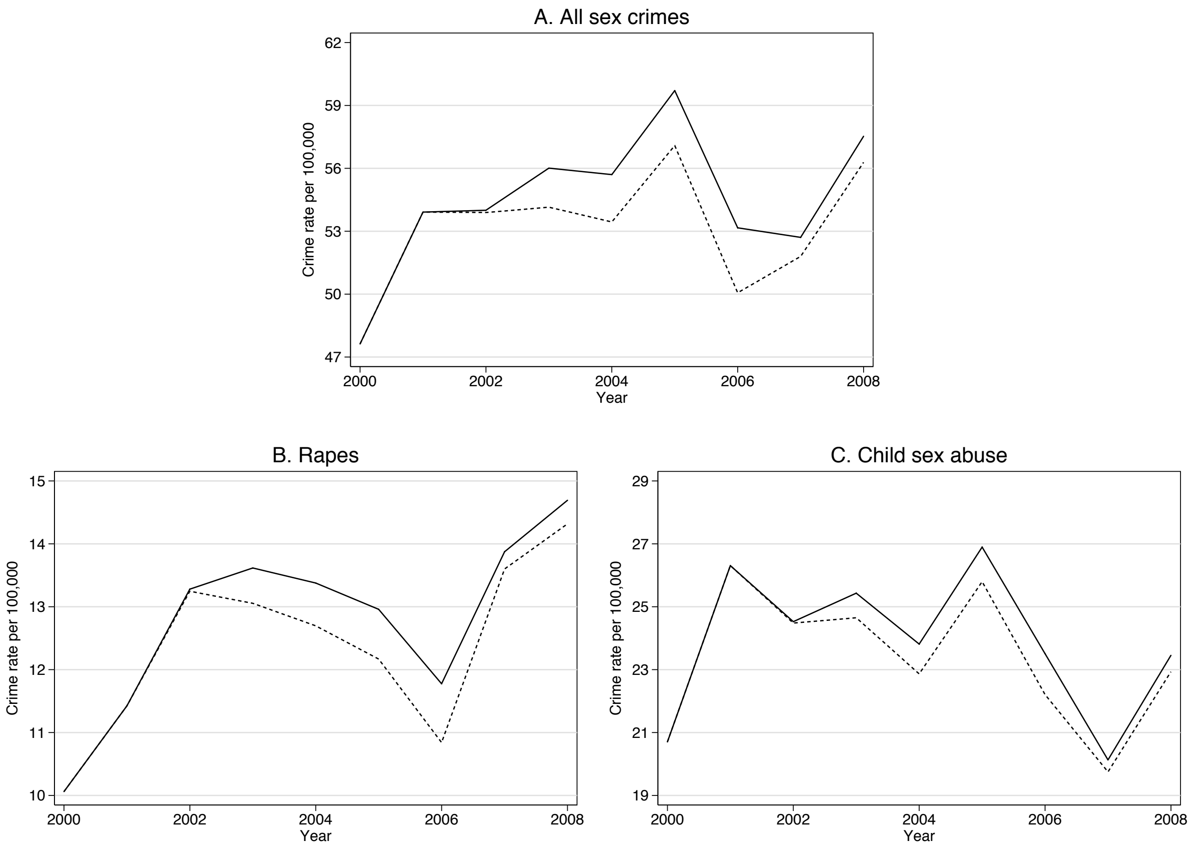


Figure 4. Actual and predicted time trends
Note: Solid line = actual crime rate. Dashed line = counterfactual crime rate in the case of no broadband expansion. The counterfactual crime rate is given by the actual crime rate minus the predicted effect of internet use on crime. In each year, the predicted effect of internet use on crime is calculated as the increase in the coverage rate in the previous year multiplied by the first stage times the LATE.

confidence in the IV-results. An important requirement for our IV-approach to be valid is that the expansion of broadband internet is unrelated to different underlying time trends in sex crime across municipalities. As a first check for this possibility we estimate municipality-specific trends using data covering the period prior to the internet expansion, namely 1993–2000. For each municipality we obtain a slope estimate $\hat{\nu}_k$. We then extrapolate pre-expansion time trends in our specification (both the first and the second stage) as follows

$$c^*_{kt} = \delta i_{kt} + x'_{kt}\beta + \alpha_k + \lambda_1 \hat{\nu}_k t + \lambda_2 \hat{\nu}_k t^2 + \tau_t + \epsilon_{kt} \tag{7}$$

which will take into account any variation in our instrument that coincides with pre-existing trends in the outcome. Columns (6) and (7) in Table 2 report the estimates based on these specifications, which are nearly identical to our baseline estimates.[15]

A second and related test follows Duflo (2001) in interacting baseline (year 2000) covariates either with a linear time trend

$$c^*_{kt} = \delta i_{kt} + x'_{kt}\beta + \alpha_k + t \textstyle\sum_j \lambda_j x_{(k,2000),j} + \tau_t + \epsilon_{kt} \tag{8}$$

or with time dummies

$$c^*_{kt} = \delta i_{kt} + x'_{kt}\beta + \alpha_k + \tau_t \textstyle\sum_j \lambda_j x_{(k,2000),j} + \epsilon_{kt} \tag{9}$$

In doing so, we allow the expansion of broadband internet to be related to different underlying time trends in sex crime across municipalities, depending on their pre-reform characteristics. The final two columns in Table 2 report the estimates based on equations (8) and (9). Again we find that these are close to our baseline estimates; if anything, the robustness check of interacting the covariates with time dummies indicate that the baseline IV specification understates the positive effect of internet use on sex crime.

We have also performed two placebo tests. The first replaces current sex crime rates and control variables with the same variables from eight years before, while maintaining broadband coverage rates from the period 2000-2008. In doing so, this pre-reform placebo test places the internet expansion in the period before the actual roll-out. Since there was no broadband internet expansion during the period 1993–2000, significant estimates would suggest that the instrument is correlated with underlying municipality-specific trends in sex crime. Table 4 reports the results

[15] We have also included municipality-specific time trends for the expansion period 2000-2008 directly in the equations (4) and (5), finding similar results.

using these pre-reform outcomes, and compares them to the reduced form effects from the baseline IV specification. It is reassuring to find no evidence of a significant correlation between the roll-out of broadband internet and the pre-reform trends in sex crime.

The second placebo test examines whether next year's internet coverage affects current sex crimes: If it did, this would suggest that there was some omitted variable causing both sex crimes and the roll-out of broadband internet. We regress the sex crime rate in year t on internet coverage in the following year $t+1$. We control for the coverage rate in year $t-1$, our instrument, to ensure that we are indeed estimating the effect of next year's internet use through future variation in our instrument.[16] Columns 3-5 of Table 4 show the results from this placebo test. It is reassuring to find in column 4 that next year's internet coverage does not affect the current rates of overall sex crimes, rape or child sex abuse. The last column includes a formal test of the difference in the effects on sex crime in year t between our instrument and internet coverage in the year $t+1$. We can reject equality at a 5% significance level for both overall sex crime and rape. For child sex abuse, we do not have enough precision to reject that the estimated effects are equal, although the point estimates differ substantially.

Next, we make sure that our estimates are not driven by observations with extreme values for sex crimes or zeros (see web appendix, Table A4, Columns (2) and (3)). Furthermore, we examine whether our results are driven by secular changes between urban and rural areas that may have coincided with the internet expansion. This may also be a concern since we found in Figure A2 that broadband internet arrived earlier in more central and larger municipalities. When we drop the five large cities from our analysis (Oslo, Bergen, Trondheim, Stavanger and Kristiansand), the estimates barely move (see web appendix, Table A4, Column (4)).

In addition, we check whether our estimates are affected by spillovers or sorting. In our analysis, crime is measured at the municipality level. This will give consistent estimates as long as sex crimes are committed in the same municipality where internet is used. In principle, sex crimes could be committed in, for example, neighboring municipalities. This would probably lead to a downward bias. To check for such spillovers we broaden our outcome measure to include sex crimes that are committed in adjacent municipalities, while controlling for the broadband internet expansion there. These estimates are very close to our baseline (see web appendix, Table A4,

[16]Since the coverage rate is cumulative, not controlling for coverage rates in the previous year would raise the concern that we are incorporating variation that precedes the sex crime rate. 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).

Table 4. Placebo tests

	Baseline	Pre-reform outcomes	Next year coverage		
			Instrument	Future	Difference
All sex crimes	0.173**	-0.091	0.171**	-0.023	0.194**
	(0.070)	(0.080)	(0.068)	(0.062)	(0.090)
Rapes	0.052**	-0.016	0.051**	-0.023	0.074**
	(0.021)	(0.021)	(0.024)	(0.026)	(0.034)
Child abuse	0.072	-0.082	0.074	0.031	0.043
	(0.049)	(0.066)	(0.048)	(0.047)	(0.068)

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the broadband coverage rate. Regressions are based on 422 municipalities × 8 years = 3376 observations. 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 coverage rates are still from the period 2001–2008. In columns (3)–(5) we display results from regressions where we regress sex crimes in year t on coverage rate in year $t+1$ (future coverage), while controlling for coverage rates at $t-1$ (our instrument). Standard errors in column (5) are bootstrapped with 200 replications. All regressions include municipality fixed effects, year dummies and controls listed in Table A1. Standard errors are heteroskedasticity robust and clustered at the municipality level. * $p < 0.10$, ** < 0.05, *** $p < 0.01$.

Column (5)).

Sorting across municipalities could also affect the interpretation of our results. In theory, individuals at risk for committing sex crimes may move to municipalities that expand broadband internet. However, the information requirements to generate such behavior are quite strict, since people would need information on the exact timing of the infrastructure expansion. Moreover, the cost of moving is probably high relative to the benefits, since broadband internet becomes available across the country after a few years, making the benefits short-lived. Also, most municipalities would have at least some coverage even in early periods, suggesting that potential moves may just as well happen within the municipality of residence. We nevertheless check whether our instrument correlates with movements into and out of the municipality, by estimating equation (5) with population growth (in percent) as the dependent variable. We estimated this effect for the population at large and separately for younger men and women who are overrepresented in the complier group. The results confirm that confounding effects due to sorting are unlikely to be a concern (see web appendix, Table A5).

Lastly, we investigate whether habit formation in sex crime and internet use will cause bias in our first stage and reduced form equations. We can introduce habit formation in the crime (second stage) and internet use (first stage) equations as follows:

$$c^*_{kt} = \kappa c^*_{k,\,t-1} + \delta i_{kt} + x'_{kt}\beta + \alpha_k + \tau_t + \epsilon_{kt} \tag{10}$$
$$i_{kt} = \xi i_{k,\,t-1} + \phi z_{k,\,t-1} + x'_{kt}\lambda + \gamma_k + \theta_t + \eta_{kt} \tag{11}$$

which gives the reduced form equation

$$c^*_{kt} = \kappa c^*_{k,\,t-1} + \delta\phi z_{k,\,t-1} + \delta\xi i_{k,\,t-1} + x'_{kt}(\beta + \delta\lambda) + (\delta\gamma_k + \alpha_k) + (\delta\theta_t + \tau_t) + (\delta\eta_{kt} + \epsilon_{kt}) \tag{12}$$

Using the within estimator to estimate (11) and (12) will in general lead to inconsistent estimates of the coefficients on the lagged dependent variables. We are, however, concerned with the estimation of the coefficients on our exogenous instrument $z_{i,\,t-1}$. The question is whether the presence of habit formation biases these estimates. Equation (24) in Nickell (1981) shows that if the exogenous regressors are uncorrelated with the lagged dependent variable, the within estimator of the coefficients on the exogenous variables is consistent.

As a first step to investigate whether presence of habit formation would bias our IV estimates, we therefore estimate the correlation between our instrument, $z_{k,\,t-1}$, and the lagged dependent variables in the first-stage and reduced form equations. We estimate the corresponding correlation by estimating the following regression

$$i_{k,\,t-1} = \alpha_z z_{k,\,t-1} + x'_{kt}\alpha_x + \alpha_k + \alpha_t + u_{kt}$$

A statistically significant estimate of α_z indicates that including habit formation will bias our first-stage estimate. Similarly, for the reduced form equations we estimate the following

$$c^*_{k,t-1} = \alpha_z z_{k,t-1} + \alpha_I i_{k,t-1} + x'_{kt}\alpha_x + \alpha_k + \alpha_t + u_{kt} \tag{13}$$

where α_z is informative about the presence of bias in our reduced form equations. Table 5 reports the corresponding estimates of α_z. As can be seen from the table, there is little, if any, correlation between our instrument and the lagged dependent variable in both the first-stage and the reduced form equations. This suggests that we can estimate equations (10) and (11) using the within estimator. Although

Table 5. Test for Nickell bias on exogenous regressor

	$\hat{\alpha}_z$	s.e.	p-value
Dependent variable			
$i_{k,\,t-1}$	-0.010	0.008	0.21
$c^*_{k,\,t-1}$:			
- All sex crimes	0.089	0.059	0.13
- Rapes	0.009	0.024	0.71
- Child abuse	0.065	0.037	0.09

the estimates of ξ and κ will most likely be inconsistent, we will obtain consistent estimates of the coefficients on our exogenous instrument. Table 6 shows the results, with columns 1, 3 and 6 reporting our benchmark results. Consider first the first-stage results in columns 1 and 2. The second column includes the additional control for lagged internet use, but our first-stage estimate barely changes from 0.124 to 0.128, confirming the small and insignificant correlation in Table 5. Habit formation in internet use therefore does not bias our first-stage results.

Column 4 in Table 6 adds the lagged outcome as an exogenous regressor to the reduced form equations. We can compare these estimates to those in column 3, which ignore the possibility of habit formation in sex crime. We see that the estimates barely move after controlling for the lagged outcome. Habit formation in crime therefore does not appear to bias our reduced form effects. As a final check, column 5 adds lagged internet use as a regressor. The estimates in this column therefore allow for habit formation in both internet use and sex crime. Again, the results are virtually unchanged compared to column 4 reinforcing the conclusion that habit formation does not appear to bias our results. For completeness, we also report the IV results allowing for habit formation in columns 6–8.

7 Mechanisms

This section explores the three mechanisms highlighted in Section 2, related to reporting, matching of potential offenders and victims, and the latent risk factor for sex crime.

Reporting behavior

To begin our investigation of reporting behavior we consider data on charges and convictions over our estimation period, 2000–2008. Columns 1–2 in Table 7 reports effects of internet use on charges and convictions for sex crimes, estimated using

Table 6. Allowing for habit formation

	FS		RF			IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All sex crimes	0.124***	0.128***	0.173**	0.179**	0.179**	1.389**	1.444**	1.403**
	(0.007)	(0.006)	(0.070)	(0.072)	(0.072)	(0.562)	(0.577)	(0.558)
Rapes	0.124***	0.128***	0.052**	0.053**	0.053**	0.419**	0.425**	0.416**
	(0.007)	(0.006)	(0.021)	(0.022)	(0.022)	(0.168)	(0.173)	(0.167)
Child abuse	0.124***	0.128***	0.072	0.080	0.080	0.583	0.641	0.627
	(0.007)	(0.006)	(0.049)	(0.051)	(0.051)	(0.391)	(0.406)	(0.395)
Extra controls:								
$y_{k,t-1}$				✓	✓		✓	✓
$i_{k,t-1}$		✓			✓			✓

Table 7. Alternative outcomes

	All sex crimes		Other reported crimes		
	Charges	Convictions	Overall	Vandalism	Theft
Effect of internet use	1.025*** (0.374)	1.019*** (0.374)	-4.281 (9.808)	-0.115 (1.332)	-3.944 (6.062)
Dep. mean	20.6	20.4	4933.8	333.4	2192.4

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the internet user rate. Standard errors are heteroskedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities × 8 years = 3376 observations. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. * $p < 0.10$, ** < 0.05, *** $p < 0.01$.

our baseline IV strategy. Importantly, we find that internet use affects not only the recording of sex crimes, but also has a significant and sizable effect on the number of offenders that are brought to justice.

We see two potential ways for internet use to increase both reports, charges and convictions of sex crimes, *without* increasing the number of sex crimes:

1. Internet leads to more sex crime reports by lowering the cost of reporting (e.g. by providing information or facilitating contact with support groups), and the new reports lead to more charges and convictions – we call it the "just reporting" hypothesis.

2. Internet raises the probability that a reported sex crime leads to a charge and conviction (e.g. by making the investigation more effective or through changes in the modes of contact between perpetrator and victim), and thereby induces more victims to report crimes – we call it the "paper trail" hypothesis.

To assess the "just reporting" hypothesis, we start by exploring whether the internet is likely to have decreased the costs of reporting. To this end, we have carefully reviewed government reports and historical accounts: These suggest that both online contact points and online information for victims of sex crimes were quite limited during this period. In particular, there was no possibility for filing police reports online, and there was no web portal serving as a contact point between victims and authorities or support groups (NOU, 2008; Johnsen et al., 2010). To investigate empirically whether changes in reporting costs are likely to be important, we note that a decrease in the cost of reporting should induce reports of weaker cases on average. This would imply that the share of reports resulting in charge or conviction should go down. However, using our baseline IV strategy on our extended data, we find no effects on the charge rate or the conviction rate (cf. columns 1–2 in Table 8, and Figure A4 in the web appendix). We also find no effect of internet use on the time elapsed between the incident occurred and the report is submitted to the police (cf. the last column in Table 8).

To assess the "paper trail" hypothesis, we use information from the police records on what grounds investigations were closed. These data tell us whether the perpetrator was unknown at the end of the investigation, and whether the investigation was closed due to lack of evidence. To check whether internet is indeed helpful to the police, we use our baseline IV model with these as dependent variables. Table 8 shows that there is no evidence that internet use increases the likelihood of identifying the perpetrator, nor that it lowers the likelihood of closing a case due to lack of evidence.

The lack of direct evidence of a paper trail effect from police reports is also consistent with the fact that we do not find effects on the *ratios* of charges to reports and convictions to reports in Table 8. Without internet, about two out of three reports do not result in a charge or conviction. One would expect that these discarded reports would also benefit from an internet induced paper trail effect; either because the investigation becomes more effective or through changes in the modes of contact between perpetrator and victim. However, the estimated effects of internet on the charge rates imply that a paper trail effect among these cases must be very small. In particular, from the point estimate for all sex crimes it follows that the impact of internet on the ratio of charges to reports is at most 0.018, compared to a baseline rate of 0.35 (in year 2000). Indeed, using the confidence interval, we can rule out effects on this rate above 0.029. The absence of a significant effect of internet on charge and conviction rates therefore also seems inconsistent with substantial paper trail effects.[17]

[17] To be precise, let r_1 and r_0 be the number of reports with and without internet, respectively, and denote the number of induced reports as $r_\Delta \equiv r_1 - r_0$. Similarly, denote the number of charges without internet as c_0, and *with internet* as $c_1 \equiv c_1^0 + c_1^\Delta$, where c_1^0 is the number of charges from the reports that are filed with and without internet, and c_1^Δ is the number of charges coming from induced reports. Then the effect of internet on the charge rate reported in Table 6 can be decomposed as follows.

$$\text{effect on charge rate} = \frac{c_1}{r_1} - \frac{c_0}{r_0} = \left[\frac{c_1^0}{r_0}\right]\frac{r_0}{r_1} + \left[\frac{c_1^\Delta}{r_\Delta}\right]\frac{r_\Delta}{r_1} - \frac{c_0}{r_0} \geq \left[\frac{c_1^0}{r_0}\right]\frac{r_0}{r_1} - \frac{c_0}{r_0}$$

where only the bracketed terms are unobserved, and the last inequality follows because $c_1^\Delta/r_\Delta \geq 0$. From our main results, we have an estimate of r_0/r_1 (0.94), and we also know the baseline charge rate c_0/r_0 (0.35). The point estimate of the effect implies that $c_1^0/r_0 \leq 0.368$, and at the upper bound of the confidence interval $c_1^0/r_0 \leq 0.379$. An analogous exercise can be applied to conviction rates.

Table 8. Effect of internet use on reporting

	Charge rate		Conviction rate		Perp. unknown		Lack evidence		Days to report	
	mean	effect	mean	effect	mean	effect	mean	effect	mean	effect
All sex crimes	0.347	-0.004	0.342	0.004	0.071	-0.001	0.465	0.006	214	-1.89
		(0.005)		(0.005)		(0.002)		(0.004)		(3.24)
Rapes	0.162	-0.007	0.153	-0.004	0.083	-0.009	0.655	0.006	81	-0.45
		(0.006)		(0.006)		(0.004)		(0.009)		(2.23)
Child abuse	0.435	0.006	0.428	0.008	0.020	0.001	0.319	0.000	241	-4.19
		(0.008)		(0.008)		(0.002)		(0.006)		(4.19)

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the internet user rate. Charge rate = # reported crimes that led to a criminal charge / # reported crimes. Conviction rate = # reported crimes that led to a conviction / # reported crimes. Perp. unknown = # reported crimes dismissed due to unknown perpetrator / # reported crimes. Lack evidence = # reported crimes due to lack of evidence / # reported crimes. Days to report = days between the date when crime was reported to the police and the date when the crime was allegedly committed (annual average at the municipality level). N = 422 municipalities × 8 years = 3376. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. Standard errors are heteroskedasticity robust and clustered at the municipality level. * $p < 0.10$, ** < 0.05, *** $p < 0.01$.

Direct and indirect effect of internet use

Although we do not find evidence of an effect of internet use on reporting, we still want to distinguish whether our estimated effects are driven by an indirect effect increasing the likelihood of matches ($\partial\lambda/\partial i$), or by a direct effect on the risk factor for sex crime itself ($\partial\rho/\partial i$). A first check considers the effect of internet on crimes other than sex crimes. This would consist of only the indirect matching effect under the assumption that for these crimes the latent crime factor is not affected by internet (i.e. $\partial\rho/\partial i = 0$). As argued above, the indirect effect could arise if internet use displaces alternative activities (both during and after use) that are more or less associated with sex crime.[18] For instance, if activities on the internet imply that people stay more at home, then this is likely to affect all crimes, not only sex crimes. We perform this check for all crimes (excluding sex crimes) as well as for the types of crime that are the most highly correlated with sex crime over time. Columns 3–5 in Table 7 report the results, suggesting that internet has no effect on non-sex crime.

A second test exploits that the effect of internet use through internet pornography should vary systematically with access to non-internet pornography: With easier prior access to pornography, the effect of internet on the latent risk factor, $\partial\rho/\partial i$, should be smaller. This suggests that the total effect will be mostly driven by the indirect effect in areas where the supply shock is small, while the direct effect will be more important in areas where the supply shock is large. Municipalities on the border with Sweden could more easily take advantage of the liberal laws governing pornography in this country.[19] Meanwhile, there is no reason to believe that there should be systematic differences in the effect through matching between border and non-border areas. The difference in the effect of internet use between these two areas should therefore reflect an effect on the propensity for sex crime, not matching. Note that the initial sex crime rate was about 25 % higher in border areas compared to non-border areas prior to the broadband expansion. Also, sex crimes are consistently more than twice as prevalent in Sweden compared to Norway (per capita).[20]

[18] Dahl and Lochner (2009) find that violent crime decreases on days with larger theater audiences for violent movies. This finding is partly due to voluntary incapacitation during movie attendance, but also because of a substitution away from dangerous activities after movie attendance.

[19] Sweden was in 1970 only the second country to legalize pornography (Kutchinsky, 1991), and pornography is easily available in stores and on television, usual with a 15-year age limit. In particular, adult stores are prevalent on the Swedish side of major border crossings to Norway. Private, a pioneer pornography producer registered on NASDAQ since 1999, was founded in Sweden by Berth Milton Sr. in 1965, and Swedish production of pornography has been substantial. Further, there is no legal ban on animal sex nor pornography, though they are governed by animal welfare laws. The ban on child pornography refers only to sexual immaturity, rather than the strict 18 year age limit enforced in Norway.

[20] Source: National Council for Crime Prevention, www.bra.se.

Table 9. Border areas

	Border	Non-border
All sex crimes	0.946	1.871*
	(0.676)	(1.009)
Rapes	0.300	0.638**
	(0.191)	(0.317)
Child abuse	0.402	0.695
	(0.466)	(0.619)
First-stage:		
$Coverage_{k,t-1}$	0.133***	0.116***
	(0.008)	(0.012)
Municipalities	162	260

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the internet user rate. Following Beatty et al. (2009), we define municipalities close to Sweden as border areas, whereas the remaining 260 municipalities are grouped as non-border areas. Standard errors are heteroskedasticity robust and clustered at the municipality level. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. * p < 0.10, ** p < 0.05, *** p < 0.01.

Results from separate IV-estimations are reported in Table 9, and show that the effects of internet use 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 sex crime propensity, $d\rho/di$, (a) is an important factor behind our results, and (b) is positive. This is in line with results from the experimental psychology literature discussed in Section 2.

8 Conclusion

Does internet use trigger sex crime? We used Norwegian registry data on crime and internet adoption to shed 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 IV estimates showed that internet use is associated with a substantial increase in both reports, charges and convictions of rape and other sex crimes. We presented 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 sex crime propensity. Our results suggested that the direct effect is positive and empirically important, plausibly as a result of increased consumption of pornography.

Our study speaks to an ongoing policy debate in many countries about whether and how internet traffic, with the abundant online supply of extreme pornography, should be controlled and monitored by government to prevent sex crime, such as rape and child sex abuse. 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 internet 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 even face execution.

Although one could argue that our findings support censorship or regulation of internet pornography because of its harms to third parties, caution is in order. While our results suggest that internet use increases the propensity for sex crime, matching effects may also be important. From a policy perspective, it is critical to pin down the exact channel: If matching effects are important, then an appropriate policy response may rather be to improve transparency and inform about the dangers on internet forums and social networking sites.

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Online Appendix — Broadband Internet: An Information Superhighway to Sex Crime?

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A Online Appendix

A.1 Supplementary tables and figures

Table A1: Variable definitions

Variable	Description
Sex crimes	
All sex crimes	The number of reported sexual offenses (as defined in the Norwegian Penal Code §§ 191-203, including rape, attempted rape, sexual abuse of children, incest, procurement, public exposé, and various other sexual offenses) allegedly 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 16 years of age (as defined in the Norwegian Penal Code §§ 195–197 and § 200) that were allegedly committed in year t in a given municipality, divided by the population size of the municipality and multiplied by 100,000.
Internet variables	
User rate t	Fraction of households residing in a given municipality at the beginning of year t, who are subscribing to broadband internet (with access speed at or above 256 kilobits per second).
Coverage rate t	Fraction of households residing in a given municipality at the beginning of year t, who can have access to broadband internet (with access speed at or above 256 kilobits per second).
Covariates of sex crimes	
Demographic factors	
Urbanization	Percentage share of the population in a given municipality residing in a densely populated locality 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.

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Variable	Description
Females	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.
Age-groups	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.
Students	Percentage share of the population aged 16 and above residing in a given municipality that is registered as students at the beginning of October each year t.
Divorced	Percentage share of the population aged 16 and above residing in a given municipality that is registered as divorced or separated at beginning of year t.
College or university	Dummy variable set equal to one if there is a registered college or university located in the municipality in year t.
Crime factors	
Baseline sex crimes	The number of reported sexual offenses (as defined in the Norwegian Penal Code §§ 191-203, including rape, attempted rape, sexual abuse of children, incest, procurement, public exposé, and various other sexual offenses) allegedly committed in year 2000 in a given municipality, divided by the population size of the municipality and multiplied by 100,000.
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.
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.
Socioeconomic factors	
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 during year t 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.
Welfare dependency	Percentage share of the population aged 16–59 residing in a given municipality that is registered as recipients of social economic assistance at beginning of year t.
Education	Average years of schooling in the age-group 16–59 residing in a given municipality at the beginning of year t.
Deterrence	
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.

Covariates of broadband	
Demand factors	
Income	Average after-tax disposable income earned during year t by individuals aged 16–59 years residing in a given municipality.
Education	Average years of schooling in the age-group 16–59 residing in a given municipality at the beginning of year t.

Variable	Description
Urbanization	Percentage share of the population in a given municipality residing in a densely populated locality at the beginning of year t.
Industry composition: Services	Percentage share of the population aged 16–59 employed in the services sector at the beginning of year t.
Industry composition: Services, private	Percentage share of the population aged 16–59 employed in the private services sector at the beginning of year t.
Industry composition: Public sector	Percentage share of the population aged 16–59 employed in the public sector at the beginning of year t.
Public services provision: Total spending	Per capita spending on municipal public services in year t (in NOK).
Public services provision: Administration	Per capita spending on municipal administration in year t (in NOK).
Public services provision: Education	Per capita spending on municipal schools and other educational institutions in year t (in NOK).
Public services provision: Health services	Per capita spending on municipal health care services in year t (in NOK).
Supply factors	
Proximity to major broadband installations: Travel time	Average travel time in minutes to municipal center at the beginning of year t.
Proximity to major broadband installations: Distance	Distance in kilometers to municipal sub-center at the beginning of year t.
Transmission channels: Road networks	Distance in kilometers covered by municipal roads at the beginning of year t.
Transmission channels: Infrastructure	Per capita spending on municipal infrastructural maintenance (incl. roads, pipes and tunnels) in year t (in NOK).

Table A2: Descriptive statistics, additional control variables

	Overall	2000	2002	2004	2006	2008
Divorce rate (%)	8.1	7.6	7.9	8.1	8.4	8.5
	(2.1)	(2.1)	(2.1)	(2.1)	(2.0)	(2.0)
Enrolled students (%)	11.3	11.5	11.6	11.3	11.2	10.6
	(1.6)	(1.7)	(1.7)	(1.6)	(1.5)	(1.4)
College or University	12.1	12.8	12.8	11.8	11.6	11.6
	(32.6)	(33.4)	(33.4)	(32.4)	(32.1)	(32.1)
Services sector (%)	30.0	28.7	30.2	30.0	30.0	31.3
	(4.8)	(5.3)	(4.8)	(4.7)	(4.4)	(4.6)
Public sector (%)	26.3	25.8	26.9	26.5	25.9	26.5
	(5.0)	(5.4)	(5.2)	(5.0)	(4.8)	(5.1)
Public spending, Total	52.1	50.2	50.3	51.2	53.4	56.1
(in 1000 NOK)	(14.5)	(13.6)	(14.3)	(14.4)	(14.2)	(15.2)
Travel distance (in km)	8.3	8.6	8.4	8.2	8.2	8.0
	(7.2)	(7.4)	(7.3)	(7.0)	(7.1)	(7.4)
Road networks (in km)	174.5	164.9	168.6	174.9	179.5	184.6
	(112.9)	(103.4)	(108.8)	(112.6)	(117.3)	(122.7)
Travel time (in minutes)	9.7	12.2	9.2	9.2	9.3	9.1
	(7.6)	(9.4)	(7.4)	(7.2)	(7.2)	(6.8)
Public spending, Administration	5.5	4.0	5.0	5.6	5.5	5.9
(in 1000 NOK)	(3.0)	(2.5)	(2.8)	(3.0)	(3.0)	(3.3)
Public spending, Education	12.8	13.2	12.7	12.7	12.9	12.8
(in 1000 NOK)	(2.5)	(2.8)	(2.5)	(2.4)	(2.4)	(2.6)
Public spending, Health	18.5	18.6	17.9	18.1	19.0	20.2
(in 1000 NOK)	(5.9)	(6.1)	(5.7)	(5.7)	(5.9)	(6.3)
Public spending, Infrastructure	7.0	7.1	7.0	6.8	7.0	7.3
(in 1000 NOK)	(3.8)	(2.8)	(3.9)	(3.6)	(3.8)	(3.9)

Note: Detailed descriptions of the variables are given in Table A1.

Table A3: Reduced form effects of broadband coverage on sex crime

All sex crimes	0.151^{**}	0.151^{**}	0.151^{**}	0.157^{**}	0.173^{**}
	(0.068)	(0.069)	(0.069)	(0.069)	(0.070)
Rape	0.049^{**}	0.049^{**}	0.049^{**}	0.050^{**}	0.052^{**}
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Child sex abuse	0.080^{*}	0.056	0.056	0.059	0.072
	(0.046)	(0.048)	(0.048)	(0.048)	(0.049)
Demographic controls		✓	✓	✓	✓
Police density			✓	✓	✓
Other crimes				✓	✓
Broadband controls					✓

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the broadband coverage rate. Standard errors are heteroskedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities × 8 years = 3376 observations. All regressions include municipality fixed effects and year dummies. Detailed descriptions of control variables are given in Table A1. * p < 0.10, ** < 0.05, *** p < 0.01.

Table A4: Additional specification checks

	Baseline (1)	No outliers (2)	No zeros (3)	No cities (4)	Incl. Spillovers (5)
All sex crimes	1.389**	1.159**	1.339**	1.440**	2.115***
	(0.562)	(0.480)	(0.574)	(0.578)	(0.772)
Rapes	0.419**	0.320**	0.372	0.450**	0.300
	(0.168)	(0.153)	(0.252)	(0.174)	(0.230)
Child abuse	0.583	0.315	0.885*	0.603	1.032*
	(0.391)	(0.310)	(0.532)	(0.404)	(0.544)
First-stage:					
$Coverage_{k,t-1}$	0.124***	0.124***	0.112***	0.124***	0.123***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
$\overline{Coverage}^{neighbor}_{k,t-1}$					0.005
					(0.007)

Note: Crime rates are calculated per 100,000 persons. Effects are reported per percentage point increase in the internet user rate. The baseline results are based on 422 municipalities × 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 99.5th percentile. In column (3), we drop all observations with zero values of the dependent variable, whereas we drop the 5 largest cities in Norway, i.e. Oslo, Bergen, Trondheim, Stavanger and Kristiansand, in column (4). Outcome variable in column (5) includes crime in neighboring municipalities. All regressions include municipality fixed effects, year dummies and all controls listed in Table A1. Standard errors are heteroskedasticity robust and clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of broadband internet on sorting

		Men		Women	
	All	16–24	25–44	16–24	25–44
Entry_{kt}	0.001	0.005	-0.016	0.034	0.001
	(0.007)	(0.024)	(0.012)	(0.026)	(0.011)
Dependent mean	0.051	0.083	0.055	0.108	0.051
Exit_{kt}	-0.001	-0.002	0.001	-0.002	-0.000
	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)
Dependent mean	0.038	0.110	0.045	0.131	0.040

Note: Entry_{kt} = # individuals moving into municipality k in year t/ size municipality k in year t. Exit_{kt} = # individuals moving out of municipality k in year t / # similar individuals in municipality k at the start of year t. Standard errors are heteroskedasticity robust and clustered at the municipality level. Regressions are based on 422 municipalities × 8 years = 3376 observations. All regressions include municipality fixed effects and year dummies.

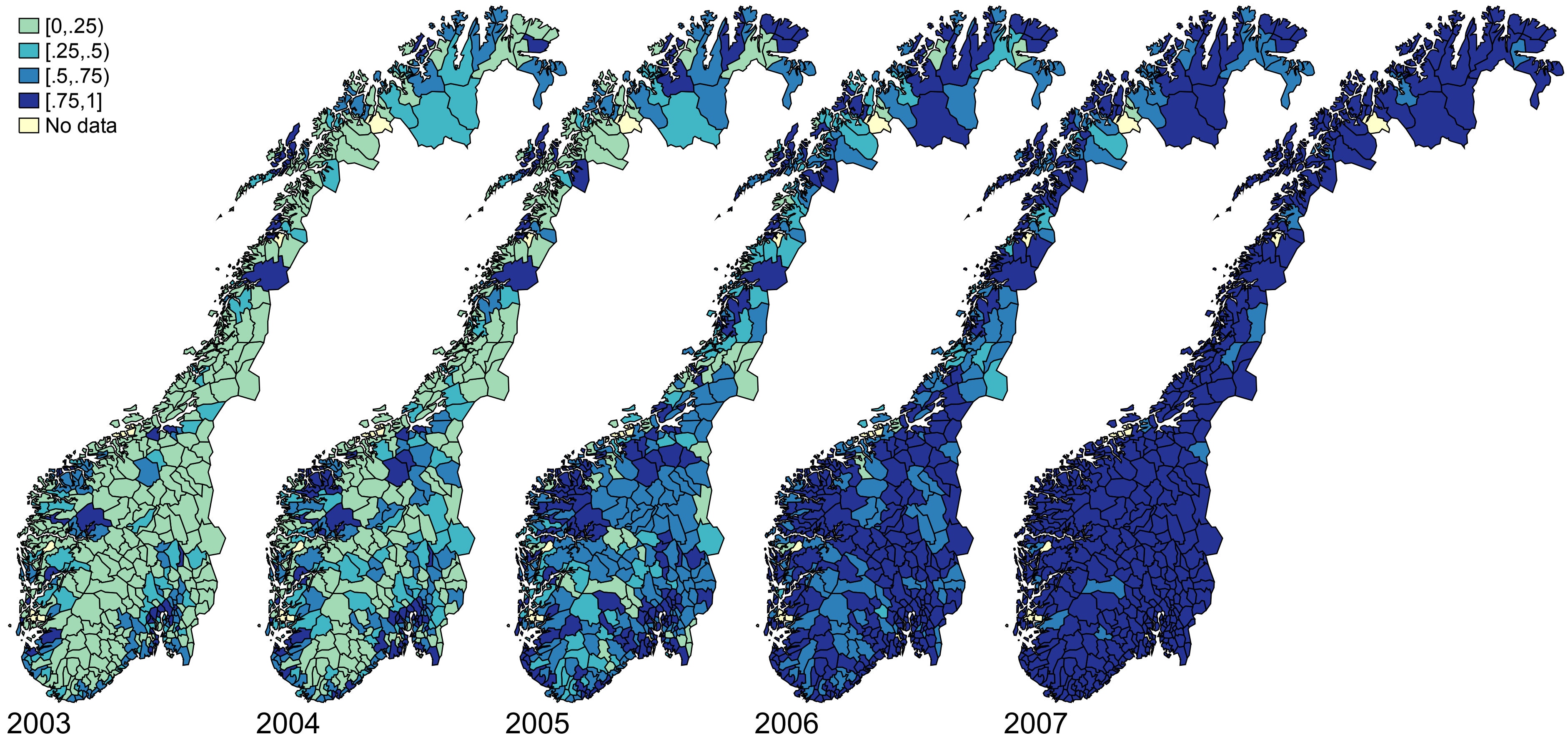


Figure A1: Broadband internet expansion across Norway

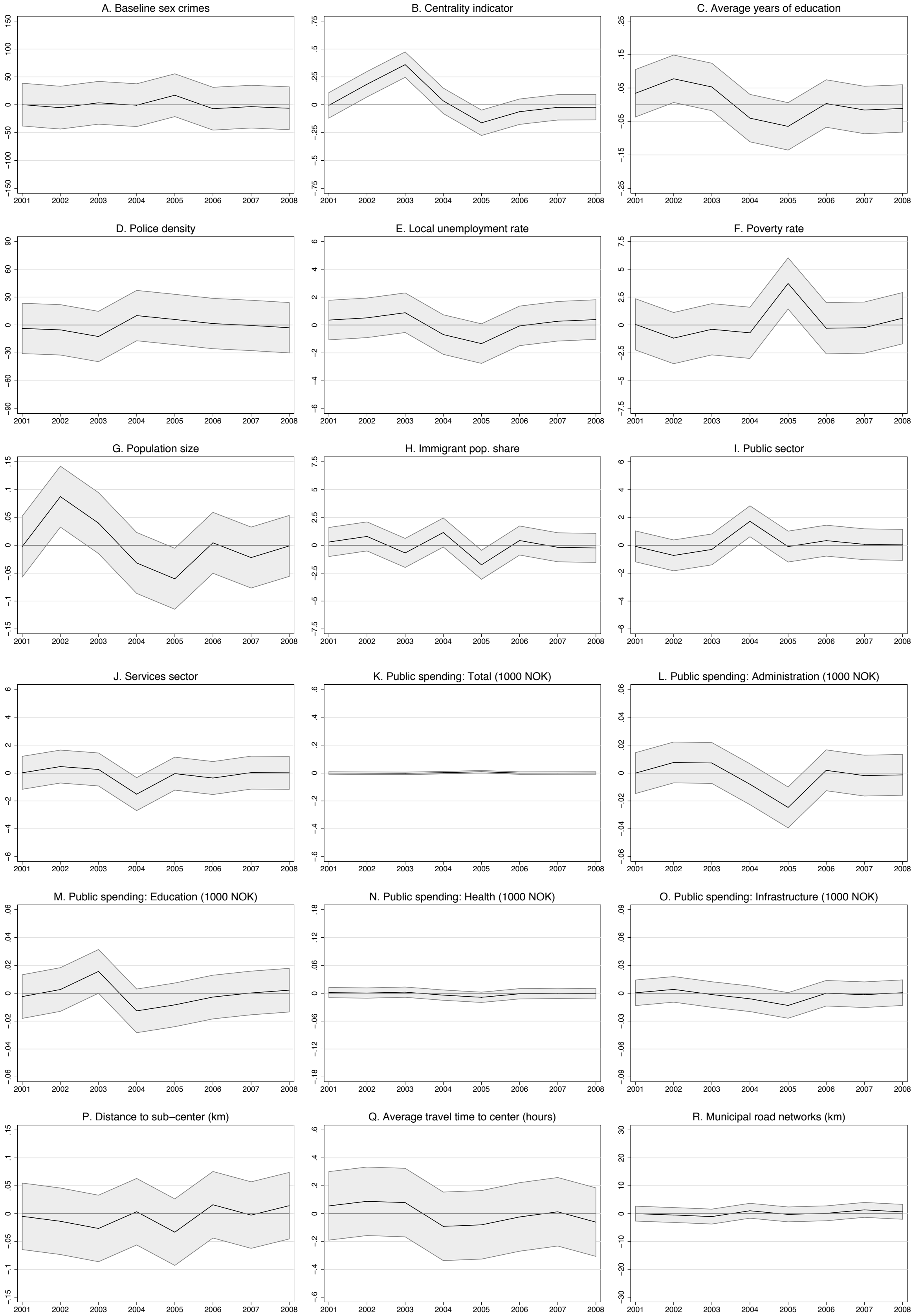


Figure A2: Broadband expansion regressed on baseline municipality characteristics, 2000–2008

Note: We regress changes in coverages rates on municipality specific baseline characteristics interacted with time dummies, controlling for municipality fixed effects. The figures plot interaction terms for each variable. The range of the axis is 6 standard deviations of the variable.

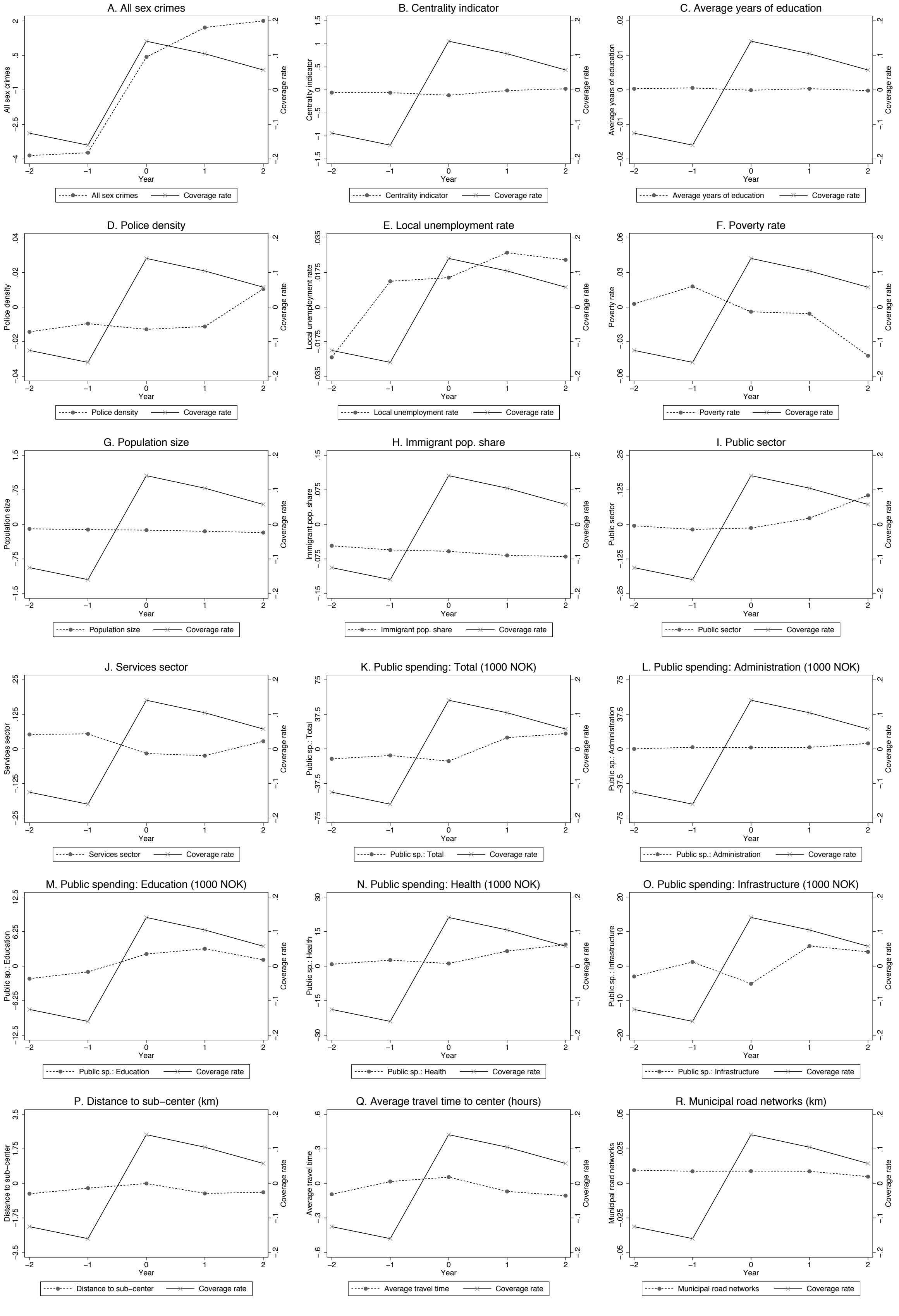
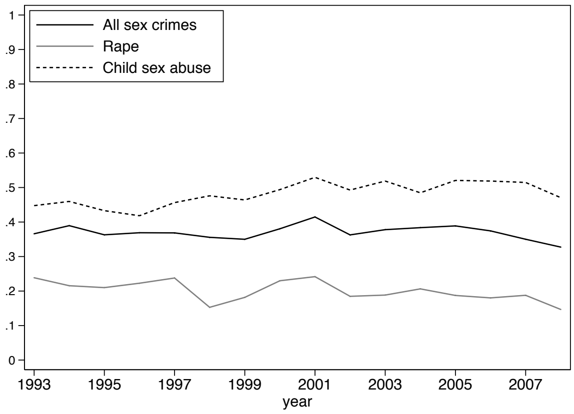
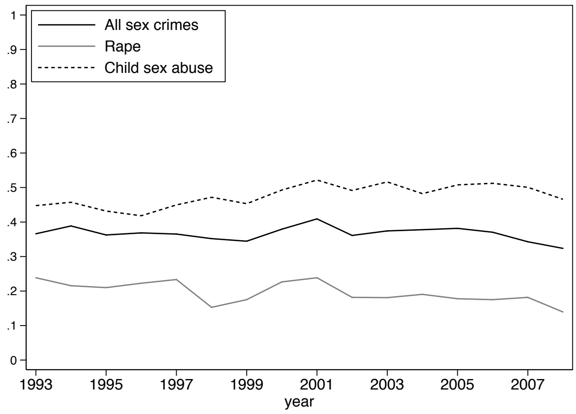


Figure A3: Graphical illustration of IV approach on sex crimes and municipality characteristics

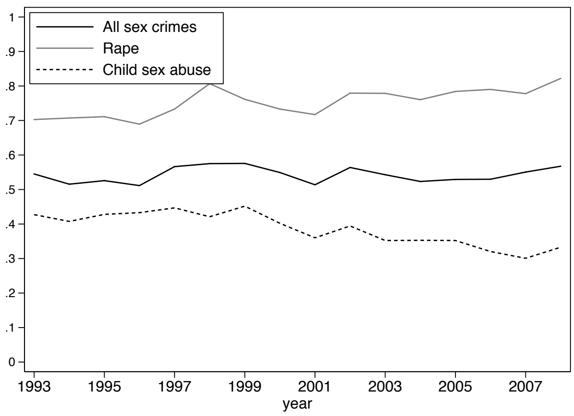
Note: For each municipality, we recenter the data such that cause and effect occur at time zero: For the coverage rate, time zero represents the year with the strongest growth in broadband coverage rate; for the user rate, the sex crime rate and the municipality characteristics, time zero represents the following year. Variables are residuals from a regression on time and municipality fixed effects. The range of the right y-axis is 1 standard deviation of broadband coverage. The range of the left y-axis is 1/10th standard deviation of the variable. The sample consists of the 413 municipalities (of 422) for whom we can construct a balanced sample of 2 years around 0.



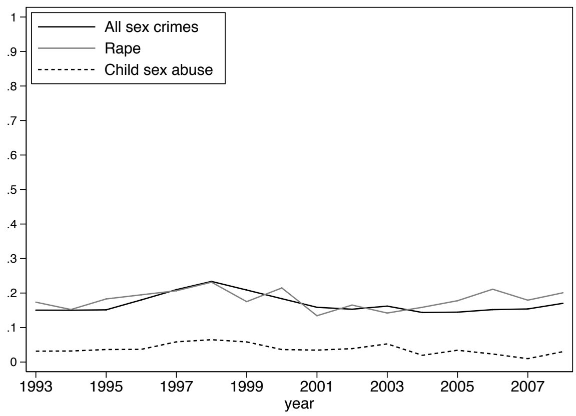
(a) Charges



(b) Convictions



(c) Lack of evidence



(d) Perpetrator unknown

Figure A4: Police evaluation of reported crimes, 1993–2008

Note: Charges = # reported crimes that led to a criminal charge / # reported crimes. Convictions = # reported crimes that led to a conviction / # reported crimes. Lack evidence = # reported crimes due to lack of evidence / # reported crimes. Perp. unknown = # reported crimes dismissed due to unknown perpetrator / # reported crimes.

A.2 Selection on observables as a guide to selection on unobservables

In this section, we discuss and implement the method proposed by Altonji et al. (2005a,b). In the context of our application this approach examines the potential bias in the IV estimates under the hypothesis that selection on time-varying observables equals selection on time-varying unobservables. This can be formally expressed as follows.

$$\frac{cov(x_{kt}^{'}\beta,\ z_{k,t-1}|\alpha_k,\tau_t)/var(z_{k,t-1}|\alpha_k,\tau_t)}{var(x_{kt}^{'}\beta|\alpha_k,\tau_t)} \equiv \frac{cov(\epsilon_{kt},\ z_{k,t-1}|\alpha_k,\tau_t)/var(z_{k,t-1}|\alpha_k,\tau_t)}{var(\epsilon_{kt}|\alpha_k,\tau_t)}. \quad (1)$$

The omitted variable bias of the IV estimator depends on the numerator on the right-hand side of equation (1), which is unobserved. The numerator on the left-hand side – the extent to which the instrument correlates with the part of the outcome explained by the observables – is however readily estimated by regressing the instrument $z_{k,t-1}$ on an estimate of $x_{kt}'\beta$. This correlation (rescaled using the identity (1)) can then be used to construct the following estimate of the omitted variable bias in the IV estimates (cf. eq. (9) in Altonji et al. (2005a)):

$$\frac{var(z_{k,t-1}|\alpha_k,\tau_t)}{\phi var(\widetilde{z}_{k,t-1}|\alpha_k,\tau_t)} \cdot \frac{var(\epsilon_{kt}|\alpha_k,\tau_t)}{var(x_{kt}^{'}\beta|\alpha_k,\tau_t)} \cdot \frac{cov(x_{kt}^{'}\beta,\ z_{k,t-1}|\alpha_k,\tau_t)}{var(z_{k,t-1}|\alpha_k,\tau_t)}, \quad (2)$$

where $\widetilde{z}_{kt-1}$ is the orthogonal projection of $z_{k,t-1}$ on x_{kt}, and ϕ is the first-stage coefficient on $z_{k,t-1}$.

Table A6 below reports estimates of equation (2) and its components. Consider first the last column showing the correlation between our instrument and the part of the outcome explained by time-varying observables. The estimates of $\rho(x\hat{\beta},\ z)$ are close to zero and generally insignificant. This is consistent with our identifying assumption that the timing of the roll-out is uncorrelated with unobserved time-varying factors of sex crime. Given that little, if any, of the variation in sex crimes that is explained by the controls is correlated with our instrument, the point estimates of the bias are going to be noisy and not convey much about selection on time-varying unobservables. The test thus lacks sufficient power due to the difficulty of predicting variation in sex crime. Indeed, as shown in Column 5, the bias estimates are very imprecise and unstable. For all sex crimes, the estimated bias even changes sign as we change the set of controls; in contrast the IV estimates are quite stable and increase slightly as we increase the number of controls, contrary to what is suggested by the estimates of the bias.

In any case, if we were to interpret the noisy point estimates based on the full set of controls, they suggest that that time-varying unobservables (a) understate the effects of internet use on overall sex crime, (b) have little impact on the estimated effect on rape, and (c) can potentially generate too large bias to draw any conclusions about the effect on child sex abuse. A more natural interpretation, however, is that the roll-out is uncorrelated with unobserved time-varying factors of sex crime – in line with the ex ante case – and that the formal test lacks power to detect bias on the order of the estimates.

Table A6: AET bias estimates

	Effect	$\frac{var(\hat{\epsilon})}{var(x\hat{\gamma})}$ (1)	$\frac{var(z)}{var(\tilde{z})}$ (2)	$\hat{\phi}$ (3)	$\frac{cov(x\hat{\beta},z)}{var(z)}$ (4)		Bias = (1) × (2) × (4)/(3)		Correlation $\rho(x\hat{\beta},\, z)$		Controls D	P	O	B
All sex crime	1.181	33.1	1.043	0.128	0.005	(0.026)	1.478	(3.746)	0.009	(0.030)	✓			
	1.180	32.7	1.043	0.128	0.006	(0.024)	1.482	(3.312)	0.009	(0.030)	✓	✓		
	1.231	27.5	1.044	0.128	-0.000	(0.027)	-0.017	(3.261)	-0.000	(0.030)	✓	✓	✓	
	1.389	22.0	1.063	0.124	-0.012	(0.030)	-2.208	(3.347)	-0.016	(0.029)	✓	✓	✓	✓
Rapes	0.384	32.1	1.043	0.128	0.002	(0.008)	0.557	(1.294)	0.010	(0.028)	✓			
	0.384	32.1	1.043	0.128	0.002	(0.008)	0.559	(1.191)	0.010	(0.027)	✓	✓		
	0.394	30.5	1.044	0.128	0.001	(0.009)	0.285	(1.241)	0.005	(0.029)	✓	✓	✓	
	0.419	25.5	1.063	0.124	0.000	(0.010)	0.090	(1.225)	0.002	(0.030)	✓	✓	✓	✓
Child sex abuse	0.436	29.7	1.043	0.128	0.027	(0.019)	6.502	(2.139)	0.058	(0.027)	✓			
	0.436	29.7	1.043	0.128	0.027	(0.019)	6.503	(2.297)	0.058	(0.027)	✓	✓		
	0.459	27.1	1.044	0.128	0.024	(0.020)	5.370	(2.259)	0.050	(0.028)	✓	✓	✓	
	0.583	20.1	1.063	0.124	0.012	(0.021)	2.136	(1.997)	0.022	(0.027)	✓	✓	✓	✓

Note: All estimates are conditional on municipality and year fixed effects. Standard errors based on 499 bootstrap replications that take clustering at the municipality level into account. Controls sets are denoted as follows: D = Demographic, P = Police density, O = Other crimes, B = Broadband controls. A full description of the control variables is available in the appendix.

References

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