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The Internet and Hate Crime:

Offline Spillovers from Online Access

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Abstract: The Internet has had profound effects on society, both positive and negative. In this paper we examine the effect of the Internet on a negative spillover: hate crime. In order to better understand the link, we study the extent to which broadband availability affects racial hate crimes in the US from 1999 – 2008. To address measurement error, we instrument for broadband availability using slope of terrain. We find strong evidence that broadband availability increases racial hate crimes. The results are stronger in areas with greater racial segregation and with more online searches for racist words, suggesting that the direct effect of the Internet on hate crime is primarily due to a heightening of pre-existing propensities to engage in hate activity. We find no evidence that the Internet has affected crime reporting. The results are robust to alternative specifications and falsification tests. These results shed light on one of the many offline spillovers from increased online access, and suggest that governmental and private regulation of online content may help reduce hate crime.

Keywords: Internet, broadband, online-offline interaction, hate crime, race

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1. Introduction

The Internet has had profound effects on society. Among the many positive spillovers include higher organizational productivity (Brynjolfsson and Hitt 2000), greater consumer surplus (Brynjolfsson et al. 2003, Ghose et al. 2006) and greater collaboration between scientists (Ding et al 2010, Forman and van Zeebroeck 2013). However, Internet technology can also be deployed in less benign ways, leading to negative spillovers from its usage. For example, Bhuller et al. (2013) document an increase in sex crimes due to Internet availability. Moreover, the Internet provides an accessible, affordable, and anonymous channel for posting and sharing hate ideologies.[1] Anecdotally, this appears to have led to an increase in hate related websites. According to a recent estimate, there are over 11,500 hate-related sites including websites, social network pages, chat forums and micro-bloggers (Lohr 2010). Indeed, extremists were among the Internet's earliest adopters. The Anti-Defamation League notes that hate groups utilized Internet Relay Chat channels (e.g., #Nazi and #Klan), online newsgroups (e.g. alt.politics.white-power and alt.revisionism) and listservs to spread hate agendas even before the first hate site, Stormfront, was launched in 1995.

While the Internet may have enabled the spread of hate ideology, the relationship between the Internet and hate crime is theoretically ambiguous (Glaeser 2005). On one hand, greater Internet availability may increase the efficiency with which extremists can spread hate ideology, and may therefore lead to an increase in the number of hate crimes committed. On the other hand, the Internet may improve interconnectivity between individuals and groups, leading to more united communities (Van Alstyne and Brynjolfsson 2005). In addition, the availability of the Internet as a rhetorical outlet may serve to reduce the number of hate crimes by allowing proponents of bigotry to express their frustrations in less physical ways (Glaser et al. 2002,

[1] Under the protection of the First Amendment, the use of the Internet to communicate hateful expression is allowed, unless the content incites imminent lawless actions or true threats . See Planned Parenthood v. American Coalition (2002). Available at *http://fl1.findlaw.com/news.findlaw.com/hdocs/docs/abortion/ppacla51602opn.pdf.*

Gentzkow and Shapiro 2011). Thus, the effect of the Internet on hate crime remains largely an empirical question.

In an effort to empirically examine this question, we use geographic and temporal variation in broadband availability to study the effect of the Internet on hate crime in the U.S. We use county-level data on the number of broadband providers and racial hate crimes between 1999 and 2008. We focus on racial hate crime because this category comprises two-thirds of all hate crimes committed in the U.S. In addition, the focus on a specific type of hate crime allows us to explore specific mechanisms driving the results. We use an instrumental variable approach to address measurement error in our measure of broadband availability. Following Kolko (2012), we instrument for the number of broadband providers using slope of the local terrain. Our results suggest that an increase in broadband access leads to an increase in racial hate crime. We also provide an in-depth discussion of the possible mechanisms that are driving the results. One hypothesized mechanism is that the Internet allows better matching between perpetrators and victims. Another hypothesized mechanism is that the Internet acts to heighten pre-existing propensities to engage in hate activity. We find no evidence that Internet availability leads to increased incidence of other crimes including burglary, murder, and robbery. However, we do find evidence that the link between Internet access and racial hate crimes is stronger in areas with higher levels of segregation and which perform more online searches for racist words. Thus, the evidence suggests that the direct effect of the Internet on hate crime is primarily due to a heightening of pre-existing propensities to engage in hate activity. Finally, we conduct a number of robustness checks, including alternative specifications and falsification tests. We also find no evidence that the Internet has affected crime reporting.

Our work contributes to several streams of literature. First, our work adds to an existing literature that examines the consequences of IT access and expansion. Extant research in this area has documented various positive outcomes of Internet growth such as facilitating the process of job search (Autor 2001, Stevenson 2008), raising wages and employment levels (Forman et al. 2012, Kolko 2012), increasing organizational productivity (Brynjolfsson and Hitt 2000), enhancing consumer surplus (Brown and Goolsbee 2002, Brynjolfsson et al. 2003, Ghose et al. 2006), and assisting research collaboration (Ding et al 2010, Forman and van Zeebroeck 2013). In contrast, other studies have detailed potentially negative outcomes from the Internet. For example, Bhuller et al. (2013) studies the link between sex crimes and Internet availability and Chan and Ghose (2013) studies the link between online classifieds and HIV. More broadly, in many cases online availability of goods leads to a displacement of offline sales (e.g.: Rob and Waldfogel 2006, Zentner 2006).[2] We add to this literature by describing another negative impact of Internet availability: the increase in racial hate crime.

More generally, we examine socioeconomic changes due to increased Internet availability. Other research in this stream includes Chinn and Fairlie (2006) and Goldfarb and Prince (2008) who investigate the role of digital divide. The link between the Internet and interactions between different groups has been examined by Van Alstyne and Brynjolfsson (2005) and Gentzkow and Shapiro (2011). More specifically within this stream, we add to a literature on hate crime. Extant research in this area has studied the prevailing trends of hate activity with respect to changing socioeconomic conditions (Beck and Tolnay 1990) and political economy (Glaeser 2005, Fryer and Levitt 2012), its relation to geographical considerations (Krueger and Pischke 1996) and law enforcement policies (Gan et al. 2011). By empirically demonstrating the role of improved connectivity via Internet availability on racial hate crime, our work sheds light on the theoretically ambiguous relationship between communication technology and hatred (Glaeser 2005).

The rest of the paper is organized as follows. In Section 2, we describe the data. Section 3 describes the empirical approach and results. In Section 4, we explore mechanisms that might

[2] For dissenting views, see Oberholzer-Gee and Strumpf (2007).

account for the effect of the Internet on racial hate crime. In Section 5 we discuss the robustness of our results to alternative explanations. Section 6 concludes with policy implications.

2. Data

To examine how the incidence of racial hate crime varies with Internet growth, we consolidated detailed data from various official sources including the FBI, FCC, and U.S. Census Bureau. Hate crime data come from FBI's annual report, *Hate Crime Statistics*. The FBI defines hate crimes as criminal offenses motivated, in whole or in part, by the offender's bias against a race, religion, disability, sexual orientation, ethnicity, gender or gender identity.[3] Since its inception in 1992, the *Hate Crime Statistics* has drastically improved its coverage of the U.S. population from 51 percent to 85.7 percent. The increase in population coverage over the short period mirrors the emphasis placed by law enforcement to educate its officers in identifying such crime (Gale et al. 2002).[4] We restrict our empirical focus to the years 1999 – 2008 because the population coverage in the data is relatively stable, with a high of 88.6 percent in 2008 and a low of 82.8 percent in 2003.

Our main dependent variable is *racial hate crime*$_{it}$, which is the number of racial hate crimes in county i in year t. We construct this variable by summing up the number of racial hate crimes across all the reporting cities in a county for a given year. We focus on racial hate crime for two reasons. First, close to two-thirds of reported hate crimes arise from racial-bias motivations, making racial hate crime the most typical form of bias-motivated crime in the U.S. There are strong motivations to understand the antecedents to the prevailing type of hate crime for effective policy implementation and intervention. Second, the nature of hate crime varies as a function of the target group, and the prevalence of each type of hate crime is affected by a different set of predictor variables (Glaser et al. 2002). To make meaningful inferences, rigorous quantitative

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[3] The *Hate Crime Data Collection Guidelines and Training Manual* provides further definitions and classifications for various hate crimes tracked by the FBI, available at http://www.fbi.gov/about-us/cjis/ucr/data-collection-manual.

[4] The high coverage of the *Hate Crime Statistics* is also an effort in part of the local agencies consistently furnishing hate crime reports to the FBI, voluntarily. In states that do not impose data collection statutes (e.g., Alabama, Georgia and Mississippi), some agencies have nevertheless chosen to submit its reports.

analysis needs to be focused on one specific type of hate crime with careful and detailed empirical modeling.

Standard deviations of the number of racial hate crimes during this period do not vary widely, suggesting that racial hate crimes are being classified consistently.[5] This observation is in line with the conclusions drawn in Boyd et al. (1996) and Martin (1995, 1996), that police departments engage in routine practices and well-defined reporting mechanisms to determine the hate-related status and hate type of an incident. Though the hate crime data do not suffer from documentation issues and ambiguous classification, it may still face the problem of under-reporting (DiIulio 1996). We address the potential effects of under-reporting through a separate test described in detail in the next section. Within our sample, though the absolute number of racial hate crimes decreased from 1999 to 2008, the incidence of these crimes became more evenly dispersed. The number of counties reporting at least one hate crime has risen by 23.5 percent from 1999 to 2008. The highest rate of racial hate crime over this period, 34.7 out of 10,000, was reported for Scott County, Arkansas in 2003.

Our main independent variable is the number of broadband providers in county *i* in year *t*, $BBProviders_{it}$, which we use to measure broadband availability. We choose to focus on broadband access to the Internet over earlier forms of access as extant research shows that broadband adoption significantly increases the overall usage of the Internet, heightens the consumption of online content in quantity and diversity, and affects a range of online and offline activities (Hitt and Tambe 2007, Kolko 2010a). More specifically, the reduction in waiting times for loading images, sounds, and videos via broadband availability facilitates the likelihood and willingness to consume hate content online. We construct the variable for broadband availability by averaging the number of broadband providers across all ZIP codes in county *i* in year *t*. The data comes from

[5] Gale et al. (2002) makes a similar observation. They reasoned that as hate crimes becomes more clearly defined, the distribution of the data would "tighten" over time. By comparing the hate crime rate from 1992 to 1995, they found a clear downward trend in the standard deviations of the data in *Hate Crime Statistics*.

FCC Form 477 which reports the number of broadband providers at each ZIP code offering broadband services at 200 kilobits per second or faster. Broadband providers include telephone-line DSL, cable modems, wireless, satellite, and power-line technologies. The FCC data is the only comprehensive indicator of U.S. broadband availability that is recorded annually since 1999 (Flamm 2006, Kolko 2010b), and is extensively used by policy makers and academics to assess broadband availability (California Public Utilities Commission 2006, Grubesic 2006, Xiao and Orazem 2011, Kolko 2012). Using data on the number of broadband providers from the FCC and proprietary data on broadband use from Forrester Research, Kolko (2010b) shows that the extent of broadband availability increases monotonically with the number of broadband providers. Given that broadband policies often work by adding providers to an area via public provision, subsidization and regulation, it is meaningful in a policy context to employ the number of broadband providers as a proxy for Internet availability.

Several limitations of the FCC data have been documented in previous work (Greenstein and Mazzeo 2006, GAO 2006, 2009, Kolko 2010b, Xiao and Orazem 2011): the FCC data does not report whether providers serve consumers or businesses, how much they charge for service, the speed of their service, and whether they provide service to the whole or only part of a ZIP code. As a result, using the FCC data to measure broadband availability introduces measurement error. For example, there may be business but not residential subscribers in a ZIP code, so the FCC's count will overstate residential broadband availability. In addition, the FCC does not report the actual number of providers for locations with 1-3 providers, instead grouping them into a single category. Following Kolko (2012), we assign a value of two to ZIP codes reported as having 1-3 providers. The provider count variable limits our ability to express the effects of additional providers on racial hate crime incidence quantitatively. Moreover, it introduces additional measurement error: ZIP codes with one or three providers will be incorrectly coded as having two providers. The presence of measurement error in our independent variable leads us to adopt an instrumental variables approach described in Section 3.1 below. For use in robustness tests, we also construct categorical measures of broadband providers. In this alternative measure, various categories (or bins) are created to denote a specific range of providers. In other robustness tests, we use state-level counts of high speed Internet lines as a measure of Internet availability. This data, also reported on FCC Form 477, is only available at the state level, and suffers measurement errors of its own. For example, providers with fewer than 250 subscribers do not need to report on subscriptions.

We combine racial hate crime and broadband availability measures with demographic, economic, and crime-related information from a variety of sources which allow us to control for the underlying propensity of racial hate crimes across locations. The U.S. Census provides county and state level information on migration rates, racial proportions, age proportions, and the number of persons below the poverty line. Estimates of these measures are available yearly from 1999 to 2008. The FBI database *Law Enforcement Officers Killed and Assaulted* provides our data on the annual number of police employees at each county. To control for general crime level, we rely on FBI's *Uniform Crime Report* for counts of crimes by county. Table 1 provides descriptive statistics of our data.

3. Empirical Approach and Baseline Results

3.1. Empirical Approach

Our identification strategy involves two approaches. First, we use fixed effects to account for location and time based factors that affect racial hate crime. These fixed effects help control for time-invariant unobserved heterogeneity that varies across counties, such as long-standing culture of prejudices or tolerance, or level of urbanization. We also include a set of time-varying demographic, economic, and crime-related covariates (described below) to account for time-varying local factors that might affect racial hate crime such as changing attitudes towards various racial groups, changing economic conditions or policies, and episodes of conflict and terrorism.

Second, we follow Kolko (2012) and instrument for the number of broadband providers using the slope of the local terrain. The instrumental variables (IV) approach helps us address attenuation bias arising from measurement error in our independent variable $BBProviders_{it}$. To be a valid instrument, slope should be correlated with broadband availability and not directly correlated with racial hate crime. Our instrument identifies cross-sectional variance in the costs to broadband providers in extending Internet service to an area. As noted by GAO (2006) and Prieger (2003), terrain features such as slope affect the cost to extend broadband service in an area. Kolko (2012) finds a strong negative correlation between slope and the number of broadband providers. The exclusion restriction holds if slope does not have a direct effect on the incidence of racial hate crime, independent of its relationship with Internet availability. Terrain characteristics such as slope are unlikely to bear direct effects on racial hate crime, as these criminal acts are largely induced by human ideologies, prejudices, and influences, and not geographic factors.

Though there may not be direct effects on racial hate crime, slope of a location may be linked to crime incidence indirectly. Locations with flat terrains are likely to be urbanized areas that may possess demographic and economic features associated with higher incidence of crime levels (Glaeser and Sacerdote 1999). To account for this possibility, a set of key demographic and economic drivers of crime rate are included in our model specifications as covariates. More specifically, we include age proportion and race proportion to account for potential demographic-based effects on crime incidence. The amount of migration is used as a covariate to account for the level of population heterogeneity and racial tolerance. The number of people below the poverty line, the number of police employees and general crime levels are included to control for the propensity of crime outbreaks in locations. Finally, the county fixed effects framework controls for any unobservable location-based effects, while the yearly dummies accounts for temporal effects that may potentially bias the validity of our instruments. We note that there are substantial differences in the number of broadband providers across the years. To allow for heterogeneous impacts on the effects of our instrument over time we interact slope with year dummies, an approach that has been widely used in past research (e.g., Forman and van Zeebroeck 2013, Stevenson 2008).

Our baseline specification to identify the effects of broadband availability on racial hate crime is as follows:

(1) $ln(racial\ hate\ crime_{it}) = \alpha + \beta\, ln(BBProvider_{it-1}) + \lambda X_{it-1} + C_i + Y_t + \varepsilon_{it}$,

where *racial hate crime*$_{it}$ denotes the number of racial-based hate crimes in county *i* in year *t*, *BBProvider*$_{it-1}$ represents the number of broadband providers, X_{it-1} is a vector of control variables, C_i denotes location fixed effects, and Y_t represents year fixed effects. We have included a lagged specification for broadband availability and controls to avoid simultaneity issues. Robust standard errors clustered over counties are used in our analyses. For the IV-approach to be valid, we require the expansion of broadband Internet to be unrelated to different underlying time trends in racial hate crimes across counties. To check for this possibility, we perform a test following Duflo (2001) that interacts the baseline year covariates with either a linear time trend

(2) $ln(racial\ hate\ crime_{it}) = \alpha + \beta\, ln(BBProvider_{it-1}) + \lambda X_{it-1} + t \sum_j \eta_j x_{(i,1999),j} + C_i + Y_t + \varepsilon_{it}$,

or with time dummies

(3) $ln(racial\ hate\ crime_{it}) = \alpha + \beta\, ln(BBProvider_{it-1}) + \lambda X_{it-1} + \tau_t \sum_j \eta_j x_{(i,1999),j} + C_i + Y_t + \varepsilon_{it}$.

This setup allows the broadband availability to be related to different underlying time trends in racial hate crime across locations.

3.2. Baseline Results

Table 2 reports our baseline results linking broadband availability to racial hate crime. Column 1 provides pooled OLS results, Column 2 provides Fixed Effects OLS results, and Columns 3-8 provide IV results. The pooled OLS results in Column 1 include demographic controls, poverty, size of the police force and general crime levels. The coefficient on the number of broadband providers is positive and significant at the 1% level, indicating a significant correlation between our measure of broadband availability and racial hate crime. However, the pooled OLS regressions do not account for unobserved heterogeneity across counties that may affect the incidence of racial hate crimes. Column 2 replicates Column 1, but includes county fixed effects and year dummies. The coefficient on broadband providers remains positive, but not significant. However, the fixed effects may exacerbate the attenuation bias arising from measurement error in our measure of broadband availability, so we turn to an IV approach.

As described in Section 3.1, we instrument for broadband availability using slope of terrain. We report the results for both the first- and second-stage regressions. As expected, increases in slope of the local terrain are associated with a lower likelihood of broadband availability. The negative relationship holds across various years under various specifications in Table 2. Values of the F-statistics on the excluded instruments in the first-stage regression range from 14.141 to 28.422, and in all cases are significant above the 1% level. We also report the Stock and Yogo (2005) critical thresholds for weak instruments. Following Stock et al. (2002), we report critical thresholds for the test that the bias of two-stage least squares regression is no more than 10% of the inconsistency of ordinary least squares regression, and that the size of the maximal Wald test for the first-stage instruments is large enough that a 5% hypothesis test rejects no more than 15% of the time. Across all models, the F-statistic surpasses these critical values, indicating that the models do not suffer from weak instrument biases.

Column 3 shows the estimates from the IV specification with only county fixed effects and year dummies. The results show a positive and significant coefficient on broadband providers, suggesting that racial hate crimes increase as broadband availability increases. The next three columns in Table 2 include control variables to investigate the extent to which the IV estimates of broadband availability are sensitive to the inclusion of time-varying observable factors. We add demographic controls in Column 4, poverty in Column 5 and the size of the police force and general crime level in Column 6. The coefficient on broadband availability is significant and similar in magnitude across all specifications.

In Columns 7 and 8, we include additional covariates constructed from the interaction terms of our observables with time trend and time dummies as per Equations (2) and (3). The coefficients on broadband availability remain significant and similar in magnitude across these additional specifications. These results provide us with some confidence that the IV-estimates are not subject to biases arising from underlying time trends in racial hate crimes. In addition, all of our IV models are overidentified and the *p*-values of the overidentification statistics indicate that the exclusion restriction of our instruments cannot be rejected at conventional levels.

We note that the OLS fixed effects results in Column 2 differ in magnitude and significance from the IV results reported in Columns 3-8. The likely explanation for this difference is measurement error of the independent variable, which will exacerbate the attenuation bias arising from measurement error. We therefore perform a Durbin-Hausman-Wu test to better understand the role of measurement error. A comparison of the OLS estimates in Column 2 with the IV estimates in Column 6 under the test reveals a statistically significant difference between the two. This test result indicates that systematic measurement error is present and the model is biased under OLS estimation.[6]

4. Mechanisms

In this section, we investigate two possible mechanisms linking the Internet to racial hate crime. To this end, we adapt the approach in Bhuller et al. (2013) and decompose the overall effect of the Internet on racial hate crime into the separate effects of (1) the Internet on racial hate crime propensity and (2) the Internet on matching. This approach helps motivate a set of related questions that we address empirically.

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[6] This issue is well documented in labor and political economy research. For examples, see Krueger and Lindahl (2001), Miguel, Satyanath and Sergenti (2004) and Cotet and Tsui (2013).

4.1. The Internet and Racial Hate Crime Propensity

One channel through which the Internet may affect racial hate crime is by changing the propensity to engage in racial hate crime. Individuals with a high racial hate crime propensity are more likely to commit such crime than others with a low propensity. However, in principle, the Internet may be equally likely to raise or lower the propensity to engage in racial hate crimes. Other researchers have carefully described these two perspectives.

One perspective is that the internet raises the propensity to engage in extremist behavior. Mckenna and Bargh (1998) argued that norms in online groups exert a stronger than usual influence over members' behaviors, as group membership provides acceptance of their ideologies and enhances their self-esteem and worth. McKenna and Bargh (1998) find that people who initially communicate a taboo identity on the Internet are subsequently likely to both incorporate that identity into their sense of self and to display it in "real life". Even without actual participation in an online hate site, individuals who affiliate themselves to similar hate ideologies can still be motivated to carry out hate agendas proposed on the Internet on their own. For example, the post-September 11 terrorist acts carried out by the independent local groups did not take direct orders from Al Qaeda. These groups see the Al Qaeda as a source of inspiration to carry out their own independent terrorist acts (Sageman 2011). Other research on extremist groups suggests that far-right music is an effective recruiting tool (CNN 2012). While hate music cannot be bought in regular music stores, the availability of such music online (e.g., hate band websites, MySpace, last.fm, and other music-listening platforms) is suggestive of a positive relationship between hate crime propensity and prevalence of online hate activity.[7] Don Black, a white supremacist and member of various hate groups reported in a TV program that he was able to recruit people via the Internet whom he would have otherwise not reached. He further commented that his website,

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[7] For instance, the music of End Apathy – a neo-Nazi band can be easily found online. Wade Michael Page, a longtime member of the band went on a mass shooting at a Sikh temple in 2012, killing six people and wounding four others.

Stormfront, provides people who support similar ideologies with a forum to communicate to one another and form a virtual community (Anti Defamation League 2001). These perspectives suggest a positive relationship between hate crime propensity and the prevalence of online hate activity.

An alternative perspective emphasizes the potential cathartic effects of participating in online hate sites, arguing that the Internet provides an open rhetorical forum for racists to express their opinions and vent their frustrations in a non-physical manner (Glaser et al. 2002). Also, Adams and Roscigno (2005) suggest that certain hate groups such as the Ku Klux Klan (KKK) use online platforms to advocate for non-violent administrative and political legislation, instead of physical expressions of their racial biases. Finally, with the presence of hate sites on the Internet, hate activity becomes more transparent to the public, allowing for active monitoring by watchdog groups and the government (Glaser et al. 2002). Anti-hate groups such as the Anti Defamation League among others have worked to debunk untrue statements made on hate sites,[8] thereby stifling potential opportunities for physical crimes to arise due to extreme ideologies posted online.

We undertake several tests to investigate whether the Internet is likely to raise or lower the propensity to engage in racial hate crimes. In these tests, we attempt to understand the differential effect of Internet availability across areas that vary in terms of hate risk propensity. We use both offline and online measures of hate risk propensity. All else equal, we expect locations with high risk propensity to exhibit a stronger effect from Internet availability than locations with low risk propensity, should Internet availability increases the propensity to commit racial hate crimes.

Historical prejudices have led to the segregation of blacks from whites in residential areas. As such, scholars have argued that residential segregation serves as an indicator for the level of racism and risk propensity in a location (Cell 1982). A commonly-used indicator of segregation is

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[8] See, for instance, http://www.adl.org/combating-hate/cyber-safety/.

dissimilarity. Dissimilarity measures the evenness of the distribution of the two social groups across an area (Cutler et al. 1999). The dissimilarity index, ranging from zero to one, is used to indicate the share of the black (or white) population that would need to change areas for the races to be evenly distributed in a city (Duncan and Duncan 1955). Another common measure for segregation is *entropy* (Massey and Denton 1988). The entropy index ranges from zero to one, where zero represents the situation where all subareas have the same population composition as the entire area (maximum integration), and where one represents the situation where all subareas contain one group only (maximum segregation).[9] We perform a median split of the counties based on their scores of dissimilarity and entropy index, and rerun our regressions on the sub-samples.

In addition, we examine the differential effect of Internet availability across locations based on an online measurement of racial animus. Using the volume of Google search queries that include racially charged language, Stephens-Davidowitz (2012) develops a non-survey proxy for racial animosity of 200 media markets in the U.S.[10] By using search data, this online based measurement of racial animus is unlikely to suffer from social censoring biases as it is easier to express socially taboo thoughts when one is alone and online (Kreuter et al. 2008). We map the 200 media markets to counties in our dataset. We then split our sample based on the median index value and run separate regressions on the sub-samples.

Columns 1 and 2 of Table 3 examine the impact of broadband availability split by dissimilarity scores, Columns 3 and 4 examine its effect by entropy index and Columns 5 and 6 examine the impact by racially charged online searches. The coefficient on number of broadband providers is positive but not significant in Column 1 (i.e., low dissimilarity), and is positive and significant in Column 2 (i.e., high dissimilarity). A similar result can be seen in Columns 3 and 4,

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[9] Gentzkow and Shapiro (2011) use similar indices of segregation to construct measures of ideological segregation.

[10] Stephens-Davidowitz (2012) creates the index by averaging the counts of searches for racial epithets in each market from 2004-2007. He notes that the racial epithet is typically preceded or followed by search terms including “hate” or “joke(s)”.

where the low measure of entropy index holds a non-significant coefficient, and the high measure of entropy index has a positive and significant effect. That is, we use two common measures of residential segregation, and find that our main results are largely driven by areas with a high degree of segregation. Next, comparing Columns 5 and 6, we see that locations with more racially charged online searches experience a positive and significant effect from broadband availability, while areas with less racially charged online searches do not. In sum, it appears that the relationship between Internet availability and racial hate crimes is due, at least in part, to a heightening of racial hate crime propensity.

4.2. The Effect of Internet on Matching

The Internet can also affect the prevalence of racial hate crimes via an indirect mechanism that involves increasing the number of matches between perpetrators and victims. Literature on online platforms shows that the Internet reduces search frictions, enabling more matches to form in the labor market (Autor 2001), the marriage market (Hitsch et al. 2010), the e-commerce market (Bakos 1997), and even the casual sex market (Chan and Ghose 2012).

There may be several ways in which the Internet can increase the number of matches between potential hate perpetrators and victims. First, the Internet expands the choice set of victims by facilitating the matches that were previously unavailable through a lack of connection in traditional social networks. The process of locating a target can now be easily achieved through social media pages and networking sites. Second, the Internet is able to reduce information constraints between the hate perpetrators. For instance, an anti-abortion group created a website which included the names, photographs, addresses, and names of spouses and children of doctors who allegedly performed abortion. The site called for its supporters to bring justice, and marked the names of doctors who were wounded by protesters or killed by anti-abortionists (Kaplan and

Moss 2003).[11] Third, the Internet provides an additional channel for racial hate crime to be committed via online targeting. For example, in one well-known case a college student sent derogatory email messages and threats to hundreds of Hispanic faculty members, students, and persons employed at various institutions.[12] Finally, the anonymous nature of the Internet allows a perpetrator to befriend and solicit information from potential victims under a fake identity, thereby gaining access to a larger pool of individuals.

While the Internet may allow perpetrators to more effectively reach potential victims *online*, it is not clear this will translate to more *offline* matches. Given widely publicized cases of cyber-stalking and the well-known risks of meeting online strangers, Internet users exercise care in divulging personal information online and are cautious in meeting online strangers alone for the first time. Furthermore, there may be a substitution effect between time spent online and time available for other activities. If more time is spent online in hate sites and social networks, there will be less time available for direct interaction with victims in real life, resulting in fewer matches. Thus, the relationship between the Internet and matching is ambiguous.

To examine whether the matching effect plays an important role in driving the observed relationship between broadband availability and racial hate crimes, we consider the effect of broadband availability on crimes other than hate crimes. This check will reveal the indirect matching effect of the Internet, under the assumption that latent risk factor for these alternative crimes are not affected by the Internet.

We focus on robbery, murder, and burglary as alternative crimes to investigate for three reasons. First, according to the FBI, these crimes do not represent the usual manifestations of racial hate crimes. Crimes that are associated with racial hate crimes will make it difficult to separate out any matching effects from the increased latent risk effect facilitated by Internet

[11] See www.cbsnews.com/2100-201_162-20910.html.
[12] See United States v. Kingman Quon (1999) Available at *www.adl.org/civil_rights/newcyber.pdf*.

availability. To formally check if these crimes are linked to racial hate crime, we separately run a regression of the number of robbery, murder and burglary on the number of racial hate crimes, controlling for county and year fixed effects and the same set of covariates as before. Results in Columns 1 to 3 of Table 4 show that racial hate crimes are not associated with burglary, murder and robbery. This result agrees with the FBI's annual reports on hate crime, which show that the proportions of hate crime executed via these crimes are extremely low.[13] Second, the latent risk factors for robbery, murder and burglary are not known to bear any relationships with Internet availability, unlike other crimes like rape and other sexual offenses (e.g., Bhuller et al 2013). Finally, perpetrators of robbery, murder, and burglary would likely need to perform some form of planning before committing the criminal act. The planning of these crimes may involve effects of matching facilitated from Internet availability.

To test for any matching effects, we perform a regression of these crimes on the number of broadband providers, separately, under the IV framework. Columns 4 to 6 in Table 4 report the results from the second stage of the IV regression. The coefficients on number of broadband providers are not significant in any model, suggesting that broadband availability has no effect on robberies, murders and burglaries. These results suggest that the link between the Internet and racial hate crime is not due to the matching effect from the Internet. Instead, it appears that the link between the Internet and racial hate crime is primarily due to heightened propensities to engage in hate crime.

5. Alternative Explanations

In this section we address alternative explanations that may account for our results. We accomplish this in several steps. First, in order to rule out misspecification, we run multiple alternative specifications, including a set of results using a different measure of broadband

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[13]According to FBI statistics, most of the hate crimes manifest through simple assault, intimidation, and vandalism/property damage, with very few (if not none) committed via robberies and vagrancy. Less than 2% of hate crimes fall under robberies, murder, or burglary.

availability and a different instrument. Our results are robust to these alternative specifications. Second, we discuss the possible effect of the Internet on the reporting of crimes, and conduct two tests to rule out reporting effects as an alternative explanation. Finally, we conduct a falsification test, which further corroborates the causal direction of our main results.

5.1. Alternative Specifications

We first examine the robustness of the baseline results to a categorical specification of broadband availability. Four different specifications of broadband provider count are tested: (1) the first bin denotes counties with 0-2 providers, the second represents that with 2-4 providers, the third indicates that with 4-6 providers, and the fourth represents that with more than six providers; (2) the first three bins are similar to the previous specification, with the fourth bin representing 6-8 providers and the last bin denoting more than 8 providers; (3) and (4) are largely similar to specifications (1) and (2) respectively, except the first bin denotes 0-3 providers and the second bin represents 3-4 providers. In all cases, we make the first bin the base group, so that we can interpret the resultant effect on racial hate crime with respect to increasing numbers of broadband providers.

Across various specifications in Table 5, we observe positive coefficients for all bins, and significance in many cases, especially for the higher bin values. These results offer further evidence of the positive impact broadband availability on racial hate crime. Furthermore, we note that the coefficient sizes tend to increase with the number of providers, but experience a diminishing effect as the number of providers extends beyond eight. This finding corresponds well with the results in Kolko (2010b) which shows a monotonic and non-linear relationship between the number of broadband providers and the extent of broadband availability.

In a second robustness check, we analyze the relationship between racial hate crime and broadband availability using a cross-sectional model that specifies our variables in a first-difference format. This cross-sectional specification explicitly models for how the change in racial hate crimes is related to the growth in broadband availability as follows:

(4) $ln(\Delta\, racial\ hate\ crime_i) = \alpha + \beta \ln(\Delta\, BBProvider_i) +\ \lambda\, X_i + \varepsilon_i,$

where Δ *racial hate crime$_i$* represents the change in racial hate crimes in county *i* between 1999 and 2008, $\Delta BBProvider_i$ denotes the change in broadband providers in county *i* during the same period and X_i is a vector of controls at the baseline year. In Columns 1 and 2 of Table 6, we report the results of the first differenced OLS analyses. As in Column 2 of Table 2, these results show no significant relationship between broadband availability and racial hate crime, potentially due to attenuation bias arising from measurement error. In Column 3 and 4 we turn to IV regressions. In the first stage, the slope coefficient holds a negative and significant relationship with the change in broadband provider count between 1999 and 2008. The values of the F-statistics of both specifications are beyond the threshold levels, supporting earlier evidence that our instrument does not suffer from weak instrument biases. Looking at the second stage IV regression results in the Table 6, we see that the change in broadband providers has a positive and significant relationship with the change in racial hate crime.

Third, we check the robustness of the main results to the removal of outlier observations. The prevalence of hate crimes may reach abnormally high or low levels in periods experiencing external shocks. For instance, the rate of racial hate crimes across the study period experiences a sharp peak and valley in the years 2001 and 2002, respectively. We suspect that shifts in hate crime rates are induced by the terror-related attacks on September 11, 2001. Anger, frustration and fear after the terror event can result in indiscriminate attacks on innocent individuals who appear to be of Muslim, Middle Eastern, or South Asian descent (Kaplan and Moss 2003). Following a sharp increase in such crimes, local enforcement task force may step up efforts to curb hate crimes, bringing about the decline in the adjacent year. Thus, in Table 7, we rerun the baseline regression models after removing observations from 2001 and 2002. The results are consistent with those reported in Table 2.

In a fourth robustness check, we work with state level data on the number of high speed Internet lines as a regressor in place of broadband providers.[14] In estimating the models involving this regressor, we adjusted all other variables to the state level. We note that slope values that are averaged across larger geographic areas become less representative of the local terrain features, thereby losing the desirable characteristics of capturing the cost of extending broadband service in the area. To alleviate this issue, we use the number of local connections to the ARPANET as an instrument in place of slope. The ARPANET is a wide area data communication network that was a predecessor of the Internet. The increase in number of connections to the ARPANET captures the variance in data communication infrastructure and human expertise with networking technologies. Availability of technical infrastructure and human expertise allow Internet providers to enjoy higher benefits and lower costs in extending Internet connections in those locations. Positive correlations between access to Internet and ARPANET connections are observed in the first stage IV regressions in previous studies involving Internet use (Forman et al. 2012, Forman and van Zeebroeck 2013). Furthermore, ARPANET connections represent historical decisions in the 1970s about connectivity to U.S. Department of Defense and university networks, thus the number of connections is unlikely to be correlated with the current prevalence of racial hate crimes, fulfilling the exclusion restriction needed for instrument validity.

In Columns 1-2 of Table 8, we report the OLS results, which show a positive but not significant relationship between broadband providers and racial hate crime. In Columns 3-6, we report results from IV analyses. In Columns 3 and 5, we note that the number of ARPANET connections has positive and significant correlations with the number of Internet high speeds lines for various years but the value of its F-statistics is below the threshold values. The values of the F-

[14] As noted above, this data also comes from the FCC's Form 477.

statistics reach an acceptable level under the limited information maximum likelihood (LIML) specification, suggesting that the estimates derived under the LIML model will be more appropriate. Similar to the baseline analysis, the instrumental variable regressions are overidentified and the *p*-values of overidentification range from 0.118 to 0.516, implying that the number of ARPANET connections satisfies the exclusion restriction. Across all four models, the number of high speed lines holds positive and significant coefficients. In sum, the set of regressions reported in Tables 5-8 provide evidence that the main results are robust to alternative specifications.

5.2. Reporting Effects

We next rule out that heterogeneity in reporting of crimes is influencing our results. If reporting does not differ systematically with broadband availability, the interpretation of our IV results would not be affected. However, it may be possible that broadband availability may increase reporting of crimes (or reduce underreporting of certain crimes), as the Internet presents an alternative way for victims and witnesses to report crimes. If broadband availability systematically affects the level of reporting, our IV estimation will overestimate the true impact of Internet on increasing the incidence of racial hate crimes. To test for this possibility, we run a set of checks using the National Crime Victimization Survey (NCVS). The NCVS is the largest ongoing survey of a nationally representative sample of residents conducted bi-annually to understand the characteristics of criminal victimization and incidence of crimes not reported to law enforcement.

For our first check, we use NCVS data from 1979 to 2004 and tabulate the proportion of crimes that are reported. We then check whether the reporting trends differ between the pre- and post-Internet periods. Given that the first commercial ISP emerged in 1990,[15] we define the years prior to 1990 to be pre-Internet and the years after 1990 to be post-Internet. A set of demographic and crime related covariates are included in the regression along with location fixed effects. We

[15] The first ISP, The World, started in 1990. See http://www.zakon.org/robert/internet/timeline/.

then compare the trend coefficients across the two periods. For robustness, we also conduct the test using alternate dates for the start of commercial Internet. As reported in Table 9, there is an increase in crime reported over time for both the pre- and post-Internet periods. Chi-square tests indicate that the difference in coefficients from the two periods are not statistically significant, suggesting that the increase in crime reporting in the post-Internet period does not differ from that in the pre-Internet period. Though this check provides some assurance that Internet availability does not affect crime reporting patterns, this test does not directly rule out a potential relationship between broadband availability and crime reporting trends, which we next address.

We next regress the percent of crimes reported on broadband availability to understand whether a systematic relationship is present between the two factors. We include a full set of demographic and crime related factors, and use location fixed effects. We control for time effects in two ways: 1) a linear time trend and 2) year dummies (i.e., year fixed effects). Table 10 shows the estimates of regression analysis. Throughout all specifications, the coefficients on broadband providers are not statistically significant, suggesting that broadband availability does not change crime reporting trends. These two tests provide evidence suggesting that the positive relationship between Internet availability and racial hate crime is not driven by an increase in crime reporting due to greater broadband availability.

5.3. Falsification Tests

Finally, we conduct two falsification tests to check whether the observed relationship is spurious. In the first test, we replace the current racial hate crime incidence with that from 1992 to 1998, while maintaining the broadband availability from the period 1999-2006. Internet access in the pre-broadband period in 1992-1998 is largely facilitated by dial-up technology. Hitt and Tambe (2007) find that quantity and diversity of information consumed in the pre-broadband adoption is lesser than that consumed after broadband adoption. With lower accessibility to online hate content in the pre-broadband period, we expect the coefficient on broadband availability to be non-significant. Significant estimates in this falsification test would suggest that the instrument variable is correlated with underlying location-specific trends in racial hate crimes. Table 11 reports the result of this falsification test and compares it to the coefficients from the baseline IV specification (see Columns 1 and 2). It is reassuring to find no evidence of a significant correlation between the broadband availability in 1999-2006 and the pre-broadband racial hate crime trends in 1992-1998. The second falsification test checks whether next year's broadband availability affects current racial hate crimes. A significant coefficient in this check would suggest that there is some omitted variable that drives the trends in racial hate crimes and broadband expansion simultaneously. We report the result of this test in Column 3 of Table 11. It is reassuring to find that the next year's broadband availability does not affect the current levels of racial hate crimes.

6. Conclusion

In this paper, we study the link between the Internet and racial hate crime in the U.S. We use slope of terrain as an instrument for the number of broadband providers to address measurement error. We provide evidence consistent with the idea that broadband availability leads to an increase in racial hate crime. In addition, we find that the effect of Internet is stronger in locations with greater segregation and more racially charged online searches. Thus, our analyses suggest that the increase in racial hate crime is induced by a positive effect of broadband availability on the risk propensity to commit racial hate crimes. Finally, we rule out a number of alternative explanations.

Our study has implications for the policy debate over content regulation on the Internet. Recently, Facebook came under criticism for not doing enough to restrict hate speech on its site.[16] Our results suggest that efforts by popular websites such as Facebook to regulate user-generated content on their sites will help reduce hate crimes. More broadly, while the First Amendment protection for online hate activity remains as a high bar to hurdle, steps have been taken over the

[16] Doug Gross, "Under Pressure, Facebook Targets Sexist Hate Speech," CNN. Available http://www.cnn.com/2013/05/29/tech/social-media/facebook-hate-speech-women/index.html. Accessed June 30, 2013.

years to abate potential abuse of the Internet as a tool for propagating hate agendas. In an effort to address the preponderance of online hate sites, the U.S. Congress passed the Communication Decency Act in 1996 and the Children Internet Protection Act in 2001. Progress is also made in rulings against the First Amendment protection of online hate content. For instance in late 2001, the Court of Appeals for the Ninth Circuit held that the anti-abortion website which shows the names and information of abortion doctors constitute a true threat of force and will therefore be not protected by the First Amendment. The jury had since ordered the site owners and operators to pay the listed doctors and abortion clinics more than $100 million in damages.

While the results of this study may argue in favor of stricter censorship and regulation of hate activity on the Internet, care needs to be exercised in order for effective policy outcomes to result. Though online hate sites may perpetrate more hate crimes, the same resource can be reconfigured as a forum for open discussion and exchange, which may help address the root problem of racial biases and misconceptions. On the other hand, enforcing censorship rules on the Internet may lead to public outcry over the denial of free expression, and may also cause hate activity to migrate into underground channels that are hard to monitor. Thus, policy makers and government agencies need to carefully weigh the various consequences of the measures for combating online hate content.

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TABLE 1: DESCRIPTIVE STATISTICS (N = 9532)

Variable	Mean	Std. Dev	Min	Max	Source
Log (Racial Hate Crimes)	0.443	0.783	0	4.779	FBI
Log (BB Providers)	1.629	0.449	0.693	3.069	FCC
Log (Slope)	0.875	0.994	0	3.539	ArcGIS
Log (Migration)	3.457	2.086	0	9.681	US Census
African American to White Ratio	0.161	0.363	0.001	5.632	US Census
Proportion Above Age 60	0.197	0.044	0.076	0.526	US Census
Log (No. in Poverty)	8.706	1.174	5.318	12.917	US Census
Log (No. Police Employees)	4.629	1.367	0	10.948	FBI
Log (No. Crimes)	5.154	2.059	0	9.956	FBI

TABLE 2: BASELINE SPECIFICATION

	Baseline Specification						Covariate Interactions	
	OLS	OLS	IV Est.	IV Est.	IV Est.	IV Est.	Time Trend	Time FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st stage DV: Log(BB Providers)								
Log (Slope)*Year=2000			-0.003	-0.001	-0.001	-0.001	-0.000	0.002
			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Log (Slope)*Year=2001			-0.030***	-0.028***	-0.028***	-0.028***	-0.025***	-0.010**
			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Log (Slope)*Year=2002			-0.047***	-0.044***	-0.041***	-0.041***	-0.038***	-0.030***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log (Slope)*Year=2003			-0.048***	-0.045***	-0.041***	-0.041***	-0.036***	-0.028***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log (Slope)*Year=2004			-0.043***	-0.039***	-0.031***	-0.032***	-0.026***	-0.021***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log (Slope)*Year=2005			-0.082***	-0.078***	-0.074***	-0.075***	-0.066***	-0.056***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log (Slope)*Year=2006			-0.072***	-0.068***	-0.062***	-0.062***	-0.052***	-0.042***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log (Slope)*Year=2007			-0.073***	-0.068***	-0.061***	-0.061***	-0.051***	-0.047***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
2nd stage DV: Log(Racial Hate Crime)								
Log (No. BB Providers)	0.094***	0.002	0.308**	0.405***	0.436***	0.432***	0.485**	0.479**
	(0.02)	(0.03)	(0.14)	(0.15)	(0.16)	(0.16)	(0.21)	(0.24)
Demographic controls	✓	✓		✓	✓	✓	✓	✓
Economic controls	✓	✓			✓	✓	✓	✓
Crime-related controls	✓	✓				✓	✓	✓
County & Year F.E.		✓	✓	✓	✓	✓	✓	✓
Observations	9523	9523	9523	9523	9523	9523	9523	9523
F-statistics (1st stage)			28.422	25.574	21.481	21.530	16.031	14.141
Stock and Yogo (2005) CV			11.39/18.54	11.39/18.54	11.39/18.54	11.39/18.54	11.39/18.54	11.39/18.55
J-statistics			7.800	7.982	7.847	7.880	7.746	10.885
Over-ID test (*p*-value)			0.351	0.334	0.346	0.343	0.356	0.144

Notes. Model 1 is a pooled OLS regression without fixed effects. Regressions in Models 2-8 include county and year dummies. Stock and Yogo (2005) critical values are reported for relative bias > 10% and maximal instrumental variable size > 15%, respectively. Robust standard errors, clustered on county, are in parentheses. The first year in the first stage regression is 1999, and is omitted in the interactions with slope as the reference group. All regressors are lagged by one period to avoid simultaneity biases. By using lagged regressors, the last year interacted with our instrument is 2007. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 3: EFFECT IN LOCATIONS WITH VARYING SEGREGATION

	Dissimilarity		Entropy Index		Online Search	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)
2nd stage DV: Log(Racial Hate Crimes)						
Log (BB Providers)	0.188	0.734**	0.214	0.809**	-1.067	0.351***
	(0.36)	(0.30)	(0.34)	(0.34)	(1.55)	(0.11)
Controls added	✓	✓	✓	✓	✓	✓

Notes. All regressions include constant term, county and year dummies. Robust standard errors, clustered on county, are in parentheses. All regressors are lagged by one period to avoid simultaneity biases. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 4: EFFECT ON ALTERNATIVE CRIMES

	OLS Estimation			IV Estimation		
	Robbery	Murder	Burglary	Robbery	Murder	Burglary
	(1)	(2)	(3)	(4)	(5)	(6)
Log (Racial Hate Crimes)	0.006	0.002	-0.002	-	-	-
	(0.01)	(0.01)	(0.01)			
Log (BB Providers)	-	-	-	-0.042	0.057	-0.002
				(0.20)	(0.19)	(0.21)
Controls Added	✓	✓	✓	✓	✓	✓

Notes. The dependent variable for each column is the log count of the crime stated at the top of the column. All regressions include county and year dummies. Second stage results are shown for Columns 4, 5, and 6. Robust standard errors, clustered on county, are in parentheses. All regressors are lagged by one period to avoid simultaneity biases. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 5: CATEGORICAL SPECIFICATION OF REGRESSOR

	(1)	(2)	(3)	(4)
2nd stage DV: Log (Racial Hate Crimes)				
Bin II	0.739*	0.760*	0.527	0.534
	(0.39)	(0.39)	(0.37)	(0.38)
Bin III	0.713**	0.783*	0.236	0.222
	(0.36)	(0.40)	(0.19)	(0.23)
Bin IV	1.274**	1.208**	0.686**	0.715*
	(0.51)	(0.54)	(0.29)	(0.39)
Bin V		1.358**		0.673**
		(0.56)		(0.32)
Controls added	✓	✓	✓	✓
Observations	9523	9523	9523	9523
J-statistics	2.548	2.384	5.62	5.539
Overidentification test (p-value)	0.769	0.666	0.345	0.236

Notes. Model 1: Bin I (0-2 Providers), Bin II (2-4 Providers), Bin III (4-6 Providers), Bin IV (> 6 Providers). Model 2: Bin I (0-2 Providers), Bin II (2-4 Providers), Bin III (4-6 Providers), Bin IV (6-8 Providers), Bin V (> 8 Providers). Model 3: Bin I (0-3 Providers), Bin II (3-4 Providers), Bin III (4-6 Providers), Bin IV (> 6 Providers). Model 4: Bin I (0-3 Providers), Bin II (3-4 Providers), Bin III (4-6 Providers), Bin IV (6-8 Providers), Bin V (> 8 Providers). All regressions include county and year fixed effects. Robust standard errors, clustered on county, are in parentheses. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 6: CROSS-SECTIONAL ANALYSIS

	OLS (1)	OLS (2)	IV Estimation (3)	IV Estimation (4)
1st stage DV: Log (Δ BB Providers 1999-2008)				
Log (Slope)			-0.117***	-0.081***
			(0.01)	(0.01)
2nd stage DV: Δ Racial Hate Crimes 1999-2008				
Log (Δ No. ISPs between '08 - '99)	0.053	-0.016	1.694*	3.249*
	(0.53)	(0.34)	(0.90)	(1.68)
Controls Added		✓		✓
Observations	1033	1033	1033	1033
F-statistics (1^{st} stage)	-	-	118.902	66.863
Stock and Yogo (2005) CV	-	-	./8.96	./8.96

Notes. See Table 2 for descriptions of the Stock and Yogo (2005) critical values. Missing Stock and Yogo (2005) critical values mean they have not been computed or do not apply. Robust standard errors, clustered on county, are in parentheses. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 7: OUTLIER TEST

	(1)	(2)
1st stage DV: Log(BB providers)		
Log (Slope)*Year=2000	-0.003	-0.000
	(0.00)	(0.00)
Log (Slope)*Year=2003	-0.048***	-0.040***
	(0.01)	(0.01)
Log (Slope)*Year=2004	-0.043***	-0.030***
	(0.01)	(0.01)
Log (Slope)*Year=2005	-0.082***	-0.074***
	(0.01)	(0.01)
Log (Slope)*Year=2006	-0.073***	-0.060***
	(0.01)	(0.01)
Log (Slope)*Year=2007	-0.074***	-0.060***
	(0.01)	(0.01)
Second stage DV: Log(Racial Hate Crimes)		
Log (No. BB Providers)	0.406***	0.533***
	(0.15)	(0.17)
Controls Added		✓
Observations	8729	8729
F-statistics (First stage)	30.426	26.287
Stock and Yogo (2005) CV	11.12/16.23	11.12/16.23
J-statistics	1.443	0.485
Overidentification test (p-value)	0.950	0.992

Notes. See Table 2 for descriptions of the Stock and Yogo (2005) critical values. Missing Stock and Yogo (2005) critical values mean they have not been computed or do not apply. Robust standard errors, clustered on county, are in parentheses. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 8: STATE LEVEL INSTRUMENTAL VARIABLE ANALYSIS

	OLS estimates	OLS estimates	TSLS estimates	LIML estimates	TSLS estimates	LIML estimates
	(1)	(2)	(3)	(4)	(5)	(6)
1^{st} stage DV: Log(High Speed Lines)						
Log (ARPANET)*Year = 2000			0.150*	0.150*	0.145	0.145
			(0.08)	(0.08)	(0.09)	(0.09)
Log (ARPANET)*Year = 2001			0.281***	0.281***	0.275***	0.275***
			(0.08)	(0.08)	(0.07)	(0.07)
Log (ARPANET)*Year = 2002			0.137**	0.137**	0.131**	0.131**
			(0.06)	(0.06)	(0.06)	(0.06)
Log (ARPANET)*Year = 2003			0.088*	0.088*	0.098*	0.098*
			(0.05)	(0.05)	(0.06)	(0.06)
Log (ARPANET)*Year = 2004			0.068	0.068	0.100**	0.100**
			(0.04)	(0.04)	(0.05)	(0.05)
Log (ARPANET)*Year = 2005			0.027	0.027	0.022	0.022
			(0.04)	(0.04)	(0.05)	(0.05)
Log (ARPANET)*Year = 2006			0.019	0.019	0.009	0.009
			(0.05)	(0.05)	(0.05)	(0.05)
Log (ARPANET)*Year = 2007			0.006	0.006	-0.007	-0.007
			(0.05)	(0.05)	(0.05)	(0.05)
2^{nd} stage DV: Log(Racial Hate Crime)						
Log (High Speed Lines)	0.020	0.075	1.294***	1.697**	1.498***	2.020***
	(0.04)	(0.20)	(0.44)	(0.69)	(0.44)	(0.73)
Controls Added	✓	✓			✓	✓
State & Year F.E.		✓	✓	✓	✓	✓
Observations	422	422	422	422	422	422
F-statistics (1^{st} stage)			3.061	3.061	3.515	3.515
Stock and Yogo (2005) CV			11.39/18.54	./3.04	11.39/18.54	./3.04
J-statistics			11.518	9.697	9.330	6.208
Over-ID test (*p*-value)			0.118	0.206	0.230	0.516

Notes. Model 1 is a pooled OLS regression without state and year fixed effects. Regressions in Model 2-6 include state and year dummies. See Table 2 for description on the Stock and Yogo (2005) critical values. The first year in the analysis is 1999 and is omitted in the interactions with ARPANet nodes as the reference group. Robust standard errors, clustered on state, are in parentheses. All regressors are lagged by one period to avoid simultaneity biases. By using lagged regressors, the last year interacted with our instrument is 2007. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

TABLE 9: REPORTING LEVELS BEFORE AND AFTER INTERNET EXPANSION

	Pre-1990	Post-1990	Pre-1991	Post-1991	Pre-1992	Post-1992
	(1)	(2)	(3)	(4)	(5)	(6)
Time trend	0.004***	0.003***	0.003**	0.003***	0.002**	0.004***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Controls added	✓	✓	✓	✓	✓	✓
χ^2 test *p*-value	0.224		0.807		0.424	

Notes. The dependent variable is the proportion of crimes reported. All regressions include a constant and MSA fixed effects. Robust standard errors, clustered by MSA, are reported in parentheses. The difference in coefficients between the pre and post periods for all specifications is not statistically significant.

TABLE 10: EFFECT OF BROADBAND ON REPORTING LEVELS

	Time Trend	Time FE	Time Trend	Time FE
	(1)	(2)	(3)	(4)
Log(BB Providers)	0.070	0.050	0.034	-0.055
	(0.05)	(0.06)	(0.06)	(0.09)
Controls added			✓	✓

Notes. The dependent variable is proportion of crimes reported. All regressions include a constant and MSA fixed effects. Robust standard errors, clustered by MSA, are reported in parentheses.

TABLE 11: FALSIFICATION TESTS

	Baseline	Pre-broadband outcomes	Next Year Broadband Avail.
2nd Stage Regression	(1)	(2)	(3)
Log (BB Providers)	0.432***	0.226	0.101
	(0.16)	(0.15)	(0.13)
Controls Added	✓	✓	✓

Notes. The dependent variable for Column 2 is the log count of the racial hate crime from 1992 to 1998. The broadband regressor for Column 3 is the next year's broadband availability. All regressions include county and year dummies. Second stage results are shown for Columns 4, 5, and 6. Robust standard errors, clustered on county, are in parentheses. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.